Better at Home than in Prison ? The Effects of Electronic Monitoring on Recidivism in France

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WP 1603 – January 2016

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Keywords:
economics of crime, prison, electronic monitoring, recidivism

JEL codes:
K42

Working Paper

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Abstract

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We would like to thank members of the French Direction of Prison Administration, INED and Aurélie Ouss for their work on the database. We also thank Nathalie Havet, Xavier Joutard, Joseph Montag, Jean-Yves Lesueur and Thomas Vendryes for their careful reading of the paper, as well as participants at various conferences and seminars for useful comments: EALE (Wien), 2nd Law and Economic Policy International Workshop (Paris Ouest Nanterre), AFSE-Trésor (Paris), GATE (Lyon), CES (Cachan).

Introduction

Many countries are slowly turning away from mass incarceration in favor of new forms of punishment. In the United States for example, after three decades of steady growth, the total prison population has been declining for five consecutive years since the peak of 2008 (Glaze and Kaeble, 2014). While the U.S. remain by far the largest prison system with 2.2 million inmates, a similar downward trend is currently observed among members of the Council of Europe, with their total prison population now under 1.7 million (Aebi and Chopin, 2014).

This slow decline in prison population is partly explained by budgetary and capacity constraints, but also by rising concerns about the effectiveness of incarceration: in the five years following release, 77% of ex-prisoners are re-arrested in the U.S. (Durose et al., 2014), while 59% are re-convicted in France (Kensey and Benaouda, 2011). This context led to the development of alternative penal sanctions which either avoid incarceration (front-door strategies) or hasten release from prison (back-door strategies). Among them, electronic monitoring (EM) is often considered as the most promising: this technology provides live surveillance of offenders[1], and therefore *some* incapacitation and deterrence, for a tenfold lower cost of operation (about $10 per day under EM compared to $100 in prison[2]). Electronic monitoring is now available in many countries, and its use is growing fast. For instance, among the 5 million offenders who are supervised in the community in the U.S. (Glaze and Kaeble, 2014), it is estimated that roughly 20 percent involve electronic surveillance (Gable and Gable, 2005). In England and Wales, 90,000 cases involved EM in 2012 (National Audit Office, 2013). In France, electronic surveillance concerns more than 20,000 offenders every year, compared to an annual inflow of 70,000 prisoners (DAP, 2015).

However, in comparison to the increasing use of EM worldwide, there is surprisingly little causal evidence on the effects of electronic monitoring in terms of recidivism. Most existing studies use observational data to estimate how recidivism rates differ between groups of ex-prisoners and EM offenders, controlling for a small set of observable characteristics[3]. Unfortunately, these estimates are likely plagued by selection bias because judges typically try to allocate electronic monitoring to the "best" offenders (those with

[1] EM offenders are located live through the electronic device attached to their ankle. Depending on the technology used, one can either track the exact location of tagged offenders (GPS tracking) or simply make sure they are present in a designated place, usually their home (Radio Frequency tracking)

[2] These U.S. estimates are from Roman et al. (2012) and Kyckelhahn (2011). Similar figures apply in France, with a daily cost of 10€ under EM and about 100€ in prison (DAP, 2013)

[3] In France for example, such methods yield a gap of about 20 percentage points in favor of EM offenders after five years (Benaouda et al., 2010)

good reentry prospects and low intrinsic risk of recidivism). In France for example, the decision to incarcerate or to grant EM to convicted offenders is highly discretionary and involves a preliminary social investigation and an hearing of eligible offenders: it is therefore very likely that judges' decisions rely not only on measured characteristics (like age or offense type), but also on a wide range of unobservable dimensions such as motivation, reentry prospects, family support, that may themselves explain recidivism. Only two existing papers deal with this selection bias convincingly, using quasi-experimental designs where similar offenders face dissimilar punishments (Di Tella and Schargrodsky (2013) in Argentina, and Marie (2015) in England and Wales). Their findings -large beneficial effects of EM compared to prison- need to be confirmed in other settings with different prison conditions, different selection of offenders, and different type of supervision under EM.

The main contribution of this paper is therefore to estimate the effect of electronic monitoring instead of incarceration on future criminal activity in a European country. France provides a good case-study for the analysis of EM as it was among the early-adopters of electronic surveillance in Europe, and is now a massive user. Moreover, the sequential introduction of EM in France represents a natural experiment: electronic surveillance was first experimented in four pilot courts in early 2000s, and later became available in more and more courts. We discuss this gradual roll-in in detail and provide evidence that endogenous selection of courts into EM is unlikely. We argue that the incremental implementation of EM generated sharp, exogenous discrepancies in eligibility to electronic monitoring between similar offenders, based on time and space. Finally, another motivation for studying France is that EM is a proper alternative to incarceration in the Penal Code: judges can fully convert any short prison sentence into electronic monitoring *before incarceration*. Therefore, all EM recipients under study in this paper were convicted to a prison sentence, but ended up serving their whole sentence at home under electronic monitoring. All these features allow us to obtain quasi-experimental estimates of the ATT effect of serving time at home under EM instead of in prison.

Our results show that simple comparisons highly overestimate the crime-preventing effect of electronic monitoring. The inclusion of a rich set of covariates reduces the gap in 5-year recidivism from 14-15 pp to 8-9 percentage points. When we additionnaly control for selection on unobservables using cross-court variation in access to EM as IV, the estimated beneficial effect of serving time at home under EM, instead of incarcerated, reduces to 6-7 percentage points. This effect remains significant statistically and economically, as it suggests a long-term reduction in recidivism by 9-11% thanks to EM

treatment. We also find that this beneficial effect is stronger on EM offenders who had prior prison convictions, received control visits at home from correctional officers while under EM, and were obliged to work. We also show that the estimated reduction in reoffending is not an artefact of short-term incapacitation at home but reveals more profound change (desistance from crime, with less recidivism and less serious new offenses), where both rehabilitation and deterrence play an active role.

The remaining of the paper is organized as follows. Section 1 discusses the potential effects of EM and prison in a simple model of recidivism, and reviews the best empirical evidence available. Section 2 presents the French institutional context and how EM was introduced in early 2000's. Section 3 shows the data and descriptive statistics. Section 4 presents our empirical strategy and provides support for the main identification hypothesis. Section 5 presents the main results and investigates the presence of qualitative effects (on offense type, severity) and heterogeneity in treatment effects (by type of offender and intensity of supervision). Section 6 discusses the mechanisms driving our results and their current validity in France. Section 7 concludes.

1 Theory and evidence

In line with the seminal model of Becker (1968), there is now compelling evidence that prison sentences prevent crime not only through incapacitation of criminals behind bars, but also by deterring potential offenders (Abrams, 2013). However, it is less clear whether the experience of incarceration also prevents recidivism among prisoners, or whether other forms of punishment might be more effective.

Theoretically, one may expect different types of effects. First, incarceration may have detrimental effects on offenders' future labor-market outcomes, due to human capital depletion inside prison and stigma after release. There is growing empirical evidence from many countries that such harmful effects occur, with incarceration leading to more fragile employment trajectories after release (Western et al. (2001), Alós et al. (2014)). Using sentencing disparities between randomly assigned judges in Illinois, Aizer and Doyle (2015) find that incarceration has large adverse effects on juveniles' future outcomes, with an estimated 13 pp reduction in high school completion and a 23 pp increase in adult incarceration[4]. In another recent paper on Texas, Mueller-Smith (2014) shows that, in addition to its adverse impact on future economic wellbeing (lower employ-

[4]Hjalmarsson (2009) provides contrasting evidence from a sentencing discontinuity in the juvenile justice system of Washington state, finding that adult incarceration reduces recidivism among juveniles at the margin.

ment and wages, higher take-up of food stamps), incarceration also disrupts family relationships with more divorce and less marriage. Conversely, alternative sanctions like electronic monitoring are far less disruptive in offenders' life-course (in terms of family, work, housing, etc.), as documented qualitatively by Hucklesby (2009).

Incarceration could also be more criminogenic than alternative sentences because of prison conditions themselves, which are often described as tough, degrading, and not rehabilitative. In Italy, Drago et al. (2009) show that recidivism, though uncorrelated with overcrowding or death rate at the prison level, is correlated with distance from the chief town of the province: post-release recidivism increases as prisons are located further away from the main cities, i.e. more isolated from families and communities. Also in Italy, Mastrobuoni and Terlizzese (2014) study the rehabilitative effect of a new "open prison" (near Milano), which offers far more freedom and activities to prisoners than the traditional Italian prisons: they find that serving more time in this open prison instead of a traditional one significantly reduces recidivism. This result is in line with a U.S. study by Chen and Shapiro (2007), where the authors exploit discontinuities in the assignment of federal prisoners to different security levels. They show that experiencing harsher prison conditions leads to more crime after release.

While stricter detention regimes may have criminogenic effects on their own, another explanation may be that prisoners who end up in high security facilities are exposed to particularly hardened criminals who exert a bad influence on others. According to this "school of crime" hypothesis, prison facilities allow criminals to learn from each other, build new networks and find new opportunities. This accumulation of criminal capital eventually converts into greater returns to crime after release, and therefore more recidivism. Recent empirical research from several countries confirms the existence of such criminogenic interactions between inmates. Studying recidivism among juvenile prisoners in Florida, Bayer et al. (2009) find that the probability to reoffend in a particular offense type is strongly related to own and peers' prior criminal experience in that offense. Similar findings are obtained for drug crime in France (Ouss, 2011) and Denmark (Damm and Gorinas, 2013), especially among cellmates of similar age. Such criminal peer effects are far less likely to occur among offenders sentenced in the community, and even more so under electronic monitoring as it usually entails home confinement for long hours, and therefore reduces potential interactions with other criminals.

Psychological factors may also apply, as documented by the large experimental and qualitative research on reciprocity, legitimacy of law, and compliance. In her interviews with EM offenders in England, Hucklesby (2009) finds that many offenders under

monitoring comply with their curfew order because of reciprocity: they are aware that the worse (incarceration) has been avoided, and that EM placement represents a second chance for them. The "gift/counter-gift" principle of Mauss (1924) could therefore explain the relative effectiveness of alternative sentences.

In sharp contrast with these arguments, specific deterrence theory suggests that the personal experience of severe punishment (in the form of incarceration for example) makes the threat of future sentences more salient, costly, and therefore deters future crime. Conversely, offenders who obtained a more lenient sentence, such as electronic monitoring, may no longer fear future punishment and eventually commit more crime. Early evidence of such an effect is found in a randomized experiment among prisoners in California in the early 1970s (Berecochea and Jaman, 1981): in this experiment, a random group of prisoners obtained 6-month early release under parole, while the control group had to serve their prison sentence normally (three years of prison on average). The results showed a somewhat larger rate of recidivism among the early-releasees than in the control group. Similarly, in the state of Georgia, Kuziemko (2013) exploits both a mass release and discontinuities in sentencing guidelines, and finds that longer incarceration actually leads to a significant reduction in recidivism. In Sweden, a country with very short average prison stays (1-2 months), Landersø (2015) also finds that exogenous increases in prison time actually promote employment after release, an effect which appears to be driven by increased rehabilitation and better preparation for release.

1.1 A simple model of recidivism

The overall picture from existing research may look puzzling, as several competing arguments about incarceration seem to all have solid empirical groundings. In order to clearly understand how all these potential effects interact and eventually affect future criminal activity, let's consider a simple model of recidivism. An offender commits a new crime after having completed a sentence of perceived severity s (with $s^{EM} < s^{Prison}$) if:

$$U(s) < (1-p) * B(s) - p * C(s) \tag{1}$$

where U is the utility derived from a law-abiding life, B is the return to a new crime (in utility), C is the utility cost of future punishment, and p is the probability of future punishment. Building on prior empirical research, we expect $U'(s)$ to be negative since experiencing harsher punishment (in the form of incarceration instead of EM) presum-

ably reduces employment prospects, family ties, and more broadly attachment to society and the rule of law. We also hypothesize that prisons are schools of crime where offenders accumulate criminal capital (criminal skills, networks, opportunities), and therefore expect $B'(s)$ to be positive. Finally, specific deterrence theory suggests that $C'(s)$ is also positive as offenders who were severely punished the first time probably expect harsh punishment in case of reconviction too. Regarding the probability of punishment, we hypothesize that p does not vary with s for simplicity[5].

Theoretically, the fact that electronic monitoring is more effective than incarceration implies that ex-prisoners who did not reoffend wouldn't have reoffended after EM either:

$$U(s^p) > (1-p)B(s^p) - pC(s^p) \Rightarrow U(s^{EM}) > (1-p)B(s^{EM}) - pC(s^{EM}) \qquad (2)$$

whereas the opposite is not true.

We briefly discuss two competing scenarios, denoting the expected utility derived from recidivism, $(1-p)B(s) - pC(s)$, as $f(s)$ for simplicity.

$\mathbf{U(0) > f(0)}$ and $\mathbf{f'(s) > 0}$ The first case corresponds to the situation of a one-time offender, with relatively high attachment to society and little interest in committing new offenses: in the absence of punishment, this individual would not reoffend. However, the experience of severe punishement may adversely affect his future behavior (through a large hardening effect, or low specific deterrence) and any increase in sentence severity leads to higher expected utility from recidivism f. In this case, depicted in Figure 1, implication 2 is verified irrespective of the slope of $U(s)$: experiencing less severe sentences, such as EM instead of prison (movement to the left), reduces the propensity to reoffend and promotes future law-abiding behavior.

The same conclusion actually applies to all situations where $U(0) > f(0)$ as long as $f'(s) > U'(s)$, such as the one depicted in Figure 2. These scenarios where alternative sentences are always preferrable to incarceration can arise in a multitude of contexts: prison facilities which are neither rehabilitative nor deterrent, and only offer inmates a chance to accumulate criminal capital behind bars; socio-economic environments were ex-prisoners face serious stigma, etc.

[5]However, we may imagine that an offender who spends time with experienced criminals inside prison can learn how to minimize risk of detection and conviction, suggesting a negative slope for $p(s)$. On the other hand, one might think that ex-detainees are more closely watched by the police, and thus that $p(s)$ exhibits a positive slope. For simplicity, we leave these potential effects aside and assume that $p'(s) = 0$.

Figure 1: $f'(s) > 0$

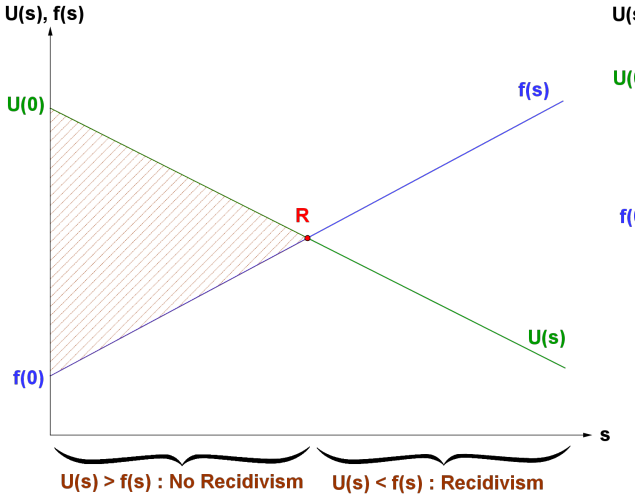
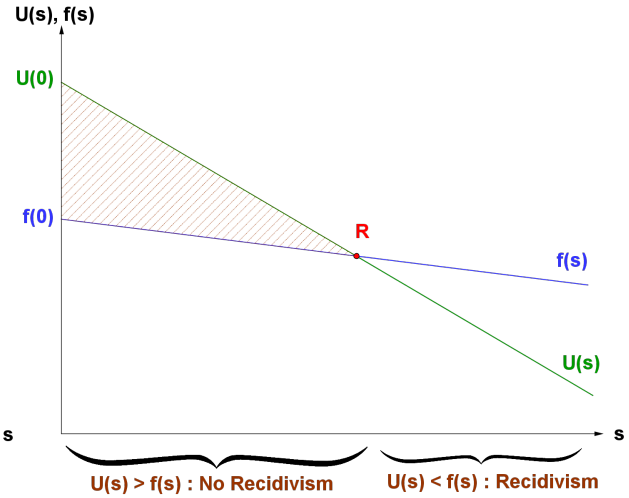
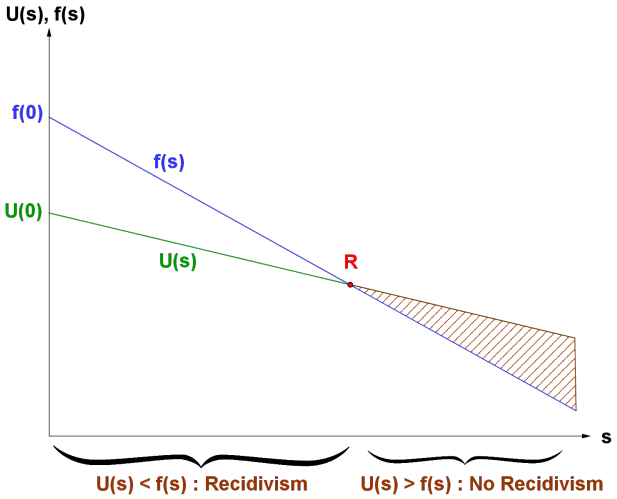


Figure 2: $f'(s) > U'(s)$



$\mathbf{f(0) > U(0)}$ **and** $\mathbf{f'(s) < U'(s)}$ This alternative case corresponds to the situation of a repeat offender, with an intrinsic proclivity towards crime: in the absence of punishement, this individual would reoffend. However, imposing a more severe sentence reduces the propensity to reoffend if the marginal decline in the value of future crime is larger than the decline in utility from a law-abiding life, or $f'(s) < U'(s)$, as shown in Figure 3.

Figure 3: $f'(s) < U'(s)$



This net beneficial effect of sentence severity arises when specific deterrence is very strong compared to criminal hardening. It could apply in case of strict solitary confinement for example, where prisoners suffer the pains of imprisonment (high specific deterrence) but are unable to build criminal capital with cellmates. Such an effect may also occur when prisoners benefit from rehabilitative programs while incarcerated

(in-prison education or work, therapeutic treatment, etc.), which increase U.

Overall, this simple model suggests that the severity of past punishment can increase or decrease the propensity to commit a new crime, depending on the magnitude of specific deterrence compared to the two competing effects on U and B, namely *desocialization* and *criminal hardening*. It remains an empirical matter to estimate the net effect on recidivism.

1.2 Existing estimates

As pointed by Aos et al. (2006) and Villettaz et al. (2006) in their meta-analyses on the effects of sanctions on recidivism, most estimates until recently were presumably contaminated by selection bias on unobservable characteristics. This threat is particularly obvious when few control variables are included in regressions or matchings, but concerns remain even when very rich data is used. Therefore, we focus on the handful of papers exploiting quasi-experimental designs where arbitrary rules or random events lead similar offenders to receive different punishments (EM or incarceration). Of course, such settings are not easy to find in practice: most judicial decisions allow some discretion from judges, who can then tailor sentences to fit the personality and situation of each offender (the opposite of random punishment).

In Buenos Aires, Argentina, criminal cases are assigned randomly to judges depending on their duty work days, which are determined by a lottery. Judges then have to decide whether alleged offenders should serve pre-trial detention inside prison or at home under electronic monitoring. Di Tella and Schargrodsky (2013) find that local judges greatly differ when making this decision: only one third of them (100/293) ever use EM in Buenos Aires during the period of study (for a total of 386 EM granted). The authors exploit these ideological differences as exogenous variations in the probability of EM treatment, and estimate the causal effect on recidivism of serving time under electronic monitoring instead of incarcerated. They find a significant 50% drop in the probability of re-arrest after EM compared to prison. This difference is confirmed when control variables are included, and when differences across judges serve as instruments. Overall, this paper is the first to provide compelling evidence of the dramatic crime-preventing effect of serving time at home under electronic surveillance, instead of in prison. However, Di Tella and Schargrodsky (2013) note that such a striking effect may well be specific to Argentina: in this country, prison conditions are particularly inhumane, with large overcrowding and little hope for rehabilitation. It is therefore crucial to gather more evidence from other advanced countries.

Marie (2015) is the first to provide quasi-experimental evidence of the beneficial effects of electronic monitoring in Europe. To do so, he exploits two administrative criteria in prisoners' eligibility to the Home Detention Curfew in England and Wales, a massive early-release program under EM. Discontinuites in eligibility, by age and sentence length, allow identification of the causal effect of obtaining early-release under electronic monitoring, instead of spending more time in prison. The Regression Discontinuity estimates show a large beneficial effect of EM, with reductions in probability of rearrest of 20% to 40% within two years. However, the program under study grants EM as an early-release device, not as a front-door substitute to incarceration, so it remains uncertain whether fully converting custodial sentences into electronic surveillance before incarceration similarly prevents recidivism.

Another closely related study of EM comes from Denmark. Andersen and Andersen (2014) investigate how electronic monitoring affects unemployment (proxied by social welfare dependence) compared to incarceration. To achieve identification, they rely on two policy reforms that expanded the use of EM in Denmark, in 2006 and 2008. Comparing pre-reform and post-reform samples of offenders (of which approximately half were granted EM), they run matching and differences-in-differences regressions and find that EM significantly decreases social welfare dependence in the first year. The beneficial effect of EM is however concentrated on young offenders (under 25 years old), while older offenders are not affected. Unfortunately, their dataset only tracks labor-market outcomes in the first year, not future criminal activity.

In France, prior work on EM and recidivism is mainly exploratory. Using the only data available at the time, Benaouda et al. (2010) compare reconviction rates between the first 492 EM offenders in France, and offenders convicted to other sentences in the North *département* in 1996. They find that EM offenders exhibit a 5-year recidivism rate (42%) that is lower than among offenders convicted to prison, probation or community service. Unfortunately, they have little individual informations to make more precise comparisons. Going one step further, Henneguelle et al. (2015) merge the dataset on the first EM recipients with a national sample of prisoners released in 2002. This dataset includes a large number of individual characteristics, which are used to restrict the control group on important criteria and to run multivariate regressions and propensity score matching. The estimated difference in recidivism decreases sharply when observable heterogeneity is accounted for (from an initial 25 pp difference to about 12 pp), but this gap remains statistically significant. However, these results may still be contaminated by selection on unobservables.

Another empirical approach is to explicitely acknowledge the presence of selection bias and test how much selection is actually needed in the data to make the estimated effect disappear (in practice, a fictious covariate is added in the model). Ouss (2013) applies this simulation-based method to the same French data and concludes that an unreasonable amount of selection bias is required to accept the null hypothesis of no effect of parole or EM on recidivism. However, it seems unclear which level of selection bias is reasonable or not, especially in the context of highly discretionary decisions made by profesional judges after individual interviews with eligible offenders. Plus, the conclusions from such sensitivity analyses highly depend on the richness of available data and on the quality of the benchmark regression, in terms of precisely controlling the main differences between the treated and non-treated.

In the current article, we take a more direct approach to estimate the causal effect on recidivism of serving time at home under electronic monitoring, instead of incarcerated. Specifically, our empirical strategy exploits the gradual introduction of EM across French courts in early 2000's as a natural experiment.

2 Institutional context

2.1 An experiment (2000-2002) followed by a gradual roll-in

On December 19th 1997, after years of parliamentary debates[6], the law no. 97-1159 introduced electronic monitoring in France as a substitute for incarceration. Legally, EM is not a criminal sentence but a way of serving a prison sentence, before or after incarceration. Though the law was passed in 1997, it took several years to prepare the introduction of EM in France[7], and the practical implementation of electronic surveillance only began in year 2000 as a pilot experiment (Kensey et al. (2003) and Lévy and Pitoun (2004)).

The experimentation of EM took place in four courts, or *Tribunaux de Grande Instance*, between October 1st 2000 and October 1st 2001: these courts were located in Agen, Aix-en-Provence, Grenoble and Lille. As explained by Lévy and Pitoun (2004), the choice of these four experimental locations was mainly motivated by whether the local judge(s) and prison head were sympathetic with the project, whether prison staff and judicial authorities worked well together locally, or whether the EM experiment

[6]Two Members of Parliament, Bonnemaison and Cabanel, wrote seminal reports on EM in 1983 and 1996 respectively. See Bonnemaison (1983) and Cabanel (1996).

[7]Between 1997 and 2000, some preliminary studies have been conducted in order to draw up a review on existing knowledge. These studies have concluded that an experimental phase was necessary, in order to test the equipments and the softwares provided by the different suppliers.

would face resistance from local unions[8]. However, as we show in detail in Section 4, these four pilot courts did not seem to differ from the other French courts on important observable characteristics such as post-prison recidivism, prison overcrowding or local crime trends.

On January 1st 2002, just after the end of the experimental phase, 143 EM had been granted to offenders from the four pilot courts, of which 120 were over. Then, the French correctional administration decided to expand electronic monitoring to the whole territory. Starting in January 2002, all French courts were allowed to grant EM to offenders who met the legal criteria. Local judges had first to request EM devices from the central administration, and could then grant tags to offenders.

However, this generalization process was prolonged and geographically heterogenous. Only one new court, located in Béziers (South of France), granted EM as a substitute for incarceration in January 2002. The first wave of adoption truely occured between December 2002 and May 2003, with a dozen new courts participating. The EM roll-in then intensified in the second semester of 2003 (78 courts had granted at least one EM by December) and in early 2004 (112 courts by May) (Lévy and Pitoun, 2004). This process continued over the next few months, and all French courts eventually adopted electronic monitoring[9]. Today, EM is massively used in France, with more than 20,000 tags granted every year, and about 10,000 offenders under EM on any given day (DAP, 2015).

Our empirical strategy exploits the early stages of this gradual roll-in (2000-2003). We use data collected by Kensey and Benaouda (2011) on the first 580 offenders who were granted EM in France, between October 2000 and March 2003. These offenders were either located in a pilot court, or in one of the 13 courts which rapidly adopted EM (between January 2002 and March 2003) as a substitute for incarceration. The map in Figure 8 shows the location of these courts over the metropolitan territory, and Table 8 reports the number of EM granted in each court by April 2003. All the other courts adopted EM later on (they do not appear in our database of the first 580 EM) and are therefore labelled as *late adopters*. We exploit these cross-court differences in access to EM to estimate the effect on future crime of serving time at home under EM instead of incarcerated. The intuition is that similar offenders had differential access

[8]Lévy and Pitoun (2004) also note that Agen hosts the national school of prison administration (ENAP), and that MP Cabanel comes from Grenoble: this may also explain why those two locations were chosen.

[9]According to discussions with practitionners, part of the explanation for the lagged introduction of EM across courts has to do with ideological resistance, since EM was quite a revolution in France in the early 2000's: it involved both a new technology, and a new philosophy on prison sentences (offenders initially convicted to prison could now fully avoid incarceration thanks to EM)

to EM depending on their location: offenders who were convicted in a pilot or early-adopter court had more chances of obtaining EM than offenders located in a neighboring late-adopter court.

2.2 The selection of offenders into EM

Conditional on EM availability at the court level, the path towards electronic monitoring is highly selective: offenders first have to fulfill several eligibility criteria, and must also receive the approval of a judge after an individual hearing. This judge, called *Juge d'Application des Peines*, has ample room for discretion.

The eligibility criteria are explicitely listed in the *Code de procédure pénale*:

- Offenders are to be convicted to a short prison sentence of no more than one year, or the remaining of their current sentence should not exceed one year [10]. In practice, about 90% of EM devices in France are granted before incarceration to short sentence offenders (Kensey et al., 2003)[11].

- Offenders are to have a place to stay equipped with a fixed-line telephone (to install the electronic device) [12].

Every offender who meets these two criteria and who is left free at trial (no bench warrant) is eligible to front-door EM. His case is automatically examined in the next weeks by a *Juge d'Application des Peines*, who decides whether EM should be granted or not. In order to make his decision, this judge requests a social investigation report to parole officers (to make sure that the landlord, family and offender give their consent, and that EM is practically possible) and later meets the offender and his lawyer for an hearing, which lasts about 20 or 30 minutes. The judge usually asks offenders questions about the current offense, the victims, past convictions, which activities the offender would or could pursue under EM (work, training, medical treatment), etc. These interviews can therefore reveal qualitative aspects of offenders' case, that are not reported in official criminal files. A couple of weeks after this interview, the offender is informed about the judge's decision.

As expected from this process, Kensey et al. (2003) show that many factors are taken into account: type of offense (driving and drug offenses are particularly frequent

[10] This length was extended in 2009 to 2 years, but remained equal to 1 year for recidivists. Our data, which are focused on the years 2000 to 2003, are not affected by this legal change.

[11] In this study, we focus on offenders who benefited from EM before incarceration to estimate the effect of a full substitution of prison by electronic monitoring.

[12] Having a fixed-line telephone is not necessary anymore for current devices, but this condition was important at the time-period of our data.

among EM offenders), length of criminal record (recidivists are less likely to receive EM), attitude towards the sentence, "maturity" or "psychological stability", etc. In fact, Kensey and Narcy (2008) show that offenders placed under EM between 2000 and 2006 are more similar to those convicted to non-custodial sentences (suspended prison, probation) than to incarcerated offenders: for instance, 92% of EM offenders were French compared to 77% among inmates; 42% had a partner, compared to 23% for prisoners. Only 18% of those under EM were illiterate or had very low schooling level, and 72% were employed before conviction (respectively 50% and 34% among inmates).

Overall, it seems that French judges use a great amount of discretion to select offenders under electronic monitoring, among the large pool of eligibles. EM offenders tend to have better reentry prospects (in terms of family support, work history, criminal background, etc.) and are therefore less likely to reoffend in the first place. These observable differences stress the need to control for a rich set of individual characteristics, and to rely on a quasi-experimental design to correct for plausible selection on unobervables too. We argue that the incremental roll-in of EM in France between 2000 and 2003 provides such a setting.

3 Data

We merge two nation-wide surveys conducted by the French Department of Prisons: the first survey consists in a cohort sample of prisoners released in year 2002, and the second is the population of the 580 first EM recipients in France (between 2000 and 2003).

Two databases. The first database contains a sample of 8537 offenders released between June 1st 2002 and December 31th 2002. It was constructed using two sources of data, penal files and criminal records. Penal files are filled by prison facilities themselves, while offenders are serving their sentence. They contain basic sociodemographic data about convicts (gender, date of birth, self-declared employment and marital status, education, home city) but also some information about offenses (date, precise infraction, sentence) and incarcerations (location, dates of entry and release, sentence reductions). Criminal records register offenders' sentences, both before and after the incarceration that led to the 2002 release. They were collected in 2008, thus enabling to investigate recidivism five years after release.

This sample is not drawn at random in the general French prison population. Indeed, some categories were fully sampled, such as women, parolees or juveniles. Though useful to study subcategories of prisoners, the sampling scheme (with weights highly skewed,

from 1 to 16) introduces much variance in the data, leading to very imprecise inference. To avoid this problem in our econometric estimations, we follow Solon et al. (2015) and include as regressors all the variables which were used for sampling. We therefore recover a representative effect of EM on recidivism.

The second database is the only existing study of recidivism among EM recipients in France. It contains individual data on the population of the first 580 EM offenders, from the inception of electronic surveillance in France until March 2003[13]. This dataset collects socio-demographic data and criminal records up to 2008, allowing a follow-up period of more than five years. Out of the 580 sampled offenders, 515 (88%) obtained EM as a full alternative to incarceration (they did not spend a single day in prison), while the others obtained early release under EM during their prison spell. We consider the former as the *treated* group (EM offenders), while the latter who went to prison (usually for quite long periods) are part of the *control* group of ex-prisoners (they only obtained EM as part of an early-release program similar to parole).

Dependent variable. We define recidivism as any reconviction, regardless of the type of new offenses and sentences. On occasion, we also focus on new prison sentences (reincarceration) to capture serious reoffending. Recidivism is measured after five years, which is typical for studies in France but much longer than most foreign research: in order to capture new offenses in the at-risk period, the clock starts on day of release for prisoners, and on first day under electronic monitoring for EM recipients[14]. In addition to reconvicted offenders, we consider as recidivists the 26 EM recipients who were sent to prison during their supervision spell due to repeated incidents or a new offense. Neglecting those "failures" would bias the comparison in favor of EM.

We acknowledge that the use of reconviction data is an imperfect measure of recidivism (some offenses are not detected and prosecuted), and does not fully inform about rehabilitation. However, there is no data on ex-prisoners' rehabilitation or self-reported crime in France. Plus, even though absence of recidivism does not guarantee rehabilitation, we argue that rampant recidivism clearly suggests a failed reentry.

Sample restrictions. Our initial sample contains 9012 individuals, 515 EM offenders (treated group) and 8497 prisoners (control group). However, we make several sample

[13]For additional details, see Kensey et al. (2003).

[14]An alternative is to start the clock for EM recipients on the end date of surveillance: this would account for potential short-term incapacitation during home curfew under EM, but it would conversely neglect new offenses during surveillance. In robustness checks, we show that our estimates are not affected by the choice of starting time.

restrictions to drop those convicts who had clearly no chance of obtaining EM, regardless of its availability in their court.

First of all, many observations are not exploitable due to death or absence of criminal record for example. Plus, some individuals exhibit missing values for important variables, such as sentence length. We decide to drop these offenders (43 EM and 1716 prisoners, representing respectively 8.3% and 20.2% of the initial samples), leaving us with a sample of 7253 individuals (6781 ex-inmates and 472 ex-EM).

Because having a home was a necessary condition to obtain EM, we also drop all ex-inmates who didn't have a domicile when they arrived in prison (965 individuals, 11.3% of the initial sample). Our sample then contains 6.288 individuals, splitted in 5816 ex-inmates and 472 ex-EM offenders.

We also focus on convicts whose incarceration, if any, took place after final conviction. We conversely drop inmates who were held in pretrial detention, as well as those whose prison sentence started exactly on the day of conviction (bench warrants). Our view is that these prisoners are inherently different from EM offenders, as judges considered their case required rapid incarceration. Conversely, the very fact that treated offenders obtained EM at home demonstrates that judges didn't view them as too dangerous. This major difference leads us to consider as controls only prisoners who were incarcerated stricly after their final prison conviction. 2821 prisoners are dropped (33.0% of the initial sample), leaving a sample of 3467 individuals, with 2995 controls and 472 treated offenders.

Finally, we exclude from most regressions individuals whose follow-up period for new convictions is shorter than 5 years. This last restriction concerns 3 EM recipients and 279 inmates (3.3% of the initial sample).

Our final study sample contains 3185 offenders convicted to prison, of which 469 obtained EM directly and 2716 spent time incarcerated.

Descriptive statistics. Table 6 presents descriptive statistics with regards to sociodemographics, judicial variables and recidivism, for the full study sample (column 1) but also both for treated (column 2) and non treated (column 3) individuals. Both subsamples are different on many aspects, even after restricting ourselves to domiciled convicts whose sentence did not start before or on day of conviction. In particular, note that EM convicts are older, more likely to be employed and in a relationship before conviction, and to be convicted for traffic offenses (DUI, driving without a license). These observations are similar to those made by Kensey and Narcy (2008) on a larger sample of EM

Table 1: **Sociodemographic and judicial variables**

Variables	Mean	Mean (EM)	Mean (Prison)	Diff.	Range
	(1)	(2)	(3)	(4)	(5)
Socio-demographic characteristics					
Male	88.6%	93.2%	87.8%	***	[0;1]
Age[a]	31.3	33.3	30.9	***	[13.6;100.6]
Standard deviation	*(11.2)*	*(11.4)*	*(11.2)*		
Employment	42.4%	64.0%	38.6%	***	[0;1]
Couple	33.3%	42.6%	31.7%	***	[0;1]
Children	44.5%	50.3%	43.5%	**	[0;1]
Prior convictions to prison					
Frequency	62.1%	69.1%	60.9%	***	[0;1]
Average number	1.3	0.9	1.4	***	[0;27]
Standard deviation	*(2.8)*	*(1.9)*	*(2.9)*		
Prior convictions to alternative sentences					
Frequency	54.0%	50.3%	54.6%	†	[0;1]
Average number	0.9	1.9	0.8	***	[0;20]
Standard deviation	*(1.6)*	*(3.1)*	*(1.1)*		
Type of initial offense					
Acts of Violence[b]	10.7%	14.1%	10.2%	*	[0;1]
Sexual assaults[c]	6.6%	4.7%	6.9%	†	[0;1]
Traffic[d]	19.5%	27.1%	18.2%	***	[0;1]
Property[e]	33.7%	29.6%	34.4%	*	[0;1]
Drugs	10.9%	8.5%	11.3%	†	[0;1]
Immigration	1.9%	0.0%	2.2%	**	[0;1]
Weapons	2.0%	2.1%	2.0%	*ns.*	[0;1]
Prison sentence					
Initial sentence (months)	8.2	5.7	8.6	***	[0;361]
Standard deviation	*(15.7)*	*(3.5)*	*(16.9)*		
Early-release	34.8%	0	40.8%	***	[0;1]
Prison characteristics					
Prison type					
Maison d'arrêt[f]	78.3%	80.0%	78.0%	*ns.*	[0;1]
Centre de détention[g]	21.7%	20.0%	22.0%	*ns.*	[0;1]
Overcrowding rate	112.1%	113.1%	111.9%	*ns.*	[26.6%;250%]
Standard deviation	*(33.8%)*	*(30.3%)*	*(34.4%)*		
Recidivism after 5 years (weighted for oversampling)					
Any reconviction	64.4%	46.9%	65.1%	***	[0;1]
Reconviction to prison	53.2%	30.5%	54.1%	***	[0;1]
Sample Size	3185	469	2716		

The sample is composed of offenders who had a home and who started serving their sentence (in prison or under EM) strictly after their date of conviction. † p<10%, * p<5%, ** p<1%, *** p<0.1%.

[a] Age at the beginning of the sentence (age at incarceration for ex-inmates or age at EM).

[b] "Acts of violence" include homicides and assault and battery.

[c] "Sexual assaults" include rapes, sexual aggressions, offenses against honor and indecencies.

[d] "Traffic" includes drink-driving and other common offenses (speeding, driving license-related violations, etc.).

[e] "Property" includes all kinds of thefts and robberies, destructions and frauds.

[f] "*Maisons d'arrêt*" are prison facilities for pretrial detainees, or for those sentenced to less than 2 years.

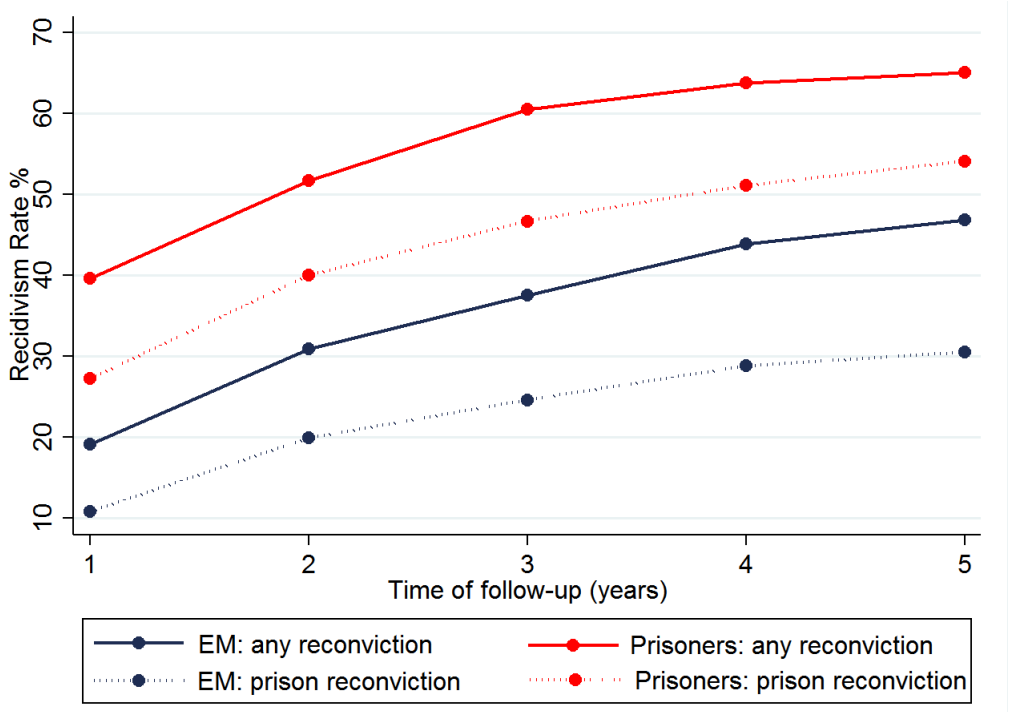
[g] "*Centres de détentions*" are prison facilities for inmates sentenced to more than 2 years.

recipients.

In terms of recidivism rates within 5 years (corrected for weighted sampling), the two groups differ dramatically: 65% of ex-prisoners are reconvicted compared to 47% ex-EM offenders, yielding a gap of 18 percentage points. This difference is even larger when focusing on new prison sentences only (gap of 24 pp).

Finally, Figure 4 shows the evolution of recidivism rates over time in both groups: the recidivism gap between ex-prisoners (red) and EM recipients (blue) is large and quite stable over the whole follow-up period.

Figure 4: Recidivism over time



4 Empirical strategy

We rely on a series of empirical strategies to obtain estimates that are robust to different assumptions. Our main strategy builds on the incremental introduction of electronic monitoring across French courts: as explained in Section 2, EM was first experimented in 4 pilot courts starting from October 2000, and then became legally available at all 193 courts after January 2002. However, EM was not rapidly implemented in practice in most courts: only 13 of them delivered at least one EM device before May 2003. Hence courts are of three types: pilot courts (n=4), early-adopter courts (n=13), and late-adopters (n = 176). We use this disparity across courts as a source of variation in the individual probability of EM treatment. Note already that we explore a differ-

ent approach later, only exploiting within-court variation in the use of EM over time, obtaining similar but less precise estimates.

The main concern with our between-court strategy is that courts which adopted EM between 2000 and 2003 may systematically differ from late-adopters. For example, some courts may have purposely decided to implement electronic monitoring to combat high recidivism, massive overcrowding, or rising crime rates. Such local differences may affect post-prison recidivism on their own, which would violate our exclusion restrictions. To investigate the hypothesis of endogenous selection of courts into EM, we collected additional data and checked whether differences are observed between EM courts and late-adopters.

First, we consider how recidivism rates differed between EM and non-EM courts before the introduction of electronic monitoring. To do so, we restrict the study sample to prisoners who were incarcerated *before* the first EM device (if any) was granted in their court. Then, we regress recidivism on the type of court (pilot, early-adopter, or late-adopter) controlling for a rich set of individual characteristics, to make sure that comparisons are not biased by differences in offenders' compositions across courts. The estimates in Table 9 (Appendix) do not support the idea that courts which adopted EM rapidly had larger recidivism rates prior to the implementation of electronic monitoring: the estimated differences are both small in magnitude (-0.02 for pilot courts, +0.01 for early-adopters) and insignificant. Similarly, we look at recidivism in the late 1990's, using a previous cohort sample from the correctional administration (Kensey and Tournier, 2005). This dataset originates from a nation-wide study of 2200 ex-prisoners released in 1996-1997, and whose reconvictions were followed until 2002. Again, covariate-adjusted differences in recidivism are small and insignficant between pilot, early-adopter and late-adopter courts. Overall, these results strongly suggest that courts which rapidly adopted EM (between 2000 and 2003) were remarkably similar to other French courts in terms of recidivism. Therefore, our IV results using between-court variation are unlikely to be driven by local differences in recidivism.

Second, we consider local crime rates. We could expect that courts decided to adopt EM to manage a surge in local crime, threatening our empirical strategy. To test this hypothesis, we rely on annual *département*-level data[15] (the Homeland Ministry doesn't provide crime statistics at the court level). We compare crime levels and crime trends in *départements* with a pilot or ealy-adopter court, and other *départements* where no court

[15] *Départements* are small administrative regions in France, like counties. France counts about 100 *départements* and the average number of High Courts per *département* is two.

adopted EM rapidly (late-adopters). In levels, pilot and early-adopters tend to display substantially higher crime rates for both violent and property offenses during the 1996-2001 period (levels are expressed in number of reported offenses per 1000 inhabitants). This is probably due to the fact that pilot and early-adopter *départements* include densely populated areas (Marseille, Lille, Paris, etc.) which exhibit higher levels of crime. However, property and violent crime follow parallel trends in the three types of *départements*. Hence, the delayed adoption of EM across *départements* didn't seem to be a reaction to different trends in local crime rates.

Third, we consider prison overcrowding: rising population/capacity ratios may have pushed some courts into electronic monitoring, which may independently affect recidivism. To test this hypothesis, we collect overcrowding rates in all prison facilities on January 1st of each year (1996, 1999, 2000, 2001, 2002) and construct weighted average rates for each type of court, based on the flow of offenders convicted in court c and incarcerated in prison p. Figure 10 shows how these overcrowding rates vary over time for the three types of courts: overcrowding tends be somewhat larger in pilot and early-adopter courts, but differences are small (less than 10 percentage points) and insignificant. Similarly, when we focus on short-term prisons (*Maisons d'Arrêt*), differences remain modest. Based on the small existing elasticities of recidivism with respect to prison overcrowding (Drago et al., 2011), we do not expect such small differences to affect our EM treatment effects. In addition, note that overcrowding in early-adopter courts was very close to late-adopter courts on January 2002 (when EM was legalized in all courts), suggesting that courts which adopted EM rapidly did not face any particular problem of overcrowding compared to other French courts.

4.1 Econometric specification

Our objective is to estimate the causal effect of serving a prison sentence at home under EM, instead of incarcerated, on individual probability of recidivism. This corresponds to the estimation of the ATT effect of EM treatment. To account for potential endogeneity of treatment, we consider a two-equation model where identification derives from two exclusion restrictions, or instruments, capturing EM availability at the court level: one dummy variable for being judged in a pilot court, and a second dummy for being judged in an early-adopter court. We therefore assume that court locations are exogenous to individual criminal decisions: offenders do not strategically choose crime locations (and therefore court of judgment) to have access to EM. This assumption seems highly credible as 98% of those who obtained EM (460 out of 469) lived in the *département*

where they were judged.

Our two-equation model writes as follows:

$$Recid^*_{i,c} = \beta_0 + \beta_1 EM_{i,c} + \boldsymbol{X}'_{i,c}\boldsymbol{\beta} + \mu_c + \epsilon_{i,c} \tag{3}$$

$$EM^*_{i,c} = \alpha_0 + \alpha_1 Pilot_c + \alpha_2 EarlyAdopter_c + \boldsymbol{X}'_{i,c}\boldsymbol{\gamma} + u_c + e_{i,c} \tag{4}$$

We account for the qualititative nature of the dependent and endogenous variables (*Recid* and EM) by estimating a recursive bivariate probit model by maximum likelihood. This model explicitely controls for the correlation between the error terms (the unobservables) of the two equations, denoted ρ. For comparison purposes, we also estimate the naïve Equation 3 alone by probit and OLS. Importantly in our cross-court design, the structure of the error term allows for intra-court correlation.

The vector $\boldsymbol{X}$ includes a rich set of individual characteristics. First, we include all the variables used in the weighted sampling scheme to obtain a representative estimate of the effect of EM treatment on recidivism[16]: this notably includes gender, juvenile (age<18), type of initial offense, and early-release under parole. We are therefore able to measure how benefiting from EM affects the probability to reoffend in a representative sample of prisoners.

We also include socio-demographic variables that are presumably correlated with both EM treatment and future crime: age and age squared, employment status, relationship status and parenthood.

We precisely control for criminal background using four variables: two dummies for prior convictions to prison or alternative sentences, and two continuous variables for the total number in each category. We also include a second-degree polynomial for the length of initial prison sentence, as prior work shows that short-sentence prisoners (those convicted for theft for example) reoffend much more on average than long-sentence prisoners (convicted for rape or murder).

We finally add a dummy for other early-release programs[17] to account for the potential effects of early-release among prisoners: this is especially important in the French context where early-release is often long to obtain, and therefore mostly benefits to long-sentence prisoners (who are not eligible to EM). We also include a linear trend for date of release to get rid of potential time trends in risk of recidivism over time (due to

[16] As recently suggested by Solon et al. (2015). This is particularly important since the sample of prisoners over-represents parolees, a positively selected group of prisoners with good reentry prospects.

[17] In addition to parole, prisoners can obtain early-release under EM, *semi-liberté* and *placement à l'extérieur*.

Table 2: Electronic Monitoring and Recidivism

	Y = Any Reconviction In The Next 5 Years			
	(1)	(2)	(3)	(4)
Probit Model				
Electronic Monitoring	-0.1404**	-0.1406**	-0.1203***	-0.0794**
	(0.0410)	(0.0410)	(0.0317)	(0.0274)
Pseudo R^2	0.19	0.19	0.24	0.29
Linear Prob. Model				
Electronic Monitoring	-0.1502***	-0.1510***	-0.1280***	-0.0922**
	(0.0443)	(0.0445)	(0.0350)	(0.0305)
Adj. R^2	0.23	0.23	0.28	0.33
Initial sentence length		x	x	x
Demographics			x	x
Past convictions				x
N	3185	3185	3185	3185

Robust standard errors in parentheses, clustered at court level. $^{+}$ $p < 0.1$; * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$. All regressions control for the variables used for weighted sampling. Probits report Average Marginal Effects.

changes in policing or economic conditions for example) between our two datasets[18].

5 Results

5.1 Benchmark estimates

We first report in Table 2 naïve estimates of Equation 3 using probit and linear probability models. They serve as a benchmark since they treat the EM treatment as an exogenous variable, and therefore do not correct for potential selection bias. We focus on the more conservative Probit estimates, but the OLS results are very similar.

The specification in Column 1 doesn't control for any individual characteristic but simply account for weighted sampling. The estimate suggests that offenders under EM are 14 percentage points less likely to be reconvicted in the next five years than those released from prison. This difference is both large in magnitude and highly significant. To account for the fact that prisoners are convicted to longer sentences than EM holders on average, we control in Column 2 for initial sentence length (quadratic polynomial). The results remain virtually unaffected. Column 3 additionnaly controls for the set of demographics: the estimated difference in recidivism drops to 12 pp. Finally, controlling for past convictions further reduces the estimated gap to 8 pp.

[18]Remember that prisoners are released in the second semester of 2002, whereas EM offenders are released in the 2000-2003 period. In addition to the inclusion of a linear time variable, we later consider the potential for non-linear time trends in robustness checks (results are robust).

Overall, the inclusion of a large set of covariates decreases the estimated gap in recidivism by more than 40%, from about 14 pp to 8 pp. This pattern supports the idea of positive selection on observables from French judges in the allocation of EM: offenders who were granted EM displayed relatively good prospects (in terms of employment, family support, criminal background, etc.) and low risk of recidivism. However, even after controlling for this large set of characteristics, the estimated difference in recidivism remains largely and significantly in favor of EM recipients compared to incarcerated offenders.

5.2 Causal estimates from cross-court variation

In order to account for potential selection on unobervables too, we exploit the differential availability of electronic monitoring between courts. Column 1 reports our benchmark probit estimate (-7.9 pp) when the EM treatment is considered as exogenous. In Column 2, we run our recursive bivariate probit model where identification is obtained from two dummy variables capturing EM availability at the court level: as expected in case of selection on unobservables, the correlation coefficient ρ is negative and the estimated average marginal effect of EM decreases. However, the estimate is still large and significant, and suggests that serving time at home instead of in prison significantly reduces recidivism after 5 years by 6.9 pp on average. In Columns 3, we estimate the same model by Two Stage Least Squares: the results are very similar.

Overall, these causal estimates tend to be somewhat smaller than the benchmark, naïve estimates (though differences are not statistically significant). This pattern, and the negative sign of ρ, support the idea that judges wisely selected EM offenders on both observable and unobservable characteristics. However, even after controlling for this selection process, a large and significant gap of 6-7 percentage points in risk of recidivism remains. We attribute it to the causal ATT effect of serving time at home under electronic monitoring instead of in prison. Given the 64% recidivism rate in the representative sample, our results suggest that EM treatment reduces recidivism by 9% to 11% after 5 years.

5.3 Robustness Checks

We run a series of robustness checks in Table 10 (Appendix). First, we limit the potential for unobserved heterogeneity between courts and socio-economic contexts: to do so, we focus on neighboring courts in a radius of 100 kilometers around each EM court, or

Table 3: Electronic Monitoring and Recidivism

	Probit	Bivariate Probit	2SLS
Y_1 = Recidivism			
ElectronicMonitoring	-0.0794**	-0.0685*	-0.0766*
	(0.0274)	(0.0307)	(0.0349)
Y_2 = Electronic Monitor.			
PilotCourt		0.2633***	0.6701***
		(0.0110)	(0.0443)
EarlyAdopterCourt		0.2116***	0.2138**
		(0.0131)	(0.0637)
Initial sentence length	x	x	x
Demographics	x	x	x
Past convictions	x	x	x
ρ		-0.110 (0.098)	
N	3185	3185	3185

Robust standard errors in parentheses, clustered at court level. $^{+}$ $p < 0.1$; * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$. All regressions control for the variables used for weighted sampling. Probits report Average Marginal Effects.

include *département* fixed effects. Both strategies yield significant negative estimates for EM and suggest that our results are not driven by unobserved contextual differences.

A second concern is that our dataset merges two samples from different time periods: ex-inmates are all drawn from a cohort of prisoners released between June 1st and December 31st 2002, whereas the EM group contains all those who were discharged from electronic surveillance from the beginning of the program (end of 2000) to March 2003. Therefore, our results could be sensitive to non-linear time trends in the risk of recidivism, for example due to temporal breaks in police forces or economic conditions. To deal with such temporal shifts, we drop all EM offenders who were discharged long before or late after the second semester of 2002. To still have a sufficient number of EM beneficiaries, we keep those who were discharged between March 1st 2002 and March 31st 2003. Again, the estimate remains similar so temporal breaks in risk of recidivism are not a concern.

Third, we drop all prisoners convicted to a sentence longer than one year, since they were ineligible to electronic monitoring. Though this new restriction reduces the sample size (to 2828), it should not affect our results since our specification controls for initial sentence flexibly using a second-degree polynomial. As expected, the estimated marginal effect (-6.3 percentage points) remains large and significant, suggesting that our results are not biased by unmodelled differences in sentence length between prisoners and EM offenders.

Fourth, we use an alternative starting date for the measure of recidivism: up to

now, we have computed recidivism as any new conviction in the 5 years following date of release (for prisoners) or date of start of electronic surveillance (for EM offenders) since they are at risk of reoffending even during their period of monitoring. However, one may argue that EM offenders are somewhat incapacitated at home while under electronic surveillance, so that the at risk period actually starts at the end of the electronic monitoring. We use this alternative starting date to compute recidivism, and find similar results again.

Fifth, we use alternative constructions for our between-court instruments. We consider either a full set of court fixed effects, or a a single dummy for EM-court or not. We also use the same two dummy instruments but exlude early-adopter courts which only granted a handful of EM over the period (less than 10). All three methods confirm our previous results.

Finally, we turn to a very different approach and only exploit within-court variation in access to EM over time. To do so, we focus on EM courts and construct a dummy variable taking 1 if the sentence started after the first implementation of EM in a given court, and 0 otherwise. Simple probit estimates yield a covariate-adjusted gap in 5-year recidivism of about 13 percentage points. When running bivariate probit regressions, the new instrument is found to have a large effect on probability to obtain EM (+0.26 percentage points, F = 61), and the estimated marginal effect of EM on recidivism is -0.066. This effect is not precisely estimated (p-value = 0.19) but note that this within-court strategy leads to a dramatic reduction in sample size (N=809), and yields an estimate very similar to our preferred estimate of -0.069 (using between-court variation). Overall, it is very reassuring to obtain such comparable estimates of the effect of EM on recidivism after using between-court and within-court sources of variation. Unfortunately, the sample size makes it difficult to combine the two approaches and obtain credible Diff-in-Diff estimates.

5.4 Type and severity of new offenses

We consider whether EM affects the type and severity of new offenses. First, we classify new offenses in three categories with high recidivism rates: property crime (22% recidivism), traffic offenses (19%), and others such as assaults, drug-related offenses, etc. (23%). We estimate these three (non-mutually-excluding) crime-specific probabilities and EM treatment simultaneously by Three Stage Least Squares. We also investigate the seriousness of new offenses, proxied by type of new sentence (custodial or not) and total length of new prison sentences accumulated over the whole 5-year follow-up pe-

riod[19]. The estimated marginal effects are reported in Tables 4 and 5. Regarding offense type, EM tends to reduce recidivism in all three crime categories, though the estimates are imprecise and reach marginal significance only for "other" offenses (with a 14% reduction in 5-year recidivism in this category).

In terms of severity, the estimates are much larger and suggest that EM reduces the probability of receiving a new prison conviction after 5 years by almost 10 percentage points (19%). When focusing on reoffenders, the large beneficial effect of EM remains, with an estimated 15% reduction in prison conviction conditional on recidivism. The same pattern emerges for total length of future imprisonment (modelled as a Tobit censored in zero for those who are never reconvicted to prison): serving time at home under EM reduces the expected prison time that reoffenders accumulate over the follow-up period by half (10 months over an average of 21 months).

Overall, we interpret the relative homogeneity of our (imprecise) crime-specific estimates as evidence that the crime-preventing effect of EM applies to all types of offenses. However, our severity estimates suggest that EM offenders commit less serious crimes than ex-prisoners in the next five years, conditional on recidivism. This interpretation is in line with the literature on criminal hardening inside prison (Abrams, 2011). However, a competing view is that EM does not affect actual crime severity, but that EM offenders simply obtain more lenient sentences from judges in case of recidivism (to further avoid incarceraton): such judicial bias in favor of ex-EM offenders would overestimate the true severity-reducing effect of EM. However, we are skeptical about this hypothesis because all sampled offenders were prison convicts already, and would still be eligible to EM again, even in case of a new (short) prison sentence[20].

5.5 Why/When is EM effective ?

We attempt to understand the conditions for the effectiveness of electronic monitoring. To do so, we run separate bivariate probit regressions on subsamples based on possibly important mediating factors.

[19] Alternative sentences are coded as zero (censored) while the few life sentences are coded as 30 years (the maximum in the sample)

[20] Moreover, judges who grant EM (or not) and judges who convict to prison (or not) are never the same person in this context.

Table 4: Crime-specific effects

	Type of New Crime		
	Property	Traffic	Other
ElectronicMonitoring	-0.0186	-0.0241	-0.0338^{+}
	(0.0165)	(0.0187)	(0.0190)
N	3185	3185	3185
Recidivism Rate	22.2%	18.8%	23.4%
Estimated % EM effect	(-8.4)	(-12.8)	-14.4

Robust standard errors in parentheses, clustered at court level. $^{+}$ $p < 0.1$; * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$. Crime-specific estimates are obtained simultaneously by 3SLS to achieve convergence. All regressions include the full set of control variables and correct for endeogeneity of EM using the same between-court instruments.

Table 5: Seriousness of new crime(s) over the Next 5 Years

	Any New Prison Conviction		Total Length of New Prison Sent.	
ElectronicMonitoring	-0.0973**	-0.1227***	-9.5066**	-10.2763***
	(0.0294)	(0.0310)	(2.9916)	(2.7212)
Conditional on Recid	No	Yes	No	Yes
N	3185	1768	3185	1768
Sample Average	53.1%	78.5%	20.55 mos	21.02 mos
Estimated % EM effect	-19.0	-15.6	-46.3	-48.9

Robust standard errors in parentheses, clustered at court level. $^{+}$ $p < 0.1$; * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$. Prison conviction estimates are obtained by bivariate probit. Prison sentence length estimates are obtained by jointly estimating a Tobit and a Probit on the sample of reoffenders. All regressions include the full set of control variables and correct for endeogeneity of EM using the same between-court instruments.

Control visits at home First, we split EM offenders in two groups, depending on whether they received control visits at home from correctional officers during curfew hours (to control that they truely respected the curfew, as suggested by the EM device). The separate regressions yield very different estimates: EM was very effective at reducing recidivism among those who received at least one visit (-10 pp) while it had little effect on those who never received a visit from correctional officers (point estimate of -0.02). One may suspect endogeneity in the choice of visiting certain offenders and not others, but note that if the most crime-prone offenders are targeted for control visits, this bias would attenuate, not exacerbate, the effect of visits on recidivism. Moreover, it appears that the probability of control visit mainly varies between courts, not between offenders within courts : for example, 97% of EM offenders were visited in Aix-en-Provence, compared to only 10% in Agen. We take this as evidence that control visits were not endogenous, case-by-case decisions, but reflected different local practices (as confirmed

by Kensey et al. (2003)). The striking difference in outcomes suggests that control visits act as a strong deterrent device, which is in line with qualitative interviews with EM offenders in England (Hucklesby, 2009).

Incidents We also restrict the EM sample to those who provoked an incident during their period of monitoring (e.g. returning home late without justification). Among them, EM has no significant effect on recidivism, whereas it is very beneficial for those who provoked no incident (-11 pp). However, note that the occurence of incidents is probably endogenous and simply signal low compliance and high risk of recidivism.

Obligations In France, judges usually require EM recipients to complete particular activities while under surveillance, such as working (355 out of 472), playing an active role in family (62), or receiving medical treatment (73). We investigate whether EM is more or less effective depending on those obligations. Our results show that EM is very effective for those who had to work while under surveillance (-8 pp), and has no significant effect on offenders who were not subject to this obligation (controlling for initial employment status). We do not find similar positive effects of family and medical obligations, either because they do not matter for crime prevention, or maybe because these obligations are less respected by EM recipients (since they are difficult to control by judges and parole officers).

Length of stay under EM We investigate whether the beneficial effect of EM are mediated by the length of electronic surveillance. To do so, we split EM offenders at the median length (63 days). The estimates show a somewhat larger effect when surveillance lasts longer than 2 months (-0.09 pp *versus* -0.05 pp). This result is in line with Di Tella and Schargrodsky (2013) who find large crime-reducing effects of EM in Argentina, where the average surveillance spell (420 days) is much longer than in France.

Demographics We then look at the mediating effect of demographics. Electronic monitoring seems to have very similar effects on those who have children or not, and those who are in a relationship or not. Regarding age, the effect of electronic monitoring seems somewhat larger among younger offenders (under the median age of 28), which echoes the findings of Aizer and Doyle (2015) and Andersen and Andersen (2014). Finally, prior employment status does not seem to play a big mediating role, as point estimates in both groups are close: this result suggests that what matters in terms of

Table 6: Heterogeneity of effects by profile and supervision

Profile	Yes	No	**Supervision**	Yes	No
Younger than 28	-0.0786*	-0.0521	Control visit at home	-0.0965***	-0.0243
	(0.0376)	(0.0359)		(0.0224)	(0.0318)
Has children	-0.0748	-0.0749**	Incident during EM	-0.0187	-0.1064**
	(0.0458)	(0.0249)		(0.0354)	(0.0366)
In a relationship[a]	-0.08022*	-0.0799^{+}	Obligation: work	-0.0787**	-0.0342
	(0.0384)	(0.0478)		(0.0297)	(0.0547)
Employed	-0.0725^{+}	-0.0697	EM length > median	-0.0882*	-0.0526^{+}
	(0.0397)	(0.0464)		(0.0391)	(0.0316)
Prior incarceration	-0.1025*	-0.0650^{+}			
	(0.0521)	(0.0349)			

Robust standard errors in parentheses, clustered at court level. $^{+}$ $p < 0.1$; * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$. All regressions include the full set of control variables.
a: estimation by 2SLS to solve convergence issues in ML.

work is not whether offenders formerly had a job or not, but whether they are *required* to work while under EM.

Prior incarceration Another interesting question is whether EM is more beneficial among offenders who were never incarcerated before (for example because they avoid the scars of prison), or among those who already had an experience of prison (because future imprisonment has a larger deterrent effect on them). We find evidence that the effect of EM is more beneficial among those who had already been convicted to prison before (-10 pp). This supports the idea that EM can trigger reciprocal behavior and maintain specific deterrence among formerly incarcerated offenders (EM is viewed as a second chance), whereas offenders who never went to prison before may consider EM as a lenient, non-deterrent punishment, generating feelings of impunity.

6 Discussion

6.1 Incapacitation, deterrence, or genuine long-term change ?

While under electronic monitoring, offenders are compelled to respect strict home curfew for long hours, with permission to leave only for work or other rehabilitative activities. One may therefore suspect that the estimated crime-preventing effect of EM is predominantly explained by incapacitation at home. However, we discard this view for several reasons: first, the average spell under electronic monitoring is only 73 days (about half of EM offenders stay no more than 2 months, and 95% less than 6 months)

Table 7: Causal effects at different time windows

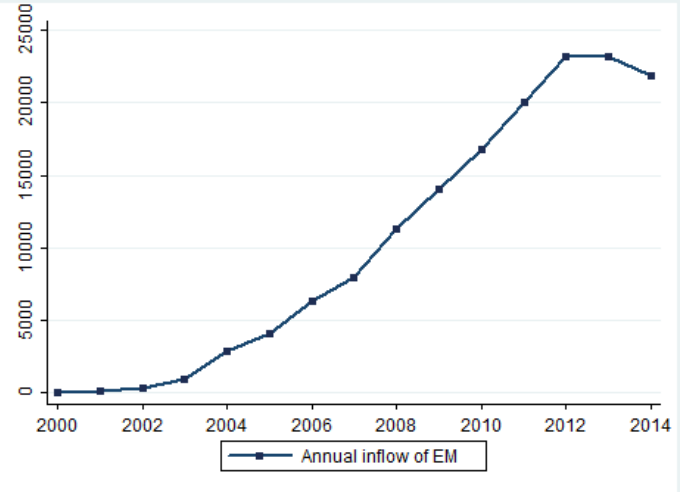
	Q1	Q2	Y1	Y2	Y3	Y4	Y5
ElectronicMonit.	-0.0211	-0.0453	-0.0750**	-0.0701*	-0.1045**	-0.0851**	-0.0685*
	(0.0278)	(0.0307)	(0.0267)	(0.0299)	(0.0337)	(0.0297)	(0.0307)
N	3467	3467	3467	3467	3467	3467	3185
Recidivism Rate	16.9%	27.1%	38.8%	51.0%	59.7%	63.1%	64.4%
EM effect in %	(-12.5)	(-16.7)	-19.3	-13.7	-17.5	-13.5	-10.6

Robust standard errors in parentheses, clustered at court level. $^{+}$ $p < 0.1$; * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$. All regressions are bivariate probits with the same two instruments, and the full set of control variables.

so it seems unlikely that such short-term incapacitation may explain the large effects observed five years later. Second, we obtain very similar estimates when we start the 5-year clock after the end of the monitoring spell (Table 10), which gets rid of the short-term incapacitation-at-home effect. Third, the percent reduction in recidivism thanks to EM is not particularly large at the beginning (while under home curfew), but actually seems to be highest in the medium run (1 to 3 years). To see this temporal pattern, we estimate the effect of EM on recidivism at different time horizons: after only one quarter, one semester, and then each year up to the fifth year after release. The results in Table 7 show that the percent reduction in recidivism is similar in the first quarter (-12.5%) and after 4 or 5 years (-13.5% and -10.6%). This pattern provides further evidence that the crime-preventing effect of EM is not driven by short-term incapacitation under home curfew: its impact remains similarly strong several years after the end of supervision.

Overall, our interpretation is that the beneficial effect of electronic monitoring on long-term recidivism derives from a mix of rehabilitation and deterrence. Indeed, our estimates in Table 6 suggest that obligation to work while under EM helps prevent recidivism, probably through better attachment to the labor market and increased opportunity cost of crime. Also note that home curfew implies spending more time with family, and conversely less time outside exposed to criminal peers and opportunities. In addition to opportunity cost, such changes in social ties may foster change in preferences, attitudes, and promote desistance from crime (Hucklesby, 2009). Second, our results show that EM is most effective on offenders who had already been to prison before: this may suggest that receiving EM is not perceived as a lenient punishment by former prisoners, but more as a second chance and an opportunity to seize: therefore, reduced deterrence (i.e. impunity effect) may not be a concern for ex-prisoners, but

Figure 5: The massive development of EM in France



only for inexperienced offenders who don't know prison. Finally, we find that EM has much more impact when it is accompanied by control visits at offenders' house. This last finding suggests that visits make the probability of detection more salient in offenders' mind (upward updating of p in the beckerian tradeoff), and that this effect lasts surprisingly much longer than simply during the period of monitoring. All in all, we take this as evidence that short-term sanctions in the community such as EM can have large, long-term effects on criminal behavior, as soon as they include ingredients of both rehabilitation and deterrence.

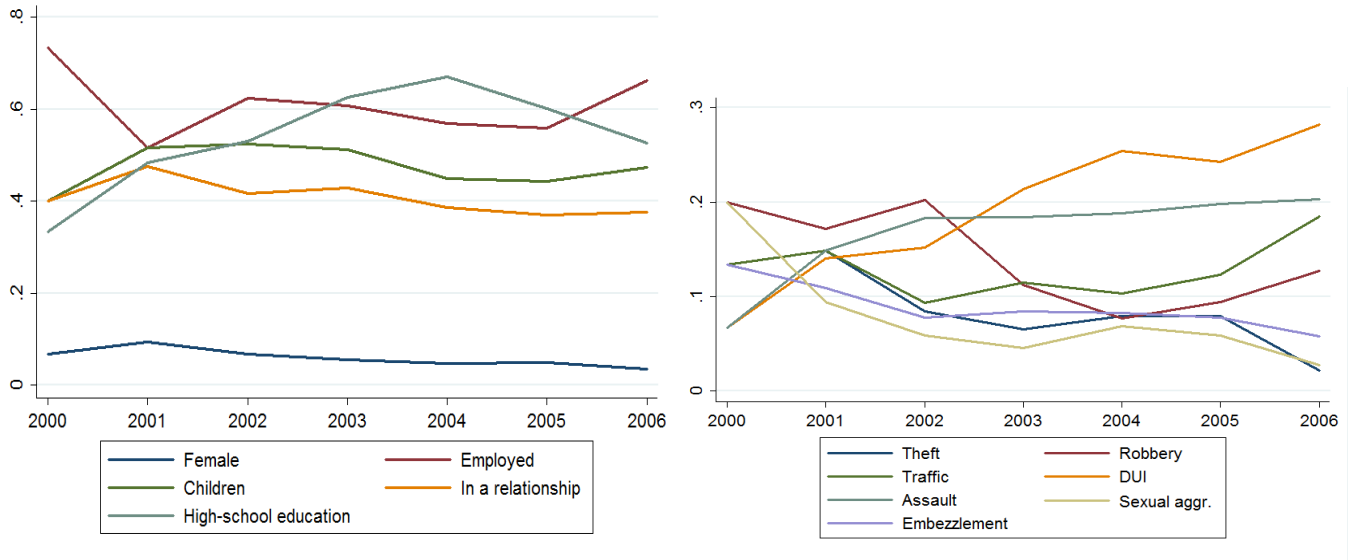
6.2 Current practices and current validity

Our results are obtained from the early stages of implementation of EM in France (from October 2000 to March 2003), a period where EM was a nascent alternative to incarceration, used in only a few hundred cases annually. As Figure 5 shows, the flow of offenders into EM rapidly increased after 2003, with about 6,000 EM delivered in 2006 for example. Today, it is estimated that about 20,000 offenders convicted to prison avoid incarceration each year thanks to electronic monitoring. This massive change in size may have large implications for the current validity of our findings. To explore this point, we exploit more recent data collected by the French Prison Administration (Kensey and Narcy, 2008) on a large representative sample of about 2,000 EM offenders, from 2003 to 2006 (covering about 20% of all EM over the period). We study how the selection and supervision of offenders under EM changed over time, in order to discuss the validity of our findings in more recent times.

Figure 6 reports the evolution of the main observable characteristics of EM offenders

between 2000 and 2006[21]. Overall, there is little change in the profile of EM offenders, except for the increase in the share of DUI offenses. This relative stability suggests that the process of selection of EM offenders remained similar between 2000 and 2006.

Figure 6: Observable characteristics of EM offenders



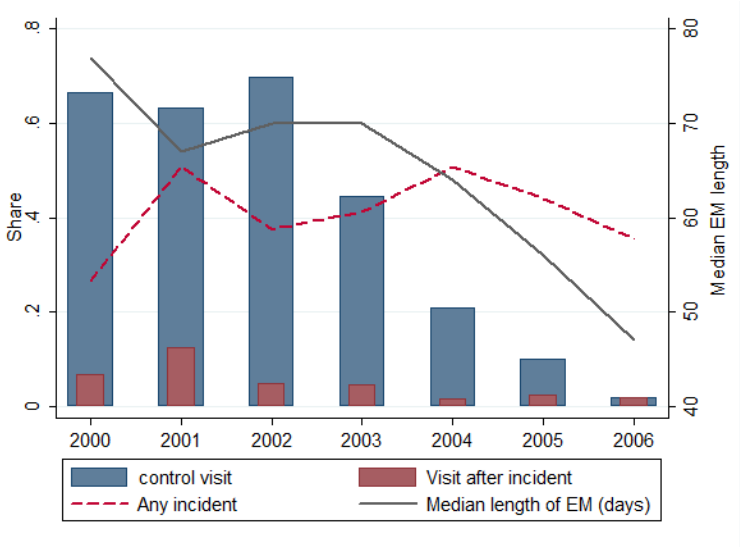
In terms of supervision however, the massive development of EM in France had large consequences. Figure 7 shows the evolution of control visits at home (blue bars): from 2000 to 2002, about two thirds of EM offenders received at least one control visit from correctional officers during their period of monitoring. After 2003, these controls dramatically dropped and almost disappeared in 2006[22]. Similarly, the median duration of EM sharply decreased over time, from about 80 days in 2000 to less than 50 days in 2006. This pattern suggests that the massive development of EM in France coincided with a sharp decrease in correctional supervision for EM offenders. On the other hand, there is no major change in the occurence of incidents (about 40% of cases throughout the period), control visits after an incident (less than 10%), or obligation to work (around 80%).

Overall, we view the dramatic fall in control visits and length of supervision as a threat to the current validity of our findings: indeed, our estimates in Table 6 suggest that EM was mainly effective in reducing 5-year recidivism among offenders who received control visits, and monitored for longer spells, while it had a lesser impact on the others.

[21] Unfortunaly the data doesn't include past incarceration, which we find as an important mediator in the effectiveness of EM

[22] Correspondance with several practicionners suggests that such control visits are very rare today too. But unfortunaly, we don't have more recent data to support this trend.

Figure 7: Supervision and Incidents under EM



7 Conclusion

This article provides new evidence on the effects of serving a prison sentence at home under electronic monitoring, instead of incarcerated, on future crime. To achieve identification of causal treatment effects, we exploit the gradual introduction of EM in France, which started as a pilot experiment in four courts (in 2000-2002) before its progressive adoption by more and more courts. We provide strong evidence to support our main empirical strategy using cross-court variation in access to EM (which assumes no endogenous selection of courts into EM), but also show estimates using only within-court changes over time.

Our results show that simple comparisons between EM offenders and ex-prisoners highly overestimate the beneficial impact of electronic monitoring on recidivism: the estimated gap in reconviction reduces from about 14-15 percentage points to 8-9 pp after including a rich set of control variables. When we additionaly correct for selection on unobservables, using the status of courts with regard to EM as instruments (pilot court, early-adopter, late-adopter), the estimated treatment effect reduces further to 6-7 percentage points. However, this effect is robust and significant statistically as well as economically: it implies a reduction in probability of reconviction of 9% to 11%. We show that this effect, measured after 5 years, is not an artefact of short-term incapacitation at home. Actually, EM seems to foster long-term change and desistance from crime, with combined reductions in the probability and seriousness of criminal activity.

We also investigate the mechanisms driving our results. It turns out that the crime-reducing effect of EM is larger on offenders who had already experienced prison, who

were required to work under EM, and who received control visits at home. We view this as evidence that labor-market attachment matters, and that electronic monitoring can generate specific deterrence when surveillance is salient (through control visits) and when offenders know what is at stake (thanks to previous experience of prison). Interestingly, this result suggests that EM and incarceration may well complement each other and be part of an effective "policy mix".

In terms of monitoring, our results suggest that, contrary to prior research on parole outcomes (Zapryanova (2014), Georgiou (2014)), the effectiveness of EM depends on the level of supervision. However, the rapid development of EM in France after 2004-2005 coincided with a dramatic change in supervision practices, with far less control visits and shorter spells of monitoring. These recent evolutions may well weaken the current validity of our findings, and inevitaby question the effectiveness of EM today. Unfortunately, there is no recent data source on EM and recidivism to conduct such an up-to-date evaluation.

With regards to existing research on EM, our results confirm that electronic monitoring can be a very effective alternative to incarceration. However, our estimates of crime reduction are smaller than those obtained by Di Tella and Schargrodsky (2013) in Argentina and Marie (2015) in England and Wales. Several reasons may explain these differences: different prison conditions (notably between Argentina and Western countries), different composition of EM recipients (in Argentina, some EM offenders were very serious criminals for example), different judicial stages (in Argentina, EM intervenes after a few days of incarceration but before trial ; in England, EM is an early-release program for prisoners ; in France, EM is a full alternative to incarceration), and finally different supervision practices under EM (in terms of length[23], obligations, control visits, etc.).

[23]In Argentina (Di Tella and Schargrodsky, 2013), the average spell under EM lasts 420 days, compared to only 2 months in the current sample from France

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Figure 8: Map of EM roll-in in French courts (2000-2003)

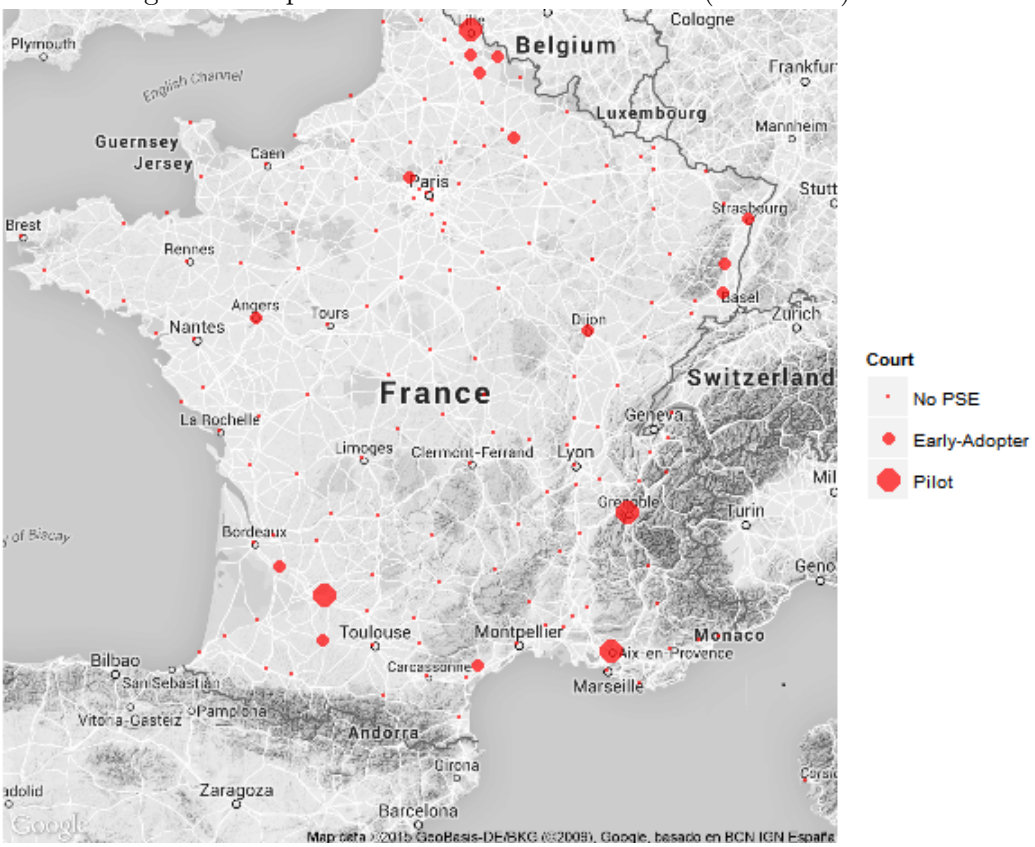

Table 8: Number of EM executed across courts by April 1^{st} 2003

Court Name	EM executed
Agen	93
Aix-en-Provence	105
Grenoble	108
Lille	90
Total Pilot courts	**396**
Béziers	25
Dunkerque	13
Pontoise	13
Cambrai	5
Auch	4
Valenciennes	4
Douai	3
Colmar	2
Dijon	2
Strasbourg	2
Hazebrouck	1
Marmande	1
Reims	1
Total Early-adopter courts	**76**
Total	**472**

Note that all French courts that are not listed here did not deliver any EM device during the 2000-2003 period, and are labelled as *late adopters*.

Figure 9: Rates of Property Crime (left) and Violent Crime (right)

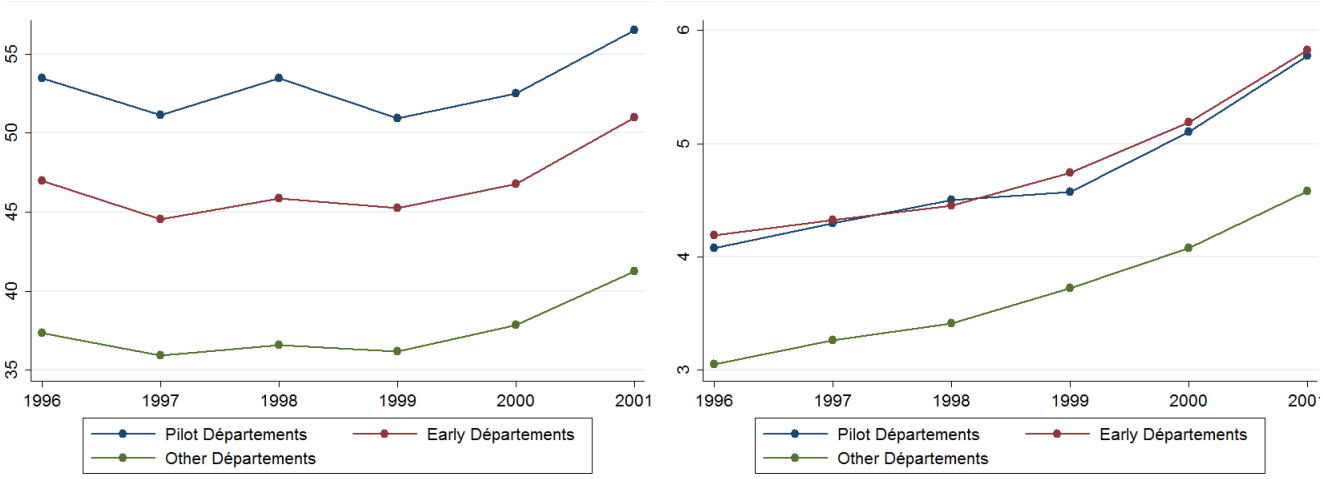

Figure 10: Prison overcrowding rates on January 1^{st}

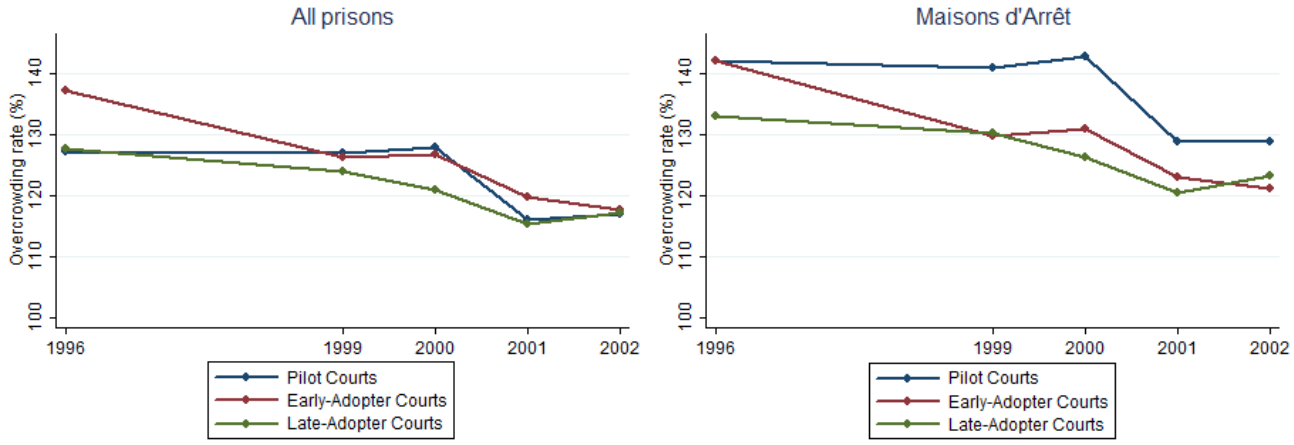

Figure 11: Density of Date of Release

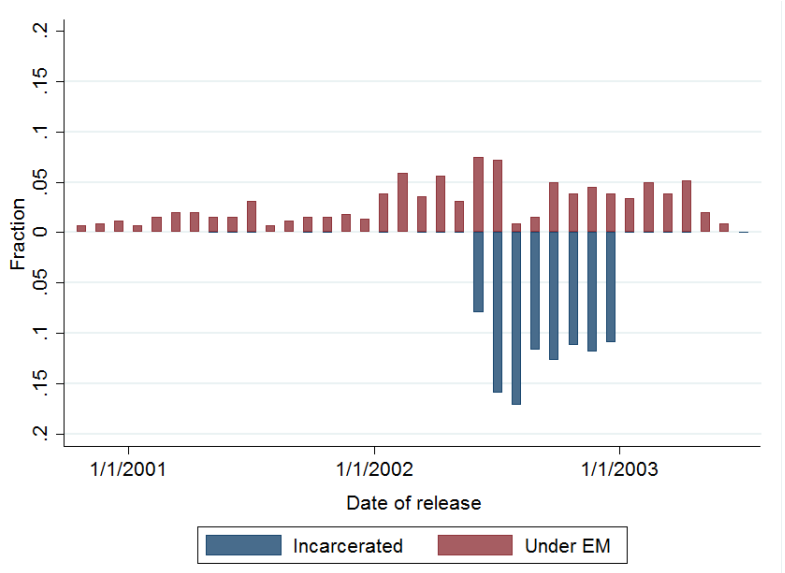

Table 9: **Differences in 5-Year Recidivism Before the Introduction of EM**

	2002 cohort	**1996-1997 cohort**
Pilot court	-0.0209 (0.0311)	-0.0142 (0.0483)
Early-adopter court	0.0113 (0.0154)	-0.0086 (0.0387)
Ref: late-adopters	0	0
Initial sentence length	x	x
Demographics	x	x
Past convictions	x	x
N	5901	2207

The table reports $\hat{\beta}$ (s.e.) from OLS regressions of 5-year recidivism after controlling for large set of individual characteristics. The 2002 sample only includes prisoners who were incarcerated before the first EM was granted in their court (if any). Prison releasees of 1996-1997 are matched to the future type of their corresponding court, based on the location of their prison. † p<10%, * p<5%, ** p<1%, *** p<0.1%

Table 10: Robustness: Electronic Monitoring and Recidivism

	Probit	Bi-Probit
100km neighboring courts	-0.0833**	-0.0773*
	(0.0299)	(0.0350)
N = 1883		
Départements FE	-0.0923**	-0.0844*
	(0.0277)	(0.0271)
N = 3185		
Released 3/1/2002 - 3/31/2003	-0.0757**	-0.0608^{+}
	(0.0290)	(0.0330)
N = 2998		
Initial sentence $\leq$ 12 months	-0.0752**	-0.0630*
	(0.0284)	(0.0317)
N = 2828		
Alt. Starting Date	-0.0826**	-0.0712*
	(0.0268)	(0.0300)
N = 3185		
Instr: Court FE	-0.0794**	-0.0718*
	(0.0274)	(0.0312)
N = 3185		
Instr: only courts with >10 EM	-0.0870**	-0.0666*
	(0.0281)	(0.0315)
N = 2994		
Instr: EM-court dummy	-0.0794**	-0.0529^{+}
	(0.0274)	(0.0315)
N = 3185		
Instr: Before/After first EM	-0.1279**	-0.0657
	(0.0425)	(0.0497)
N = 809		

Robust standard errors in parentheses, clustered at court level. $^{+}$ $p < 0.1$; * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$. All regressions include the full set of control variables. Average Marginal Effects reported.