# Criminal Recidivism after Prison and Electronic Monitoring[*]

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## Abstract

We study criminal recidivism in Argentina by focusing on the re-arrest rates of two groups: individuals released from prison and individuals released from electronic monitoring. Detainees are randomly assigned to judges, and ideological differences across judges translate into large differences in the allocation of electronic monitoring to an otherwise similar population. Using these peculiarities of the Argentine setting we argue that there is a negative causal effect on criminal recidivism of treating individuals with electronic monitoring relative to prison of the order of 15 percentage points.

JEL: K42
*Keywords*: electronic monitoring, prisons, recidivism

## I. Introduction

Every year a large number of convicted criminals are sent to prison. Given that prisons are expensive to build and run, and often involve cruel treatment of fellow citizens, possibly contributing to the conversion of inmates into 'hardened' criminals, it is unsurprising that alternatives to imprisonment have been tried out. One of the more intriguing experiments in this area is the substitution of incarceration for electronic monitoring (EM).[1] 'Electronic tagging', as it is also sometimes called, involves fitting offenders with an electronic device (on the ankle or wrist) that can be monitored remotely by employees of a correctional facility who can verify whether the individual is violating a set of pre-established conditions. One that is common is to stay at home, although in many cases a provision for attending work or school is included. Technological progress has fostered the use of these devices, making them safer, cheaper, and more complete over time (for example, recent versions of the basic device can include voice recognition and transdermal measurement of alcohol and drug consumption). By 2010, more than 500,000 people in the US and Europe alone had been 'treated' with electronic monitoring, in spite of the obvious complexity of a full cost-benefit analysis. In this paper we seek to contribute to an evaluation of electronic monitoring by providing evidence on one of the estimates needed for such an exercise: the difference between the recidivism rate of offenders formerly under electronic monitoring and the recidivism rate for offenders released from a standard prison.

Theoretically, the difference in these two recidivism rates is ambiguous. On the one hand, specific deterrence theory suggests that spending time under electronic monitoring rather than incarceration might make low punishment salient, implying a positive relationship between light punishment (electronic monitoring) and recidivism. On the other hand, several theories point out to a negative relationship. For example, imprisonment might be criminogenic through harsh prison conditions or peer effects that are not present under electronic monitoring. In particular, electronic monitoring could prevent contact with hardened criminals, or reduce the perception that society is 'mean' and 'deserving of the

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[1] See, for example, the discussions in Schwitzgebel (1969), Petersilia (1987), Schmidt and Curtis (1987), Morris and Tonry (1990), Tonry (1998), Payne and Gainey (1998) and Renzema and Mayo-Wilson (2005).

crime it receives' (one variation is in Sherman and Strang, 2007). Moreover, electronic monitoring could differ from prison in its effect on the offender's labor market prospects or social integration (perhaps through stigmatization or through an effect on the accumulation of skills).[2]

A simple comparison of recidivism rates across the prison and electronic monitoring samples, however, is typically not very informative. There are at least two practical empirical problems in trying to derive a causal estimate, one of which can be called a problem of selection and the second a problem of differential risk of the target population. The problem of selection refers to the fact that at least one potential criterion for the granting of electronic monitoring to an offender is her/his risk of recidivism. Thus, low post-release recidivism of a group of offenders treated with electronic monitoring could simply reflect the success of the legal system at the selection stage if the objective was to target 'kind' types (i.e., the group of low risk offenders within the population). The problem of the 'differential risk of the target population' refers to the possibility that electronic monitoring programs are restricted to low risk populations (for example, those whose most serious crime is drunk driving). The failure to detect a negative effect of electronic monitoring on recidivism could simply reflect that this population is already at very low risk of crime (and the control population receives a very light treatment such as short prison sentences with good prison conditions).

In practice, these and other problems have interfered with the evaluation of electronic monitoring. In a recent review by Renzema and Mayo-Wilson (2005) the authors conclude that *"applications of electronic monitoring as a tool for reducing crime are not supported by existing data."* A similar conclusion is reached in the review by Aos *et al*. (2006), who *"find that the average electronic monitoring program does not have a statistically significant effect on recidivism rates"*.

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[2] Reviewing the vast literature on these issues is beyond the scope of this paper. But many relevant aspects are covered in the recent review by Bushway and Paternoster (2009). See also Nagin (1998) on the evidence on deterrence, as well as Sherman and Berk (1984), Smith and Gartin (1989), Stafford and Warr (1993) and Piquero and Pogarsy (2002) for discussions of different aspects of deterrence. On peer effects, see, for example, Glaeser *et al*. (1996) and Bayer *et al*. (2009). An early reference on the correlation between cognitive skills and imprisonment is Banister *et al*. (1973). Stigmatization following incarceration (by the self or others) is discussed, for example, in Schwartz and Skolnick (1962).

In this paper we study electronic monitoring in the Province of Buenos Aires, Argentina. We measure recidivism through re-arrest rates of offenders treated with electronic monitoring since the program's inception in the late 1990's. As a benchmark, we take a sample of former prisoners of similar observable characteristics treated with incarceration. We find a large, negative and significant correlation between electronic monitoring and re-arrest rates.

Two features of the institutional setting we study suggest that a reasonable interpretation of our estimate is that it is the causal effect of treating an apprehended offender with electronic monitoring instead of prison. The first feature is that offenders are randomly matched to judges. And the second is that the likelihood an offender is sent to electronic monitoring instead of prison differs substantially across judges. This second feature occurs, in part, because of the usual ideological differences across judges (see, for example, Waldfogel, 1991); and in part because any ideological difference becomes exaggerated in Argentina. One likely reason for this exaggeration (and resulting ideological variance) is that the vast majority of people in Argentine prisons (and in our sample) are on pre-trial detention (i.e., offenders who have not received a final sentence in a full trial).[3] Thus, intervening judges are not passing sentences in lengthy trials but rather making a decision about the conditions for supervision until final trial. Given that this involves a minimum of information gathering, there is ample room for a judge's ideological predisposition to play a role. Additionally, Argentine jails and prisons have frequently been denounced as excessively cruel by human rights organizations (see, for example, ADC, 2006). This results in two stereotypes: judges who frequently allocate

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[3] Since individuals are not yet convicted it might be more accurate to refer to them as "presumed offenders" or "arrestees". On the extensive use of pre-trial detention in Latin America and for some comparative data, see Schonteich (2008). The widespread use of pre-trial detention is both due to the extremely slow workings of the legal system (which means that most people on pre-trial detention have not been through their first trial); and because of the contribution of several specific legal institutions in Argentina. One example is the fact that the transition detained-pre-trial to imprison only takes place when found guilty, which takes place when the appeals process has been exhausted. In contrast, in several common law countries including the US, individuals are on pre-trial detention only until their first trial. Thereafter, they are imprisoned, even if they are appealing the first sentence. Strictly speaking, the US has pre-trial detention whereas in Argentina it should be called detention prior to final sentence (although, as most detainees in our sample are waiting for their first trial, we use the term "pre-trial" detention).

electronic monitoring (often called "*garantistas*" –from their emphasis on "individual guarantees", which in the US would approximately correspond to Liberal) and judges that never do so (these are often called "*mano dura*" –literally "tough hand", which in the US would approximately correspond to Conservative). Since the assignment of judges is exogenous to prisoners' characteristics (whenever a person is detained by the police, she/he is assigned to the judge who was on duty on that day, and duty turns are assigned by a lottery), it is possible to instrument the decision to send an offender to prison or electronic monitoring with a proxy for the judge's ideology. The instrumental variable results reveal a robust, negative and significant effect of electronic monitoring on later re-arrest rates of the order of 15 percentage points.

These institutional features of the Argentine setting also ensure that electronic monitoring is applied to offenders that have committed relatively serious crimes, thus addressing the problem of differentially low risk of the target population. Note also that EM in Argentina is associated with the objective of lessening the cost of the pre-trial period. In other words, the counterfactual for the group under EM is imprisonment. This contrasts with the phenomenon of 'net widening' in the US, whereby EM is linked to an increased punitiveness of the penal system, as it is often applied to former prisoners who would have otherwise been on lower supervision (e.g, parole supervision). Finally, it is worth emphasizing that in Argentina EM does not complement other programs (education, work, anger management, drug addiction, alcohol abuse, etc) as a requisite, something that facilitates the interpretation of our treatment.[4]

Previous work on electronic monitoring using data from the US has been inconclusive. For example, Courtright *et al*. (1997) compares recidivism for drunk driving offenders treated with electronic monitoring versus those receiving jail sentences. The recidivism rates following release were extremely low for both groups (and the difference was not significant). The paper by Gainey *et al*. (2000) finds some evidence of lower recidivism

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[4] The evidence available from the US and Europe typically refers to concomitant programs, where electronic monitoring is only one of the treatments received (see, for example, Bonta *et al*. 2000, and the description in Gainey *et al*. 2000). Renzema and Mayo-Wilson (2005) discuss studies focused on groups judged to have intermediate and high risk of recidivism, which are still on the low side when compared to the groups we study.

amongst (mostly low risk) offenders who spend time under electronic monitoring, but the effect is not robust to the inclusion of control variables. Previous work has found it hard to control for the possibility that offenders treated with prison might be particularly dangerous and inherently more likely to commit crimes.[5] Renzema and Mayo-Wilson (2005) review the literature and find only two studies with random assignment and with recidivism as the dependent variable, including Petersilia and Turner (1990). Unfortunately they describe several limitations in these studies (including incomplete administration of the program) and conclude that they do not help in the evaluation of electronic monitoring.[6] An interesting paper is Marklund and Holmberg (2009), which evaluates a Swedish program that allows prisoners to apply to electronic monitoring as a substitute for prison (early release) as long as they have an occupation and they subject themselves to regular sobriety controls.[7] They find that participation in the electronic monitoring program is associated with lower recidivism.

Our paper is also related to work studying the effect of imprisonment on recidivism, where a similar selection problem is present (see, for example, Lerman, 2009, and Villettaz, Killias and Zoder, 2006). Two comprehensive reviews by Gendreau, Goggin and Cullen (1999) and by Nagin, Cullen and Jonson (2009) conclude that incarceration appears to have a null or mildly criminogenic effect on future criminal behavior, but that the evidence is not sufficiently strong to be used in policy.[8] Two recent papers by Chen

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[5] Interestingly, papers that look at re-arrest rates of people with different lengths of time on electronic monitoring (but that are all treated) suffer less from this criticism. The fragility of the results in Gainey *et al*. (2000) is thus particularly disappointing.

[6] It is worth pointing out that the sign of the bias introduced by selection problems depends on the nature of the program. For example, Finn and Muirhead-Steves (2002) describe the application of electronic monitoring to violent offenders who would otherwise have been released in Georgia, US. It is compared with a group of violent offenders who were released and finds no difference in recidivism rates. Given that this is a case of net widening, the selection problem has the opposite sign: those selected for continued supervision are potentially at a higher risk of recidivism, so the similarity in recidivism rates is consistent with socially positive effects of electronic monitoring.

[7] The average age of the EM group in that program was 38. Of them, 19% received help in finding a job, while 28% had arranged participation in a program organized by the state employment agency, with the rest having regular jobs that they had organized themselves. In comparison to the prison population sentenced to a similar term in prison (more than two years), the group of successful applicants to the EM program contained a smaller proportion of individuals with more than a single prior conviction and/or who had used drugs during their time in prison. They were also more often married or had a partner with whom they lived than the prison population.

[8] Nagin, Cullen and Jonson (2009) explain "Remarkably little is known about the effects of imprisonment on reoffending. The existing research is limited in size, in quality, in its insights into why a prison term

and Shapiro (2007) and Kuziemko (2007) pay special attention to selection and reach somewhat opposite conclusions. Chen and Shapiro (2007) exploit the fact that there is a discontinuity in the mechanism that assigns prisoners to security levels (and hence prison conditions) in the US. Thus, they are able to observe recidivism rates of former prisoners that were ex-ante very similar (i.e., on both "sides" of the cutoffs) and conclude that, if anything, harsher prison conditions lead to slightly higher recidivism rates (see also Camp and Gaes, 2009). On the other hand, Kuziemko (2007) finds that recidivism falls with time served using two different identification strategies. In one, she exploits "an over-crowding crisis" which resulted in the release of 900 prisoners on a single day, so that conditional on the original sentence, the length of time served for this group was determined by the date the sentence began. The second is a regression-discontinuity design using the variation in time served generated by cut-offs in parole-board guidelines. Katz *et al*. (2003) and Drago *et al*. (2009) also study the relationship between prison conditions and recidivism and do not find that harsher conditions reduce post-release criminal activity. See also Song and Lieb (1993), Helland and Tabarrok (2007), and Bhati and Piquero (2008).[9] It is also worth mentioning that one of the identification strategies used here, based on random assignment to judges with different ideological inclination, is not new. For example it is very much related to the one employed by Kling (2006) in his study of the effects of incarceration length on employment and earnings. He finds no consistent effect using instrumental variables for incarceration length based on randomly assigned judges with different sentencing propensities.[10]

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might be criminogenic or preventative, and in its capacity to explain why imprisonment might have differential effects depending on offenders' personal and social characteristics. Compared with noncustodial sanctions, incarceration appears to have a null or mildly criminogenic effect on future criminal behavior. This conclusion is not sufficiently firm to guide policy generally, though it casts doubt on claims that imprisonment has strong specific deterrent effects."

[9] See also Needels (1996). Iyengar (2010) discusses the difficulties in interpretation of experiments that have not been widely communicated to the public.

[10] On inter-judge variation in sentencing, see Waldfogel (1991), Payne (1997) and Anderson *et al*. (1999). Using variation in sentencing across randomly assigned judges, Green and Winik (2008) find similar recidivism rates after incarceration and probation while a recent paper by Aizer and Doyle (2011) uncovers large increases in the likelihood of adult incarceration for those incarcerated as juveniles. In a related spirit, Doyle (2008) uses the randomization of families to child protection investigators to estimate causal effects of foster care on adult crime.

The structure of the paper is as follows. Section II describes the implementation of electronic surveillance in the Province of Buenos Aires. Section III describes our data and empirical strategy. Section IV presents our main set of results, while Section V provides a welfare discussion that includes the problem of escapees. Section VI concludes.

## II. Judges, Prisons and the use of Electronic Monitoring in Argentina

In this section we discuss three important elements of the institutional and theoretical background suggesting that it is possible to use an instrumental variables strategy. In the first subsection we describe the legal process that individuals must follow, from the time they are arrested until they reach trial, with special attention to the role of the judge. In the second subsection we describe anecdotal evidence suggesting very large ideological differences across judges (this issue is formally revisited in section III after we introduce the data). Finally, in the last subsection we present a brief model with several of the features of the Argentine setting and where ideological judges determine EM allocation.

As background, it is worth noting that crime in Latin America is a major social and economic problem. Deaths due to violence in Latin America around the year 2000 were 200% higher than in North America and in the Western Pacific, 450% higher than in Western Europe, and 30% higher than in the former Communist bloc (Soares and Naritomi, 2010). Our data come from Argentina, a country with traditionally low levels of crime which has conformed to the Latin American patterns of high crime rates during the 1990's. Within Argentina, our focus is Buenos Aires, the largest and economically most significant, province with a population of almost 15 million people (about 38% of the population of the country). This province was the first place in Latin America where an electronic monitoring program was implemented.

*II.a. The Process: How Alleged Offenders end up in Prison or EM*
By 2007, the Penitentiary Service of the Province of Buenos Aires hosted a population of approximately 26,990 inmates, which represented 44.5% of the total population under

penal supervision of the whole country.[11] The conditions of confinement are extremely poor, perhaps because the inmate population held in prisons and in jails experienced a large increase (from 12,223 in 1994 to a peak of 30,721 in 2005) without a corresponding increase in infrastructure investment. By the end of our sample period in 2007, the overpopulation of the Buenos Aires penal system is estimated to be well in excess of 30%, with most of the inmates lodged in prisons (almost 88%, with only 12% lodged in jails). Figure 1 provides a picture. For the purposes of this paper, it is important to focus on three institutions of the penal system in the province of Buenos Aires: jails, prisons, and the electronic monitoring program. Jails are local facilities run by the provincial police force in close connection with the legal institutions of the judicial district (in particular the judges and prosecutors deciding on the alleged offenders which are the focus of this paper). Given that they are typically part of a police station, the ratio of officials to detainees is relatively high. This means that, even though the conditions of detention are extremely poor, they are not as chaotic as prisons. Another important feature is that, typically, detainees in local jails are relatively close to their families. Prisons, on the other hand, are run by the Province's Penitentiary System. With relatively few, large prisons in the province, alleged offenders lodged in them are often far from their relatives. Several international organizations gathered evidence of overcrowding and lack of food and hygiene in both jails and prisons, which included at least one documented example of detention in a container with neither ventilation nor sanitation services (with detainees forced to defecate in plastic bags).[12] This led to several episodes of violence and rioting, particularly in the prisons of the Province of Buenos Aires in the 1990's.[13] Recently, the annual report of an official committee in charge of monitoring the

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[11] Data in this section is compiled from several sources in the report UJ (2009). The imprisonment rate of the Province of Buenos Aires in 2007 (188 per hundred thousand population) is higher than the country's rate (156). As a reference, consider that this rate for the US is 758, 282 for Chile, 220 for Brazil, 197 for Mexico, 215 for Uruguay, 114 for Canada, 97 for France and 92 for Germany. Isla and Miguez (2003) provide an account of urban violence in Argentina using ethnographic evidence from low income areas, prisons and gangs.

[12] See, for example the recommendations of the United Nations Committee in charge of monitoring Argentina's compliance with the Convention against Torture and Cruel, Inhuman and Degrading Treatment, Conclusions and Recommendations, Argentina, 10/12/2004, CAT/C/CR/33/1, as described in ADC (2006).

[13] In one episode, which was particularly violent, the leaders of the mutiny killed and cooked several rival prisoners in the prison bakery and fed them to the prison guards without their knowledge. The editorial of a leading newspaper noted the spread of HIV/AIDS and its use as a weapon (through in-prison rape) and

Buenos Aires prisons denounces that "The prisons are schools of criminals where detainees study bad arts and graduate with more violence and social resentment."[14]

The third correctional institution is the electronic monitoring program. In late 1997, the Province of Buenos Aires pioneered in Latin America the use of electronic monitoring for the custody of inmates.[15] Under the program, offenders stay at home wearing a bracelet in their ankle.[16] The bracelet transmits a signal to a receptor installed in the offender's house. The receptor has a battery in case there is an electric stoppage. If the signal is interrupted, manipulation is detected, or vital signs of the individual are not received, the receptor sends a signal to the service provider through a telephone line. The provider investigates the reason for the signal and, whenever necessary, reports to the EM office of the Buenos Aires Penitentiary system which sends a patrol unit to the inmate's house. The contractor is the South American representative of a leading international provider. The fee paid by the provincial government in May 2007 was $32 per day (equivalent to approximately 10.4 dollars of May 2007).

The electronic monitoring program is relatively small, with a capacity of handling a maximum of 300 detainees simultaneously. A specialized division within the Province's

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remarked on the increasing violence demonstrated in one of these episodes: "never before a judge had been taken hostage and the mutineers have probably killed one of the hostages and some of the other prisoners – in this case as settlement of old *disputes*– to a degree that is also higher than the usual" (See "Mutinies and Death in Prison", *La Nación*, April 3, 1996). One apparent reason is a remarkably low staff/prisoner ratio (see Isla and Miguez, 2003). UJ (2009) reports a ratio of 1 to 50 for 2007 in the province of Buenos Aires (to be compared with a 1:1 ratio in federal prisons). On the quality of life in prisons in the province of Buenos Aires, see Alzua, Rodriguez and Villa (2010) and Borda and Pol (2007). For Latin America, see, for example, "Latin American Prisons: Inhuman Hell on Earth -Rights Violations, Violence are Rampant", *The Seattle Times*, February 17, 1997.

[14] See "The Prison, School of Criminals", *La Nación*, August 15, 2010.

[15] Other countries in the region have since adopted similar programs. A GPS monitoring system was implemented in Bogotá (Colombia) in 2009, while the Peruvian Congress approved the use of EM in 2010 (not yet implemented). The Justice Minister of Chile presented a similar proposal to Congress in 2011. In 2012, proposals are under consideration in Brazil, Uruguay and Costa Rica.

[16] Gomme (1995) explains that the first electronic monitoring device was developed by Harvard psychologist Robert Schwitzgebel as a humane and inexpensive alternative to custody. 'Dr. Schwitzgebel's Machine,' as it was called, consisted of a battery pack and a transmitter capable of emitting a signal to a receiver within a quarter-mile. In 1977, Judge Jack Love of Albuquerque, New Mexico was inspired by an episode in the Spiderman comic book series to explore the possible use of EM for offenders. Spiderman, the comic book hero, had been tagged with a device that allowed a villain to track his every move. Judge Love persuaded an electronics expert, Michael Goss, to design and manufacture a monitoring device and in 1983, Love sentenced the first offender to house arrest with EM (Gomme, 1995). See *The John Howard Society of Alberta*, 2000.

Penitentiary system, employing fewer than 20 individuals, runs it. This office receives the formal requests from the judges and allocates the bracelets on a first come, first serve basis. At its inception, electronic monitoring was granted to the old and terminally ill, with the objective of allowing them to spend their final days with their families and under house arrest. Soon, all new entries to the EM program were individuals on pre-trial detention (or, more precisely, detainees awaiting the final sentence). Buenos Aires legislation only allows the use of EM as a way to improve the conditions under which individuals await trial. Thus, the coverage shifted over time towards individuals under criminal indictment awaiting final sentence (average age in our sample is 27.4, with s.e. 5.8). In practice there were no restrictions on the types of crime and individuals accused of any crime, even rape or murder, qualified for the use of electronic monitoring (Table 1 illustrates). Given the very slow functioning legal system, the period of detention prior to the first trial and through the appeals process until a final sentence can be quite substantial so that a large proportion of individuals under the supervision of the penal system are in this category and hence qualify for EM. In the province of Buenos Aires, up to 84% of detainees were in this category during our sample period. Since its inception, and up until October 2007, more than 910 offenders had been at some point under electronic surveillance.

The theoretical process of allocating a bracelet is as follows (see Figure 2 for a timeline that summarizes the main stages). When a person is arrested by the police, the prosecutor is notified (stage 1 in Figure 2). He/she is in charge of running the investigation and ultimately presenting the accusation at the trial. Within a maximum of 48 hours, the prosecutor has to free the alleged offender or order that the apprehension be converted into a detention (if it is reasonable to think that a case against him can be built). The majority of cases are immediately converted because they involve flagrance (i.e., individuals apprehended while they commit crimes) and the alleged offender is put in the local jail (stage 2 in Figure 2).[17] Also at the time of arrest, the case is assigned to the

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[17] One informant explained that a common description of the system is that "it detains only criminals that crash into police cars". Marchisio (2004) finds that, in a sample of the cases that enter the judicial system in the city of Buenos Aires, 94% refer to criminal acts that took place in the previous 24 hours, and interprets this as evidence of the prevalence of flagrance in the system.

judge who is on duty on that day in that judicial district. Thus, the identity of the judge that will be put in charge varies depending on which court was on duty in that district on the day of the apprehension (there is only one judge in each court; we use court/judge interchangeably). One turn on duty lasts for one or two weeks and duty turns are assigned by a lottery at the judicial district (the province is geographically divided in 18 judicial districts). Thus, the allocation of alleged offenders to judges is exogenous to his/her characteristics.[18]

The prosecutor gathers the police reports and any other evidence he/she thinks might be relevant, including a description of the accusation and a report on how the detainee pleads. It also includes the alleged offender's criminal history, although it is typically limited to previous crimes committed at the local level but not crimes committed in other provinces, and only if there was a sentence. Remarkably, if he was imprisoned but freed even one day before he was sentenced he would still have a clean criminal record (remember that the vast majority of those imprisoned do not reach trial). Of course, the judge could request information on previous entries to the Province's Penitentiary System, or even entries to the systems of other provinces. This is often the case, although presumably judges that do request this information place more weight on the victim's rights (conservative judges).[19] Within 20 days, the prosecutor must present a request to the judge that the detainee is incarcerated until the trial.[20] With the alleged offender still detained in a local jail, the judge has up to 5 days to decide on this request (these are stages 3 and 4 in figure 2). If the judge grants the request, the alleged offender is transferred to a prison until trial (the term in Spanish is *prisión preventiva* -"preventive imprisonment"). The main criterion for a positive answer is that the alleged offender represents a flight risk. Legally, there is one other possible concern to the judge, which is

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[18] In theory, a criminal could find out who is the judge on duty on a given day prior to committing a crime. In practice, this possibility seems remote. First, it is not always trivial to obtain this information. Second, key informants (which included defense lawyers for low income groups) reported to have never heard of such a case. When asked to suggest how this could happen, one informant answered that it could perhaps apply to very sophisticated criminals –operating in bands – but that he himself had not heard of it. Note that drug trafficking is a federal offense and is not part of our sample.

[19] One awkward consequence of this institutional feature is that very ideological judges on the liberal end of the spectrum would end up making decisions with less information than the one we have available.

[20] In some circumstances it can be extended by another 20 days.

that the alleged offender might interfere with the investigation. This intends to cover the possibility that the alleged offender might locate and threaten the witnesses. I

In theory, the legal code allows for the use of bail. In practice, however, it appears to be rarely used. While we confirmed this from several sources, the reasons invoked varied somewhat. The most often cited reason is that there is a legal request that the amount of bail not be set independently of the offender's socio-economic status. Given that the overwhelming majority of cases involve alleged offenders of very limited economic means, this aspect of the penal code translates into a very low upper limit to the amount of bail that is permissible. Such levels of bail do not offer magistrates any reassurance that alleged offenders, often caught in flagrance, will return and subject themselves to trial. One alternative is to free alleged offenders after they make an oath to come back although, understandably, it appears to be only used in the case of crimes that are not serious, such as larceny. There are also legal restrictions to granting bail to detainees accused of serious crimes: the penal code stipulates that bail (of either type: real or through an oath) cannot be granted, with some exceptions, if the alleged crime has a minimum penalty that is over 3 years (or a maximum penalty that is over 8 years). Note that this suggests that we should tend to have relatively few people in the sample that end up in preventive prison for larceny and other minor crimes.

We note that the evidence is broadly consistent with a more infrequent use of bail for harsher crimes. For example, Table 1 suggests that relatively few prisoners have been charged with larceny and other minor crimes (only 7.6% of the sample), which is comparable to the proportion imprisoned for the most serious crimes (homicide and attempted homicide sum up to just over 7%). If less serious crimes are more prevalent than serious crimes one possible explanation for this pattern is that individuals accused of larceny are given bail and hence do not enter the database of the penitentiary system reported in Table 1. Similarly, one might expect that those that were denied bail and enter the Buenos Aires prison system accused of larceny, represent higher risks conditional on the crime category (for example, higher number of previous entries) than those accused of crimes for which bail is not used. The evidence is again consistent with this

hypothesis, as the percentage of people with criminal history that are imprisoned for larceny (43%) or other minor crimes (37%) is almost double those that are imprisoned accused of homicide (15%) or aggravated robbery (22%). This evidence is only suggestive, so we return to the issue of bail once we introduce more data in section *III.b*.

Once the judge observes that the alleged offender represents a flight risk (or might interfere with the investigation) and rules on pretrial imprisonment, he/she can decide, at his/her discretion, to "attenuate" it by granting electronic monitoring. There are three new legal requirements at this stage, which seem fairly bureaucratic (given the enormous desirability of EM relative to prison): a "technical" report on the availability of a telephone line and the suitability of the house to install EM; a "social-environmental" report on the family and neighborhood conditions; and a declaration of a family member accepting to take care of the alleged offender.[21] A real and major final problem is the possibility that EM equipment may not be available. If this is the case, the detainee is incorporated into a waiting list. The list is unique (for the whole province), with no quotas per judicial district, judge or type of crime. The sources we consulted reported that the judge does not request information on its length at the moment of deciding on EM assignment. The legal files of all the individual cases we consulted did not contain such requests of information prior to the decision to allocate EM.[22]

Note that a recurrent problem is the unavailability of space in prisons. This means that the initial period of detention in jail is sometimes extended and can last several months (until prison space opens up). Thus, in practice, such pretrial detention in jail can occur even after the judge's order of preventive imprisonment (i.e., even after stage 3 in Figure 2).

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[21] Our informants suggested that these conditions did not stand in the way of EM allocation. Note that one requirement is that the offender has access to a telephone (and this is a hard constraint in the sense that no amount of good will from social services can substitute for this). In the very first phase the system required a fixed line, although later cellular phones (with GPS incorporated to guarantee that they are physically "fixed") were also allowed. Obtaining a telephone is relatively cheap. The telephone company confirmed that within a maximum of 30 days a fixed connection could be obtained in urban areas of the Province of Buenos Aires. Obtaining a cellular phone is often cheaper (and immediate).

[22] In the Buenos Aires legal system the communications between the judge and the Provincial Penitentiary System are included in these files.

This means that, in our data, some alleged offenders on EM do not spend time in prisons (they can go directly from jail to EM).

Following the decision to imprison alleged offenders and to send some of them to EM, individuals must await trial. Given the slow functioning of the legal system, the evidence available to the prosecutor "decays", which means that it is less likely to be available during trial in its original form (one often cited example is that witnesses are more likely to forget or confuse details, or even recant their statements, as time goes by). Thus, at some point the prosecutor prefers to settle and free the alleged offender rather than go to trial with odds that appear increasingly unattractive. Given the option to go free or wait an uncertain amount of time for a trial with an uncertain outcome, most alleged offenders accept the offer (stage 5 in Figure 2). Some alleged offenders reach trial, and then exhaust the appeals process receiving a final sentence (they are those at stage 6 in Figure 2). These are not included in our sample.

*II.b. Background and some Anecdotal Evidence on Ideological Differences across Judges*
Although the Argentine legal system de jure gives less discretion to judges than in common law countries, de facto judges have ample room to express their views. Heterogeneity in views comes from a combination of ideology and practical considerations. Of particular relevance in the case of Argentina is differences across judges over what to do with individuals accused of crimes before they receive a final sentence. Indeed, given the slow rate at which individuals accused of crimes are brought to trial and receive a final sentence, a pressing decision for judges is what to do with these individuals from the moment they enter the oversight of the penal system until they are either released or receive a final sentence. Note that release comes either when offenders reach the maximum length of their potential sentence (and they would have to be released even if found guilty in a trial) or settle for time served.[23]

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[23] Note that the vast majority of cases are in the second category (they are released before receiving a final sentence).

Two extreme judicial positions have been widely reported in the media: *garantistas* vs *mano dura*, which broadly corresponds to the debate in the US between liberal and conservative judges. Liberal judges (or *garantistas*) in Argentina often take the position that poor prison conditions violate basic human rights and thus, should be rarely used for pre-trial detention. Thus, individuals that do not have exhausted the appeals process and do not have a final sentence, and hence are formally still innocent, should be either free or under minimum supervision, as long as they do not represent a flight risk or could interfere with the investigation.

On the other hand, conservative (or "*mano dura*") judges typically emphasize in their rulings the rights of victims and their families. They certainly consider prisons to be in bad shape, but not out of line with other problems in the country and largely out of their sphere of influence. Moreover, they take the position that individuals coming before them are already likely to be guilty: given that the police does not cast a very wide net, it brings to the attention of the legal system only cases where there is flagrancy or other clear evidence against the detainee. There is ample evidence on the prevalence of flagrance cases and is consistent with the fact that the system incarcerates the vast majority of those accused of criminal acts, even before they have been convicted in their first trial (see, for example, Marchisio, 2004). Interestingly, while in other countries there has been an attempt to introduce procedures that harmonize treatment, at least when it comes to sentencing, so as to remove the arbitrary component of the judge's identity (for example, sentencing guidelines have been adopted to encourage consistency of sentencing across judges in the US and the UK), these are absent in Argentina.[24]

The resulting institutional setting is one where judges have very different criteria when it comes to assigning electronic monitoring. Liberal judges regularly assign it, while conservative judges never do so. The rhetoric used is consistent with these differences. As an illustration of the liberal position consider the case of Eugenio Zaffaroni, a

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[24] In the US, for example, efforts to reduce sentencing disparities (through sentencing commissions and presumptive sentencing guidelines) led to emphasis on criminal history and offense category (see, for example, chapter 2 in Morris and Tonry, 1990). One example is the grid of the Minnesota sentencing guidelines, which gives the presumptive sentence for each offense/criminal history combination.

Supreme Court judge who explains that electronic monitoring violates basic human rights and introduces the danger that we could all be monitored in a prison-society, but that it should not be denied to individuals detained without a sentence whose only alternative is confinement in overcrowded prisons.[25] As another illustration consider Judge Schiavo, who stated that "denying electronic monitoring because a person is 'dangerous' would violate the law and the National Constitution". Judge Schiavo is noteworthy because a detainee to whom he had assigned EM in spite of a violent prior conviction (involving a triple murder) went on a killing spree in an episode known as the "Campana massacre", which led to the suspension of the EM program in October 2008 (see section *V* below).[26]

As an illustration of the conservative position, consider the statement of Judge Ramos Padilla when rejecting the pre-trial release of an individual accused of robbery, with 15 prior penal convictions: "I am unwilling to face the accused again if he were in the future to be accused of murder during a robbery, and to have to give explanations to the family of whomever might be his victim".[27] Another illustration comes from simply noting the political demands for judges to be more punitive.[28]

*II.c. A model of EM assignment*

We provide a simple model to study the decision judges' face based on the analysis of optimal punishment of Becker (1968) and, in particular, the model on a judge's tolerance

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[25] See "Electronic Monitoring is today's shackle with a Bloody Iron Ball", by Eugenio Zaffaroni in *Critica*, October 1st, 2008.

[26] Schiavo's statement to the media regarding the inadmissibility of using evidence on "dangerousness" at the time of deciding on conditions of pre-trial detention is particularly noteworthy because it was made following the Campana massacre. See, for example, "Should Judge Schiavo stand trial?" by María Helena Ripetta, Luciana Geuna and Santiago Casanello, in *Critica*, October 5th, 2008.

[27] He then added "I can't make a generalized criticism of colleagues who probably take into account the shortcomings of prison institutions, the lack of resources of the judicial system, the excessive work load, and the deficiencies in some laws, and then proceed to take responsibility for situations that, at the end of the day, correspond to other branches of the State. … each one of the branches of the State must accept its responsibilities and judges must act according to the mandate in the preamble of the Constitution, attending to the concrete realities faced by the penal system…". See "Judge rejects freedom-pending-trial and criticizes 'garantista' colleagues", in *El Dia*, Monday, October 3rd 2009.

[28] This includes former Argentine President Nestor Kirchner, who attacked magistrates for 'liberating and liberating' criminals. See, for example, "It is time for the Judicial System to put on the long trousers", in *La Nación*, Thursday, October 30th 2008. To which Supreme Court judge Eugenio Zaffaroni replied: "Some hypocrites expect that everyone is locked up and that judges act as executioners of the poor and the excluded. They ask that children are sentenced to prisons were they will be raped so that they emerge as psychopathic killers". In "Kirchner is badly mistaken", *Crítica*, November 2nd, 2008.

for error by Andreoni (1991), as well Reinganum (1988) study of the effect of sentences on pretrial bargaining. The judge is faced with an alleged offender (AO), and she must decide on pretrial detention in a prison or with EM. We first consider the case of a serious crime, one for which the judge cannot consider the possibility of releasing on bail. As in Andreoni (1991), the evidence that is presented to him/her suggests that with probability $p$ the AO is guilty. There is randomness in the amount of evidence coming before the judge, so we can take $p$ as i.i.d.

A judge that makes a correct decision has a certain enjoyment, normalized to zero. Mistakes, on the other hand, are costly. The judge is particularly concerned with two types of mistakes. On the one hand it might send an AO who ends up being innocent to prison.[29] Call this "injury to prisoner". On the other hand, assigning EM to an AO that is in fact guilty means that the judge is facilitating his/her escape. Call this second cost "flight risk".

Assume "injury to prisoner" means that the judge incurs a cost $U(\alpha k)$ of sending an innocent AO to prison who suffers in the amount $k$. And that "flight risk" means that the judge pays a cost $U(\beta f)$ of sending a guilty AO to EM whose escape costs society $f$. These are losses and we assume the judge is risk averse, so that $U(.)<0$, $U'<0$ and $U''>0$.

If the judge thinks that there is a probability $p$ of the AO being guilty, then the decision to send him to prison yields expected utility

$$EU_P = p\,0 + (1-p)\,U(\alpha k)$$

And the decision to send him to EM yields expected utility equal to

$$EU_{EM} = p\,U(\beta f) + (1-p)\,0$$

We know that the amount of evidence that a judge requires to send somebody to prison is such that $p>p^*$, where

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[29] We do not call these mistakes type I and type II errors because it is unclear what is the natural null (alleged offenders are presumed innocent and the formal aspects of the legal process reflect this; on the other hand, alleged offenders have typically been caught in flagrance, and many informal aspects of the legal process reflect a heavy presumption of guilt, particularly the decision to send alleged offenders to prison prior to the first trial).

$$p^* = \frac{U \propto k}{U \propto k \ + U \ \beta f}$$

Characterizing liberal judges as having a higher $\alpha$ and/or a lower $\beta$, we obtain

$$\frac{d\,p^*}{d \propto} > 0 \text{ and } \frac{d\,p^*}{d\beta} < 0$$

A more liberal judge (who is more sensitive about $k$, the extent of the "injury to prisoner") can be characterized as having a higher $\alpha$. In this setting she demands more convincing evidence before sending the alleged offender to prison (i.e., the cutoff $p^*$ is higher for a liberal judge). A similar reasoning leads to the same effect of lower $\beta$: a liberal judge (who is less sensitive about the social cost of escapees, f) will also demand more convincing evidence before using prison instead of EM. This is the main prediction of the model: liberal judges are more likely to assign EM than conservative judges. A similar conclusion emerges if, as in our sample, there are several judges, characterized by differences in their $\alpha$'s and $\beta$'s, all drawing alleged offenders from the same pool of individuals (with potentially similar evidence and hence distribution of $p$).

Several points are worth mentioning briefly. First, the model is focused on the utility of the judge. With a limited number of bracelets, it is possible to argue that judges should take into account that each time they assign EM they are taking up a slot that other judges might use. Such altruistic tendencies are ignored here. Similarly, the set up ignores strategic considerations (in fact, a very conservative judge might want to use EM on some cases, depending on their assessment of the ideology of other judges).

A second key feature emphasized is the common pool/sequential nature of the judge's decision-making process, without comparisons across alleged offenders, as requested by law. Indeed, it is possible that a judge might abstain from using one of the few bracelets in the hope of having one available in the future in case he/she would come across a more deserving case. We ignore this possibility in the model because, given the large number of judges, all drawing EM from the same pool of 300 bracelets, abstaining from assigning

an EM bracelets in one case does not significantly change the expected probability that the judge will be able to use it in a future case. Thus, judges deciding on a case do not take into account the possible characteristics of other cases they may face in the future. This explains why judges in our sample are observed to sometimes assign EM to serious criminals, who have served time on previous occasions, possibly contributing to the unavailability of bracelets to younger alleged offenders who may have never been in trouble before, even if this might have greater value for society. Note that the model still predicts that people with criminal history would be less likely to receive EM, and is consistent with liberal judges being more sensitive to particular types of evidence.

Third, note that the main prediction of the model does not change when there are a limited number of bracelets so that assignment by the judge does not ensure success at the allocation stage. Note that, given that the judges do not request information about bracelet availability before making the assignment decision, the cutoff $p$ is unaffected (trivially) if we replace the expected utility of EM assignment in its derivation above for

$$EU_{300EM} = zEU_{EM} + (1 - z) EU_P$$

where $z$ is the probability that one of the 300 bracelets is available at the time. The model can still be extended to the case of a series of judges deciding on several alleged offenders, but now we must assume that both liberal and conservative judges are similarly optimistic about success at the EM assignment stage (otherwise a liberal judge who faces a "mean" type might withhold EM assignment under overly optimistic expectations that in the future he might both face a more deserving type and be successful in obtaining one of the 300 bracelets).[30]

Finally, note that the analysis can readily be extended to crimes for which there is the possibility of pretrial release on bail. Indeed, note that releasing an alleged offender on bail involves a similar flight risk than putting him on EM and hence similar $f$ (the cost of prison $k$ is unchanged). Thus, we expect the cutoff $p*$ to be largely unaffected by the use

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[30] Note that the speed to which a case is brought to trial and eventually reaches a final sentence depends on judicial institutions at the judicial district and provincial level.

of bail. Liberal judges who use bail would certainly have fewer subjects in our sample (which only captures individuals released from EM or prison). Presumably, subjects released on bail would be judged to be at particularly low flight risk, so those remaining in our sample should be "mean" types (or more generally, at high risk of reoffending), suggesting an upward bias in the basic recidivism rates of subjects that stood before a liberal judge in the sample. Note that in Argentina bail mainly applies to minor crimes (see section *II.a.* and *III.b.* below).

## III. Data, Empirical Strategy and First Stage

In this section we present our data and discuss evidence that justifies the assumptions required for an instrumental variables empirical strategy.

*III.a. Data*

Our aim is to compare the effect of electronic monitoring with the effect of imprisonment on criminal recidivism. Our data were compiled from two sources within the administrative records of the Penitentiary Service of the Province of Buenos Aires. Broadly speaking, the first data source provided information regarding prison vs EM assignment, whereas the second source provided the bulk of the data on recidivism.

Specifically, the first data source, which could be called the Selection sample, provides information on the identity and characteristics of all offenders released from the Buenos Aires penal system from January 1, 1998 until October 23, 2007, which is the date when we were allowed to start collecting the data. The collection process was inordinately long, in part because the EM program was suspended in October 2008 after the Campana massacre (see section V below). From this database we obtained two groups. The first group (the electronic monitoring group) is made up of individuals whose last period under the supervision of the penal system was spent under electronic monitoring. Given that the involvement in criminal activity is mainly a male phenomenon and declines with age (see for example, Freeman, 1996 and Hansen, 2003), we focus on men below 40

years of age (born after January 1, 1957). We also exclude from the sample offenders that died while under electronic monitoring, those that were sick or characterized as dangerous, and those with missing data on the specific type of crime, the intervening judge, their birth date, their detention date, or their release date. This gives a monitoring surveillance group of 386 individuals.[31]

The second group (the comparison group) is constructed using a similar criterion. It starts with the group whose last period under the supervision of the Buenos Aires penal system was spent in prison. We first exclude a few inmates hosted in the provincial penitentiary system but under federal jurisdiction (as they would not qualify for EM). We then restrict to males below 40 years of age and exclude offenders that passed away, the sick, those characterized as dangerous, and those with missing data. We then exclude 4,788 released sentenced inmates, as there are no condemned offenders under EM.[32] This leaves a sample of 24,027 individuals who were released from prisons without a final sentence. Table 1 shows the pattern of crimes for these two populations. A unique feature of the Argentine system is immediately apparent: many of the offenders under electronic monitoring are being prosecuted for serious offenses. A second feature visible in Table 1 is the broad similarity in the distribution of alleged offenses for the EM and prison groups (we return to this theme in section *III.b.* below).

This source of data does not have information on current inmates so it is not a good source on recidivism. It only provides data on recidivism for one sub-group: those that

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[31] We also exclude a small group (of 7 individuals) who spent time under EM but later went back to prison (because they received a final sentence or because of misconduct). Note that they may distort our estimates if they are particularly "bad types" (as that would generate a selected sample of those in EM). However, a really "bad type" would escape supervision altogether and avoid being re-sent to prison. Escapees do not pose a problem as they count when they commit new crimes (see the discussion in section V below, where we also report the relatively high recidivism rate of escapees in our sample). We run robustness tests including the 7 "returnees" in our sample of EM. Even if we use the most pessimistic assumptions we find that our main results are not affected. As an illustration note that a back-of-the-envelope calculation suggests that if we count the 7 of them and then assume that they recidivate (i.e., commit crime and are apprehended and then detained and then imprisoned) at the rate of the prison sample, the recidivism of the EM sample would rise to 13.4% (from 13.21%). Alternatively, if was assume they recidivate at a rate of 100% (i.e., all of the 7 are assumed to re-offend and get apprehended by the police), the average rate would rise to 14.8%. Thus, the raw recidivism difference between EM and prison offenders would be 7.2 percentage points (instead of 9).

[32] Note the low percentage of individuals released from the Buenos Aires penitentiary system with a sentence: only 16.6%.

committed crimes but are not currently in prison (i.e., those released from prison, apprehended and currently released). Thus, our measure of recidivism is completed using a second data source, which has data on current inmates. It is not publicly available and was kept separately. When we approached the Buenos Aires Penitentiary Service with our request to access this second data source, which can be called the Recidivism sample, it was granted (after several requests) under the condition that the data was copied by hand (i.e., the files could not leave their premises). This meant that copying the information for the full sample with three research assistants was impractical. We then decided on the following matching criteria. For each prisoner in the first group (released from electronic monitoring), we identified all those prisoners with similar age (+/- 6 months), similar imprisonment date (+/- 6 months), similar imprisonment length (+/- 20 percent), same type of crime, similar judicial status (our sample only involves detainees that have been sent to prison or electronic monitoring and have been released before reaching a final sentence), and same number of episodes of previous imprisonment. Finally, from this group (the matching group of prisoners identified for each offender under EM), we randomly selected three individuals for each individual released from EM. Note that the features of the pre-trial detention setting and the fact that we can select matches from a very large group of former prisoners means that we do not run into the "curse of dimensionality" (see Nagin, Cullen and Jonson, 2009 on the practical problems in previous work that have too many variables to match).

This second source used in constructing the Recidivism sample had more detailed information (besides recidivism), which also allowed us to reconfirm the information we had already collected, and to correct multiple entries (when individuals re-offending had been given slightly different names on the second entry into the penal system). This procedure gave us complete information for a total database of 1,526 individuals (1,140 formerly in prison and 386 formerly under electronic monitoring). Note that after this detailed information is used as filter the remaining data is no longer exactly matched 3:1 (2.95:1 instead of 3:1).

*III.b. Evidence on Judge Heterogeneity and the First Stage*

A first pass at the data suggests that several individuals on EM have been accused of very serious crimes. Table 1 provides the distribution of crimes for individuals that are under pretrial EM. There are 30 individuals accused of homicide who receive electronic monitoring, which constitutes 7.8% of the EM sample. It is also worth noting that there are 224 individuals on EM that stand accused of aggravated robbery, which constitutes 58% of the EM sample. This suggests that neither the severity of the crime nor the expected recidivism risks are strong criteria for EM assignment.[33]

Table 1 also contains the types of crimes committed by the pretrial subsample of the prison population. For example, there are 1,406 imprisoned individuals accused for homicide, which is 5.85% of the sample.[34] For attempted homicide, the number is 398, or 1.66% of the sample. Given that these numbers are remarkably close to those in the EM sample (compare with 7.77% and 2.07% respectively), it is possible that the distribution of crimes is similar for the two samples.[35] Judges in our sample do not appear to be selecting the "kind" types to send to EM: there are not large differences, particularly in the more serious categories.

The observed assignment to EM is not the product of a uniform tendency of judges. Instead, some judges in the sample tend to assign EM more frequently than others. Table 2 shows that from the universe of 300 judges acting on the 24,413 cases, only a third of them have ever used electronic monitoring. Thus, two thirds of judges never used electronic monitoring when it was available to them, whereas the other third sends 2.68%

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[33] Robbery is the category with highest recidivism rate in our sample. Langan and Levin (2002) report recidivism rates in the US, by type of offense. It shows that the percent of released prisoners who, within 3 years, were re-arrested was highest for property crimes (79% for motor vehicle theft) and lowest for violent offenses (under 41% for homicide; no controls for age included). Recidivism rate for robbery was 70.2%. The classification used is not identical to that used in Argentina. Drug-trafficking is a federal offense so individuals accused of this crime are not part of our sample.

[34] We note that it is inadmissible for judges to use the type of crime (or the expected penalty) as a justification for denial of electronic monitoring (see the discussion on the ruling in the Diaz Bessone case by the Camara de Casacion Penal, "Diaz Bessone, Ramón Genaro s/recurso de inaplicabilidad de ley' - CNCP 30/10/2008).

[35] Indeed, we test if the distribution of crimes in the electronic monitoring sample is similar to the distribution of crimes in the prison sample and we cannot reject equality. This test does not take into account the severity of the crimes and weighs equally similarity in any category. For a comparison considering crime severity, see Table 4 below.

of the offenders standing before him/her to EM.[36] This is consistent with ideological judges (constrained by the 300 bracelet limit). Of course, this could also be considered a noisy indicator of the judge's inclination to use EM. For example, some judges might have used it initially by accident or to experiment or under an incomplete understanding of its implications, and subsequently decided not to use it. Alternatively, some judge's that appear as not having sent anyone to EM might have done so but were unsuccessful in obtaining it given that the capacity of the electronic monitoring program was small.

Table 3 exploits the information on the intervening judge's ideology to investigate an alleged offender's tendency to receive EM. It starts by constructing *% Judge sent to EM*, the proportion of all alleged offenders that stood before the intervening judge which were assigned to EM -excluding the individual in question. Column (1) of Table 3 reports that EM assignment (the dependent variable is a dummy equal to 1 if the alleged offender received EM) is positively correlated with *% Judge sent to EM*, controlling for judicial district fixed effects (which is at the level where the randomization occurs, by matching alleged offenders to judges). The implied effect is large: a one standard deviation increase in the judge's EM rate (0.0772) would imply an increase in the likelihood of EM assignment of 0.05 (0.6456*0.0772). Since the proportion of alleged offenders on EM in the sample used in Column (1) is 0.016 the increase is 314.16 % of the mean EM rate in our sample (0.0498/0.016=3.1416).

Column (2) includes the crime category for the most serious alleged offense (the omitted category is *Homicide*). We note that the seriousness of the alleged offense has no predictive power and, in fact, those accused of *Homicide* are if anything more likely to receive EM than those accused of less serious crimes (the difference with *Larceny*, for example, is significant). The adjusted R squared remains almost unchanged between Columns (1) and (2). As a benchmark, we note that the adjusted R squared of a baseline regression of EM assignment exclusively on judicial district fixed effects is 0.032. Similarly, when such a baseline regression includes both district fixed effects and the

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[36] In our final sample of 1,538 offenders, judges using EM are, by construction, overrepresented (of the 300 judges in the full sample, our results use evidence from approximately two thirds of them, with only 100 using EM).

alleged offense categories (but excludes % Judge sent to EM), the adjusted R squared is 0.035. Thus, our instrument contributes to the model fit considerably more than the dummies capturing the seriousness of the imputed offense (the adjusted R squared jumps from 0.032 to 0.049 versus 0.035).

Column (3) includes the alleged offender's criminal history and reveals a negative and significant coefficient. Although it is unclear why the severity of the crime should have such a different impact than criminal history in the judge's evaluation of flight risk, we note that the R squared remains unchanged.[37] Importantly for our purposes, the coefficient on *% Judge sent to EM* is unaffected by the inclusion of the crime categories as a control or the alleged offender's criminal history. Column (4) includes other personal controls that are available and reaches a similar conclusion. Interestingly, age and nationality predicts EM assignment. Finally, in Column (5) we add a proxy for the judge's ideology incorporating a time dimension: *Judge already used EM*, a dummy which equals 1 if the judge has ever previously sent an alleged offender to EM prior to facing the particular individual, and equals 0 otherwise. This instrument is also significant reinforcing that judge's preferences affect EM assignment.

Finally, we can explore more formally if the evidence is consistent with the assumption of random assignment to judges. While it is not possible to test whether alleged offenders standing in front of conservative judges are particularly "mean" (because this involves an unobservable trait), we can study if their observable characteristics differ from those of alleged offenders standing in front of liberal judges. Accordingly, Table 4 presents the observable characteristics of alleged offenders in our sample across liberal and conservative judges, where "liberal" is defined as having *% Judge sent to EM* above the median in the sample.[38] In the first column we have the unconditional mean of the covariates for conservative judges. We would like to take make comparisons taking into account district fixed effects, which is the level at which the randomization takes place. A

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[37] We note that some *garantista* judges have argued that it is unconstitutional to use the type of crime as a criterion in the allocation of EM (see the quotation of Judge Schiavo in section II.b. above; see also the Diaz Bessone case mentioned in footnote 33).

[38] As the distribution of the instrument is strongly asymmetric, with many observations at zero, we use the median as a measure of centrality instead of the mean.

simple way to present the data, following Aizer and Doyle (2011), is to have in the next column the predicted mean (adjusted to control for the effects of the judicial district dummies). These emerge from separate OLS regressions for each covariate on a dummy for whether the intervening judge was above the median (i.e., liberal) and district fixed effects. Consistent with random assignment of alleged offenders to judges on duty, the means of the cases standing in front of liberal and conservative judges are remarkably similar.

Alleged offenders are predominantly Argentine (98%), on average 27.1 years old, with similar dates of entry into the supervision of the penal system, and court size. Approximately 1 in 4 have a previous entry to the Province's Penitentiary System. Given that some offenders have multiple previous entries, the average number of previous imprisonments is 0.4.[39] Again, the difference is not significant across cases standing before judges with low and high EM rates. These differences are estimated in the "Selection sample" (24,042 observations). We also have available an indicator of the alleged offender's profession which is matched to the average income that this profession earns in the General Household Survey. The sample drops to 14,658 observations (35% drop in column 1 and 42% in column 2). Although care should be exercised given the imprecise nature of these data, column 3 reports no significant difference across the high and low EM samples. An indicator of whether the alleged offender has a spouse or partner is available for a subsample of 19,730 observations, and also reveals no significant differences across columns 1 and 2.

The most serious alleged offenses of the cases standing before the two types of judges are broadly similar. There are significant differences in only two categories, but these are small in size and do not appear to involve a clear difference in the severity (or "meanness") of the cases. In other words, the two small observed differences do not suggest that the distribution of crimes in the cases faced by High EM judges is dominated by more serious cases. The final rows of Table 4 investigate this further by comparing

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[39] This includes both previous entries that end up in a final sentence and entries where the individual spent all his time on pretrial imprisonment.

measures of the severity of the crime of the alleged offenders facing the two groups of judges. The first three measures *Serious Crimes, Middle Crimes* and *Minor Crimes* reveal no differences. Specifically, *Minor Crimes* is a dummy that equals 1 for prisoners whose most serious crime is larceny, attempted larceny or other minor crimes and 0 for any other crime. There are no significant differences. Note that this test also provides indirect evidence on the differential use of bail because cases where bail was granted do not enter our database. If bail were in fact in widespread use, liberal judges would presumably tend to use it more frequently, so that *Minor Crimes* should be higher for conservative judges. The difference (9% versus 8%) is consistent with this hypothesis, although it is small and not statistically significant. In the same spirit, we can test if within serious crimes, there are differences in the proportion of extremely serious cases facing the two groups of judges. To construct a test, we first ignore relatively light crimes (and drop observations involving anything with a shorter potential sentence than that for aggravated robbery). We then define a new variable, *AR within serious crimes*, which is a dummy that equals 1 for prisoners whose most serious crime is aggravated robbery and equals 0 for prisoners whose most serious crime is homicide, attempted homicide, sexual offenses or other serious crimes (again, note that any other crime counts as missing value in this test). The value of *AR within serious crimes* for the group of conservative judges is 0.80 (while it is 0.82 for liberal judges; difference not significant). This suggests that the cases facing the two groups of judges are similar in terms of severity, even when we restrict attention to relatively serious crimes (which is the bulk of the sample).

*III.c. Empirical Strategy*

We compare the recidivism rate of the electronic monitoring and prison population running the following regression model:

$$\text{Recidivism}_i = \alpha + \beta \, EM_i + \varepsilon_i \qquad (1)$$

where *Recidivism$_i$* is a dummy variable that indicates whether individual *i* went back to detention in the Province of Buenos Aires after his release; *EM$_i$* is a dummy variable that indicates whether individual *i* was in the electronic monitoring group. We also include as

controls type of crime dummies, age, age squared, Argentine, number of previous imprisonment, judicial district dummies, and year dummies (although note that in the recidivism regressions the sample is already matched following age, imprisonment date, imprisonment length, type of crime, and number of episodes of previous imprisonment). Robust standard errors are clustered at the judge level which is at the level at which we observe changes in the instrument.

An obvious concern with this strategy is that the allocation of electronic monitoring to offenders is potentially non-random but instead follows the type of criminal. In particular, the concern is that EM is assigned to individuals that have a "kind" type or that have a lower risk of re-offending following release, so that any difference in the recidivism rates across the EM and prison samples simply captures success at the selection stage. In order to deal with the possibility that such unobservable characteristics bias the OLS estimate downward, we rely on an instrumental variables approach, using an indicator of the judge's ideology as instrument. The main instrument we use is *% Judge sent to EM*, the percentage of alleged offenders that this judge has placed on EM, excluding the individual in question. Given random assignment of alleged offenders to judges within each district, we interpret differences in their rates of recidivism as the causal effect of the change in the likelihood of EM assignment of judges with different ideologies. We also introduce *Judge already used EM*, a dummy variable for whether the judge has previously sent an alleged offender to EM (prior to facing the offender in question), to capture a time dimension of ideology. Instruments are calculated in the original database of 24,413 alleged offenders.

## IV. Results

As a benchmark it is helpful to note that, in the raw data, the prison recidivism rate (i.e., the proportion of individuals released from prison that have returned for another crime) is 22.22%. It is 13.21% for the group released after spending time under electronic monitoring, with a difference of 9 percentage points. The period over which the

likelihood of recidivism is calculated varies across individuals depending on how early they were released from penal supervision but note that the EM and prison samples are matched. The end date is common to all individuals, as we collected our data in October 2007. On average, the post-release period in our sample is 2.86 years.[40]

*IV.a. OLS (& Probit) Results*

Table 5 presents a set of basic OLS and Probit results as reference. Column (1) runs recidivism on a dummy indicating if the person was released from electronic monitoring. The coefficient repeats the observation that the difference between the two groups is 9%. Column (2) presents the regression including the above mentioned set of controls plus judicial district and year dummies. The coefficient on EM does not change (the sample is matched along these controls, with the exception of the geographic information). Column (3) repeats the exercise with a Probit regression. Column (4) restricts the sample to offenders accused of aggravated robbery, the most common type of crime (and one for which bail is not allowed), and finds similar results. Note that

The extent of selection can be studied (albeit indirectly) by including a dummy for whether the judge ever sent an offender to EM (see Column (5) in Table 5). Indeed, there are three groups in the sample: those that went to EM, those that went to prison sent by a liberal judge, and those that were sent to prison by a conservative judge. If the liberal judges were in fact judges selecting the good types (low recidivism risk) for treatment with EM, then those standing before that same judge that were not selected for EM should be bad types (high recidivism). In particular, their average type should be worse than the average type of the conservative judges who did zero selection. In other words, the point estimate on the dummy *Judge ever uses EM* should be positive (as the base category is those that were sent to prison by judges who never sent anyone to EM). Instead, this dummy variable is insignificant, with a point estimate of -0.02. Thus, the implied estimate of the difference in recidivism across former prisoners who stood before the two different types of judges (excluding those that received EM) is suggestive of no

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[40] Thus, the average yearly recidivism rate for the prison sample (i.e., the proportion of those released from prison that are back within a year) is 7.8=22.22/2.86, while that for the EM sample is 4.6=13.21/2.86.

selection on the part of judges (although, of course, the size of the EM program would have to be larger for this test to be convincing). Using the raw data to make the same point reveals that the recidivism rate for conservative judges is 22.4% (105/469). All of these went to prison. On the other hand, there are two types of alleged offenders who faced liberal judges: those eventually released from prison (with a recidivism rate of 22.1%=151/683); and those released from EM (with a recidivism rate of 13.2%=51/386). Similar results are obtained in Column 6 of Table 5 using the same liberal/conservative classification of Table 4.

*IV.b. Using Judge Ideology to estimate the effect of EM on Recidivism*

Column (1) in Table 6 uses judge dummies as an instrument (the F-stat for the joint significance is 4.36). There are 172 judges in this sample, so this approach has limitations. Column (2) in Table 6 uses as instrument the percentage of offenders that the judge sent to electronic monitoring (excluding each particular offender). We calculate these percentages using the full sample with 24,413 offenders and restrict attention to judges with more than ten offenders. The coefficient is still negative and significant and somewhat larger in absolute size than the OLS estimate. The instrument is highly significant in the first stage. Column (3) adds a different dimension of ideology depending on how early the judge started using EM. Both instruments are statistically significant in the first stage. The second stage shows a negative and significant effect of electronic monitoring on recidivism of -0.17. In column (4), we combine these two instruments in an IV Probit regression, with similar results. Finally, in column (5), we run the same first-stage regression of column (3) not in the recidivism sample of 1,513 observations, but in the full sample of alleged offenders of the Buenos Aires penitentiary system where EM bracelets are actually assigned (exactly the regression in Column (5) of Table 3). We then use the predicted EM assignment from this full sample regression as an instrument for Electronic Monitoring in our recidivism sample.[41] All these specifications show a negative and significant effect of electronic monitoring on recidivism of around 15 percentage points. This translates into a difference of 5.3% in the average yearly

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[41] On generated instruments see Wooldridge, 2002 (pp. 115-118).

recidivism rate for the two samples (i.e., the proportion of those released from prison that are back within a year is 7.8=22.22/2.86, while that for the EM sample is 2.5=7.22/2.86).

One concern with these results is that, although electronic monitoring could reduce recidivism rates, perhaps those that do recidivate might perceive that "they got it cheap" and then commit harsher crimes (a weakening of the specific deterrence effect). We can study this issue using again the minimum and maximum penalty established by the penal code for each type of crime as a cardinal measure of crime severity, now for the recidivistic crimes committed after release from EM or prison (imputing zero for those that did not recidivate). Using our base specification of Column (3) of Table 6, the first column of Table 7 shows a significant reduction of 0.8 years in the minimum legal penalty of the recidivistic crime, while Column (2) shows a reduction of 2.7 years of prison for the maximum penalty. In Column (3), we instead consider the difference between the minimum penalty of the recidivistic and original crime. Column (4) performs the same exercise for the maximum penalty, always imputing zero for the posterior crime if the individual did not recidivate. In both cases, we again find a negative and significant effect of EM on the severity of recidivism (the effects are smaller, as the original crimes were slightly more severe for the high EM sample; see Table 4).

Table 8 presents several tests exploring the robustness of our results. Columns (1) to (4) include additional information on the prisoners collected by the Penitentiary system. Column (1) includes our measure of income, while column (2) includes an indicator on whether the individual has a spouse. The results are similar, but we note that the number of observations drop. Column (3) includes information on whether relatives visit the alleged offender in prison, while column (4) includes all the available information in one regression with similar results, although now we only have close to a third of the original sample (the jump in the coefficient on EM is due to this reduction). Column (5) controls for the total extension of pretrial detention (and a squared term), with no effect on our results. Finally, column (6) repeats the basic regression in a sample that includes only

those accused of aggravated robbery, the most common type of crime. The results are again similar.[42]

Finally, we can wonder within those assigned to electronic monitoring whether a higher proportion of time spent under EM over total detention time leads to lower recidivism. Column (1) of Table 9 suggests that is the case using the natural log of the ratio, although the statistical significance is lower when the straight ratio is used (t of 1.44).

## V. Discussion

*V.a. Escape*

In our electronic monitoring sample 66 individuals (17% of the sample) fled from the supervision of the penal system by breaking their electronic bracelets. Instead, there are no registered escapes from prison in our sample. Note that our previous estimates (and the welfare analysis that follows) already incorporate the presence of escapees. When an escapee re-offends and is apprehended, he is counted as a recidivist in our sample. Indeed, 18 of the 66 who escaped were apprehended again, for a recidivism rate of over 27%.[43] In Columns (2) and (3) of Table 9, we analyze within the electronic monitoring sample how the variables that are available to the judge (at least in principle) at the time of the allocation decision can predict recidivism or evasion. The results show that previous imprisonment is a significant predictor of both recidivism and evasion. Still, mandating EM for offenders with a previous criminal record could be defended if it were particularly effective in reducing their recidivism. However, we could not find evidence of this (we could not reject the hypothesis of equal effect of EM across the group with prior imprisonment and the rest of the sample when including an interaction term; results available upon request). Thus, Table 9 suggests that a reasonable assignment rule would

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[42] The results also show no change if we include the seven individuals who spent time under electronic monitoring but later went back to prison (see fn. 32).

[43] A different potential problem concerns differential geographic movements of escapees versus those released from prison. Escapees are theoretically (we don't have evidence on this) less likely to move around the country (or to other countries) as doing so would make them more vulnerable to routine checks by the police.

probably exclude offenders with a previous criminal record from the EM system as their evasion and recidivism rates are high.

We note that there seems to be no effect of the seriousness of the alleged offense on escape and recidivism. Still we expect a reasonable assignment rule to exclude offenders accused of serious crimes because there is by now some evidence of the political repercussions of serious crimes committed by escapees. Indeed, the EM program of the Province of Buenos Aires was highly criticized in the media and later on suspended after one Angel Fernandez, who was under EM accused of illegal possession of a handgun, killed a family of four (children aged 8 and 10) in an episode known as the "Campana massacre" in August 2008. Fernandez had a prior entry into the penal system: in 1987 he had been convicted to 25 years in prison for robbery, rape, followed by triple murder, but had been released after only 15 years because a law at the time mandated that days in prison without a final conviction count double. Similar episodes occurred in the UK in 2003 and Bogota after the program was implemented in 2009 when offenders under electronic monitoring committed new crimes.[44]

*V.b. Welfare*

A full derivation of the welfare effects of adopting an EM program are beyond the scope of this paper. However, given the centrality of the parameter estimated here in such an exercise, we can provide a back of the envelope calculation of the welfare implications of EM, although we emphasize that some of the issues we ignore (such as the value to society of having a more humane penal system) are potentially very important.[45]

Two dollar values are readily available: the cost of EM relative to prison and the cost of a crime (that is avoided by using EM). First, the cost of prison per inmate per day in the Province of Buenos Aires can be estimated at 34 dollars per day (calculations based on

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[44] See Nellis (2006); and "Criminal removed tag before horrific murder," Rochdale Observer, March 22, 2005 and "Policía sorprendió a un hombre que violó detención monitoreada con manilla del Inpec," Elespectador.com, Colombia, March 24, 2010.

[45] Quantifying the benefit to society of having a more humane penal system is difficult, in part, because it will depend on the type of beliefs that prevail in society. For work showing that people who believe luck (rather than effort) pays tend to support lighter punishments, see Di Tella and Dubra (2008).

CELS, 2011, and Gobierno de la Provincia de Buenos Aires, 2011), whereas the electronic monitoring fee paid by the provincial government to the private provider was approximately U$ 10 per day. Given that prisons in Argentina are poorly run and that the EM program is the first and only in the country, these numbers underestimate the fiscal gains of EM.[46] The average detention length in our sample of EM offenders is 660 days. This means that the EM program induced a direct fiscal savings of U$ 15,840 (= 660 days * the cost difference which is estimated at U$ 24).

The estimate for the victims' cost of crime in Argentina comes from Ronconi (2009). Using victimization surveys and different data sources, he estimates the material losses for aggravated robbery, the most common alleged crime in our sample, at U$ 508, and costs of physical and mental health and lost income of U$ 1,990, giving a total cost of this crime of U$ 2,498. With this information, we extrapolate (using the length of the punishment as a proxy for the severity) and construct the cost for each crime category. Using the distribution of crimes post prison, we come to the expected cost of a crime of U$ 1,672.

The expected social value of someone released from prison during one year is the return from work times the probability that he will work, plus the total cost of his crimes times the probability he returns to being a criminal. We use U$ 2,880 as the legal annual income from working (based on their declared professions and assuming a 20% unemployment rate). This is multiplied by the probability he will not recidivate. The second term (a negative number) is U$ 1,672 times *n*, the total number of crimes he will commit during one year times the probability of recidivism post prison. Similarly for someone released from EM. Thus, from the point of view of society, after incurring the cost of prison and EM, the expected benefit EM ($V_{EM}$) relative to prison ($V_P$) within the first year of release is $V_{EM} - V_P$, or

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[46] For the US, for example, Aos *et al.,* (2006) estimate the operating cost of imprisonment at U$ 62 per day, which add to U$ 87 considering capital costs (similarly, PEW, 2010, estimates the daily imprisonment cost in the US at U$ 80). Instead, they report that an average EM program costs less than U$4 per offender. Another example is from the state of Washington, US, where the cost of EM is U$ 5.75, while the cost of a place in jail is about $61 per day. For the United Kingdom, the fiscal costs of electronic monitoring have been on a downward trend and represent about 20% of the cost of custody (National Audit Office, 2006). Ratios between 30% and 60% have been provided for Sweden, Finland, Peru, and Chile.

$$(1 - R_E)\ 2{,}880 - R_E\ 1{,}672 * n\ -\ (1 - R_P)\ 2{,}880 - R_P\ 1{,}672 * n$$

where $R_E$ and $R_P$ are the true recidivism rates of the two groups. Our key estimate suggests that 7.8% of those released from prison are back within a year, while we expect this for only 2.5% of the EM sample (for a difference of 5.3%). Of course, the true recidivism rates are higher as only a fraction gets detained. Call $d$ the detection rate.[47] Then, computing the difference in fiscal costs, the expected benefit of EM relative to prison after one year of release is

$$15{,}840 + (2{,}880 + 1{,}672 * n)\ \frac{0.053}{d}$$

Using the victimization survey (given that reported crimes underreport true levels of crime) and estimates from the Ministry of Justice, we can obtain an estimate of $n=13$. Obtaining a defensible estimate for $d$ is harder, but we note that even with 20%, which appears implausibly large, the net benefit of having sent an alleged offender to EM (instead of prison) within just one year of release is U$ 22,363 (or 2.9 times the average GDP per capita).[48]

This estimate ignores the possibility that crimes committed by those released from EM might be less severe (some evidence that this is indeed the case is presented in Table 7). A second reason why our calculation might be an underestimate of the true benefit of EM is that we have ignored all the police and judicial costs originated by recidivistic crimes. It also ignores all the public and private pecuniary prevention costs triggered by additional crime, as well as the welfare cost of non-pecuniary prevention measures incurred by citizens (see Di Tella, Galiani and Schargrodsky, 2010).

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[47] We assume that arrest rates, conditional on having committed a crime, are similar for the two samples (on the "gambler's fallacy" applied to criminals -whereby apprehended offenders think they will have better chances of avoiding capture in the future, see Pogarsky and Piquero, 2003 and Clotfelter and Cook, 1993). However, if the electronic monitoring sample has given out more information to the penal system (for example, an address or family contacts) then the EM sample would be more likely to be re-arrested, so the correlation presented in this paper could be an underestimate of the true causal effect. Our sources from the Penitentiary Service report that this is not the case because, in their opinion, the police do not follow up their investigations in that detail but mainly focus on flagrant cases.

[48] The per capita GDP for Argentina for 2009 was U$ 7,732. This is the year of Ronconi (2009)'s estimates. Source, IMF - World Economic Outlook.

On the other hand, substitution of prison for EM might lead to reduced general deterrence effects because the punishment appears to be lower. This effect might be minor with a small program like the one implemented in Buenos Aires, but are likely to appear if EM/prison substitution is intensified (but see Lee and McCrary, 2005 for some evidence regarding myopic behavior on the part of offenders). This might be balanced if with cheaper, more humane punishment society decides to expand the supervision of the penal system (if expanding the use of EM more than compensates the reduction in prisoners).

## VI. Conclusion

All societies must decide what to do with those that commit crimes. While historically, harming apprehended criminals (e.g., amputation or the death penalty) was common, today the alternative used the most in modern democracies is prison confinement for a period of time. Although some details have changed, the basic prison technology appears to have remained basically constant since their first descriptions 3,000 years ago. Imprisonment has become common, leading some to wonder about the possible consequences and what alternatives might be available to society when dealing with a criminal offender.[49] One of the most intriguing policy proposals is to use electronic bracelets to monitor offenders. These new technologies have been becoming increasingly cheaper and safer in recent years. In this paper we seek to contribute to this debate by providing an estimate of the effect on recidivism of sentencing a person to time under electronic surveillance instead of prison.

Previous work on this issue is inconclusive (see, for example, Renzema and Mayo-Wilson, 2005, and Aos *et al.*, 2006). One of the key challenges in answering this question is that, ideally, we would like to compare similar individuals after their release from electronic monitoring and prison. This is rarely observed in practice because judicial

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[49] Indeed, at least since Jeremy Bentham, who in 1791 proposed the *Panopticon* (a glass prison where inmates could be watched continually by guards who could not be seen –see Figure 3-), society has considered how technological and institutional advances could be used to substitute for standard prisons.

allocation decisions are typically heavily influenced by the offenders "meanness" and risk of recidivism. In this paper we study the performance of an electronic monitoring program in Argentina, where EM is used to substitute for imprisonment for detainees awaiting final sentence. Alleged offenders are randomly matched to judges with different propensities to use electronic monitoring. Such differences are large in part because judges decide on imprisonment before reaching a final sentence in a trial. And in part because very bad prison conditions and the slow working of the legal system mean that even small ideological differences across judges can lead to extreme differences in the allocation of electronic monitoring. In fact, liberal leaning judges have allocated electronic monitoring to individuals accused of very serious crimes (for example homicide) and with prior records of imprisonment. Moreover, they have done so with some regularity, while others have never done it since the start of the EM program.

Exploiting the inclination of judges to assign electronic monitoring to other inmates as an instrument, our main IV estimates suggest that treating alleged offenders with electronic monitoring instead of prison induces a reduction in recidivism of about 15 percentage points. Electronic monitoring might not only be cost effective, but also reduce recidivism by avoiding the criminogenic effect of prisons.

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Figure 1: Conditions of Confinement in the Province of Buenos Aires.
Source: http://procesalpenal.wordpress.com/2007/11/17/habeas-corspus-colectivo-presentado-por-el-cels-en-causa-verbitsky-horacio-s-habeas-corpus-csjn-03052005/

Figure 2: Theoretical timeline for an alleged offender on pre-trial supervision: our sample comprises stages 1-5 only.

Prosecutor has up to 20 (+20) days to decide
if he has a case; if no, individual is freed; if
yes, prosecutor can request detention until
trial; Judge has up to 5 days to decide on
pretrial imprisonment; if no individual is
freed; if yes, individual is moved to a prison

Individual is freed prior to trial
because he settles for time
served or reaches maximum
sentence. Case closed.

Police apprehends;
prosecutor notified

1                                            3              5

In jail                          Most sent to prison
                                    Some assigned to EM

2                                            4                                   6

within 48 hours prosecutor
must request detention; if no,
individual is freed; if yes,
Judge on duty is notified and
individual is sent to jail; if
crime is non-serious, he
might be set free (under oath
to return)

Some judges assign
Electronic Monitoring (EM);
these prisoners are placed
on EM when bracelets
become available

individual brought to trial; can
appeal sentence to chamber; can
appeal to Provincial Supreme
Court; in some cases can appeal to
Supreme Court; if acquitted,
individual is freed; if convicted
individual stays in prison with a
"convicted" status; if on EM,
individual sent to prison

Figure 3: Panopticon blueprint, by Jeremy Bentham 1791.

A type of prison that allowed prisoners to be monitored at all times (without them being aware of when they are being watched). Bentham himself described the Panopticon as "a new mode of obtaining power of mind over mind, in a quantity hitherto without example".
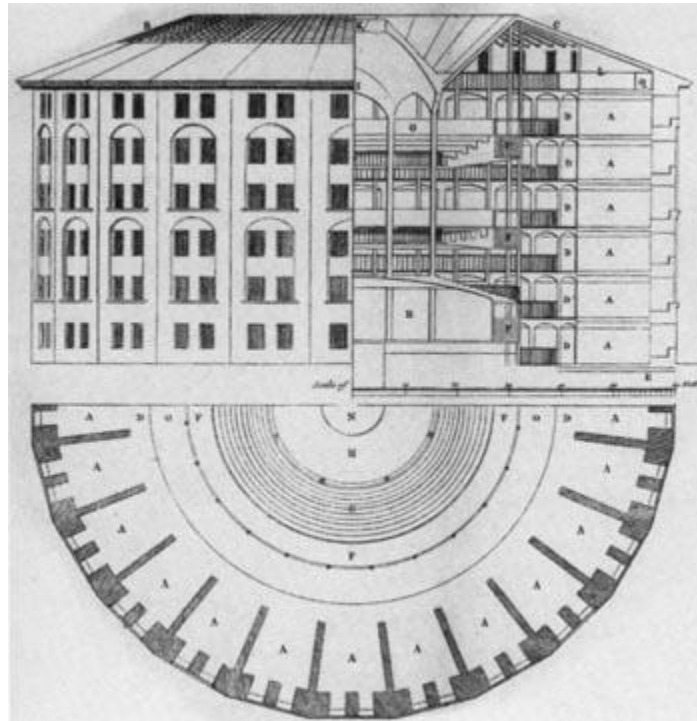
| Table 1: Type of crime for the electronic monitoring and prison population, 1998-2007 | | | | | |
|---|---|---|---|---|---|
| **Type of Crime** | **Offenders released from EM** | | **Offenders released from Prison** | | **Difference** |
| | Freq. | Percent | Freq. | Percent | Percent |
| Homicide | 30 | 7.77 | 1,406 | 5.85 | 1.92 |
| Attempted homicide | 8 | 2.07 | 398 | 1.66 | 0.41 |
| Sexual offenses | 10 | 2.59 | 450 | 1.87 | 0.72 |
| Other serious crimes | 10 | 2.59 | 484 | 2.01 | 0.58 |
| Aggravated robbery | 224 | 58.03 | 11,673 | 48.58 | 9.45 |
| Attempted aggravated robbery | 12 | 3.11 | 1,817 | 7.56 | -4.45 |
| Robbery | 25 | 6.48 | 2,935 | 12.22 | -5.74 |
| Attempted robbery | 22 | 5.7 | 1,924 | 8.01 | -2.31 |
| Possession of Firearms | 18 | 4.66 | 1,103 | 4.59 | 0.07 |
| Larceny / Attempted larceny | 4 | 1.04 | 889 | 3.7 | -2.66 |
| Other minor crimes | 23 | 5.96 | 948 | 3.95 | 2.01 |
| Total | 386 | 100 | 24,027 | 100 | |

**Notes:** Distribution by type of crime of all male alleged offenders below 40 years of age released from the Buenos Aires penal system before reaching a final sentence from January 1, 1998 until October 23, 2007.

| Table 2: Judges | |
|---|---|
| Number of judges | 300 |
| Number of judges that ever used electronic monitoring | 100 |
| % of judges that ever used electronic monitoring | 33.33% |
| average % of offenders under electronic monitoring | 1.59% |
| average % of offenders under electronic monitoring for judges that use it | 2.68% |

**Notes:** Use of electronic monitoring for the judges in charge of the 24,413 alleged offenders of Table 1.

| Table 3: Electronic Monitoring Assignment and Type of Crimes | | | | | |
|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | 5 |
| % Judge sent to EM | 0.6456*** | 0.6424*** | 0.6427*** | 0.6462*** | 0.6421*** |
| | (8.37) | (8.32) | (8.35) | (8.13) | (7.38) |
| Judge already used EM | | | | | 0.0061*** |
| | | | | | (2.64) |
| 1-Attempted homicide=1 | | -6.54e-04 | -7.01e-04 | -8.60e-04 | -8.22e-04 |
| | | (0.09) | (0.09) | (0.12) | (0.11) |
| 2-Sexual offenses=1 | | 1.40e-03 | 1.25e-03 | 1.09e-03 | 3.44e-04 |
| | | (0.15) | (0.13) | (0.12) | (0.05) |
| 3-Other serious crimes=1 | | -3.30e-04 | 3.58e-04 | 1.02e-03 | 7.24e-04 |
| | | (0.05) | (0.05) | (0.15) | (0.10) |
| 3-Aggravated robbery=1 | | -3.66e-03 | -3.27e-03 | -2.16e-03 | -2.30e-03 |
| | | (0.91) | (0.81) | (0.53) | (0.57) |
| 3-Attempted aggravated robbery=1 | | -0.01*** | -0.01*** | -0.01** | -0.01** |
| | | (3.33) | (3.22) | (2.53) | (2.53) |
| 4-Robbery=1 | | -0.01*** | -0.01*** | -0.01** | -0.01** |
| | | (2.85) | (2.60) | (1.99) | (1.99) |
| 4-Attempted robbery=1 | | -0.01** | -0.01 | -4.67e-03 | -5.03e-03 |
| | | (1.99) | (1.60) | (1.02) | (1.08) |
| 5-Larceny / Attempted larceny=1 | | -0.01*** | -0.01** | -0.01 | -0.01* |
| | | (2.68) | (2.26) | (1.60) | (1.71) |
| 5-Possession of Firearms=1 | | -0.01 | -0.01 | -0.01 | -0.01 |
| | | (1.57) | (1.24) | (1.07) | (1.12) |
| 5-Other minor crimes=1 | | 4.93e-03 | 0.01 | 0.01 | 0.01 |
| | | (0.76) | (0.94) | (1.17) | (1.21) |
| # of Previous Imprisonments | | | -4.07e-03*** | -4.82e-03*** | -4.87e-03*** |
| | | | (3.52) | (3.83) | (6.14) |
| Age | | | | -9.37e-06** | -9.57e-06** |
| | | | | (2.41) | (2.37) |
| $(Age)^2$ | | | | 4.82e-10*** | 4.95e-10** |
| | | | | (2.62) | (2.57) |
| Argentine=1 | | | | 0.01*** | 0.01*** |
| | | | | (3.06) | (3.62) |
| Judicial district dummies | Yes | Yes | Yes | Yes | Yes |
| Year dummies | No | No | No | Yes | Yes |
| Adjusted R2 | 0.048 | 0.049 | 0.049 | 0.054 | 0.048 |
| Observations | 24,342 | 24,342 | 24,342 | 24,307 | 24,025 |

**Notes:** OLS regressions. The dependent variable is a dummy that equals 1 if the offender received electronic monitoring, and 0 otherwise. *% Judge sent to EM* indicates, for each offender, the percentage of offenders of the same judge sent to electronic monitoring, excluding him. *Judge already used EM* is a dummy that equals 1 if, before the alleged offender, the judge has previously used EM, and equals 0 otherwise. The base category of crime is Homicide. *# of Previous Imprisonments* is the number of times that a prisoner has been imprisoned before. In column 5, the sample is restricted to judges with at least 10 offenders. Standard errors clustered at judge level. Absolute values of robust t statistics in parentheses. * Significant at 10%; ** significant at 5%; *** significant at 1%.

| | Table 4: Comparison of Means of Pre-Treatment Characteristics | | |
|---|---|---|---|
| **Pre-Treatment Characteristic** | **(1)** **Cases before Judges w/ Low EM Rate** | **(2)** **Cases before Judges w/High EM Rate** | **(3)** **p-value** |
| % Judge sent to EM | 6.804e-04 | 0.021 | 0.000 *** |
| Argentine=1 | 0.976 | 0.976 | 0.995 |
| Age (in days) | 9,898.93 | 9,844.39 | 0.414 |
| Entry date | 15,305.69 | 15,354.87 | 0.692 |
| Court size | 207.23 | 203.31 | 0.843 |
| Ever imprisoned=1 | 0.265 | 0.265 | 0.953 |
| # of Previous Imprisonments | 0.404 | 0.408 | 0.854 |
| Income-Profession | 1,003.87 | 1,009.47 | 0.598 |
| Spouse=1 | 0.612 | 0.604 | 0.443 |
| Homicide=1 | 0.055 | 0.055 | 0.981 |
| Attempted homicide=1 | 0.018 | 0.017 | 0.841 |
| Sexual offenses=1 | 0.021 | 0.021 | 0.996 |
| Other serious crimes=1 | 0.026 | 0.020 | 0.008 *** |
| Aggravated robbery=1 | 0.480 | 0.509 | 0.069 * |
| Attempted aggravated robbery=1 | 0.073 | 0.070 | 0.549 |
| Robbery=1 | 0.122 | 0.120 | 0.743 |
| Attempted robbery=1 | 0.078 | 0.071 | 0.211 |
| Larceny / Attempted larceny=1 | 0.037 | 0.036 | 0.739 |
| Possession of Firearms=1 | 0.045 | 0.038 | 0.123 |
| Other minor crimes=1 | 0.045 | 0.043 | 0.648 |
| Serious crimes | 0.600 | 0.623 | 0.162 |
| Middle crimes | 0.311 | 0.297 | 0.284 |
| Minor crimes | 0.089 | 0.081 | 0.214 |
| AR within serious crimes | 0.800 | 0.818 | 0.179 |

**Notes:** Categories in columns 1 and 2 defined by the median of the instrument: *% Judge sent to EM*. Column 1 is the unconditional mean. Column 2 is the predicted value from an OLS regression of the characteristic on the indicator that the judge's incarceration rate is higher than this median, controlling for district fixed effects. p-values are for the difference between the two columns, calculated using standard errors clustered at judge level. The sample is limited to judges with at least 10 offenders in the full sample. Number of observations: 24,042 (207 judges). *Income-Profession* is an estimate based on reported profession, using the General Household Survey. *Spouse* is an indicator if the alleged offender has a wife or partner. *Minor crimes* is a dummy that equals 1 for prisoners whose most serious crime is larceny, attempted larceny or other minor crimes, and 0 for any other crime. *AR within serious crimes* is a dummy that equals 1 for prisoners whose most serious crime is aggravated robbery, and equals 0 for prisoners whose most serious crime is homicide, attempted homicide, sexual offenses, or other serious crimes (note that any other crime counts as missing value in this test). * Significant at 10%; *** Significant at 1%.

| | 1 | 2 | 3 | 4 | 5 | 6 |
|---|---|---|---|---|---|---|
| **Table 5: Recidivism and Electronic Monitoring** | | | | | | |
| Electronic monitoring=1 | -0.09*** | -0.09*** | -0.09*** | -0.09*** | -0.09*** | -0.09*** |
| | (4.83) | (4.66) | (4.91) | (3.21) | (4.50) | (4.43) |
| Judge ever uses EM | | | | | -0.02 | |
| | | | | | (0.85) | |
| High EM Rate | | | | | | -0.02 |
| | | | | | | (1.06) |
| Attempted homicide=1 | | 2.46e-03 | 0.01 | | -8.37e-04 | -2.51e-04 |
| | | (0.04) | (0.16) | | (0.01) | (0.00) |
| Sexual offenses=1 | | -0.05 | -0.09 | | -0.06 | -0.06 |
| | | (1.02) | (1.15) | | (1.06) | (1.04) |
| Other serious crimes=1 | | 0.18** | 0.21** | | 0.18** | 0.17** |
| | | (2.06) | (2.17) | | (2.05) | (2.04) |
| Aggravated robbery=1 | | 2.70e-03 | 0.02 | | 1.53e-03 | 1.63e-03 |
| | | (0.08) | (0.38) | | (0.04) | (0.05) |
| Attempted aggravated robbery=1 | | -0.04 | -0.03 | | -0.04 | -0.04 |
| | | (0.85) | (0.61) | | (0.87) | (0.88) |
| Robbery=1 | | 0.01 | 0.02 | | 0.01 | 0.01 |
| | | (0.20) | (0.31) | | (0.16) | (0.17) |
| Attempted robbery=1 | | 0.03 | 0.04 | | 0.03 | 0.03 |
| | | (0.52) | (0.67) | | (0.52) | (0.52) |
| Larceny / Attempted larceny=1 | | -0.04 | -0.04 | | -0.04 | -0.04 |
| | | (0.51) | (0.46) | | (0.50) | (0.50) |
| Possession of Firearms=1 | | 0.06 | 0.10 | | 0.06 | 0.06 |
| | | (1.05) | (1.43) | | (1.01) | (1.01) |
| Other minor crimes=1 | | -0.02 | -3.16e-03 | | -0.02 | -0.02 |
| | | (0.34) | (0.05) | | (0.37) | (0.36) |
| # of Previous Imprisonments | | 0.16*** | 0.14*** | 0.14*** | 0.16*** | 0.16*** |
| | | (7.38) | (8.10) | (3.98) | (7.41) | (7.41) |
| Age | | -1.39e-04*** | -9.42e-05* | -5.64e-05 | -1.41e-04*** | -1.40e-04*** |
| | | (2.79) | (1.91) | (0.93) | (2.85) | (2.85) |
| $(Age)^2$ | | 4.96e-09** | 2.80e-09 | 1.45e-09 | 5.08e-09** | 5.03e-09** |
| | | (2.18) | (1.19) | (0.52) | (2.24) | (2.23) |
| Argentine=1 | | -0.06 | -0.07 | -0.17** | -0.06 | -0.06 |
| | | (1.03) | (1.00) | (2.03) | (1.02) | (1.02) |
| Judicial district dummies | No | Yes | Yes | Yes | Yes | Yes |
| Year dummies | No | Yes | Yes | Yes | Yes | Yes |
| Adjusted $R^2$ | 0.009 | 0.155 | | 0.169 | 0.154 | 0.155 |
| Observations | 1,526 | 1,526 | 1,513 | 854 | 1,526 | 1,526 |

**Notes:** OLS regressions (except Probit in column 3).The dependent variable is a dummy that equals 1 if the offender went back to prison for a new crime at the Province of Buenos Aires, and 0 otherwise. Marginal Probit effects presented in column 3. Sample in column 4 is restricted to offenders prosecuted for Aggravated Robbery. Absolute values of robust t (or z) statistics in parentheses. Standard errors clustered at judge level.* Significant at 10%; ** significant at 5%; *** significant at 1%.

| Table 6: Recidivism and Electronic Monitoring. IV Regressions | | | | | |
|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | 5 |
| **Second stage:** | | | | | |
| ElectronicMonitoring=1 | -0.10** | -0.14* | -0.17** | -0.11* | -0.15** |
| | (2.19) | (1.89) | (2.34) | (1.86) | (2.18) |
| Adjusted $R^2$ (second stage) | 0.16 | 0.16 | 0.15 | - | 0.15 |
| **First stage:** | | | | | |
| Set of judge dummies | Yes | | | | |
| % Judge sent to EM | | 3.09*** | 2.94*** | 3.11*** | |
| | | (9.90) | (9.19) | (7.01) | |
| Judge already used EM | | | 0.05** | 0.05 | |
| | | | (2.06) | (1.57) | |
| Large-sample estimated EM | | | | | 4.73*** |
| | | | | | (10.09) |
| Adjusted $R^2$ (first stage) | 0.30 | 0.26 | 0.26 | - | 0.26 |
| Observations | 1,503 | 1,503 | 1,503 | 1,494 | 1,503 |

**Notes:** Instrumental Variables regressions (IV Probit in column 4, 2SLS in the rest). The dependent variable is a dummy that equals 1 if the offender went back to prison for a new crime at the Province of Buenos Aires, and 0 otherwise. All the regressions include as controls type of crime dummies, age, age squared, Argentine, number of previous imprisonment, judicial district dummies, and year dummies. The sample is limited to judges with at least 10 offenders in the full sample. In column 1, the instruments are a set of dummy variables indicating the judge that tried the offender. The F-stat of the joint significance test of all the dummies in the first stage is 4.36***. In column 2, the instrument is *% Judge sent to EM*, the percentage of alleged offenders the judge sent to EM, excluding him. This and all other instruments used in this table are calculated in the original database of 24,413 alleged offenders. In column 3, we add *Judge already used EM,* a dummy that equals 1 if, before the alleged offender, the judge has previously used EM, and equals 0 otherwise. In column 4, we again combine these two instruments in an IV Probit regression (marginal Probit effects presented for the second stage). The same first-stage regression of column 3 is run in the full sample of 24,413 individuals (see column 5 of Table 3), and the predicted EM assignment is the instrument used in column 5. Absolute values of robust t (or z) statistics are in parentheses. Standard errors clustered at judge level. * Significant at 10%; ** significant at 5%; *** significant at 1%.

| Table 7: Severity of Recidivistic Crime and Electronic Monitoring. IV Regressions | | | | |
|---|---|---|---|---|
| | 1 | 2 | 3 | 4 |
| **Second stage:** | | | | |
| Electronic Monitoring=1 | -0.81** | -2.72** | -0.78* | -2.66** |
| | (2.00) | (2.42) | (1.90) | (2.33) |
| Adjusted R$^2$ (second stage) | 0.08 | 0.10 | 0.66 | 0.59 |
| **First stage:** | | | | |
| % Judge sent to EM | 2.93*** | 2.93*** | 3.04*** | 3.04*** |
| | (9.04) | (9.04) | (8.91) | (8.91) |
| Judge already used EM | 0.06** | 0.06** | 0.04 | 0.04 |
| | (2.29) | (2.29) | (1.46) | (1.46) |
| Adjusted R$^2$ (first stage) | 0.27 | 0.27 | 0.26 | 0.26 |
| Observations | 1,451 | 1,451 | 1,371 | 1,371 |

**Notes:** Instrumental Variables regressions based on column 3 of Table 6. The instruments are the *% Judge sent to EM* (the percentage of alleged offenders the judge sent to EM, excluding him) and *Judge already used EM* (a dummy that equals 1 if, before the alleged offender, the judge has previously used EM, and equals 0 otherwise). Both instruments are calculated in the original database of 24,413 alleged offenders. The sample is limited to judges with at least 10 offenders in the full sample. In column 1, the dependent variable is the minimum penalty (in years) for the new crime committed after release according to the Penal Code of the Province of Buenos Aires if the offender recidivated, and 0 otherwise. In column 2, the dependent variable is the maximum penalty for the new crime committed after release if the offender recidivated, and 0 otherwise. In column 3, the dependent variable is the minimum penalty for the new crime committed after release (0 if the offender did not recidivate) minus the minimum penalty for the original crime. In column 4, the dependent variable is the maximum penalty for the new crime committed after release (0 if the offender did not recidivate) minus the maximum penalty for the original crime. All the regressions include as controls type of crime dummies (for the original crime), age, age squared, Argentine, number of previous imprisonment, judicial district dummies, and year dummies. Absolute values of robust t statistics are in parentheses. Standard errors clustered at judge level. * Significant at 10%; ** significant at 5%; *** significant at 1%.

| Table 8: Recidivism and Electronic Monitoring. Robustness (IV regressions) | | | | | | |
|---|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | 5 | 6 |
| **Second stage:** | | | | | | |
| Electronic Monitoring=1 | -0.22*** | -0.14* | -0.15** | -0.24*** | -0.15** | -0.18* |
| | (3.16) | (1.90) | (2.18) | (3.00) | (2.16) | (1.75) |
| Income-Profession | 4.46e-05* | | | 3.81e-05 | | |
| | (1.95) | | | (1.57) | | |
| Family visits=1 | | 0.05 | | -0.01 | | |
| | | (1.37) | | (0.17) | | |
| Spouse=1 | | | -0.04 | 0.07** | | |
| | | | (1.42) | (2.38) | | |
| Total Detention Length | | | | | 1.72e-04*** | |
| | | | | | (2.61) | |
| Total Detention Length$^2$ | | | | | -4.17e-08 | |
| | | | | | (1.10) | |
| Adjusted $R^2$ (second stage) | 0.14 | 0.16 | 0.14 | 0.13 | 0.16 | 0.16 |
| **First stage:** | | | | | | |
| % Judge sent to EM | 3.29*** | 2.84*** | 3.65** | 3.38*** | 2.93*** | 2.71*** |
| | (8.38) | (9.67) | (10.25) | (8.83) | (9.17) | (6.98) |
| Judge already used EM | 0.03 | 0.03 | -4.66e-03 | -0.01 | 0.06** | 0.09*** |
| | (1.06) | (1.01) | (0.19) | (0.44) | (2.35) | (2.57) |
| Adjusted $R^2$ (first stage) | 0.28 | 0.37 | 0.26 | 0.36 | 0.26 | 0.26 |
| Observations | 941 | 1503 | 1147 | 874 | 1,503 | 829 |

**Notes:** Instrumental Variables regressions based on column 3 of Table 6. The instruments are the *% Judge sent to EM* (the percentage of alleged offenders the judge sent to EM, excluding him) and *Judge already used EM* (a dummy that equals 1 if, before the alleged offender, the judge has previously used EM, and equals 0 otherwise). Both instruments are calculated in the original database of 24,413 alleged offenders. The dependent variable is a dummy that equals 1 if the offender went back to prison for a new crime at the Province of Buenos Aires, and 0 otherwise. All the regressions include as controls type of crime dummies, age, age squared, Argentine, number of previous imprisonments, judicial district dummies, and year dummies. The sample is limited to judges with at least 10 offenders in the full sample. *Income-Profession* is an estimate based on reported profession, using the General Household Survey. *Spouse* is an indicator if the alleged offender has a wife or partner. *Family Visits* is an indicator that equals 1 if the prisoner receives a family visit. *Total Detention Length* is the amount of time under detention in prison or ME. In column 6, we only consider aggravated robbery. Absolute values of robust t statistics are in parentheses. Standard errors clustered at judge level. * Significant at 10%; ** significant at 5%; *** significant at 1%.

## Table 9: Recidivism and Escape within EM

| | Recidivism 1 | Recidivism 2 | Escape 3 |
|---|---|---|---|
| Ln (EM detention length/Total detention length - prison or ME) | -0.05** (2.09) | | |
| Attempted homicide=1 | 0.10 (0.78) | 0.10 (0.72) | -0.10 (1.05) |
| Sexual offenses=1 | 0.14 (1.00) | 0.12 (0.90) | 0.22 (1.23) |
| Other serious crimes=1 | -0.10 (1.13) | -0.10 (1.17) | 0.18 (0.99) |
| Aggravated robbery=1 | 0.00 (0.05) | 0.00 (0.01) | -0.01 (0.16) |
| Attempted aggravated robbery=1 | 0.07 (0.74) | 0.06 (0.60) | -0.10 (0.96) |
| Robbery=1 | 0.13 (1.14) | 0.13 (1.11) | -0.07 (0.50) |
| Attempted robbery=1 | -0.04 (0.34) | -0.05 (0.51) | -0.03 (0.34) |
| Larceny / Attempted larceny=1 | -0.13 (0.92) | -0.15 (1.08) | 0.05 (0.29) |
| Possession of Firearms=1 | -0.02 (0.22) | -0.04 (0.48) | 0.03 (0.26) |
| Other minor crimes=1 | -0.05 (0.50) | -0.07 (0.65) | -0.08 (0.74) |
| # of Previous Imprisonments | 0.10** (2.48) | 0.12*** (2.69) | 0.14*** (2.84) |
| Age | -7.44e-05 (0.87) | -7.47e-05 (0.86) | -3.15e-05 (0.26) |
| $(Age)^2$ | 2.59e-09 (0.66) | 2.67e-09 (0.67) | -7.96e-11 (0.01) |
| Argentine=1 | 0.23** (2.49) | 0.22** (2.05) | 0.10 (1.02) |
| Judicial district dummies | Yes | Yes | Yes |
| Year dummies | Yes | Yes | Yes |
| Adjusted $R^2$ | 0.15 | 0.14 | 0.04 |
| Observations | 386 | 386 | 386 |

**Notes:** OLS Regressions. Only for alleged offenders released from EM. In columns 1 and 2, the dependent variable is a dummy that equals 1 if the offender went back to prison for a new crime at the Province of Buenos Aires after release, and 0 otherwise. The dependent variable in column 3 is a dummy that equals 1 if the offender escaped from the electronic monitoring system, and 0 otherwise. Absolute values of robust t statistics are in parentheses. * Significant at 10%; ** significant at 5%; *** significant at 1%.