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Alessandro Barbarino
Giovanni Mastrobuoni

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Alessandro Barbarino
Federal Reserve Board

Giovanni Mastrobuoni
Collegio Carlo Alberto,
CeRP and IZA

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IZA

P.O. Box 7240
53072 Bonn
Germany

Phone: +49-228-3894-0
Fax: +49-228-3894-180
E-mail: iza@iza.org

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ABSTRACT

The Incapacitation Effect of Incarceration: Evidence from Several Italian Collective Pardons*

We estimate the “incapacitation effect” on crime using variation in Italian prison population driven by eight collective pardons passed between 1962 and 1995. The prison releases are sudden – within one day –, very large – up to 35 percent of the entire prison population – and happen nationwide. Exploiting this quasi-natural experiment we break the simultaneity of crime and prisoners as in Levitt (1996) and, in addition, use the national character of the pardons to separately identify incapacitation from changes in deterrence. The elasticity of total crime with respect to incapacitation is between -20 and -35 percent. A cost-benefit analysis suggests that Italy’s prison population is below its optimal level.

JEL Classification: K40, K42, H11

Keywords: crime, pardon, amnesty, deterrence, incapacitation

Corresponding author:

Giovanni Mastrobuoni
Collegio Carlo Alberto
Via Real Collegio 30
Moncalieri (TO), 10024
Italy
E-mail: giovanni.mastrobuoni@carloalberto.org

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1 Introduction

Despite the recent consensus by researchers on crime and punishment that elements of the judicial system, such as increased police forces and incarceration rates, are effective in reducing crime (Levitt, 2004), there is no consensus on the size of the reduction nor on the exact channels through which such reduction is achieved (Donohue, 2007). This paper provides a detailed empirical analysis of both.

In the United States the response to the unprecedented spike in crime rates in the 90s has been an increase in policing and, to a much larger extent, in incarceration. The US prison population is now the highest in the world.[1] Moreover, the large stock of inmates has the potential to create a second crime wave as sentences expire (Raphael and Stoll, 2004; Duggan, 2004). These facts call into question the effectiveness of a further expansion in incarceration as opposed to alternative policies and prompts a further inquiry on the marginal benefit of imprisonment.

To this end it is crucial not only to have a precise measurement of the total effect of incarceration on crime, but also to disentangle the deterrence effect of corrective measures from their incapacitating effect (Shavell, 1987). Indeed, recent studies attempt to isolate either the deterrence effect or the incapacitation effect exploiting detailed aspects or policies of the judiciary system. For instance, Kuziemko (2006) exploits parole boards in Georgia, Owens (2009) sentence enhancements in Maryland, Kessler and Levitt (1999) sentence enhancements in California, Helland and Tabarrok (2007) three strikes laws in California, and Weisburd et al. (2008) different strategies to enforce payments of court ordered fines.[2]

In the same spirit our paper focuses on *collective pardons*, release policies based on general criteria that lead to large reductions in prison population. In particular, we study a series of

[1]Police forces increased around 20 percent over the last 20 years, while due to harsher punishments incarceration rates increased fourfold in the U.S. over the last 30 years. In 2005 there were 737 inmates per 100,000 US residents (International Center for Prison Studies 2007) compared with a world average of 166 per 100,000 and an average among European Community member states of 135 Raphael and Stoll (2009)). McCrary (n.d.) warns that prison population is not in steady state yet and that the reforms of the past 5-30 years have still to produce their full-fledged effects.

[2]In the literature review we try to provide the reader with a brief summary of such work.

pardons enacted in Italy during the years 1962-1995, that lead to the release of prisoners whose residual sentence length was less than a given number of years, usually two or three. It turns out that these policies generate a large variation in prison population across time and regions. For instance, the last collective pardon, passed on July 31, 2006, led within a day to the release of 22,000 inmates, around 30 percent of the total (DAP, 2006).

These sudden changes can be used to break the classical simultaneity between crime and prisoners (Levitt, 1996). Unlike most other policy experiments found in the literature, pardons generate *nationwide*, *immediate*, *measurable* and *large* changes in prison population that, we argue below, are not related to other factors that influence crime.

A first identification strategy, based on monthly time-series crime data and the exact date the pardon gets passed leads to an estimated elasticity of crime with respect to prison population of respectively 15 percent. The identification of the total elasticity (deterrence + incapacitation) with the monthly time series estimates requires few assumptions, since it is based on discontinuous changes in crime rates following pardons measured at a high frequency. However the elasticity is only approximate since we do not have monthly data on prison population. By contrast, the identification based on yearly panel data allows us to control for deterrence funneled by criminals expectations and allows us to disentangle deterrence from incapacitation. Given that immediately after a pardon gets passed criminals should be less prone to commit crimes–a) the next pardon is unlikely to happen very soon, and b) pardoned sentences might be added to the a new sentence (see Drago et al., 2009)–, the incapacitation effect should be larger than the total effect. This is what we find. The elasticity of total crimes based on the panel data, equal to 35 percent, is indeed larger than the one based on monthly data.

We classify the different types of deterrence that might arise in our experiment. Criminals expectations are potentially an important channel through which deterrence is at work. Criminals might try to predict the timing of pardons and change their behavior accordingly. Hence failing to control for criminals' expectations might bias the estimates towards zero since changes in criminals expectations might make the variation induced by the policy endogenous. We control for criminals' expectations making use of the nationwide nature of pardons. The intuition is the following: pardons are national laws that are outside the control of regional administrations and are homogeneous across regions. As such, they should affect criminal expectations (and deterrence) in a similar way across the country. Controlling for time absorbs the deterrence effect that works through criminals expectations. Notice that if pardons were regional we would not be able to control for this type of deterrence. We consider additional types of deterrence that might be associated with our experiment such as congestion and crowding out effects but we find that criminal expectations are the most important channel through which deterrence is at work.

Levitt (1996) is the closest paper in the literature to this work. In his paper a set of indicator variables capture the status of overcrowding litigation, which generate an exogenous variation in the US prison population. Sometimes court decisions led to fewer offenders sentenced to prison terms, sometimes to early release programs and other times to the construction of new prison facilities and to a reallocation of prisoners across institutions. He estimates the combined effect of deterrence and incapacitation using a quasi-natural experiment based on aggregate U.S. states-level data and his estimated elasticities are indeed larger than our estimates.

With our estimates in hand we then move on to study the efficiency level of the Italian prison population. Heterogeneity of criminal types generates a distribution of criminal-specific social costs. The sum of these social costs per released prisoner are approximately 3 times as large as the cost of keeping him in prison, indicating that Italy has a prison population that is below its optimal level. The mainly unselective pardons that have been enacted recently are thus very inefficient, as the release of potential criminals has a social cost greater than the cost of incarceration.[3] Indeed, in case of overcrowding an expansion in prison capacity should be preferred to an unselective pardon, and has lately been planned by the Italian government.

Research using data from different countries helps evaluating the robustness of the extant

[3]In the spirit of this "selective incapacitation," the Italian penal code establishes that pardons and amnesties should not be given to recidivist, recurrent, or career criminals (art. 151). Despite this article, in the 1990 and 2006 pardons and in the 1990 amnesty, the legislators decided to extend the benefits to career criminals. Moreover, because of evidence that criminal activity decreases with age, the legislators have sometimes increased the number of pardoned years for older criminals (usually defined as being older than 65 or 70 years of age). Other preferred treatments for elderly inmates were introduced in 1974 (law n. 220).

findings that are all based on US data and can support useful cross-country comparisons at times where many researchers wonder about the optimal size of the US prison population.[4]

1.1 Related Literature

In this section we provide a brief review of the literature on the relationship between crime and incarceration that is close to the issues raised in our paper.

Studies on the Correlation of Crime and Prison Population – Several papers have tried to estimate the effect of prison population on crime. Early studies do not control for endogeneity and use state level time series data and regressions. Stemen (2007) reviews these studies: the elasticity of crime with respect to incarceration range from positive figures down to -28 percent. Not controlling for endogeneity clearly biases the results upwards. Among the most notable studies Marvell and Moody (1994) proceed by rejecting that crime Granger causes prison population, and later estimate an elasticity of crime with respect to prison population of -0.16. Spelman (1994) finds similar effects.[5] The studies above fail to account for the simultaneity between crime and imprisonment. Given that when for whatever reason crime rises the prison population will mechanically increase the simultaneity pushes the estimated correlations between crime rates and incarceration rates towards zero. Levitt (1996) controls for the simultaneity using an instrumental variables (IV) approach and finds elasticities that are two to three times larger than before.

Studies that Isolate Deterrence – Among the many studies that focus on deterrence it is worth mentioning Kessler and Levitt (1999) who exploit sentence enhancements to isolate deterrence from incapacitation. Helland and Tabarrok (2007) use the deterrent effect of California's "three-strike" law to isolate deterrence. Weisburd et al. (2008) use a randomized field trial of alternative strategies for incentivising the payment of court ordered fines to estimate deterrence. Levitt (1998) isolates deterrence based on the discontinuity in punitiveness at the

[4]Few researchers have explored Italian data using an economic approach, with Buonanno et al. (2009), Buonanno et al. (2008), Drago et al. (2009), and Marselli and Vannini (1997) being rare exceptions.

[5]See also Raphael and Winter-Ebmer (2001), Marvell and Moody (1997), Donohue and Levitt (2001), and Spelman (2000, 2005).

age of majority and finds evidence of it. In contrast, Lee and McCrary (2005) examine a longitudinal database of individual level arrest records in Florida. They take advantage of data on the exact date of birth of arrestees and look for discontinuous changes in offending right at the age of cutoff using a regression discontinuity design, but find no sizable declines at the age of majority. Drago et al. (2009) exploit random variation in sentence length due to a recent collective pardon to isolate deterrence.

Studies that Isolate Incapacitation – Owens (2009) uses a one-time exogenous change in sentence enhancements for 23-25 year-old inmates in the State of Maryland to isolate incapacitation but she estimates the effect that incarceration has on individual recidivism, rather than crime. Recidivism might not be a proper measure of crime if arrested criminals tend to commit different types of crimes or a different number of crimes than non arrested ones. It might also not properly capture congestion and replacement effects:[6] the increased supply of criminals due to pardons might reduce the probability of being detected, and consequently attract new entrants in the criminal market. In contrast, released criminals might also drive some of the old criminals out of the market, making the total effect on crime ambiguous.Whenever several prisoners are released at once peer effects might be at work as well. Moreover, whenever large numbers of prisoners are released the prison administration might face more binding constraints in assisting released prisoners to provide job counseling, accommodation, etc. We later test for this additional effect that depends on the size of the released prison population by splitting the sample based on the group of regions with releases above or below the median.

Finally, a special issue of *Quantitative Criminology* (2007) contains a thorough overview of studies on incapacitation.

Studies on Pardons – Only a few papers have studied the effect of pardons on crime. The reason is that most empirical research on the criminal justice system focuses on the United States, where pardons are rare (Levitt and Miles, 2004) and release small numbers of inmates. (Mocan and Gittings, 2001) estimates the deterrence effect of gubernatorial pardons of persons

[6]See Cook (1986), Freeman (1999) and Miles and Ludwig (2007) for a more thorough discussion of the replacement and spillover effects.

on death row and finds that three additional pardons generate 1 to 1.5 additional homicides. Kuziemko (2006) studies parole boards in Georgia and exploits overcrowding litigation and a collective pardon of 900 inmates to find out the relationship between time served and recidivism and the efficiency of parole boards but she does not concentrate on the estimation of incapacitation nor on the evaluation of pardons.

In Italy, despite the recurrent use of pardons, there has been only one empirical study on the relationship between pardons and crime. The study Tartaglione (1978) headed by a judge that was killed in that same year by the Red Brigade terrorist group, finds that after the 1954, 1959, 1966, and 1970 pardons, national changes in crime tend to be above average. The exceptions are the 1963 pardon, in which only one year was pardoned, and the 1968 pardon, which applied only to certain crimes committed during student demonstrations. The study also documents that pardoned inmates have a recidivism rate of 31.2 percent, which is not that different from 32.9 percent, the recidivism rate of prisoners who are released at the end of their term. Standard errors are not shown, so we do not know whether these differences are significant or not. The judges who worked on this pioneering study did not use regression methods, which makes it impracticable to analyze the link between prison population and crime or to use regional variation in the fraction of released prisoners. The judges also made no attempt to value the monetary cost of the increased crime, or to separate the incapacitation effect from the total effect.

2 Italy's Collective Pardons and Prison Population

Historical remarks – Pardons and amnesties are deeply rooted in the Italian legislative history and culture. Between the unification of Italy in 1865 and the defeat of Mussolini in 1943 there have been approximately 200 pardons or amnesties, though some of these were just fiscal pardons, or amnesties for very specific crimes. Some of these pardons were aimed at easing social tensions, others were passed to magnify the royal family. A pardon was passed when the Prince of Naples was born (1869) and one when he got married (1896). Other pardons followed the colonization of Eritrea, Somalia, Cyrenaica, Tripolitania and later Ethiopia, the peace treaty between Italy and Austria (1919), the annexation of Slavic territories in the North-East of Italy. Though sometimes even local tensions led to pardons (wood thefts in the Montello region, illegal cutting of olive and mulberry trees (1920), crimes committed in occupied Greece, etc.) The use of amnesties and pardons in Italy has been the norm, and the fact that an entire article of the 1945 Constitution is devoted to these acts (art. 79) shows that after World War II nothing changed. Between 1945 and today there have been more than a dozen pardons (mostly coupled with amnesties). Although these were firstly aimed at reconciling a politically divided nation, in more recent times an additional goal has been to reduce prison overcrowding.

Legal definition – Starting in 1992, collective amnesties and pardons in Italy have been issued by the legislators with an absolute majority requirement of two-thirds (constitutional law n.6 of 1992). Before that year, the President could issue them but only after they had been mandated by a simple majority of the parliament. The main difference between amnesties and pardons is that amnesties eliminate both the sentence and the crime, as if they never happened, whereas pardons eliminate only part of the sentence. Given that Italian prosecutors are required by law to investigate all felonies (art. 112 of the Constitution), pardons are usually followed by amnesties.[7] Otherwise, prosecutors would have to spend time and effort investigating pardoned crimes, even if it was impossible to actually punish the perpetrators. Another difference between the two is that whenever the pardoned prisoner recommits a crime within five years, the commuted prison term gets added to the new term.[8] Amnesties, instead, are permanent.[9] [10]

[7]The 2006 pardon was an exception to this rule.

[8]The incapacitation that we estimate represents, therefore, a lower bound of the typical incapacitation. Drago et al. (2009) exploit this rule to identify the deterrence effect of prison.

[9]The great majority of pardoned prisoners are convicted criminals, though some might be in preventive detention with an expected sentence that is below the maximum number of pardoned years. For example, in 2006 when the number of pardoned years was three, 10.7 percent of the prisoners that were freed were in preventive detention (Marietti, 2006).

[10]Pardons and amnesties also reduce the number of arrestees who are subject to restrictive measures that are different from imprisonment namely, social work outside prison, semi-liberty, and house arrest. Between 1975, the year in which these measures were introduced in Italy, and 1995, 19 percent of apprehended criminals (or alleged criminals) were subject to these alternative measures. It has been shown that recidivism rates for these individuals are significantly lower than those for prisoners (Santoro and Tucci, 2004) and that some of these individuals might commit crimes even while subject to these alternative measures. Nevertheless, changes in

Overcrowding – The left panel of Figure 1 shows that the official prison capacity (measured as the number of beds per 100,000 Italian citizens) declined between 1962 and 1975, significantly reducing the cushion between the total prison population and the total capacity. Although 81 new prisons were build between 1971 and 2003, 87 older and obsolete ones were dismissed during the same period(de Franciscis, 2003). As a result, between 1975 and 1991, prison capacity was basically flat at almost 50 beds per 100,000 residents. Only in more recent times has capacity increased. In 1983, as a result of flat capacity and a steady increase in crime, the prison population exceeded the "official" capacity (even if aggregated at the national level) for the first time. If necessary the prison administration can add new beds to existing cells, to reach what is defined as "tolerable" capacity. The 1986 pardon was the first one to solve a situation of overcrowding. Partly because of the tougher majority requirements, 16 years passed between the most recent pardon, in 2006, and the pardon before that. During the same period, the prison population tripled from about 20,000 to 60,000, dropping to about 35,000 after the 2006 pardon.

Pardons and Prison Population – Figure 2 shows the log-changes in prison population and the fraction of pardoned prisoners.[11] It is evident that collective pardons induce an almost one-for-one change in prison population. Overall the fraction of inmates that gets freed can be as high as 35 percent, and it sometimes reaches 80 percent in single regions. But the effect of pardons on prison population appears to be short-lived. Within one year, the inmate population recovers more than half of the size of the initial jump. Between 1959 and 1995, for example, the inmate population increased, on average, by 449 inmates per year, but with large fluctuations that were driven by the pardons. The inmate population decreases by an average of 3,700 inmates after pardons, but increases by an average of 2,944 inmates immediately afterwards. In all other years the average increase is by 1,165 inmates. In other words, in the year immediately after the pardons, and excluding the year of the pardon, the inmate population grows two and a half times faster.

crime might be due in part to these additional pardoned individuals.

[11] The Italian Statistical Office (ISTAT) groups together pardoned and amnestied prisoners.

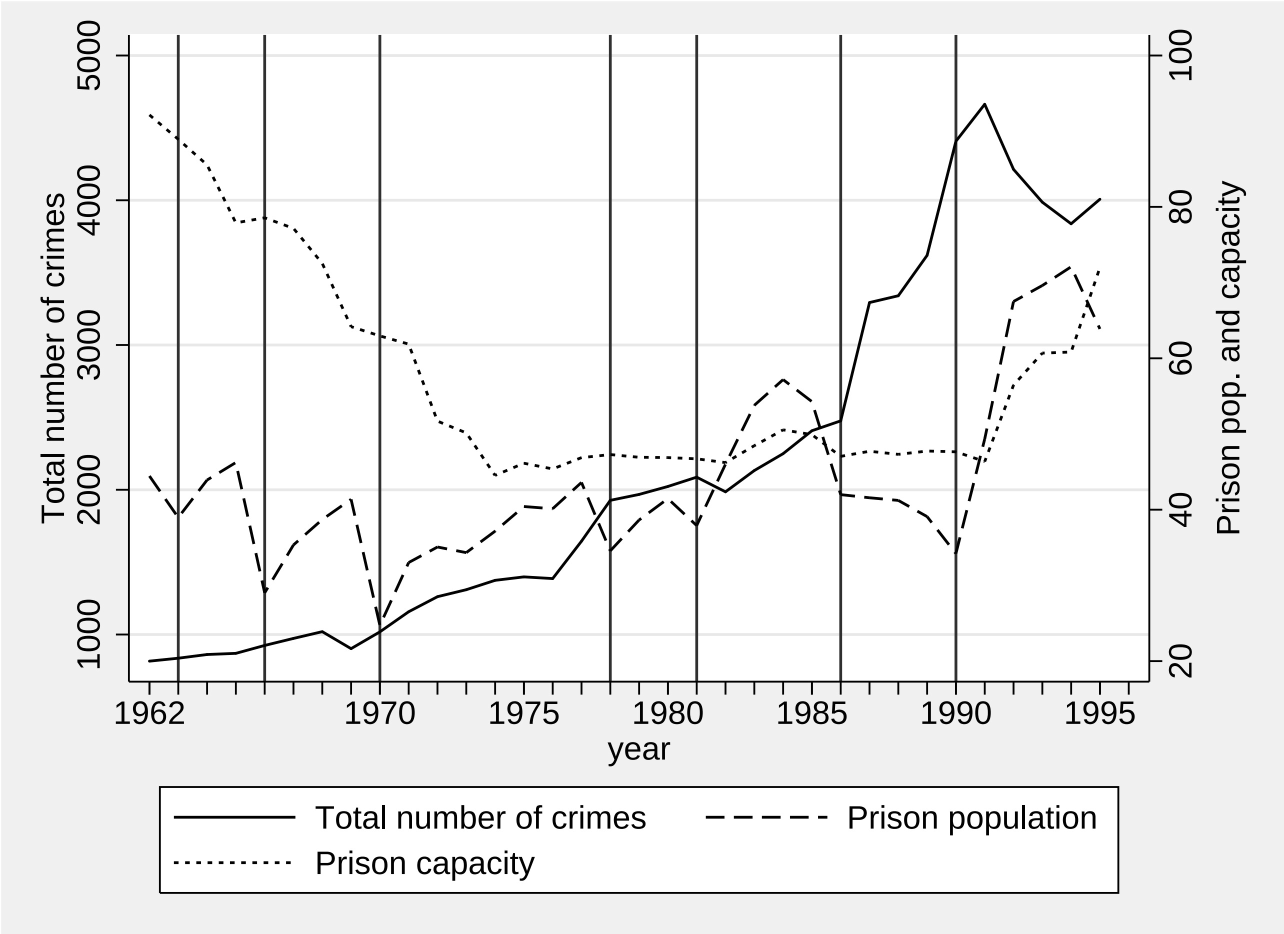

Figure 1: End of the year prison population, prison capacity, and the total number of crimes

Notes: Vertical lines represent years in which pardons or amnesties have been passed.

Pardons generate also variation across regions. Table 1 shows the fraction of pardoned inmates across regions. Table 2 shows, for example, that in the Abruzzo and Molise regions, aggregated because of data limitations, the 1966 pardon freed 85 percent of the inmate population, while in Sardinia only 38 percent left the jail. The 1968 pardon, which applied to crimes committed during student demonstrations, led to a release of very few prisoners. Two years later,instead, in five regions—namely, Abruzzo, Molise, Friuli-Venezia Giulia, Liguria, and Trentino-Alto Adige—more than 70 percent of prisoners were freed. Later pardons have led to fewer releases. The last pardon in our sample happened in 1990, as the judicial data about the 2006 pardon are not available yet.

The next section presents the additional crime data that are used to measure how other

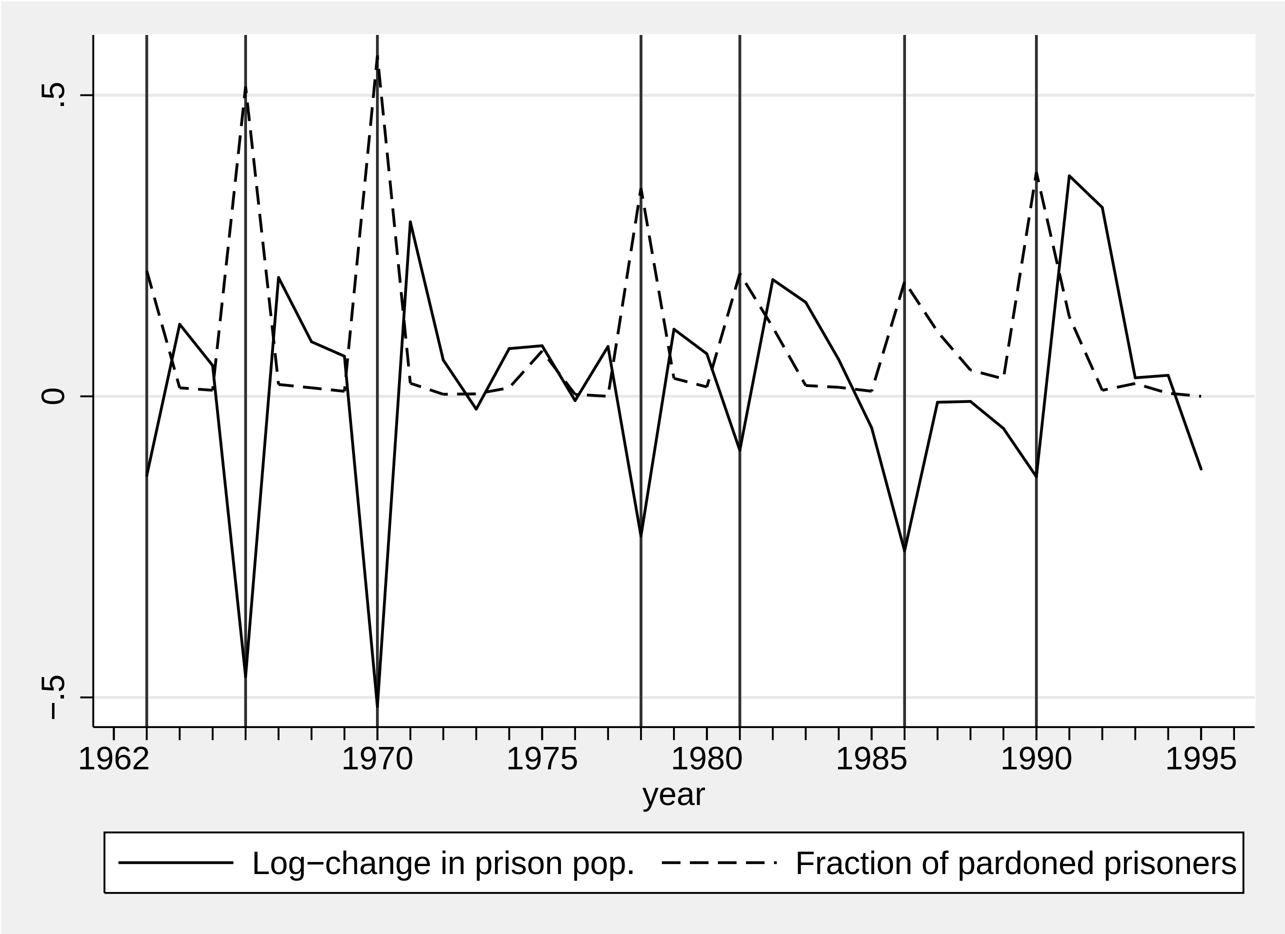

Figure 2: Fraction of pardoned inmates and change in inmate population

Notes: Vertical lines represent years in which pardons or amnesties have been passed.

types of crime respond to collective pardons and amnesties in particular, those that were passed between 1962 and 1995. We have chosen to collect information on crime and on the prison population through 1995, because 1990 represents the last year in which a pardon was passed for which these data are available. No regional data are yet available for the 2006 pardon. The value added of using region level data is that we can try to isolate the pure incapacitation effect.

3 Data

The Italian Statistical Office (ISTAT) publishes a yearly statistical supplement about the Italian judicial system. From these supplements, we collected information about the evolution of the prison population and about crime for 20 Italian regions between 1962 and 1995. ISTAT publishes two sets of crime statistics: those collected directly by the police corps (*Polizia di Stato*, *Carabinieri* and *Guardia di Finanza*) from people's complaints (*Le Statistiche della Delittuosità*), and those collected by the judicial system (*Le Statistiche della Criminalità*) when the penal prosecution, which in Italy is mandatory, starts. The two sets of statistics differ whenever at least one of the following things happen: i) the initial judge decides that the complaint does not depict a crime; ii) the judicial activity is delayed with respect to the time that the crime was committed; iii) a crime is reported to public officials who do not belong to the police corps. Since the exact timing of our statistic is important in most of our analysis we use crime as measured by the police. When single crime categories are unavailable in the police data, and as a robustness check, we also use the judicial statistics.[12]

Table 3 shows the summary statistics of the variable that we use. Variables are weighted by the resident population. Between 1962 and 1995, there were on average 42 inmates per 100,000 residents. Levitt (1996) shows that during a similar time frame in the United States the inmate population was 168, exactly four times as large as in Italy. Our statistics indicate that the total amount of crimes per year per 100,000 residents was 1,983. This number is significantly smaller than Levitt's number for the United States (approximately 5,000), which might be due to underreporting. In 1984, ISTAT started separating reported crimes into more specific categories. Some categories are identical to those reported by Levitt, and allow a comparison between Italy and the United States. Burglaries seem less frequent in Italy (285 versus 1,200), and so seem larcenies (265 versus 2,700), though the definition of these crimes might differ as well. For motor vehicle thefts, where the definition is clear, and where underreporting and multiple offenses are less frequent, the two countries are similar: 420 per 100,000 residents in Italy and 402 in the United States.

Full-year Equivalence – Given that some released prisoners get rearrested within a year, we would like to estimate how crime rates vary immediately after a pardon gets enacted.

[12]In 1984, ISTAT changed the categorization of crimes in the police statistics, providing a more detailed crime categorization. Instead, for the judicial data we can use a sample on single crime categories that starts in 1970 (Marselli and Vannini, 1997).

But pardons and amnesties are sometimes passed in the middle of the year, and we have no access to monthly regional data. Fortunately, we can use the date on which the pardon gets passed to adjust the change in the prison population and the number of pardoned prisoners to produce "full-year equivalent" pardoned prisoners—that is, prisoners who can potentially commit crimes for a whole year. Take, for example, the 1978 pardon. The law was issued on August 5. Assuming that after the pardon criminal activity was uniformly distributed over time, recidivist prisoners would have been able to commit crimes for five months in 1978. One way to take this timing into account and produce "full-year equivalent" prisoners is to reduce the number of pardoned prisoners by 7/12 in the year of the pardon and add these prisoners to the year after the pardon, year in which they can potentially commit crimes for the whole year.[13]

More generally, based on the day of the year, d, on which the pardon becomes active, full-year equivalent pardoned prisoners are

$$PARDONED^*_{t,r} = \frac{365-d}{365} PARDONED_{t,r}$$

in the year of the pardon and

$$PARDONED^*_{t,r} = \frac{d}{365} PARDONED_{t,r-1} + PARDONED_{t,r}$$

in the year after the pardon, and

$$PARDONED^*_{t,r} = PARDONED_{t,r}$$

in all other years. We also adjust the prison population accordingly. Later we are also going to see how robust our results are when we use different ways to adjust prison flows to the dating of pardons.

[13] In 1990, the amnesty occurred in April, while the pardon occurred in December. As a result, the weight is going to be the average of the two periods weighted by the fraction of released prisoners who got released because of the pardon (80 percent) and because of the amnesty (20 percent) (Censis, 2006).

4 The Estimated Incapacitation Effect

4.1 Identification using monthly time-series variation

As for how pardons affect crime, ideally one would compare monthly crime-level statistics with the monthly number of pardoned criminals. Monthly data on crime are available for the years 1962-1983, but not for prison population. The top left panel of Figure 3 shows the monthly data, while all other panels simply zoom into a two year window around each pardon. From the figures one can see that crime rates tend to increase right after pardons get passed (the horizontal lines). A simple comparison of pre/post differences, meaning the distance between the horizontal lines, shows that apart from the last panel crime rates tend to be larger immediately after the pardons than immediately before. The estimated differences shown in the title are significantly different from zero. The discontinuity is less clear cut in 1963 and 1982, years where the released fraction of prison population has been relatively small (approximately 10 percent). Later we will show that overall these differences are statistically significant even when first-differencing the data to control for preexisting trends.

For the 2006 pardon we also have data on monthly prison population, which we match with data on a specific crime, bank robberies. The two panels of Figure 4 show the number of bank robberies per 100 bank branches between January 2004 and December 2007. The vertical line represents the end of July, when the prison population dropped from 60,710 to 38,847 (-37 percent). The right panel shows the estimated regression discontinuity using the region level monthly data with a cubic term of time. Within a month the number of bank robberies jumped by 0.31 per 100 branches (with a standard error of 0.07) from around 0.5 to 0.8. Given that prison population decreased by 37 percent the estimated elasticity is close -160 percent (0.3/0.5/0.37). The dotted line shows the estimated discontinuity when we use 12 month averages just before and after the pardon. The estimated change is equal to 0.18 (with a standard error of 0.04). The estimated elasticity on a yearly basis is closer to 1.

We should keep in mind that such an elasticity represents the sum of the (negative) incapacitation and (likely positive) deterrence effects. We speculate that the net deterrence effect

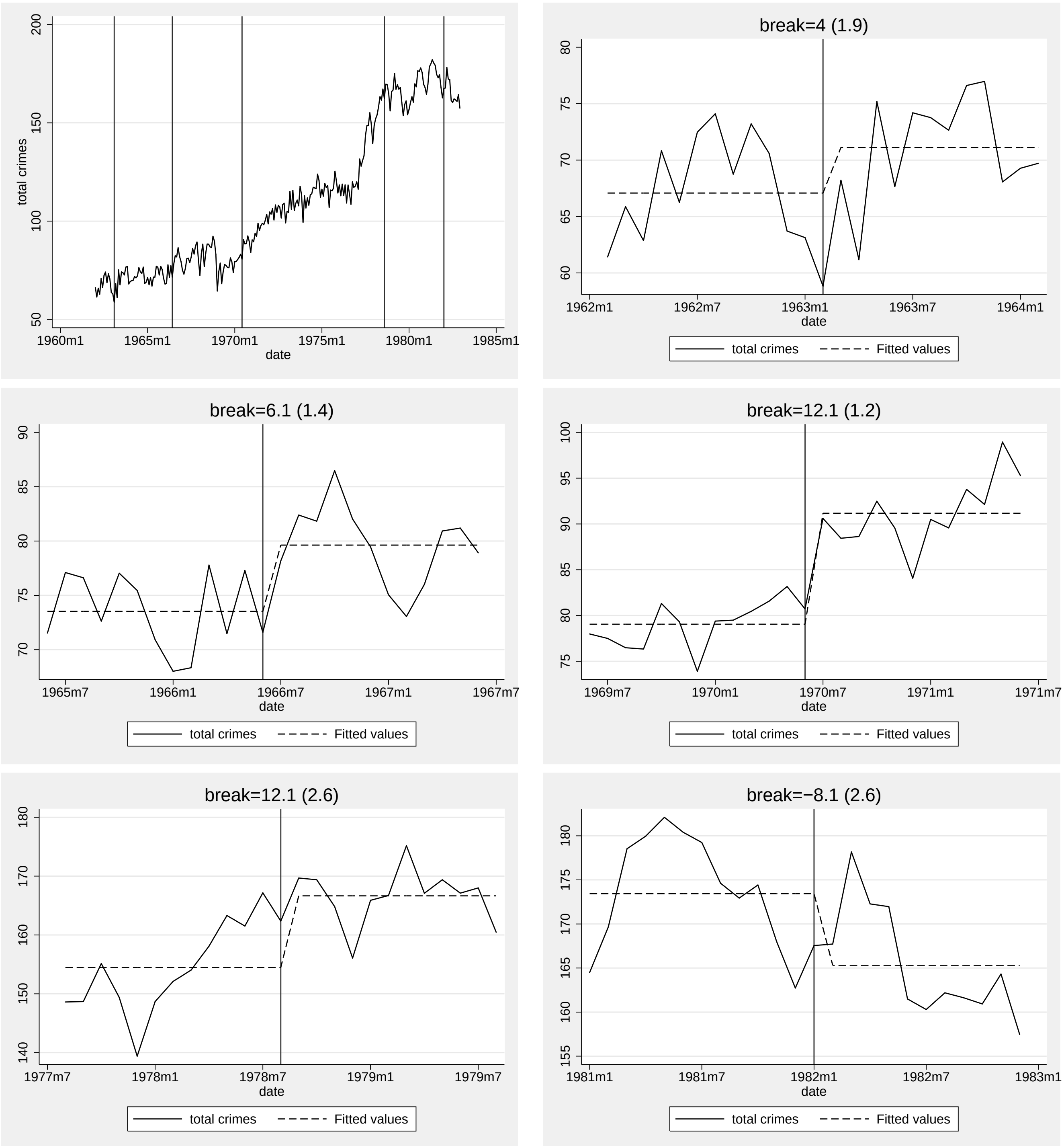

Figure 3: Monthly crime rates per 100,000 residents, and structural breaks)

Notes: The break is estimated using a two year window centered at each pardon. The standard errors are in parentheses. Vertical lines represent months in which pardons or amnesties have been passed.

is positive since immediately after a pardon gets passed criminals do not expect a new pardon to be passed in the near future and inmates that were released because of a pardon would risk to see their old sentence added to the new one if they got rearrested.

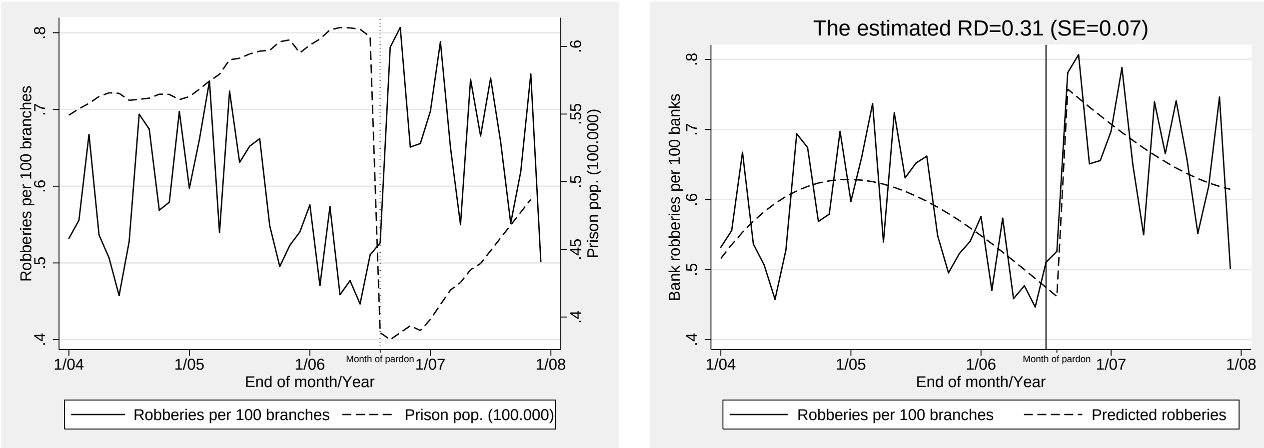

Figure 4: Monthly bank robberies per 100 branches

We just showed that bank robberies had a sizeable increase after the 2006 pardon. Should we expect similar increases for other kinds of crime? In other words, who are the prisoners that are typically released? The last column of Table 4 shows the changes in prison population for different types of criminals, while the distribution and the rank of the different types of crimes committed by the inmates are shown in the remaining columns. Overall the distribution of the type of crimes committed by prisoners serving jail just before and just after the July 2006 pardon are very similar. Most changes in prison population are close to the overall 37 percent decline. Since Mafia related crimes are excluded from pardons, criminals who had committed these crimes were less likely to exit jail in August. Seventeen percent of them did leave jail, probably, by having pardoned the part of their crime that was not related to the "mafia-type criminal association" felony (*Associazione per Delinquere di Tipo Mafioso*, art. 416 of the penal codex). The 2006 pardon did not apply to some drug-related criminals, which is why their decline is smaller than the average decline. Criminals who committed crimes against persons are less likely to exit jail than criminals that have committed crimes against wealth, but the differences are small. We do not have the month-by-month distribution of crime types for the other pardons but a quick look at past pardon bills shows that historically very few crime categories have been excluded from such clemency bills, suggesting that differences are likely to be negligible. This means that in terms of criminal background pardoned inmates are similar to those inmates that are released after serving their entire sentence. Later, when we use pardoned inmates to instrument changes in prison population, the resulting estimates should therefore represent average incapacitation effects rather than incapacitation effects related to some specific inmates.

Now we are going to show that even first-differencing the data shown in Figure 3 one can estimate the total incapacitation effect. There are different ways to estimate the total elasticity (incapacitation plus deterrence) that lead to similar results. Since we don't have monthly data on prison population we simply regress crime rates on dummy variables indicating whether in that particular month a pardon or an amnesty was passed. Given the clear non-stationarity of crime rates in Table 5 we first-difference the data. In the first three columns we use crime levels, while in the last three we use logs. In columns 3 and 6 we also control for seasonality adding month fixed effects. Consistent with what was shown in Figure 3 for the bank robberies, there is strong evidence that after a month crime rates increase by more than 4 crimes every 100,000 inhabitants (column 3), or 5.6 percent (column 6). Given that the average fraction of released prisoners during this period is 36.8 percent the implied elasticity is 5.6/0.368 or 15.21 percent.

The next section shows that the results based on panel data tend to give overall elasticities that are consistent with the time-series results. But most importantly, the identification strategy based on panel data allows us also to control for deterrence, and doing that shows that the incapacitation is in absolute terms larger than the total effect: deterrence lowers the total change in crime.

4.2 Identification using yearly panel data

In this section we propose a taxonomy of deterrence for our experiment and explore how to isolate incapacitation. To this end we exploit collective pardons that trigger simultaneous regional variations in crime that arguably are exogenous and unrelated to factors that influence crime. We also explicitly allow criminals to respond positively to expected future pardons through changes in deterrence. Hence, our identification strategy revolves around instrumenting changes in regional prison population with the number of pardoned prisoners released in the region *with the extra precaution of controlling for deterrence funneled by expectations.* Controlling for time allows us to kill two birds with a stone: on the one hand, time controls purify our estimates from expectation-driven deterrence effects leaving us with the sole incapacitation effect; on the other hand, they neutralize the danger that criminals' expectations about pardons make these policies be as if they were endogenous.

Long-term deterrence effect – As mentioned pardons might generate changes in deterrence through criminals expectations. Since pardons reduce the expected sanction, everything else being equal, we should expect crime rates to be higher in a society that occasionally makes use of them. Given the unavailability of a counterfactual Italian society without pardons, this effect is hard to estimate but is going to be absorbed by the constant term.

Pre-pardon deterrence effect – Criminals might also try to strategically time (around the time of the pardon) their criminal activity in order to minimize their expected sanction (pre-pardon deterrence). This effect is severely dampened by the rule that pardons only apply to crimes committed up to a specific date, usually three to six months before the signing of the law. The risk of committing a crime that is too close to a pardon, and therefore excluded from the pardon, is likely to significantly reduce the incentive to commit pardonable crimes shortly before the law passes.

Pre-pardon deterrence would lead to an increase in crime rates just before the pardon, biasing our estimates toward finding no effect on crime when prison population drops. Anecdotal evidence seem to suggest that this bias is hardly at work. First, there is an endless sequence of pardon bills on the Parliament floor which are likely to be the prime source of information to predict pardons. Table 6 shows that there are so many proposals (for example around April 2005, October 2002 and August 2001) that never became law that criminals would have a hard time predicting the timing of a new pardon. Second, a cursory inspection of the monthly data available for the 2006 pardon shown in Figure 4 would exclude that in that year the short term deterrence effect is at work. Before July 2006 bank robberies were actually trending down, which is not consistent with criminals expecting a new pardon and timing their crimes accordingly. Moreover, monthly data allow us to try to predict the implementation of a pardon using the information available until right before it is passed. In particular Table 7 shows that using monthly data it is impossible to predict the exact timing of pardons based on crime rates during the past 3, 6, or 12 months. Using high frequency data one can isolate very narrow intervals around pardons, showing that the estimated discontinuities are not subject to simultaneity bias. There is no evidence that pardon are passed depending on recent patterns of crime.

Post-pardon deterrence effect – Expectations on pardons are likely to be updated immediately after pardons get passed. And this is the largest and most worrisome deterrence effect because criminals are going to be less likely to commit crimes: i) they know that the next pardon is unlikely to happen within their expected sentence length, and ii) released prisoners would see their pardoned sentenced added to the new one if they were rearrested.[14] The lowered propensity to commit crimes immediately after a pardon would again lead to underestimating incapacitation. Fortunately these laws are nationwide laws (outside the control of regional administrations, and homogeneous across regions when implemented), meaning that the implied changes in expectations are arguably the same across the country and will be fully absorbed by time controls.

Assuming that once we control for time effects there is enough variation left in the number of released prisoners across regions, the structure of our experiment allow us to control for post-pardon deterrence, isolating incapacitation. The variation in the prison population that we exploit is the variation in the fraction of prisoners who are pardoned across regions at a given point in time. This fraction depends on the distribution of the residual prison time of the inmate population, which at the time of the pardon is certainly predetermined.[15] [16]

[14]Drago et al. (2009) use this rule to isolate deterrence effects.

[15]Kuziemko (2006) uses a similar variation to estimate the effect of time served on recidivism.

[16]The link between regional prison population and regional crime depends crucially on the law establishing that each arrested criminal must first be incarcerated in prisons that are located inside the competent judicial jurisdiction where the crime has been committed (*Competenza per Territorio*, Article 8 of the *Codice di Procedura*

The variation in number of released inmates comes from two sources: 1.) for a given crime, variation in the residual sentence length that is due to variations in the date of arrest or in the date of conviction, depending on whether the judge decides to keep the criminal in jail during his trial;[17] 2.) for a given date of conviction, variation in the residual sentence length which might or might not be due to differences in the distribution of crime seriousness.[18]

Policing, congestion and replacement effects – There is also the possibility that the release of a large mass of prisoners might change other factors that affect deterrence, like increased policing or other changes in police actions, that in our model corresponds to changes in $p_{t,r}$. But these effects are measurable and we think that we have fairly good proxies for these changes.

There might also be congestion and/or replacement effects: on one side, the increased supply of criminals due to pardons might reduce the probability of being detected, and consequently attract new entrants in the criminal market; one the other side, released criminals might drive some of the old criminals out of the market, making the total effect on crime ambiguous. But again, these effects would generate non-linearities between crime and prison population that we can test.

Other potential sources of biases – Another possible source of endogeneity of our instrument is the possibility that increased crime rates may lead, if no new prisons are built, to prison overcrowding, which may lead to a collective release: this chain of events would make our policy endogenous (not necessarily through criminals' expectations rather through the national Government reaction function).[19] We already showed that pardons do not seem to be related to previous crime rates. And since pardons are unlikely to depend on year to year changes in crime we adopt the precaution of differencing the data, working with changes in crime instead

Penale) and might later be transferred to a prison that is closest to where the respective family resides (Article 42 of the 26 of July 1975 n. 354 law). Each region has one or more jurisdictions, with the exception of the Valle D'Aosta and Piedmont regions which share the jurisdiction of Torino.

[17]Preliminary judges can keep suspected criminals in jail if at least one of these three risks is present: i) reiteration of the same crime, ii) escape, and iii) removal of the evidence

[18]It has been shown that even under federal sentencing guidelines the same crime might be judged differently by different judges (Anderson et al., 1999) and the ability of lawyers is also likely to influence the sentence length for the same kind of crime.

[19]Tartaglione (1978) argues that pardons in the 60s and 70s were difficult to justify other than for a political preference for clemency, but Figure 1 does show that after 1982 prisons started to be overcrowded.

of levels.[20]

Regions that had and might still have higher crime rates might simply release more prisoners so that the fraction of pardoned prisoners in a region might depend on the level of crime in the previous period in the same region. If this was the case regional lagged crime rates would be able to predict the fraction of released prisoners. Table 8 tests whether this is the case by regressing the fraction of pardoned prisoners at time t on the logarithm of crime at time $t-1$ using a sample of regions where at least 1, 5, or 10 percent of prisoners are released. No matter the sample we choose the coefficient is quite precisely estimated to be close to zero. Thus, there is no evidence that regions with higher crime rates at time $t-1$ release a larger fraction of prisoners, meaning that a compositional bias is unlikely to arise.

Average effects and local effects – With a variation in the distribution of crime seriousness that differs across regions and over time our estimated incapacitation effect might not measure the average effect, but rather a local one. If, for example, in Piedmont criminals commit frequent but petty crimes, while in Sicily crimes are less frequent but more serious, a pardon would tend to release more prisoners from Piedmont. The incapacitation effect would, therefore, give more weight to crimes which are on average less serious. The opposite would be true if criminals who are caught recidivating commit crimes more frequently, because these criminals receive sentences that are increased by at least a third (art. 81 of the Italian penal codex). We neutralize the variation in the distribution of crime seriousness by focusing on specific types of crime and by interacting the average (log) sentence length of the same crime types with the fraction of pardoned prisoners.[21] We exploit the regional variation given that approximately 90 percent of inmates get arrested in the region they reside (ISTAT, 1961-1995).[22]

The Behavioral Model – Let us introduce a simple model of criminal behavior to disci-

[20] Differencing the data is also important in case crime levels and prison population are non-stationary. A regression in levels might then give spurious results.

[21] Ideally we would like to measure the region-specific and crime specific average sentence length of *pardoned* prisoners and not the one of the *whole* prison population, though the two are likely to be correlated since pardoned prisoners are part of the prison population. The two measures would also be correlated within regions if sentence lengths contained a judge-specific fixed effect, though we do not have data to test for the existence of these fixed effects.

[22] We do not find evidence of criminal spillovers to contiguous regions.

pline our reasoning and to formalize the mechanics of deterrence and incapacitation that leads naturally to our empirical specification. The model, a revised version of Kessler and Levitt (1999)'s model, can be viewed as a reduced form of the search model of crime developed in Lee and McCrary (2005) and McCrary (n.d.). Suppose criminal i (the mass of criminals is normalized to 1 by dividing the number of criminals by the regional population), who is ex-ante identical to all other criminals, faces the following dichotomic problem at time t:

$$\max E[b_{i,t} - p_{t,r} J(S_t)|I_t] C_{i,t}$$

where $C_{i,t}$ takes the value 1 if the criminal chooses to commit the crime; the return from crime, $b_{i,t}$, is, for simplicity, uniformly distributed between 0 and B; the joint probability of apprehension and conviction varies across regions and the distribution of the disutility from jail, $J(S_t)$, depends on the expected sentence length, conditional on the information available up to time t, including information about possible future pardons.

Differences in the probability of apprehension and conviction are assumed to be temporary, with mean $E[p_{t,r}] = p_t$. Later in the empirical specification we deal with possible systematic differences by i) controlling for proxies of p, ii) differencing the data, and iii) controlling for regional fixed effects. Information about pardons, I, does not vary across regions. The criminal will commit a crime if $b_{i,t} > p_t E[J(S_t)|I_t] = p_t J_t$.

In the simplified case of a sentence length of one year, the law of motion of criminals is

$$C_{t,r} = \underbrace{1}_{\text{total criminal pop}} - \underbrace{\left[\frac{p_t J_t}{B}(1 - p_{t-1,r} C_{t-1,r})\right]}_{\text{fraction deterred of free population}} - \underbrace{p_{t-1,r} C_{t-1,r}}_{\text{fraction incapacitated}} .$$

It is possible to relax, in a reduced-form approach, the assumption that sentence length, S, equals 1. If S is equal to 2 the model becomes

$$C_{t,r} = \underbrace{1}_{\text{total criminal pop}} - \underbrace{\left[\frac{p_t J_t}{B}(1 - p_{t-1,r} C_{t-1,r} - p_{t-2,r} C_{t-2,r})\right]}_{\text{fraction deterred of free population}} - \underbrace{p_{t-1,r} C_{t-1,r} - p_{t-2,r} C_{t-2,r}}_{\text{fraction incapacitated}},$$

and, after rearranging,

$$C_t = 1 - \frac{p_t J_t}{B} - \left(\frac{p_t J_t}{B} p_{t-1} - p_{t-1}\right) C_{t-1} - \left(\frac{p_t J_t}{R} p_{t-2} - p_{t-2}\right) C_{t-2}.$$

Generalizing to sentence lengths up to duration $S_{\max}$ gives the following:

$$C_{t,r} = 1 - \frac{p_t J_t}{B} - \sum_{s=1}^{S_{\max}} \left(\frac{p_t J_t}{B} p_{t-s} - p_{t-s}\right) C_{t-s,r} \ .$$

Now let us introduce a pardon. The effect of pardoning Z years is to free $W_{t,r}$ criminals at the beginning of period t, $1 - \frac{p_t \tilde{J}_t}{B}$ of whom will recommit crimes during the year:

$$\tilde{C}_{t,r} = 1 - \frac{p_t \tilde{J}_t}{B} \left(1 - \sum_{s=1}^{S_{\max}} p_{t-s,r} C_{t-s,r} + W_{t,r}\right) - \sum_{s=1}^{S_{\max}} p_{t-s,r} C_{t-s,r} + W_{t,r} \ .$$

We allow the pardon to have an effect on future expected sentence lengths, $\tilde{J}_t$. The difference between the scenarios with and without a pardon will be:

$$\tilde{C}_{t,r} - C_{t,r} \quad = \quad \left(\frac{p_t J_t}{B} - \frac{p_t \tilde{J}_t}{B}\right) \left(1 - \sum_{s=1}^{S_{\max}} p_{t-s,r} C_{t-s,r}\right) + W_{t,r} \left(1 - \frac{p_t \tilde{J}_t}{B}\right) \ . \qquad (1)$$

The first summand measures the change in crime due to deterrence, the second summand the change due to incapacitation. In particular, $\left(1 - \frac{p_t \tilde{J}_t}{B}\right)$ measures the fraction of crimes that are attributable to the released criminals, the incapacitation effect.

The Empirical Model – Given our discussions above, we are ready to set up our empirical model. We do not observe the counterfactual criminal scenario of a "pardon year" without a pardon. In our empirical specification we proxy for the counterfactual of crime using years that are contiguous to the pardon. The dependent variable is going to be the first difference in crime rates. To isolate the incapacitation effect, we need to realize that in Italy pardons are nationwide policies and that the deterrence effect is, therefore, unlikely to vary across regions. If time effects, and time-varying variables capture changes in the deterrence effect, then the coefficient on the number of pardoned prisoners captures the incapacitation effect, $1 - \frac{p_t \tilde{J}_t}{B}$.

When we analyze the effect of the prison population on total crime the model is

$$\Delta CRIME_{t,r} = \beta \Delta PRISON_{t,r} + f(t) + \delta' X_{t,r} + \gamma_r + \epsilon_{t,r},$$

where the main variables are expressed in logarithmic terms. Changes in prison population are instrumented using the fraction of pardoned prisoners. Notice that the IV's reduced-form equation in levels,

$$\Delta CRIME_{t,r} = \tilde{\beta} PARDONED_{t,r} + \tilde{f(t)} + \tilde{\delta}' X_{t,r} + \tilde{\gamma}_r + \tilde{\epsilon}_{t,r}$$

is directly related to equation 1, with the counterfactual scenario being replaced with the scenario in the previous year. The term $f(t) + \gamma_r + \delta' X_{t,r}$ is supposed to capture the deterrence effect and isolate the incapacitation effect $\beta = 1 - \frac{p_t \tilde{J}_t}{B}$. All variables except the average sentence length are first-differenced (which controls for systematic differences in the levels) and all but the average sentence length and the probabilities are expressed in terms of 100,000 residents. All regressions include regional fixed effects, which control for systematic differences in trends (for example, long-term changes in the probability of apprehension and conviction, or changes in the attractiveness of the legal labor market, etc.), though results without regional fixed effects are almost identical.

Although yearly fixed effects represent the methodologically correct tool to control for time effect in our experiment, they absorb most of the variation in prison population that is needed for identification when some years of data are unavailable. For this reason we introduce two alternative ways to control for time effects. These controls should approximate the evolution of criminals' expectations.

In one specification we control for a cubic spline using three-year intervals; in the other, we control for pardon-specific linear time trends. The use of splines assumes that criminals' changes in expectations evolve smoothly, without discontinuities. The complexity of the legislative process that leads to pardons makes it difficult to forecast their date of enactment. Moreover, criminals have to forecast not only the date of pardon but also its ending date of coverage. This is likely to smooth the deterrence effect. In the other specification we use pardon-specific linear trends, which assumes that criminals' expectations jump to a new level in the year of the pardon and evolve linearly thereafter. Both the constant term and the coefficient on time are allowed to have a different evolution between each pair of pardons. In other words we simply interact the constant term and time with pardon-specific dummy variables.

The different time controls are shown in Figure 5. The dotted line represents the estimate of $f(t)$ using year fixed effects. The estimated time effects are smoother when we use the three-year cubic spline (solid line), especially during the 1980s and 1990s. But the pardon-specific linear time trends (dashed line) are close to the fixed effects during the 1960s (it is the decade with the highest number of pardons).

4.3 Results

Panel A of Table 9 shows the results of a first-stage regression of the change in prison population on the number of pardoned prisoners. Only where time controls, $f(t)$, are estimated using a time fixed effects the fraction of pardoned prisoners lead to a reduction in the log of prison population that is less than one. The F-statistic is simply the square of the t-statistic, and is largely above the rule-of-thumb threshold level of 10 (Staiger and Stock, 1997). When we control for year fixed effects (column 3), absorbing the nationwide variation in the number of pardoned prisoners, the F-statistic is considerably lower, $(0.376/0.0878)^2 = 4.28^2 = 18.31$, but still above 10.

Panel B shows the reduced form regression, the Two Stage Least-Squares (IV) regression, and the Ordinary Least Squares (OLS) regression results, where the dependent variable is log-changes in crime.[23] The reduced form regressions show patterns that are similar to the

[23] A special event took place in Italy in July 1990: the World Cup soccer tournament. In the 12 regions that hosted at least a game, log-changes in crime were, compared with the remaining regions, 12 percentage points larger in 1990 than in either 1989 or 1991 (p-value of 8 percent). Prisoner flows, however, did not seem to differ significantly because of the World Cup. To control for changes in crime that are due to the World Cup, all regressions control for whether in 1990 the region hosted at least one World Cup game. We also add a dummy equal to one for the region Umbria in 1991 to control for an apparent data error. After the 1990 pardon and amnesty Umbria is the only region that appears to have more pardoned prisoners in 1991 than in 1990.

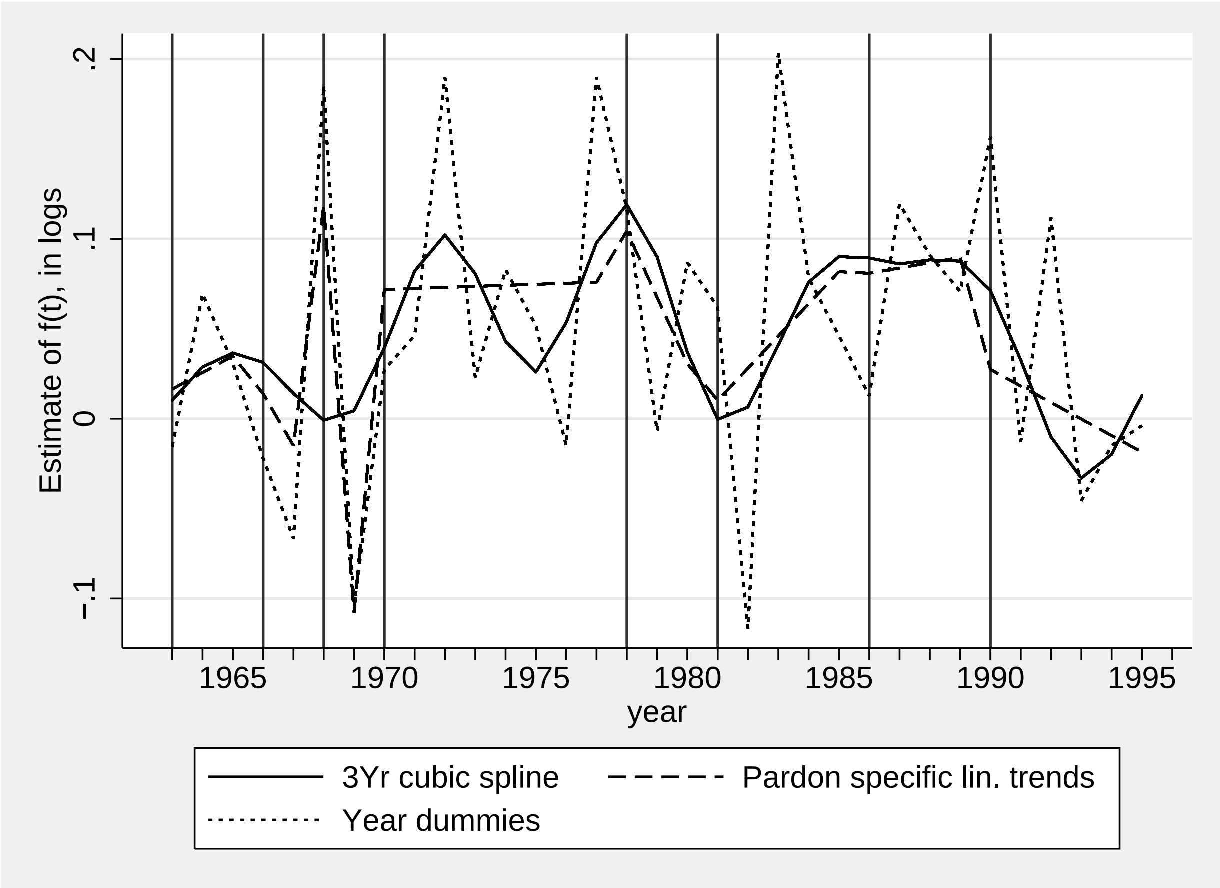

Figure 5: Estimated time effects of the log of total crime ($f(t)$)

Notes: Vertical lines represent years in which pardons or amnesties have been passed.

first stage results. The elasticities between crime and the fraction of pardon prisoners are close to 20 percent with the only exception of the one estimated using time effects that is approximately 50 percent lower. The IV estimates, which correspond to the ration between the reduced form elasticities and the first stage elasticities tell us that a 10 percent reduction in prison population increases the estimated number of crimes by between 1.84 percent and 3.52 percent. As expected the elasticities are closer to zero when we don't control for time fixed effects. Given that not controlling for $f(t)$ the elasticities are quite close to the ones we get using splines or pardon-specific time trends such functional form might not fully capture changes in deterrence.[24] The price one has to pay to add the year fixed effects is a considerable

Moreover, the number is larger than the total prison population (see Statistiche Giudiziarie Penali, Tavola 17.5, on page 629). Later we check whether the results are robust to the exclusion of this dummy variable.

[24]Notice that because of the simultaneity between crime and prison population OLS estimates are biased

loss of precision. When we use year dummies the p-value is 5.6 percent, considerably higher than when using the other functional forms to approximate $f(t)$. Because of data limitations most of the analyses that follow use a smaller sample (fewer years), making the identification that uses year dummies often impractical because of a lack of power. Since deterrence biases the estimates toward zero we are going to stay on the conservative side using pardon-specific time trends each time we need more precision. But we should keep in mind that we are most likely understating the elasticity of crime with respect to prison population.

Robustness Checks – Table 10 addresses several initial robustness checks. Regressions (1) and (4) are just replica of the ones shown in Table 9 shown for reference. The first represents the one with pardon-specific trends, the second the one with year dummies. Regression (2) replicates regression (1) but uses per capita jail population to weight the regressions instead of the resident population. Regressions (3) and (5) show the elasticity when no weighting is used. The results are definitely robust to different weighting procedures. Regression (6) replicates regression (1) but clustering the standard errors by year instead of regions. Such a clustering does increase the standard errors but the results are still significant. Regression (7) adds regions fixed effects, allowing for region specific trends. Regression (8) shows that using variables in levels instead of logs does not alter the results. Each additional released criminal leads on average to 44 additional crimes. Regression (9) shows that the Umbria 1991 dummy does not alter the results. In regression (10) we compute full-year equivalent figures of prison population and of the number of pardoned prisoners only for the first year and not in the following year. The elasticities tend to be smaller because the initial months present the most significant changes in crime. Not adjusting for the exact timing of the pardon, instead, introduces sever measurement error and biases the results toward 0. Notice that since the misclassification error is on average close to 50 percent (it is 0 whenever pardons happen at the very beginning of the year and 1 whenever they happen at the very end) the coefficient is approximately half the size of the elasticity in regression (1).

In regression (12) we test for non-linearities that might be driven by spillovers: congestion

toward zero.

or replacement effects. If criminals in regions with larger reductions in prison population have, because of congestion, a smaller probability of detection, we should expect in absolute terms the elasticities to be larger the larger the reduction in prison population. If instead a larger release of prisoners emphasizes competition between criminals we should expect the opposite to happen. Regression (12) adds the squared change in prison population around the average change (instrumented using the squared fraction of pardoned prisoners). Adding the squared term does not change the coefficient on the linear terms and the coefficient of the squared term is not significantly different from zero. The coefficient is positive, which would be consistent with replacement effects (the larger the reduction in prison population the smaller the change in crime), but is not statistically significant.

Regressions (13) and (14) control for the lagged change in prison population and the lagged changes in crime. If pardons were passed after crime rates have been particularly high, leading to overcrowding, the elasticity estimated in regressions (1) to (12) might just capture correlations between past levels of crime and thus prison population and current changes in prison population. The results show that adding lagged values of prison population or changes in prison population does not alter the results.[25]

Results for Total Crime Conditional on Other Covariates–In Table 11, we additionally control for variables that might affect pardons and crime rates. Since some of the additional controls are available only for the years 1985-1995, the sample size drops from 594 to 198. For this reason we use pardon-specific time trends instead of year dummies to gain precision. Despite the smaller sample size the estimates are precisely measured and are larger than the elasticities estimated before, indicating that the incapacitation might have increased over time. The elasticity drops from -19.6 to -30.7 percent. Less punitive amendments against recurrent and professional criminals during the 1986 and 1990 pardons are likely to be main the reason for this finding.

Changes in the probability that the perpetrator of the crime has been identified by the

[25]To make sure that the elasticity is not driven by a single region or a single pardon we estimate the elasticity of incapacitation excluding any single region or any single pardon the results were always statistically significant and of similar magnitude. The results are available upon request.

police represents one way to measure the productivity of law enforcement. Pardons might reduce the backlog of criminal cases and influence the productivity of law enforcement agencies. An increase in this probability increases the expected sentence length and, therefore, might influence crime. Controlling for sentence length and for changes in the probability that the perpetrator is known leaves the IV elasticity practically unchanged. Changes in GDP are supposed to proxy for legal opportunities of criminals, while changes in consumption are supposed to capture illegal opportunities. In Column 3 we are also controlling for the change in the fraction of population aged 15 to 35, the change in the population with high school degree and the change in the population with university degree. Controlling for these opportunities does not change the elasticity.

Police enforcement might respond strategically to the legislatures's pardons. Depending on their objective function, police officers might either increase or decrease their efforts to apprehend criminals. On the one hand, the supply shock of criminals after a pardon is likely to increase the probability of apprehension (p) and also police activity (A) if police officers' goal is to equate expected marginal benefits $pB(A)$ to marginal costs $C(A)$ and if $B_{AA} < 0$, $C_{AA} > 0$. On the other hand, pardons are likely to weaken the police officers motivations and, therefore, productivity. Pardons do more than nullify part of the officers' past efforts. Criminals who commit a crime before the pardon, but get arrested only after the pardon, can also benefit from the pardon. Thus, even post-pardon arrests might end up with an early release. For these reasons, in columns 4 and 9 we control for changes in the number of police officers and for changes in the number of controlled people. The IV estimates are robust to this inclusion, indicating, at least, that police activity does not change as abruptly as the inmate population.

Finally, we control for changes in the fraction of inmates staying in dormitories and for the change in the rate of overcrowding (inmates divided by available beds). The reason is that changes in prison quality might have a deterrence effect (Katz et al., 2003). Although the change in the rate of overcrowding captures part of the variability that is due to the pardons, there are again no significant changes, which suggests that pardons can be credibly treated as exogenous and that there is no need to control for all these variables.

Results for Different Crime Categories – The results based on total crimes hides some heterogeneity by crime types, in part because some crimes were not subject to pardons. We do not have regional level data on changes in prison population by crime type, thus the independent variable is going to be the same as the one used for total crimes. As we said, not all criminals are pardoned and some restrictions always apply, and a number of crimes, like mafia-related crimes, kidnappings, and sexual assaults, are always excluded from pardons.

But even if a criminal was convicted mainly for one of these crimes, and crime types are recorded based on the most serious offense, he/she might still have committed additional pardonable crimes. This is why Table 4 showed that during the last pardon even some mafia members were released from prison. Also, even if none of the criminals that committed these crimes were released, pardons might still influence these crimes if criminals do not fully specialize in given crimes. Excluded crimes are thus not a perfect placebo test, and Table 12 shows that between 1984 and 1995 the coefficients on the types of crime that were explicitly excluded from pardons, like mafia murders, kidnappings, and sexual assaults, tend to be less precisely estimated. Bank robberies show an elasticity of 41 percent (almost significant at the 5 percent level), and drug-related crimes have an elasticity of 52 percent, even though some drug-related crimes were excluded from the 1990 pardon (but not from the 1990 amnesty).

The effects of pardons on larcenies are not significantly different from zero but Italian victimization surveys show that only around half of those crimes get reported to the police.Muratore et al. (2004). This measurement error is likely to inflate the standard errors of the estimated elasticities. Indeed, motor vehicle theft which, unlike other thefts, are known to be measured with high precision (the rates of reporting are close to 1 due to car insurance), have an elasticity of 27.8 percent. Row 14 and 15 of Table 12 shows that using judicial crime data instead of police data strengthens the overall incapacitation effect (28 percent versus 21 percent). Given that "judges for the initial investigation" (*giudice delle indagini preliminari*) are supposed to dismiss all irrelevant cases before reporting a crime, this result is likely be due to increased precision in the measurement of crime. Consistent with this possibility, the elasticity for all thefts, which in include larcenies and burglaries, is equal to 40.8 percent. Frauds have an elasticity of 32.5

percent, and even the coefficient for homicides (murder and attempted murder) is significantly different from zero (34.6 percent). The judiciary data has robberies, extortions and kidnappings all under one category, and the elasticity is 25.5 percent.

In Section 4.2 we mentioned that regions whose prisoners serve on average shorter terms release on average more prisoners when pardons get enacted. If these released prisoners tend to commit crimes more frequently than average, it is important to control for the average sentence length to rule out a spurious relationship between pardoned prisoners and crime. In Table 13 we rerun the same specification as in Table 12 with in addition the demeaned average log sentence length in the particular crime categories and its interaction with changes in prison population instrumented with its interaction with the fraction of pardoned prisoners. The coefficient on the interaction is never significant and the incapacitation effects are very close to the ones estimated without controlling for sentence length, which indicates that selection is not a concern and that most of the variability in the fraction of released pardons is due to the variability in the date of arrest or in the date of conviction, depending on whether the judge decides to keep the criminal in jail during his trial.

5 Policy Implications

When attempting to solve the problem of prison overcrowding, the important question to ask is whether a forward-looking society would benefit from building new prisons or expanding alternative measures to imprisonment, instead of constantly relying on pardons. Collective pardons and collective amnesties have been shown to increase the total number of crimes. What is still to be determined is whether the marginal social cost of these crimes, when compared with the marginal cost of incarceration, is large enough to make pardons an inefficient policy.

The Marginal Cost of Incarceration – Let us start with the cost of incarceration. Regressing the total budgetary cost of the penitentiary administration (in 2004 euros) on prison population over the past 17 years, we obtain a marginal cost per prisoner of 42,449 euros (95 percent confidence interval [11,066-73,832]) when we use OLS and of 57,830 euros (95 percent confidence interval [44,092-71,568]) when we use a median regression. Dividing the budget by the prison population instead, we get an average cost of 46,452 euros, with a range that varies between 35,496 euros (97 euros per day) and 70,974 (194 euros per day).[26] Notice that these costs do not include tax distortions (it costs more than 1 euro to collect 1 euro in taxes), rehabilitation of the criminal, retribution to society DiIulio (1996), inmates' wasted human capital, their potential increased criminal capital (Chen and Shapiro (2007) focus on the much smaller yearly wave of released prisoners from federal prisons and indeed find that harsher prison conditions worsen recidivism), their post release decline in wages, and the pain and suffering of inmates and of their families (including that due to overcrowding).

The Marginal Cost of Crime – Calculating the marginal cost of crime is more difficult and requires the use of different sources and several assumptions. Table 14 reports the estimated elasticity (ϵ), the probability of reporting (p), the marginal effect of incarceration ($\beta = \frac{\epsilon}{p} \times \frac{\overline{crimes}}{\overline{prison-pop}}$), the cost per crime ($c$), and the social cost ($s = \beta \times c$).[27] The marginal effects are based on the average crime rates in 2004, which is the last year for which the published crime statistics are available. Notice that these social costs are based on the incapacitation effect only and might be larger if deterrence were taken into account. All but two cost-per-crime estimates and the probabilities of reporting a crime come from ISTAT's 2002 victimization study (Muratore et al., 2004) and from Detotto and Vannini (2010). Italy's Value of a Statistical Life (VSL), used to value a lost life due to intentional homicide, are comparable with those from several other studies done in the United States.[28] The social cost of frauds comes from a study by the Italian association of retailers (Confesercenti, 2006).[29] [30] For drug-related crimes, we

[26]These costs tend to be much larger than in the United States (Levitt, 1996), probably because the inmate-to-staff ratio is two to six times larger in Italy than it is in the United States. At the beginning of 2007, the Italian prison system employed more than 45,000 people, with an inmate-to-staff ratio close to 1 (www.polizia-penitenziaria.it). In 2001 the inmate-to-staff ratio, ranged between 1.7 in Maine (with an average cost of 122 dollars per day) and 6.8 in Alabama (with an average cost of 22 dollars per day, www.ojp.usdoj.gov).

[27]As in Levitt (1996) we need to assume that reported and unreported crimes are subject to the same elasticities, an assumption that, since criminals do not know a priori whether a crime gets reported, seems to be reasonable.

[28]Estimates of the VSL for Italy range from 1,448,000 euros to 2,896,000 euros (Albertini and Scarpa, 2004). See Ashenfelter and Greenstone (2004b,a) for an overview of recent estimates of the VSL.

[29]The study uses the following sources for its estimate, fiscal police (*Guardia di Finanza*), customs police (*Agenzia delle Dogane*), survey data, and the anti-fraud phone (*Telefono antiplagio*).

[30]We could not estimate some elasticities, marked with a question mark, while for other elasticities, based on

could not find any cost estimate, while for attempted murder, which also has a positive elasticity, we use the possibly conservative estimate for assaults taken from Levitt (1996). Notice that when computing the total social cost, question marks are treated as zeros, a conservative approach. With the exception of bank robberies, for which such data are available, the cost estimates do not include preventive measures taken by people to fight crime (insurance policies and the like). Apprehensions are also socially costly, because resources must be spent to rearrest pardoned prisoners. But since these costs are difficult quantify they also are excluded.[31]

Policy Implications – Intended to be taken with a grain of salt, given the assumptions, the total social cost amounts to 77,762 euros, a value that is considerably higher than the marginal cost of prisoners.[32][33] The most socially costly crimes after a pardon are frauds (17,700 euros) and non-mafia-related murders (21,000 euros). Next are other robberies (16,800 euros), motor vehicle thefts (a total of 8,500 euros), and bank robberies (6,400 euros).

While some of these costs are simple transfers to criminals we follow Levitt (1996) and do not take their utility into account.[34] To understand how our policy implication depends on these transfers we can compute the relative value lost needed to keep the marginal cost of pardons equal to the marginal cost of incarceration. Assuming that transfers apply only to property crimes and using a marginal cost of prisoners of 42,449 euros the relative loss has to be lower than 1/3 to make pardons efficient. In other words more than 2/3 of the value of the

institutional details of the pardons, we have a conservative guess of zero, marked with a zero and a question mark.

[31]Notice that we are implicitly assuming a linear social function. In case of risk aversion individuals would like to equate their marginal expected (dis)utility from crime with their marginal tax devoted to financing the prison administration. Given that crime involves risk to the public, people should be willing to pay even more than the marginal cost of incarceration to keep criminals in jail.

[32]Even if we exclude the social cost related to frauds, which is the only cost not entirely based on representative victimization surveys or on police reports only, the social cost is still above any measure of marginal cost of prisoners.

[33]We exclude from the cost-benefit analysis pardoned individuals who were subject to alternative measures of detention. The reason for the exclusion is that we do not have region-level data on these measures. We do know, though, that pardons affect the prison population, and the population subject to alternative measures of detention, in the same way. Since the population subject to alternative measures of detention is likely to recidivate less and cost less than the prison population, including them in the cost-benefit analysis is likely to reduce the marginal cost of imprisonment, thereby making the case against pardons and amnesties even stronger.

[34]A justification for doing so might be that only 14 percent of Italians surveyed in 2007 supported the 2006 pardon (EURISPES, 2007), while more than 70 percent of them believed that the pardon had led to an increase in crime.

stolen property (including the damages) need to be transferred from victims to criminals to justify pardons.

6 Conclusions

We use an atypical judicial policy—namely, Italy's collective pardons and amnesties—to estimate the causal effect of incapacitation on crime. We show with a simple model that whenever pardons and amnesties are nationwide policies, the incapacitation effect can be identified separately from the deterrence effect. We can also control for the possible endogeneity of the policy that arises whenever criminals expect a sentence-reducing policy before the policy gets enacted. Ignoring this endogeneity could bias our estimates toward zero. Compared with the elasticities found in Levitt (1996), which uses the status of overcrowding litigation in the U.S. states as an instrument and estimates the sum of incapacitation and deterrence, our elasticities of "just" incapacitation are indeed smaller. Nevertheless, they tend to be larger than previous "non-experimental" estimates. Our OLS estimates as well as the estimates that do not control for year dummies are shown to be biased toward zero casting doubt on estimates of the incapacitation effect that do not try to overcome the simultaneity issue or that do not try to control for deterrence. We also estimate the incapacitation effect using monthly data, exploiting the exact timing of the pardons. These estimates are lower than those estimated based on the panel data, which is consistent with a post-pardon deterrence effect.

Collective pardons and amnesties could represent a more cost-efficient imperfect screening device than individual parole boards. This idea was certainly present in the minds of the legislators. "Formalized" habitual criminals were typically excluded from pardons, and elderly prisoners, believed to have lower recidivism rates, sometimes received larger sentence reductions.

This view could potentially lead to the definition of an optimal release policy, which would likely be several times more efficient than the typical Italian pardon. We leave this topic for future research and perform a cost-benefit analysis that evaluates the net social costs of prison releases. We find the social cost of a release to be significantly larger than the cost of incarceration. In the absence of cost-efficient alternatives to incarceration this finding suggests that that marginal changes in prison population generate more costs than benefits, indicating that prison population might be below its optimal level. It also suggests that pardons should be abandoned or be designed to be more selective.

A Definitions of crime, years available, and source

Bank robberies, 1984-95, police records The seizing property from a bank through violence or intimidation.

Burglaries, 1984-95, police records The unlawful entry of a structure to commit a theft.

Drug-related crimes, 1984-95, police records

Frauds, 1970-1995, judicial records The deceiving of someone to damage him usually, to obtain property or services unjustly. Examples are false advertising, identity theft, false billing, forgery of documents or signatures, false insurance claims, investment frauds, etc.

Homicides, 1970-1995, judicial records

Kidnappings, 1984-95, police records

Larcenies, 1984-95, police records The unlawful taking of property from the possession of another

Mafia murders, 1984-95, police records Intentional homicides related to the organized crime

Motor vehicle theft, 1984-95, police records

Sexual assaults, 1984-95, police records The carnal knowledge against someone's will

Theft and aggravated thefts, 1970-1995, judicial records

Robberies, 1970-1995, judicial records The seizing property through violence or intimidation. Includes extortions and kidnappings.

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Table 1: Fraction of the prison population that is pardoned.

	1963	1966	1968	1970	1978	1981	1986	1990
Abruzzo & Molise	0.301	0.847	0.007	0.732	0.426	0.184	0.274	0.459
Basilicata	0.285	0.642	0.007	0.445	0.287	0.119	0.153	0.348
Calabria	0.248	0.382	0.02	0.378	0.313	0.154	0.137	0.337
Campania	0.175	0.464	0.008	0.698	0.377	0.179	0.211	0.358
Emilia Romagna	0.218	0.619	0.003	0.675	0.318	0.231	0.2	0.433
Friuli-Venezia Giulia	0.276	0.62	0.003	0.709	0.429	0.289	0.333	0.514
Lazio	0.204	0.427	0.036	0.319	0.307	0.212	0.144	0.276
Liguria	0.192	0.579	0.007	0.715	0.392	0.234	0.236	0.372
Lombardia	0.223	0.556	0.028	0.617	0.339	0.214	0.161	0.366
Marche	0.199	0.747	0.025	0.695	0.418	0.152	0.119	0.341
Piemonte & Valle d'Aosta	0.23	0.55	0.014	0.676	0.272	0.15	0.171	0.428
Puglia	0.216	0.512	0.005	0.508	0.397	0.246	0.277	0.401
Sardegna	0.132	0.387	0.004	0.389	0.266	0.202	0.205	0.243
Sicilia	0.192	0.447	0.007	0.497	0.369	0.199	0.103	0.419
Toscana	0.224	0.69	0.012	0.579	0.357	0.239	0.256	0.281
Trentino-Alto Adige	0.247	0.591	0.091	0.772	0.638	0.32	0.415	0.504
Umbria	0.172	0.385	0	0.573	0.425	0.205	0.47	0.316
Veneto	0.251	0.62	0.011	0.549	0.366	0.193	0.287	0.462

Table 2: Pardoned inmates per 100,000 residents.

	1963	1966	1968	1970	1978	1981	1986	1990
Abruzzo & Molise	5.4	16.3	0.2	24.1	14.2	7.9	12.5	19.0
Basilicata	10.7	24.5	0.3	17.2	14.9	5.7	8.2	13.1
Calabria	12.1	15.8	0.9	14.9	13.9	7.4	8.3	12.4
Campania	11.4	28.4	0.4	35.3	18.7	6.6	14.5	15.8
Emilia Romagna	6.0	16.0	0.1	14.4	9.1	6.7	6.5	13.8
Friuli-Venezia Giulia	10.1	18.9	0.1	18.9	14.8	9.2	11.3	13.7
Lazio	8.8	19.1	1.4	11.9	15.3	13.3	9.3	12.0
Liguria	10.2	29.8	0.3	29.0	18.8	11.1	12.4	13.4
Lombardia	8.0	17.4	0.7	14.6	9.1	5.9	6.1	12.2
Marche	2.8	13.8	0.4	13.7	5.0	2.8	4.0	6.9
Piemonte & Valle d'Aosta	9.6	19.8	0.5	19.6	10.4	6.3	9.1	18.8
Puglia	10.1	20.2	0.2	18.6	18.2	12.7	12.9	14.1
Sardegna	7.0	18.7	0.2	15.5	11.8	11.3	10.7	11.5
Sicilia	13.3	29.3	0.4	24.8	22.4	11.7	7.3	19.5
Toscana	7.2	19.0	0.3	14.1	9.9	7.2	9.9	10.3
Trentino-Alto Adige	7.4	17.3	2.1	17.7	13.7	10.1	14.0	15.1
Umbria	5.7	13.0	0.0	22.0	16.6	5.4	7.3	8.5
Veneto	5.8	12.9	0.2	10.6	7.5	4.3	7.3	8.4

Table 3: Summary statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
		Judiciary data			
	Monthly average sentence				
Theft and aggravated theft	7.264	3.007	2.747	25.313	468
Attempted and committed intentional homicide	124.322	33.146	0	360	468
Robberies, extortions and kidnappings	32.211	14.075	0	139.13	468
Frauds	7.354	1.925	2.667	18.557	468
Total	12.245	3.967	5.044	26.781	468
	Number of recorded crimes				
Theft and aggravated theft	2443.668	1156.177	238.676	8078.645	468
Attempted and committed intentional homicide	3.936	3.252	0.257	23.585	468
Robberies, extortions and kidnappings	60.767	65.034	0.995	306.061	468
Frauds	47.678	33.739	11.367	298.439	468
Total	3724.753	1591.018	788.667	11623.533	468
	Other				
Fraction of known perpetrators (in %)	21.322	14.879	0	73.915	612
		Police data			
	Number of recorded crimes				
Mafia murders	0.565	1.205	0	7.971	255
Sexual assaults	1.371	0.511	0	3.605	255
Kidnappings	1.154	0.49	0	2.578	255
Drug related c.	46.264	28.907	0	159.845	255
Larceny	265.133	192.141	0	1073.249	255
Burglary	285.311	119.974	0	754.677	255
Motor vehicle theft	420.964	283.699	0	1174.157	255
Bank robberies	1.584	2.383	0	12.75	612
Total	2159.907	1355.672	536.903	7696.002	612
	Other				
# of police forces	414.48	180.539	112.932	1008.553	288
# of police controls	46381	22102	0	125820	255
		Prison data			
Prison population	43.977	18.03	7.504	100.916	612
Pardoned prisoners	3.599	6.01	0	35.552	612
Fraction in dormitories (in %)	12.504	6.675	0	36.113	611
		Other data			
GDP per capita (/1000)	14.137	3.639	7.273	21.515	288
Consumption per capita (/1000)	11.192	1.857	7.325	17.361	288
Unemployment rate (in %)	8.630	4.195	3.189	24.137	288
Population between age 15 and 35	0.296	0.122	0	0.641	288
Fraction with high school degree	0.153	0.048	0.076	0.408	288
Fraction with university degree	0.033	0.011	0.015	0.084	288

Notes: Whenever applicable variables are expressed per 100,000 residents.

Table 4: Distribution of criminal types that are in jail before and after the July 2006 pardon.

	July 2006	rank	September 2006	rank	% Change
Crimes against wealth	0.309	1	0.277	1	-0.43
Crimes against persons	0.149	2	0.167	2	-0.29
Drug related crimes	0.146	3	0.166	3	-0.28
Illegal possession of weapons	0.141	4	0.144	4	-0.36
Public trust	0.048	5	0.041	5	-0.46
Crimes against the public administration	0.038	6	0.032	7	-0.47
Crimes against the justice department	0.034	7	0.027	8	-0.50
Third book of administrative sanctions	0.025	8	0.025	9	-0.37
Mafia related crimes	0.025	9	0.033	6	-0.17
Other crimes	0.085	.	0.088	.	-0.35
Total	1	.	1		
Total number of prisoners	60,710		38,326		-0.37

Notes: Based on DAP (2006). The % Change represents the percentage change in the number of prisoners by main crime typology.

Table 5: Time-series Evidence of the Relationship Between Crime Rates and Pardons

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	Total crime rate					
	levels	Δ levels	Δ levels	logs	Δ logs	Δ logs
Total crimes (t-1)	0.826***					
	(0.038)					
Log total crimes (t-1)				0.736***		
				(0.045)		
Pardon month	-2.738	-2.653	-1.501	-0.042*	-0.038	-0.027
	(2.414)	(2.509)	(1.923)	(0.025)	(0.026)	(0.021)
Pardon month (t-1)	5.906**	6.504**	4.310**	0.062**	0.077***	0.056***
	(2.418)	(2.509)	(1.922)	(0.025)	(0.026)	(0.021)
Pardon month (t-2)	1.346	0.866	0.014	-0.002	-0.007	-0.017
	(2.416)	(2.509)	(1.921)	(0.025)	(0.026)	(0.021)
Time	0.259	0.142	0.133	0.007***	0.001	0.001
	(0.192)	(0.198)	(0.148)	(0.002)	(0.002)	(0.002)
Time sq.	0.023**	-0.006	-0.005	0.000*	-0.000	-0.000
	(0.010)	(0.008)	(0.006)	(0.000)	(0.000)	(0.000)
Month dummies	no	yes	no	yes	no	yes
Observations	250	250	250	250	250	250
R-squared	0.979	0.034	0.484	0.973	0.044	0.451

Notes: Monthly time-series ranging from January 1962 to December 1982. Standard errors in parentheses. * significant at 5 percent; ** significant at 1 percent.

Table 6: Laws proposed before the July 2006 pardon.

	Legislature	House of rep.	Senate
July 2006	XV	4	1
June	XV	2	4
May	XV	5	0
April	XV	0	0
TOTAL	XV	**11**	**5**
January-March	XIV	0	0
December 2005	XIV	0	1
November	XIV	1	0
May-October	XIV	0	0
April	XIV	2	3
August 2003-March 2005	XIV	0	0
July 2003	XIV	2	0
February-June	XIV	0	0
January	XIV	1	0
December 2002	XIV	0	0
November	XIV	3	3
October	XIV	1	0
September	XIV	4	0
July-August	XIV	0	0
June	XIV	0	2
March-May	XIV	0	0
February	XIV	1	0
January	XIV	0	1
October-December 2001	XIV	0	0
September	XIV	2	0
August	XIV	0	0
July	XIV	2	0
June	XIV	1	0
TOTAL	XIV	**20**	**10**
May 1996-May 2001	XIII	12	7
March 1994-April 1996	XII	3	2
April 1992-April 1994	XI	3	1

Table 7: Probability That in a Given Month a Pardon or an Amnesty is Passed

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.	Probability that during the month a pardon is passed					
Total crimes	-0.203	-0.358				
	(0.192)	(0.259)				
Total crimes ($t-1$)	-0.012	0.356				
	(0.215)	(0.311)				
Total crimes ($t-2$)	0.060	-0.264				
	(0.215)	(0.310)				
Total crimes ($t-3$)	0.105	0.244				
	(0.194)	(0.261)				
Average crimes during the last 6 months			-0.029	-0.008		
			(0.141)	(0.144)		
Average crimes during the last year					-0.044	-0.047
					(0.172)	(0.174)
Cubic in time	yes	yes	yes	yes	yes	yes
Observations	234	234	230	230	218	218
R-squared	0.01	0.05	0.01	0.04	0.01	0.04

Notes: Monthly time-series ranging from January 1962 to December 1982. Standard errors in parentheses.

Table 8: Testing the endogeneity of pardons

	(1)	(2)	(3)	(4)	(5)	(6)
	Fraction. of pardoned inmates (adj.)					
	1 Percent sample		5 Percent sample		10 Percent sample	
Crime (t-1)	-0.0179	-0.0180	-0.0211*	-0.0172	0.0250*	0.0303*
	(0.0103)	(0.0109)	(0.0114)	(0.0122)	(0.0139)	(0.0150)
Region FE	No	Yes	No	Yes	No	Yes
Observations	324	324	216	216	189	189
R-squared	0.005	0.024	0.010	0.074	0.017	0.119

Notes: The sample is restricted to those region-years that have at least 1 or 10 percent of prisoners released because of a pardon. Standard errors clustered by region in parentheses. * significant at 5 percent; ** significant at 1 percent.

Table 9: (Log-) changes in crime on (log-) changes in prison population, 1963-1995.

		(1)	(2)	(3)	(4)
Dependent var.		Panel A: Δ log prison pop.			
FIRST	Frac. pardoned	-1.192***	-1.240***	-0.376***	-1.051***
STAGE	prisoners	(0.1000)	(0.106)	(0.0878)	(0.0812)
	R-squared	0.500	0.525	0.729	0.362
Dependent var.		Panel B: Δ crime			
REDUCED	Frac. pardoned	0.219***	0.244***	0.132*	0.207***
FORM	prisoners	(0.0313)	(0.0410)	(0.0749)	(0.0298)
	R-squared	0.321	0.317	0.374	0.089
IV	Log-change	-0.184***	-0.197***	-0.352*	-0.197***
	in prison pop.	(0.0261)	(0.0266)	(0.184)	(0.0219)
OLS	Log-change	-0.0694**	-0.0896**	0.0108	-0.108***
	in prison pop.	(0.0252)	(0.0317)	(0.0344)	(0.0219)
	R-squared	0.295	0.303	0.371	0.082
Year controls		spline	time trends	dummies	none
Observations		594	594	594	594

Notes: All regressions are weighted by the resident population and include a 1990 Soccer World-cup dummy equal to one for the regions where at least one game was played and a year 1991 dummy for the region Umbria due to data inconsistencies. Standard errors clustered by region in parentheses. * significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent.

Table 10: (Log-) changes in crime on (log-) changes in prison population, 1963-1995.

	Dependent variable	Log-Change in crime rates			
Log-Change in prison pop. (adj.)		coefficient	SE	R-squared	N.obs.
(1)	Baseline; pardon specific trends; weighted by pop.	-0.196***	(0.0267)	0.176	594
(2)	Weighted by per capita jail population	-0.243***	(0.0315)	0.146	594
(3)	Unweighted regression	-0.220***	(0.0297)	0.147	594
(4)	With year dummies	-0.352*	(0.184)	0.235	594
(5)	Unweighted regression with year dummies	-0.319*	(0.176)	0.175	594
(6)	SE clusterd by year	-0.196**	(0.0845)	0.176	594
(7)	With region dummies	-0.219***	(0.0296)	0.150	594
(8)	In levels	-44.08***	(9.013)	-	594
(9)	Without the Umbria 1991 dummy	-0.196***	(0.0266)	0.175	594
(10)	Adjusting prison population only for the pardon year	-0.360***	(0.0703)	0.085	594
(11)	No adjustment for the exact timing of the pardon	-0.115***	(0.0365)	0.157	594
(12)	Squared polynomial of prison population	-0.184***	(0.0521)	0.177	594
	squared term	0.149	(0.496)		594
(13)	With lagged change in prison population	-0.207***	(0.0269)	0.186	576
	lagged term	-0.0719	(0.0546)		
(14)	With lagged change in crime	-0.198***	(0.0283)	0.180	576
	lagged term	-0.0507	(0.0493)		

Notes: All regressions are 2SLS regressions. The baseline regression is weighted by the resident population and includes a 1990 Soccer World-cup dummy equal to one for the regions where at least one game was played and a year 1991 dummy for the region Umbria due to data inconsistencies. Standard errors clustered by region in parentheses. * significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent.

Table 11: The incapacitation elasticity after controlling for additional factors.

	(1)	(2)	(3)	(4)	(5)
	Log-change in crime, reduced form				
Log-Change in prison pop. (adj.)	-0.307***	-0.311***	-0.319***	-0.301***	-0.305***
	(0.0340)	(0.0356)	(0.0464)	(0.0404)	(0.0422)
Log sentence length		0.0253**	0.0301**	0.0324**	0.0265*
		(0.0126)	(0.0152)	(0.0149)	(0.0153)
Log change in probability		0.0197	-0.00300	0.00177	0.00104
perpetrator is known		(0.0175)	(0.0176)	(0.0196)	(0.0249)
Log change in GDP			0.0751	-0.00976	-0.0493
			(0.324)	(0.327)	(0.310)
Log change in consumption			0.805	0.562	-0.0402
			(0.569)	(0.611)	(0.685)
Log change in unemployment rate			-0.0677	-0.0821	-0.0653
			(0.0608)	(0.0581)	(0.0520)
Log change in pop. 15-35			4.729*	3.687*	4.164
			(2.662)	(2.228)	(2.750)
Log change in pop.			-0.298**	-0.317**	-0.313**
with high school degree			(0.149)	(0.142)	(0.139)
Log change in pop.			0.103	0.101	0.0758
with university degree			(0.121)	(0.117)	(0.114)
Log change in police officers				0.0548	-0.0174
				(0.152)	(0.121)
Log change in number				0.147***	0.124***
of people controlled by the police				(0.0435)	(0.0474)
Log change in the fraction of					-0.0556***
inmates staying in dormitories					(0.0172)
Log change in overcrowding					0.0744**
					(0.0332)
Observations	198	198	198	198	198
R-squared	0.50	0.50	0.51	0.52	0.52

Notes: All IV regressions are weighted by the resident population and include pardon-specific time trends, a 1990 Soccer World-cup dummy equal to one for the regions where at least one game was played, and a year 1991 dummy for the region Umbria due to data inconsistencies. Standard errors clustered by region in parentheses. * significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent.

Table 12: The incapacitation effect for different types of crime for the years 1985-1995.

	Dep. variable	Coeff.	SE	Obs.
		Police data 1985-1995		
(1)	Mafia homicides	-0.590	(0.374)	91
(2)	Sexual Assault	-0.242*	(0.135)	216
(3)	Kidnappings	-0.176	(0.139)	216
(4)	Drug deals	-0.517***	(0.0701)	216
(5)	Larceny	0.0208	(0.201)	216
(6)	Burglary	-0.162***	(0.0355)	216
(7)	MV thefts	-0.278***	(0.0498)	216
(8)	Robberies	-0.406**	(0.161)	216
(9)	Total	-0.325***	(0.0321)	216
		Judiciary data 1970-1995		
(10)	Thefts	-0.408***	(0.0907)	450
(11)	Homicides	-0.346***	(0.0792)	450
(12)	Robberies	-0.255***	(0.0744)	450
(13)	Frauds	-0.325*	(0.177)	450
(14)	Total crimes (judiciary)	-0.285***	(0.0599)	450
(15)	Total crimes (police)	-0.212***	(0.0316)	450

Notes: All IV regressions are weighted by the resident population and include pardon-specific time trends, a 1990 Soccer World-cup dummy equal to one for the regions where at least one game was played, and a year 1991 dummy for the region Umbria due to data inconsistencies. Standard errors clustered by region in parentheses. * significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent.

Table 13: The incapacitation effect for different types of crime for the years 1970-1995. Controlling for selection.

	(1) Thefts	(2) Homicides	(3) Robberies	(4) Frauds	(5) All
Log-change	-0.398***	-0.347***	-0.253***	-0.315*	-0.277***
in prison pop.	(0.110)	(0.0733)	(0.0768)	(0.181)	(0.0599)
Log sentence	-0.00344	0.0310	0.00136	-0.0604	-0.0254
	(0.0191)	(0.0683)	(0.0209)	(0.0478)	(0.0204)
Interaction	0.257	0.0925	0.118	0.802	-0.0260
	(0.295)	(0.398)	(0.128)	(0.692)	(0.156)
Observations	450	440	438	438	438

Notes: All IV regressions are weighted by the resident population and include pardon-specific time trends, a 1990 Soccer World-cup dummy equal to one for the regions where at least one game was played, and a year 1991 dummy for the region Umbria due to data inconsistencies. Standard errors clustered by region in parentheses. * significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent.

Table 14: Social benefit from incarceration

	Total	Elasticity	Reporting prob.	Marginal effect	Cost per crime	Social cost
Against the person						
Massacre	24	0?	-	-	-	-
Mafia related murder	299	0.00	1.00	0.00	0.00	0.00
non-Mafia related murder	1,249	0.35	1.00	0.01	2,679,690	21,052
Attempted murder	1,542	0.35	1.00	0.01	?	?
Infanticide	6	?	-	-	-	?
Voluntary manslaughter	83	?	-	-	-	?
Involuntary manslaughter	8,294	?	-	-	-	?
Sexual assault	4,571	0.24	1.00	0.02	50,600	1,018
Other (including assault, battery, pornography)	290,612	0 ?	-	-	-	?
Against the family, the morale, the animals						
	18,180	0 ?	-	-	-	-
Against property						
Motor vehicle theft (motorbikes)	80,494	0.28	0.95	0.43	2,156	928
Motor vehicle theft (cars)	182,470	0.28	0.87	1.07	7,145	7,618
Other thefts	1,252,117	0.41	0.54	17.16	326	5,594
Bank robbery	2,683	0.41	1.00	0.02	324,809	6,465
Other robberies	47,046	0.26	0.50	0.44	38,330	16,856
Extorsion	8,024	?	-	-	-	-
Kidnappings	196	0.00	-	-	-	-
Harm to things, animals, property, etc.	300,352	?	-	-	-	-
Fraud	301,428	0.33	1.00	1.78	9,953	17,727
Against the economy and the public trust						
Commercial fraud	8,583	0.33	1.00	0.05	9,953	505
Drug related crimes	33,417	0.52	1.00	0.31	-	?
Other (forged currency, counterfeit)	193,095	0.33	1.00	1.14	-	?
Against the State and the public order						
	74,610	0 ?	-	-	-	-
Other crimes						
	153,878	0 ?	-	-	-	-
					Total social cost	77,762.61
					Total social cost excluding frauds	60,035.34

Notes: See Section 5 for the list of sources and assumptions used.