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CRIMINAL RECIDIVISM AFTER PRISON AND ELECTRONIC MONITORING

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Criminal Recidivism after Prison and Electronic Monitoring
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ABSTRACT

We study the re-arrest rates for two groups: individuals formerly in prison and individuals formerly under electronic monitoring (EM). We find that the recidivism rate of former prisoners is 22% while that for those 'treated' with electronic monitoring is 13% (40% lower). We convince ourselves that the estimates are causal using peculiarities of the Argentine setting. For example, we have almost as much information as the judges have when deciding on the allocation of EM; the program is rationed to only some offenders; and some institutional features (such as bad prison conditions) convert ideological differences across judges (to which detainees are randomly matched) into very large differences in the allocation of electronic monitoring.

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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] 'Tagging', as it is also sometimes called, involves fitting offenders with an electronic device (typically on the ankle) 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. The most common of these conditions is to stay at home, although in some cases a provision for attending work or school is included. By 2007, more than 250,000 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 (and more broadly to the debate about the effectiveness of using prisons) by providing 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 ulterior 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 crime it receives' (one variation is in Sherman and Strang, 2007). Moreover,

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

electronic monitoring could differ from prison in its effect on the improvement of skills (social, applied or cognitive) and labor market prospects.[2]

A simple comparison of recidivism rates across the prison and electronic monitoring samples, however, is typically unlikely to be 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 criterion for the granting of electronic monitoring to an offender is her/his potential risk of recidivism. Thus, low post-release recidivism of a population 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' (low risk offenders). The problem of the 'differential risk of the target population' refers to the possibility that electronic monitoring programs are applied to low risk populations (for example, drunk drivers). The failure to detect a negative effect of electronic monitoring on ulterior recidivism could simply reflect that this population is at very low risk of crime in general and that the control population receives a very light treatment (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"*.

[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 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 group 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. The correlation survives different specifications.

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 main reason is that offenders are randomly matched to judges and the likelihood an offender is sent to electronic monitoring instead of prison differs substantially across judges. This occurs, in part, because of the usual ideological differences across judges, and in part because these differences become exaggerated when liberal-leaning judges are reluctant to send offenders on pre-trial detention (i.e., who in most cases have not received a final sentence in a full trial) to Argentine prisons frequently denounced as too cruel by human rights organizations. Indeed, some judges (often called "*garantistas*" –from "individual guarantees", which in the US would approximately correspond to liberal) often send offenders to electronic monitoring whereas other judges never do so (these are often called "*mano dura*" –literally "tough hand", which in the US would approximately correspond to conservative). 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. With extreme ideological differences (and judges behaving like automata who never send an offender to prison), the process results in a correlation that can be directly interpreted as the causal effect of electronic monitoring on recidivism. Alternatively, it is possible to instrument the decision to send an offender to prison or electronic monitoring with a proxy for the judge's ideology. This also reveals a negative and significant effect of electronic monitoring on later re-arrest rates.

Two other features of this setting contribute to a causal interpretation of our findings. First, the judge in charge of allocating EM typically does so without meeting the offender

and immediately after arrest rather than after a lengthy trial. This means that the information available to the judge is very close to the information available to us, so that controlling for observables is likely to circumvent a substantial part of the selection problem. Second, we can exploit the fact that the EM program is relatively small, with capacity to supervise a maximum of 300 offenders at any point in time. Thus, even without random assignment to judges, those that receive EM are likely to be similar to other offenders sent to prison by that same judge.

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 electronic monitoring in Argentina is associated with the objective of lessening the punishment during the pre-trial period. In other words, the counterfactual for the group under electronic monitoring is incarceration. This is to be contrasted to the phenomenon of 'net widening' in the US, whereby electronic monitoring is linked to an increased punitiveness of the penal system, as it is applied to former prisoners who would have otherwise been on lower supervision (e.g, parole supervision). Finally, it is worth emphasizing that EM in Argentina 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.[3]

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 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

[3] 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.

to control for the possibility that offenders treated with prison might be particularly dangerous and inherently more likely to commit crimes.[4] 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.[5] 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.[6] 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 incarceration on recidivism, where a similar selection problem is present (see, for example, Lerman, 2009 and Villettaz, Killias and Zoder, 2006). A meta analysis by Gendreau, Goggin and Cullen (1999) covers fifty studies estimating the effect of length of time in prison on recidivism and concludes that prison produced slight increases in recidivism. Two recent papers by Chen and Shapiro (2007) and Kuziemko (2007) pays special attention to selection and reach somewhat different 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

[4] 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.

[5] 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 positive effects of electronic monitoring.

[6] The average age of the electronic monitoring group was 38. Of them, 19% received help from the probation service 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 electronic monitoring program contained a smaller proportion of individuals with more than a single prior court 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.

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.[7] 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. See also Song and Lieb (1993) and Drago, *et al* (2009), and the discussion in Gainey, *et al* (2000).[8] It is also worth mentioning that one of the main identification strategies used here, based on random assignment to judges with different ideological inclination, is not new.[9] For example it is very much related to the one recently 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.

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 discussion that includes the problem of escapees. Section VI concludes.

II. Crime and Electronic Monitoring in Argentina

Crime in Latin America is a major social and economic problem. For example, deaths due to violence in Latin America is 200% higher than in North America and in the

[7] See also Needels (1996).

[8] There are difficulties in using an experimental design to the study of the effect of punishment, in part because the public (perhaps seeking retribution) might object to standard randomization strategies. Note that in order to study deterrence effects, the public needs to be informed of penalties. See Iyengar (2010) who illustrates the difficulties in interpretation of experiments that have not been widely communicated to the public.

[9] Other papers studying measures of inter judge variation in sentencing include Waldfogel (1991), Payne (1997) and Anderson, *et al* (1999).

Western Pacific, 450% higher than in Western Europe, and 30% higher than in the former communist bloc (Soares and Naritomi, 2010). Our data comes from Argentina, a country with traditionally low levels of crime which has conformed to the Latin American patterns of high crime rates during the early 1990's. Within Argentina, we focus on the largest province, Buenos Aires, which has been the first district in Latin America to implement an electronic surveillance system for the custody of offenders. The Province of Buenos Aires is the most significant economically and the most populated of Argentina, with a population of almost 15 million people (about 37.9% of the population of the country). In 2007, the Penitentiary Service of the Province of Buenos Aires hosted a population of approximately 25,170 inmates, which represents 41.5% of the total imprisoned population of the whole country.[10]

The system of electronic monitoring (EM) in the Province of Buenos Aires started in December of 1997.[11] 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 electronic monitoring program were detainees awaiting the final sentence (Argentine legislation only allows the use of EM as a reduction in the severity of punishment to those awaiting trial). Over time the coverage shifted towards individuals under criminal indictment awaiting trial. As far as we can tell (from the data presented in this paper and from the interviews with key informants), there were no restrictions and any individual accused of any crime qualified for the use of electronic monitoring. Given the very slow functioning legal system, this period can be substantial and a large proportion of individuals under the supervision of

[10] The imprisonment rate of the Province of Buenos Aires (188 per hundred thousand population) is higher than the country's rate (156). As a reference, consider that this rate for the US is 737, 262 for Chile, 211 for Brazil, 198 for Mexico, 193 for Uruguay, 107 for Canada, 85 for France and 93 for Germany.

[11] 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 electronic monitoring 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 electronic monitoring (Gomme, 1995). As cited in a *The John Howard Society of Alberta*, 2000.

the penal system are awaiting a definitive sentence. In the province of Buenos Aires, up to 85% of detainees were in this category during our sample period. Since its inception, and up until April 2007, more than 910 men had been at some point under electronic surveillance.

The electronic monitoring system in the province of Buenos Aires consists of a bracelet worn in the ankle or wrist of the offender. The bracelet transmits a signal to a receptor installed in the inmate'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.[12] The provider tries to investigate the reason for the signal and, whenever necessary, reports to the penitentiary system which sends a patrol unit to the inmate's house.[13] The contractor is the South American representative of a leading international provider. The fee paid by the provincial government in May 2007 was $32 (approximately U$10) per month. The Buenos Aires Penitentiary Service has a small office (employing fewer than 20 employees) that is in charge of the administration..

An important factor is how the monitoring surveillance system is allocated. The surveillance program is relatively small, with a capacity of handling a maximum of 300 detainees simultaneously. The process of allocating a bracelet is as follows. When a person is arrested, the police must first decide whether to "convert" the apprehension into a detention. The overwhelming majority of cases are immediately converted (for example, because they involve flagrance, i.e., individuals apprehended while they commit crimes) and assigned to a State-appointed defense attorney, a prosecutor (in charge of the investigation) and a judge. The identity of the judge that will be put in

[12] Note that a requirement is that the offender has a telephone, so there could be a problem of selection if some offenders do not have access to a telephone. It was explained to us that in practice this does not occur, perhaps because of the enormous desirability of EM relative to prison. Obtaining a telephone is relatively cheap. We checked with the telephone company, which confirmed that within a maximum of 30 days a connection can be obtained in the province of Buenos Aires. We also obtained census data which confirmed that a significant fraction of low income people have access to telephone service. For example the 2001 census reveals that, within the lowest income group (characterized by having unmet basic needs, which accounts for 13% of the population of the province of Buenos Aires), 40% has a telephone.

[13] More modern versions, like the one introduced in Bogota, Colombia, in 2009, perform the monitoring globally through a global positioning system (GPS).

charge varies depending on who was on duty in that district on the day of the apprehension. One turn on duty lasts for one or two weeks and duty turns are assigned by a lottery.[14] Thus, the allocation of judges to prisoners is exogenous to prisoners' characteristics. With the offender under police custody, the prosecutor can ask the judge to detain the offender "preemptively" until the trial if he/she represents a flight risk (or might endanger/interfere with the investigation). While it is possible in principle for the judge to require more information for her/his decision (and even interview the offender), this is extremely rare in practice.[15] At the discretion of the judge, he/she can detain the offender at home with electronic monitoring instead of prison.[16] If there are no available equipments, then the detainee is incorporated into a waiting list. The program was discontinued in October 2008 after the Fernandez massacre (see below). Our sample period ends October 2007 (when we were allowed to start hand collecting the data).

III. Data and 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. The first data source, which was relatively easy to obtain, does not have information on recidivism but has data on other characteristics of offenders. For the purposes of inclusion in our sample, we first consider all the men that went through the Buenos Aires penal system from January 1, 1998 until October 23, 2007. Given that the involvement in

[14] A potential problem is that criminals could find out who is the judge on duty on a given day and decide their criminal behavior based on that information. In practice, it was explained to us by key informants (which included defense lawyers for low income groups) that this never occurs. When asked to suggest how this could happen, one informant answered that it could possibly apply to 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.

[15] We have explicitly asked about to several informants. None reported knowing of such a case. A system of bail is only used for economic crimes.

[16] 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 rulings on the Verbitsky case and, in particular, the Diaz Bessone case by the Camara de Casacion Penal, "Diaz Bessone, Ramón Genaro s/recurso de inaplicabilidad de ley' - CNCP 30/10/2008).

criminal activity 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).[17] This first cut leaves a sample of 43,618 men.

We then construct 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. We exclude from this group offenders that are in our sample but that died while under electronic monitoring. We also exclude those that were sick and those with missing data on the specific type of crime, their birth date, their detention date, or their release date. This gives a monitoring surveillance group of 454 individuals.

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 penal system was spent in prison. We then exclude offenders that passed away, the sick, those characterized as dangerous, and those with missing data on the specific type of crime, birth date, detention date, or release date. This leaves a sample of 37,378 individuals who were released from prisons. Table A 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.

Data on recidivism for these individuals 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, 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, same number of episodes of previous imprisonment,

[17] The average age in our sample is 27. The upper limit on 40 is a compromise between the assumption that offending peaks in the mid twenties and the findings of Piquero *et al* (2001), who show that many more offenders are on trajectories that are non-declining in age when incapacitation periods are take into account.

and with similar judicial status. Finally, from this group (the matching group of prisoners identified for each offender under EM), we randomly selected three individuals.

This second source had more detailed information (besides recidivism) which allowed us to reconfirm the information we had already collected (in particular, on the intervening judge), and correct multiple entries (when individuals re-offending had given slightly different names on the second entry into the penal system). A small group (of 7 individuals) spent time under electronic monitoring but later went back to prison (because of misconduct or because they received a final sentence) and are also excluded.[18] This procedure gave us complete information for a total database of 1,538 individuals (1,152 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 (2.98:1 instead of 3:1). The second information source also provided data on the number and type of visitors each individual had while imprisoned and a measure of their estimated income based on their profession.

III.b. Empirical Strategy

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

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

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; $Electronic\ Monitoring_i$ is a

[18] The reason for their return to prison varies across cases. Note that they may distort our estimates if they are particularly "bad types" (as that would generate a selected sample of those in electronic monitoring). 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 *IV.c.* below, where we also report data on the relatively high recidivism rate of escapees in our sample). We run some robustness tests including the 7 "returnees" in our sample of electronic monitoring. 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 all 7 of them and then assume that they all recidivate at the rate of the prison sample, the recidivism of the electronic monitoring sample would rise to 13.4% (from 13.21%). If all of the 7 are assumed to re-offend and get apprehended by the police, then the rate would rise to 14.8%. Thus, the effect of electronic monitoring would be 7.5 percentage points (instead of 9), or a fall in the recidivism rate of 33% (instead of 40%).

dummy variable that indicates whether individual *i* was in the electronic monitoring group. We also include in some specifications a set of controls (although note that in the basic regressions the sample is matched following age, time since release, detention time, previous imprisonment, type of crime and year of release).

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 electronic monitoring is assigned to individuals that have a "kind" type or that have a lower risk of re-offending following release. We provide several pieces of evidence that reasonably suggest that this is not a serious concern in our sample and suggest different approaches to obtain a causal estimate of the parameter of interest. Some of this evidence originates in differences across judges.

Institutional Features of the Argentine Context

Although the Argentine legal system gives de jure 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 (whereas in the US judicial ideology gives rise to differences in sentencing across judges). 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 as they enter the oversight of the legal system and until they receive a firm sentence, either because they have reached the end of the appeals process or because they have opted for not appealing their sentences (very few cases). Two extreme judicial positions have been widely reported in the media: *garantistas* vs *mano dura*, which, mutatis mutandis, corresponds to the debate in the US between liberal and conservative judges. A liberal judge (or *garantista*) may take the position that in Argentina prisons have poor conditions that violate basic human rights and thus, should

be used very rarely for pre-trial detention.[19] Moreover, individuals that do not have a final sentence (for example, because they have appealed their conviction in a lower court) are innocent and therefore should be either free or, theoretically, if they are unable to provide economic guarantees (individually or through a family member) that they do not represent a flight risk to the court, then with minimum supervision (because such lack of guarantees are derived from low socioeconomic status rather than actions for which individuals themselves are responsible).

On the other hand, a conservative (or "*mano dura*") judge, would emphasize the rights of victims and their families. They might also consider prisons to be in bad shape, but not out of line with other problems in the country. Moreover, he/she may take the position that individuals coming before him or her are already likely to be guilty (given that the police is unable to cast a very wide net, it brings to the attention of the legal system only cases where there is clear evidence against the detainee). This would explain why the system incarcerates the vast majority of those accused of criminal acts, even before they have been convicted in their first trial. 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.

This results in an institutional setting 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 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

[19] On overcrowding and prison conditions in the Province of Buenos Aires, see Borda and Pol (2007). See "Latin American Prisons: Inhuman Hell on Earth -Rights Violations, Violence are Rampant", *The Seattle Times*, February 17, 1997. Katz, *et al* (2003) show that even in the US, prison conditions can have a large effect on behavior.

confinement in overcrowded prisons.[20] 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 he assigned electronic monitoring to a certain Angel Fernandez, accused of illegal possession of a handgun, a relatively minor offense. While under electronic monitoring, Fernandez killed a family of four (children aged 8 and 10). 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 a murder.[21]

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".[22] Another illustration comes from simply noting the political demands for more punitive judges. A newspaper reported "Former Argentine President Nestor Kirchner followed up the assault initiated by his wife, Cristina Kirchner against the Judicial power by stating that it is time for magistrates to 'put on their long trousers' and stop 'liberating and liberating' criminals."[23]

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

[21] He had been released after only 15 years because a law at the time mandated that days in prison without a final conviction count double. Schiavo's statement about the inadmissibility of using evidence on "dangerousness" at the time of deciding on conditions of pre-trial detention was made to the media following the Fernandez affair. See, for example, "Should Judge Schiavo stand trial?", by María Helena Ripetta, Luciana Geuna and Santiago Casanello, in *Critica*, October 5th, 2008.

[22] 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 powers 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' collegues", in *El Dia*, Monday, October 3rd 2009.

[23] See, for example, "Néstor Kirchner: 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 jails were they will be raped so that they emerge as psychopathic killers". In "Kirchner is badly mistaken", *Crítica*, November 2nd, 2008.

Data on Judges in our Sample

Besides the rhetorical evidence on judge heterogeneity, we can formally examine differences in pre-trial detention practices across judges. The assignment to electronic monitoring or prison by a judge (who happens to be on duty the day of apprehension) takes place with a minimum of information, the main one being the type of crime for which an individual is accused. If these judges were selecting the "kindest" criminals for treatment with electronic monitoring (retribution logic), we would see no person accused of homicide in the EM sample.[24] Yet, the anecdotal evidence discussed above suggests this is not the case. Table A shows that this is not an isolated case. There are 36 individuals accused of homicide who receive electronic monitoring, which constitutes 7.93% of the EM sample. A t-test comfortably rejects equality to zero. This is relatively strong evidence that selection on retribution grounds is not guiding assignment.

Alternatively, if these judges were selecting the criminals with lowest risk of recidivism (deterrence logic) we would expect to see no person accused of robbery in the EM sample.[25] Yet, Table A shows that there are 246 individuals accused of aggravated robbery who receive electronic monitoring, which constitutes 54.41 % of the EM sample. A t-test comfortably rejects equality to zero. This is relatively strong evidence that selection on deterrence grounds is not guiding assignment.

Table A is also informative because it contains the types of crimes committed by the prison population.[26] For example, there are 2,687 individuals imprisoned for Homicide, which is 7.03% of the sample. For attempted homicide, the number is 545, or 1.55% of the sample. Given that these numbers are remarkably close to those in the electronic monitoring sample (compare with 7.93% and 1.76% respectively), it is possible that the

[24] Note that the "retribution" logic is being applied in a sample of individuals accused but not yet tried in court (although given the slow/inefficient legal system, only strong cases are brought forward).

[25] 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.

[26] Note that drug-trafficking is a federal offense so individuals accused of this crime are not part of our sample.

distribution of crimes is similar for the two samples.[27] The hypothesis that judges are selecting the "kind" types to send to electronic monitoring requires (at least) that the electronic monitoring sample has relatively low frequency for the more serious crimes. The evidence suggests that there are no differences in the more serious categories (in fact the point estimate is higher for the electronic monitoring sample). It is also possible to run a regression of electronic monitoring on the types of crimes (this can be done for the full sample of 37,832 offenders). For illustrative purposes, Table B presents the results of three simple OLS regressions where the dependent variable is a dummy equal to 1 if the offender received electronic monitoring at some point and 0 otherwise. Column (1) includes only a dummy equal to 1 if the court ever sent an offender to electronic monitoring. The coefficient is positive and highly significant. Column (2) repeats this estimation but includes the indicators for the type of crimes (homicide is the base category). The shaded categories are crimes that are broadly similar in terms of seriousness (defined as those categories for which the Argentine legislation provides broadly similar penalties). As can be seen, severity of the crime has no predictive power on the allocation of electronic monitoring (more appropriate estimation strategies, such as probit estimation, yield similar results). Moreover, the point estimate of *Court ever sent to EM* experiences no significant change when these controls are included (the adjusted R squared is 0.01, also unchanged; compare with 0.0005 in column 3).

Further evidence on this issue can be gathered by looking at data on judges. Table C uses data on the 199 judges in our sample. Of these, only 101 (or 50.7%) have ever used electronic monitoring. Thus, we have evidence that approximately half our sample of judges have never used electronic monitoring when it was available to them. 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 it. 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 electronic

[27] 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.

monitoring might have done so but were unsuccessful in obtaining it given that the electronic monitoring program was small. Note in the bottom half of Table B that some judges are effective in sending a high proportion of people to electronic monitoring.

IV. Results

We present three different ways of exploiting the environment described to arrive at causal estimates of the effect of electronic monitoring on recidivism. As a baseline note that a simple test of means suggest differences in recidivism between the electronic monitoring and the prison population. The recidivism rate (i.e., the proportion of individuals released from the penal system that have returned for another crime) is 22.22% for offenders released from prison, whereas it drops to 13.21% for those released after electronic monitoring.[28]

IV.a. Selection on Observables

The first identification approach exploits the fact that the judge allocating EM has to make the decision with very little information. Thus, a plausible assumption is that we have available the same amount of information as the judge has at the moment of allocation. Note that the judge is not initially sentencing the offender at a trial where the objective is to find out if the accusations are true and where judges might want to find out more about the accused. He/she is simply replying to a request by the prosecutor to keep a person detained until trial, evaluating only if the offender represents a flight risk (the only other criterion is if the offender can interfere with the investigation, and it usually plays a minor role). In principle, they could ask to interview the offender. However, we are unaware that it ever happens. In practice, the decision of the judge is relatively straightforward, perhaps because the police only bring forward a relatively small set of cases, the vast majority of which involve offenders caught during a criminal act (or, in some occasions, very shortly thereafter). Thus, a simple way to address the potential

[28] The period for which is calculated varies across individuals. On average, 7.1% (10.5%) of those released from EM (prison) re-offend and are apprehended within the first year.

selection bias in our estimates is to collect data on the observables available to the judge so as to ensure that we make comparisons across similar individuals that only differ in their prison experience.[29] Our sample is matched along types of crimes for which the offenders are being accused, which is the main variable potentially affecting the decision.[30] They are also matched for age, imprisonment date, imprisonment length, same number of episodes of previous imprisonment, and judicial status.

Column (1) in Table 1A presents an OLS regression between recidivism and a dummy indicating if the person was released from electronic monitoring. The coefficient repeats the observation that difference between the two groups is 9.01%. Column (2) repeats the regression including the above mentioned set of controls plus a set of geographic dummies. The coefficient on EM does not change (the sample is matched along these controls, with the exception of the geographic information), while those on the types of crime are consistent with what we expect from other studies (with highly significant, like robbery and attempted robbery). Column (3) repeats the exercise with a probit regression. The implied marginal effect is again 9 percentage points. Column (4) repeats the exercise restricting the sample to the most common type of crime (Robbery or Aggravated Robbery), finding similar results.

In the first three columns of Table 1B we exploit some of the added information regarding the prison population. As explained above, the judge does not seem to have (and is not supposed to use) this extra information. The approach, however, shows the robustness of the results, perhaps allowing comparison with different types of prisoners. Two dimensions are explored: income (captured through the offender's profession, converted into a monetary value using the average income earned by people with these professions in the Buenos Aires household survey) and family connections (spouse, number of visits by the spouse, number of visits by the inmate's children and number of

[29] Heckman, Ichimura, Smith and Todd (1998) show that efforts to focus on groups that are similar along observable dimensions to the group of interest (for example using propensity scores) can be as effective as random assignment. For an interesting discussion on the role of age and criminal history in sentencing, see Bushway and Piehl (2007).

[30] Note that, as emphasized by *garantista* judges (see the excerpts above), using the type of crime as a criterion in the allocation of electronic monitoring is unconstitutional (this point was later made explicitly in the so-called Diaz Bessone case mentioned above).

visits by the inmate's siblings). The results are extremely similar, though the sample size falls due to some missing data.[31]

IV.b. Capacity Limits of the Program

The electronic monitoring program is limited to 300 offenders at any one time. This is very small compared to the population in prison. The prison population is approximately 25,000 detainees, of which approximately 85% are on pre-trial detention and in principle could receive EM. Thus, the program covers only 1.4% of the population that can theoretically receive EM, with long waiting lists compiled by the Buenos Aires Penitentiary system with the requests of the judges. Offenders are placed on EM following the order in which the requests arrive.[32] Given this, and the institutional setting described above, a plausible assumption is that allocation of EM is determined by availability of one of the bracelets, which is essentially random. Note that a causal interpretation is possible even if matching to judges is non-random (and, for examples, all "bad" types go to one judge).

Thus, the OLS estimate in column (1) in Table 1A is directly interpretable as causal, now not because of successful selection on observables, but rather because of random allocation (following order of arrival) of limited EM equipment.

Perhaps, one would want to make sure that these judges are indeed sending people to EM. In this case we would want to compare those placed on EM with those released from prison, but only for the sample that stood before a judge that sent someone to EM. These judges were sensitive to the possibility of sending pre-trial detainees to EM. Column (1) in Table 2 repeats the base regression (column 2, Table 1A) but restricts the sample to offenders detained by courts that sent at least one offender to EM. The results are similar. Column (2) in Table 2 conducts a similar exercise but with the full sample and includes a

[31] The small changes in the size of the coefficient can be traced to the changing sample rather than the influence of the covariates.

[32] A potential problem is corruption: if some offenders are able to bribe their way to the top of this list our estimates would be biased (though the likely bias is down, as these would presumably be "high risk" offenders). Note, however, that after the Fernandez scandal mentioned above, which led to the interruption of the program, an investigation of the division in charge of EM of the Buenos Aires penitentiary service did not find evidence of misconduct.

dummy if the court ever sent an offender to EM. This dummy variable is insignificant, with a point estimate of -0.0035. This is helpful because we now have an estimate of the difference in recidivism across former prisoners who stood before the two different types of judges (besides those that received EM). The evidence is consistent with no selection on the part of judges. The reason is as follows. There are three groups in the sample: those that went to electronic monitoring, those that went to prison sentenced by a judge that sent someone to EM, and those that were sent to prison by a judge that never sent offenders to EM. If the liberal judges were in fact judges selecting the good types (low recidivism risks) for treatment with EM, then those 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 *Court ever uses EM* should be positive (as the base category in column 2 is those that were sent to prison by judges who never sent anyone to EM). The Table below makes the same point using the raw data on recidivism rates.

Liberal Judges	Conservative Judges
EM=13.21% (51/386) Prison= 22.11% (151/683)	Prison= 22.39% (105/469)

IV.c. Fully Ideological Judges

Differences across judges in the amount of people sent to EM arise in their ideological differences. Given the details of the decision (for example, involving pre-trial detention) it is possible that judges that assign EM do so as a matter of principle. Thus, it is possible that they act like automata, sending to EM all offenders that come before them. There is still the question of why they do not send 100% of offenders to EM, but given the small size of the program (300 at any one time) this is to be expected. If this "automata" assumption is accepted then we have a situation where the assignment of electronic

monitoring is fully determined by the luck of the draw of which judge happened to be on duty at the time and place of apprehension.

Thus, the OLS estimate in column (1) in Table 1A is then directly interpretable as causal, now not because of successful selection on observables, nor because of the mechanical limit on program capacity, but rather because of random allocation across ideologically different judges who do not select offenders as a matter of principle.

Perhaps, one would want to restrict the sample to offenders that stood before judges that sent at least 10% of their offenders to electronic monitoring. This is the group of judges in our sample that were most likely to be ideological and to behave like automata, sending all offenders to EM instead of prison, regardless of their type. The effect of electronic monitoring on recidivism is still negative and significant, with a point estimate of -0.12 (results available upon request). Most interesting is the fact that we can reject the hypothesis of a lower point estimate relative to column (1) in Table 1A. If judges that sent offenders to EM are really selecting, then those in this group (the group that most frequently sends) should be the least likely to be selecting (with the success or not in obtaining electronic monitoring status being determined by availability of the bracelets). Therefore, the ideological judges are the most likely to have the smallest difference in recidivism between those with EM and those sent to prison. Thus, the point estimate should be smaller in absolute value than the one in column (2) in Table 1A (which is the opposite of what we observe).

IV.d. Somewhat Ideological Judges

As mentioned above, some judges might have had a fuzzy understanding of how the system worked and its implications so the automata assumption used above is too strong. Thus, even if the capacity of the EM program was substantially larger, judges would not fully take up all of the available bracelets because they are exercising some discretion. This suggests that an instrumental variable strategy can be used using the ideological differences across judges in the first stage predicting the decision to send offenders to electronic monitoring.

Column (1) in Table 3A uses court dummies as an instrument (the F-stat for their joint significance is 3.96). There are 199 judges in our sample, so this approach has limitations. Column (2) in Table 3A uses as instrument the percentage of offenders that the court sent to electronic monitoring (excluding the particular offender). We calculate this using the full sample with almost 37,832 offenders and restrict attention to courts with more than ten offenders in the sample (sample size falls by 25 -to 1,513). The coefficient is still negative and significant and somewhat larger in absolute size than the OLS estimate. Column (3) explores a small variation of this instrument by including a dummy for whether the court ever sent a prisoner to electronic monitoring (which is a more flexible approach).

In column (4) we use a different dimension of ideology depending on how early the court started using EM. Specifically, the instrument is *Court has already sent to EM*, a dummy which equals 1 if the court has ever previously sent an offender to electronic monitoring, and equals 0 otherwise. In column (5), we combine the two instruments. In column (6), we again combine these two instruments in an IV Probit regression, with similar results.

Table 3B provides some robustness results, using our base specification (the two instruments in all regressions in this table are: the % of inmates of the same court sent to electronic monitoring, excluding himself, and a dummy which, for each offender, equals 1 if before himself, the court has ever previously sent an offender to electronic monitoring, and equals 0 otherwise). The covariates used are similar to those explored in Table 1B above. The conclusion that electronic monitoring causes a reduction in recidivism is robust. We also experimented with alternative measures of our treatment with similar results.[33]

[33] For example, rather than a dummy for whether the individual was in electronic monitoring we used the proportion of the sentence that was spent under electronic monitoring.

V. Escape and Discussion

Finally, with a system of (close to) random allocation of electronic monitoring, we should expect a considerable amount of escape. Indeed, in our sample 66 individuals (17% of the sample) flee by breaking their electronic bracelets and evading from the supervision of the penal system altogether. In terms of our estimates this introduces a potential problem because the subsample containing the worst types in the electronic monitoring sample spends more time out of supervision and has more time to offend again. Indeed, 18 of the 66 who escape are apprehended again, for a recidivism rate of over 27%. Thus, our basic coefficient of interest (e.g, -0.9 in Table 1A) could be an underestimate of the true causal effect of treating an offender with EM instead of prison.

There is also the possibility of generating rules that might improve the system. Table 4A presents the observable characteristics of these escaped offenders. Of them, 19 (almost 29%) had been imprisoned before (this was at least their second entry into the penal system). This is tested more formally in Table 4B where the probability of escape of the electronic monitoring sample is estimated against the observables to the judge at the time of assignment of EM. We analyze how the variables that were observable to the judge at the time of the allocation decision predict recidivism or evasion from electronic monitoring. The results show that previous imprisonment is a significant predictor of both recidivism and evasion.

Although electronic monitoring could be particularly effective in reducing the recidivism of offenders with a previous criminal record, their evasion and recidivism rates are high.[34] Also for retribution reasons, we might expect offenders accused of homicide, attempted homicide and rape to be excluded. When we repeat the base regression (column 2 in Table 1A) but limiting the sample to exclude these 4 groups (those with previous imprisonment and those accused of homicide, attempted homicide or rape), the

[34] Granting EM to groups with high recidivism or escape rates would certainly be unpopular. Also, we could not find evidence that EM is particularly effective in reducing the recidivism rates of offenders with prior imprisonment (we could not reject the hypothesis of equal effect of EM across the group with prior imprisonment and the rest of the sample; results available upon request).

estimated reduction in recidivism from treatment with electronic monitoring is still 9 percentage points (-0.088 with a t-stat of 3.8).

Note that in our study arrest rates are similar for the two samples, with only harshness of punishment varying (so differential recidivism cannot be explained by the "gambler's fallacy" -whereby apprehended offenders think they will have better chances of avoiding capture in the future, see Pogarsky and Piquero, 2003). However, if the electronic monitoring sample has given out more information to the penal system (for example, an address or family contacts) then the electronic monitoring sample would be more likely to be re-arrested, so our correlation is an underestimate of the true causal effect.[35]

Finally, it is worth noting that several dimensions (beyond recidivism) matter when society makes these decisions. On the one hand, it is hard to quantify the benefit to society of having a more humane penal system, in part because this number will depend on the type of beliefs that prevail in society.[36] Even on the fiscal side, precise numbers seem hard to calculate, although it is clear that electronic monitoring could be considerably cheaper than sending people to prison.[37] Note that given the explosion of people in prison in some countries (like the US) fiscal considerations will likely play an important role in favoring the adoption of electronic monitoring. As a final example of the difficulty in the decision to adopt such a program, note that a proper estimation of the deterrent effect of putting people on electronic monitoring instead of prison should take into account the possibility that a much larger fraction of the population might be put under the supervision of the penal system without building new prisons. In this paper we do not address these important questions.

[35] Our informants 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.

[36] For example, if people believe luck (rather than effort) pays, they will tend to support lighter punishments. See Di Tella and Dubra (2008) for a model and some evidence.

[37] Although simple comparison of costs of prison *vs* electronic monitoring requires assumptions about the activities that will be allowed under electronic monitoring (if work is allowed, even labor taxes and the substitution of social services have to be computed), and the proportion of the total cost paid by the offenders under monitoring (in the US this is often a significant fraction), amongst others.

VI. Conclusion

All societies must decide what to do with those that commit crimes. Historically, one approach has been to harm individuals guilty of certain crimes. For example, corporal punishment, amputation and even death have all been used as part of the penal system around the world. The alternative used the most in modern democracies, however, is prison confinement for a fixed period of time. Although some small details may have changed, the basic technology used in prisons appears to have remained constant since their first descriptions (for example in the Old Testament). This approach has become common, leading some to wonder about the possible consequences of using prisons and what alternatives might be available to society when dealing with a criminal offender. Indeed, at least since Jeremy Bentham, who in 1791 proposed the *Panopticon* -a glass jail where inmates could be watched continually by guards who could not be seen-, society has considered how technological and institutional advances could be used to substitute for prisons. One of the most intriguing policy proposals in recent years is to use electronic bracelets to monitor offenders. Electronic monitoring is an increasingly popular form of surveillance, with over 250,000 offenders having passed through the system in the US and Europe by 2007. 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). 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 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 it is used to substitute for imprisonment for detainees awaiting final sentence. Three features of the institutional setting we study in Argentina help with a causal interpretation of our main estimate. First, judges make the decision before passing the final sentence in a trial, so the amount of information they have is very limited. In most cases the

information available to the judges is similar to the one we have, so a strategy based on selection on observables is possible. Second, the capacity of the program is limited to only 300 bracelets at any one time. Third, 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.

We find that recidivism rates among offenders under electronic monitoring are 9 percentage points lower than offenders who went to prison. Since recidivism rates for former prisoners is close to 22.2 percentage points on average, the causal effect of electronic monitoring on recidivism is approximately 40.5%.

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Figure 1: 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".

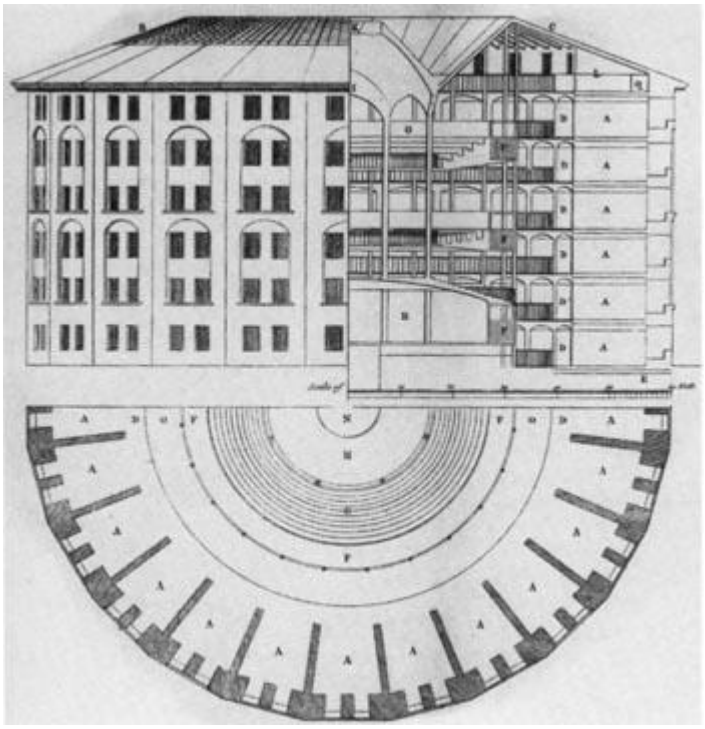

Table A: Type of crime for the electronic monitoring and prison population, 1998-2007					
Type of Crime	**Offenders released from EM**		**Offenders released from Prison**		**Difference**
	Freq.	Percent	Freq.	Percent	
Homicide	36	7.93	2,626	7.03	0.90
Attempted homicide	8	1.76	579	1.55	0.21
Sexual offenses	13	2.86	899	2.41	0.45
Other serious crimes	13	2.86	919	2.46	0.40
Aggravated robbery	247	54.41	18,493	49.48	4.93
Attempted aggravated robbery	16	3.52	2,571	6.88	-3.36
Robbery	36	7.93	4,452	11.91	-3.98
Attempted robbery	29	6.39	2,757	7.38	-0.99
Possession of Firearms	24	5.29	1,494	4.00	1.29
Theft / Attempted theft	7	1.54	1,298	3.47	-1.93
Other minor crimes	25	5.51	1,290	3.45	2.06
Total	454	100	37,378	100	

Table B: Electronic Monitoring Assignment and Type of Crimes			
	1	2	3
Court ever sent to EM	0.02***	0.02***	
	(14.80)	(14.89)	
1-Attempted homicide=1		1.65e-03	1.60e-03
		(0.33)	(0.02)
2-Sexual offenses=1		1.63e-03	1.60e-03
		(0.39)	(0.17)
3-Other serious crimes=1		2.90e-03	2.90e-03
		(0.70)	(0.10)
3- Aggravated robbery=1		4.55E-04	-3.43e-04
		(0.20)	(0.15)
3-Attempted aggravated robbery=1		-0.01**	-0.01**
		(2.40)	(2.44)
4-Robbery=1		-4.79e-03*	-0.01**
		(1.80)	(2.07)
4-Attempted robbery=1		-3.22e-03	-3.20e-03
		(1.09)	(1.06)
5- Theft / Attempted theft=1		-0.01	-0.01**
		(1.59)	(2.22)
5-Possession of Firearms=1		2.55e-03	2.50e-03
		(0.73)	(0.65)
5-Other minor crimes=1		0.01**	0.01
		(1.99)	(1.50)
Adjusted R^2	0.01	0.01	5.0e-04
Observations	37,832	37,832	37,832

Notes: The dependent variable is whether the offender received electronic monitoring. OLS regressions. Absolute values of t statistics in parentheses. Court ever sent to EM is a dummy equal to 1 if the court sent at least one offender to EM. The base category of crime is Homicide. * Significant at 10%; ** significant at 5%; *** significant at 1%.

Table C: Judges		
	Considering every type of crime	Robbery and Aggravated Robbery
Number of judges	199	192
Number of judges that ever used electronic monitoring	101	64
% of judges that ever used electronic monitoring	50.70%	33.00%
% of offenders under electronic monitoring for the five judges with highest ratio	16.60%	25.00%
	22.50%	35.71%
	31.25%	44.44%
	45.45%	50.00%
	80.00%	62.50%

Table 1A: Recidivism and Electronic Monitoring				
	1	2	3	4
Electronic monitoring=1	-0.09***	-0.09***	-0.42***	-0.08***
	(4.26)	(4.11)	(3.99)	(2.87)
Attempted homicide=1		0.03	0.21	
		(0.49)	(0.62)	
Other serious crimes=1		0.18**	0.71**	
		(2.31)	(2.14)	
Sexual offenses=1		-0.02	-0.27	
		(0.41)	(0.61)	
Aggravated robbery=1		0.03	0.19	
		(0.88)	(1.11)	
Attempted aggravated robbery=1		1.8e-03	0.07	
		(0.04)	(0.30)	
Robbery=1		0.05	0.25	0.01
		(1.09)	(1.18)	(0.34)
Attempted robbery=1		0.09	0.40*	
		(1.61)	(1.74)	
Theft / Attempted theft=1		0.03	0.13	
		(0.41)	(0.29)	
Possession of Firearms=1		0.11*	0.60**	
		(1.88)	(2.12)	
Other minor crimes=1		0.04	0.21	
		(0.65)	(0.81)	
Age		-1.9e-04***	-6.8e-04***	-1.7e-04***
		(4.05)	(3.09)	(2.72)
$(Age)^2$		7.2e-09***	2.4e-08**	6.4e-09***
		(3.34)	(2.29)	(2.26)
Argentine=1		-0.05	-0.22	-0.11
		(0.82)	(0.85)	(1.26)
# of previous imprisonment		0.17***	0.67***	0.16***
		(7.10)	(7.46)	(5.63)
Time under arrest (prison or ME)		7.9e-05	3.7e-04*	1.3e-04
		(1.34)	(1.74)	(1.38)
$(\text{Time under arrest (prison or ME)})^2$		1.6e-08	5.1e-08	-1.3e-08
		(0.46)	(0.50)	(0.23)
Great Buenos Aires		-0.01	-0.03	0.01
		(0.31)	(0.25)	(0.22)
Large city		0.04	0.16	0.08*
		(1.06)	(1.17)	(1.88)
Adjusted R^2	0.01	0.16	0.19	0.15
Observations	1,538	1,538	1,538	984

Notes: The dependent variable is a dummy = 1 if the offender went back to prison for a new crime at the Province of Buenos Aires. OLS regressions (except probit in column 3). Year dummies indicating the year of release from prison or electronic monitoring included in regressions 2-4. Sample in column 4 is restricted to offenders prosecuted for Robbery and Aggravated Robbery. Absolute values of robust t (or z) statistics in parenthesis.* Significant at 10%; ** significant at 5%; *** significant at 1%.

Table 1B: Recidivism and Electronic Monitoring, Robustness					
	1	2	3	4	5
Electronic Monitoring=1	-0.11*** (4.17)	-0.09*** (2.86)	-0.11*** (2.85)	-0.09*** (6.35)	-0.09*** (5.06)
Spouse visiting			0.06 (1.45)		
Number of children visiting			0.02* (1.93)		
Number of siblings visiting			-3.1e-03 (0.37)		
Income	2.7e-05 (1.12)				
Spouse		0.04* (1.73)			
Adjusted R^2	0.15	0.15	0.23	0.16	0.16
Observations	959	1,171	647	1,538	1,538

Notes: OLS Regressions in columns 1 to 5. The dependent variable is a dummy = 1 if the offender went back to prison for a new crime at the Province of Buenos Aires. All the regressions include as controls type of crime dummies, age, age squared, Argentine, number of previous imprisonment, total time under arrest (prison or ME), total time under arrest squared, Great Buenos Aires, large city dummy, and year dummies. Columns 4 and 5 are similar to Column 2 of Table 1A, but clustering the standard errors at the judicial district and court levels, respectively. Absolute values of robust t statistics are in parentheses. * Significant at 10%; *** significant at 1%.

Table 2: Recidivism and Electronic Monitoring		
	1	2
Electronic monitoring=1	-0.08***	-0.08***
	(3.42)	(3.73)
Court ever uses EM		-3.5e-03
		(0.14)
Adjusted R^2	0.16	0.16
Observations	1,069	1,538

Notes: OLS Regressions. The dependent variable is a dummy = 1 if the offender went back to prison for a new crime at the Province of Buenos Aires. All the regressions include as controls type of crime dummies, age, age squared, Argentine, number of previous imprisonment, total time under arrest (prison or ME), total time under arrest squared, Great Buenos Aires, large city dummy, and year dummies. Column 1 restricts the sample to offenders that stood in front of a court that sent at least one offender to EM. Court ever uses EM is a dummy equal to one if the court sent at least one offender to EM. Absolute values of robust t statistics are in parentheses. * Significant at 10%; *** significant at 1%.

Table 3A: Recidivism and Electronic Monitoring, IV Regressions						
	1	2	3	4	5	6
Second stage:						
Electronic Monitoring=1	-0.08** (2.21)	-0.13*** (2.76)	-0.12*** (2.65)	-0.12 (1.41)	-0.13*** (2.72)	-0.53** (2.50)
Adjusted R^2 (second stage)	0.16	0.16	0.16	0.16	0.16	0.16
First stage:						
Set of court dummies	Yes					
% Court sent to EM		3.88*** (17.59)	3.13*** (13.69)		3.45*** (14.64)	3.45** (11.44)
Court ever sent to EM			0.24*** (9.62)			
Court has already sent to EM				0.24*** (10.45)	0.12*** (4.98)	0.12*** (4.92)
Adjusted R^2 (first stage)	0.30	0.21	0.26	0.11	0.22	0.22
Observations	1,538	1,513	1,513	1,513	1,513	1,513

Notes: Instrumental Variables regressions in columns 1 to 5. The dependent variable is a dummy = 1 if the offender went back to prison for a new crime at the Province of Buenos Aires. All the regressions include as controls type of crime dummies, age, age squared, Argentine, number of previous imprisonment, total time under arrest (prison or ME), total time under arrest squared, Great Buenos Aires, large city dummy, and year dummies. In the first column, the instruments are a set of dummy variables indicating the court that tried the offender. The F-stat of the joint significance test of all the dummies in the first stage is 3.96***. In column 2, the instrument for each inmate is the % of offenders of the same court sent to electronic monitoring, excluding him. The number of observations in columns 2 through 6 falls from 1,538 to 1,513 because we exclude courts with less than ten offenders. In column 3 we also include as an instrument a dummy for whether the court ever sent a prisoner to electronic monitoring. In column 4, the instrument is a dummy which, for each inmate, equals 1 if before himself, the court has ever previously sent an inmate to electronic monitoring, and equals 0 otherwise. In column 5, we combine the two instruments. In column 6, we again combine these two instruments in an IV Probit regression. All the instruments are calculated in the original database of 37,832 offenders. Absolute values of t (or z) statistics are in parentheses. * Significant at 10%; ** significant at 5%; *** significant at 1%.

Table 3B: Recidivism and Electronic Monitoring, Robustness (IV regressions)							
	1	2	3	4	5	6	7
Electronic Monitoring=1	-0.17** (2.92)	-0.14** (2.21)	-0.20** (2.31)	-0.18 (1.66)	-0.17* (1.86)	-0.13* (1.80)	-0.13** (2.36)
Spouse visiting			0.04 (0.75)	0.23*** (2.19)			
Number of children visiting			0.02* (1.80)	0.03 (1.61)			
Number of siblings visiting			-4.8e-03 (0.58)	-1.2e-03 (0.33)			
Income	3.3e-05 (1.33)			-1.4e-05 (0.32)			
Spouse		0.04* (1.70)		-0.14* (1.55)			
Adjusted R^2	0.14	0.15	0.23	0.23	0.16	0.16	0.16
Observations	946	1,155	637	463	1,513	1,513	1,513

Notes: Instrumental Variables regressions in columns 1 to 7. The dependent variable is a dummy = 1 if the offender went back to prison for a new crime at the Province of Buenos Aires. In all columns, the two instruments are: the % of offenders of the same court sent to electronic monitoring, excluding himself, and a dummy which, for each inmate, equals 1 if before himself, the court has ever previously sent an inmate to electronic monitoring, and equals 0 otherwise. The instruments are calculated in the original database of 37,832 offenders. All the regressions include as controls type of crime dummies, age, age squared, Argentine, number of previous imprisonment, total time under arrest (prison or ME), total time under arrest squared, Great Buenos Aires, large city dummy, and year dummies. Column 5 includes judicial district dummies. Columns 6 and 7 are similar to Column 5 of Table 2, but clustering the standard errors at the judicial district and court levels, respectively. Absolute values of t statistics are in parentheses. * Significant at 10%; ** significant at 5%; *** significant at 1%.

Table 4A: Escaped, Crime Categories and the Previously Imprisoned				
	Escaped		Escaped (minus those previously imprisoned)	
	Frequency	Percent		
Homicide	5	7.58	4	8.51
Rape	3	4.55	3	6.38
Other serious Crimes	3	4.55	3	6.38
Aggravated Robbery	40	60.61	27	57.45
Attempted Aggravated Robbery	1	1.52		
Robbery	4	6.06	3	6.38
Attempted Robbery	4	6.06	3	6.38
Theft/Attempted Theft	1	1.52	1	2.13
Other Minor Crimes	1	1.52	1	2.13
Possession of Firearms	4	6.06	2	4.26
	66	100	47	100

Table 4B: Escape and Recidivism within EM		
	1 Recidivism	2 Escape
Attempted homicide=1	0.06	-0.12
	(0.49)	(1.67)
Other serious crimes=1	-0.08	0.19
	(1.05)	(1.08)
Sexual offenses=1	0.07	0.21
	(0.61)	(1.28)
Aggravated robbery=1	-4.6e-03	-3.3e-03
	(0.07)	(0.04)
Attempted aggravated robbery=1	0.04	-0.09
	(0.20)	(0.08)
Robbery=1	0.22**	-0.03
	(2.01)	(0.24)
Attempted robbery=1	0.02	0.01
	(0.21)	(0.08)
Theft / Attempted theft=1	-0.13	0.11
	(1.45)	(0.52)
Possession of Firearms=1	-0.06	0.06
	(0.67)	(0.52)
Other minor crimes=1	-0.02	-0.10
	(0.22)	(1.14)
Age	-8.9e-05	-6.6e-05
	(1.08)	(0.69)
$(Age)^2$	3.1e-09	1.9e-09
	(0.83)	(0.43)
Argentine=1	0.20**	0.11
	(2.50)	(1.19)
# of previous imprisonment	0.14***	0.15***
	(2.91)	(3.08)
Great Buenos Aires	0.06	0.03
	(1.45)	(0.70)
Large city	0.26***	0.08
	(2.50)	(0.76)
Adjusted R^2	0.09	0.03
Observations	386	386

Notes: OLS Regressions. The dependent variable in column 1 is a dummy = 1 if the offender went back to prison for a new crime at the Province of Buenos Aires after release. The dependent variable in column 2 is whether the offender escaped from the electronic monitoring system. In both regressions, we restrict attention to offenders that received electronic monitoring. Absolute values of robust t statistics are in parentheses. * Significant at 10%; ** significant at 5%; *** significant at 1%.