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

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We estimate the "incapacitation effect" on crime using variation in Italian prison population driven by eight collective pardons passed between 1962 and 1990. 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 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 −17 and −30 percent. A cost-benefit analysis suggests that Italy's prison population is below its optimal level. (*JEL* K42)

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).[1] This paper provides a detailed empirical analysis of both by measuring the total effect of incarceration on crime but also attempting to disentangle the deterrence effect of corrective measures from their incapacitation effect (Shavell 1987).

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[1] 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. 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 (Raphael and Stoll 2004; Duggan 2004; Johnson and Raphael 2012).

With this objective in mind, we exploit as quasi-natural experiments a series of collective pardons enacted in Italy during the years 1962–1990. The collective pardons that we study are release policies based on general criteria that lead to the release of prisoners whose residual sentence length is less than a given number of years, usually two or three. These policies generate a large variation in prison population across time and across 18 Italian regions.[2] For instance, the last collective pardon (which we exploit to some extent but that we do not end up using in our main results due to some missing data), passed on July 31, 2006, led within a day to the release of 22,000 inmates, around 30 percent of the total (Dipartimento dell'Amministrazione Penitenziaria 2006). Hence, 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.

We use these sudden exogenous changes in prison population to break the classical simultaneity between crime and prisoners.[3] In addition to controlling for simultaneity, we address two issues that have plagued the literature that has tried to estimate the effect of prison population on crime using aggregate crime regressions (see Durlauf and Nagin 2010):[4] (i) the use of ad hoc model specifications (added controls, functional forms, etc); (ii) prison population is not a policy variable but rather an outcome of the certainty and the severity of punishments, which makes the interpretation of its coefficient difficult, to say the least. We address the first criticism showing that pardons generate such extreme reductions in prison population that the results are robust regardless of the controls we use or the functional form of the regression (log or levels) or of the way we control for time effects. As for (ii), we exploit a policy where prison population does become a policy variable with clear and well defined implications for deterrence.

Usually policies that cause large reductions in prison population, i.e., generalized reductions in sentence lengths or increased use of alternative sentencing, lead to a decrease in deterrence as criminals incorporate the reduced severity of punishment. By contrast pardons lead to an increase in deterrence, since: (i) the next pardon is unlikely to happen very soon, and (ii) pardoned sentences might be added to the new sentence (see Drago, Galbiati, and Vertova 2009).[5] This means that without controlling for deterrence one can use pardons to construct a reasonable lower bound: the incapacitation effect is going to be at least as negative as the total effect. Estimates based on the other policies mentioned above can at best identify a much less informative upper bound to incapacitation. For example, in Levitt (1996), a set of indicator variables capture the status of overcrowding litigation, which generate variation in US prison population. Sometimes court decisions led to fewer offenders sentenced to prison terms, sometimes to early release programs and other times

[2] Italy has 20 regions but the crime and judiciary statistics of the Italian Statistical Office combine together Piemonte/Valle d'Aosta and Abruzzo/Molise.

[3] Moreover, evidence based on a high-frequency monthly time series shows that such policies are not related to past national crime rates. Our identification strategy relies on yearly data but we make occasional use of available monthly data to supplement our arguments.

[4] Section A lists all major papers that are cited in Durlauf and Nagin (2010) critique.

[5] Criminals might try to predict the timing of pardons and change their behavior accordingly. This would lead to more crime just before pardons and less crime just after, biasing the estimates toward zero.

to the construction of new prison facilities and to a reallocation of prisoners across institutions. Such decisions cause reductions in prison population, but also, most likely, reductions in deterrence.[6]

Consistent with results in Drago, Galbiati, and Vertova (2009), our estimated elasticity of total crime with respect to prison population controlling for deterrence is more negative (−22 percent) than the elasticity estimated without controlling for deterrence, and also larger than the estimates based on monthly time-series data (see the online Appendix A.A4).[7]

In addition to having an experiment that allows us to identify a lower bound to incapacitation, we strive to control for a host of deterrence effects and get as close of an estimate as possible of a pure incapacitation effect. In this regard the most important feature of our experiment is the nationwide nature of Italian pardons. Pardons are national laws beyond the control of regional and prison administrations, and follow the same identical rules across regions. As such, they should affect criminal expectations and the deterrence that depends on them in a similar way across the country. This implies that controlling for time effects should absorb the deterrence effect, which works through criminals' expectations. Notice that if pardons were regional we would not be able to control for this type of deterrence. We also consider additional types of deterrence that might be associated with our experiment such as congestion and crowding out effects but find little evidence of them.

We highlight that the assumptions that are needed to isolate a pure incapacitation effect, namely, that conditional on time-varying observable characteristics and on time controls there are no more systematic differences in deterrence across regions (or that such differences are uncorrelated with the fraction of released inmates).

These assumptions might be violated. Pardons might generate local deterrence effects that require unidentifiable time-regions fixed effects to be controlled for. Nonetheless, we can verify the extent to which our assumptions are violated in specific cases. For instance regions might differ in the fraction of residents who are on the margin between committing and not committing a crime. Although it is impossible to measure the fraction of residents who are on the margin of committing a crime, this fraction should be related to the degree by which crime rates and prison population rates vary between and within regions. The larger such fraction the more one would expect crime and prison population to vary over time. In this case we show that local effects are indeed present, but they are small and do not change our conclusions.

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. We sum them and find that the social cost per released prisoner is larger than 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. Given the recurrent

[6] See also Durlauf and Nagin (2011) for a discussion on how the interpretation of Levitt's (1996) results depend on how the offenders' beliefs about punishment change after such court orders.

[7] Our estimates are less negative than ones found in Levitt (1996). This squares with the fact that he estimates a combined effect of deterrence and incapacitation in an experiment that entails a decrease in deterrence.

problems of overcrowding in recent years we conclude that an expansion in prison capacity should be preferred to such unselective pardons.

A. *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 ranges from positive figures down to −28 percent. Among these 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.

According to Durlauf and Nagin (2010, 2011) these early studies fail to account for the simultaneity between crime and imprisonment.[8] Given that when crime rises the prison population will mechanically increase ceteris paribus, simultaneity biases the estimated correlations between crime rates and incarceration rates toward zero. Donohue (2009) reviews five additional studies, Levitt (1996); Becsi (1999); Spelman (2000, 2005); and Johnson and Raphael (2012); which account for such biases.[9]

Levitt (1996) controls for simultaneity using overcrowding litigation status as an instrument and finds elasticities between −0.26 and −0.42, about two to three times larger than in previous studies. Johnson and Raphael (2012) use a convincing alternative instrumental variable approach, namely the predictive power of changes in steady state incarceration rates that are driven by past shocks to crime rates, and also find that IV estimates are larger than OLS.[10] The studies mentioned above estimate the combined effect of incapacitation and deterrence.

Studies That Isolate Incapacitation.—There are two other studies that isolate incapacitation, Owens (2009) and Buonanno and Raphael (2013).

The first uses a one-time exogenous reduction in sentence enhancements for 23–25 year-old inmates in the state of Maryland to isolate incapacitation. Within a seven month period released inmates are on average arrested for almost three criminal acts. This is clearly a selected group of inmates, and the author 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

[8] They also criticize such studies for the use of ad hoc model specifications and for treating prison population as if it was a policy variable.

[9] A final paper that Donohue (2009) reviews, Liedka, Morrison, and Useem (2006), much in the spirit of Marvell and Moody (1994) dismisses endogeneity issues using Granger causality arguments.

[10] The estimates of Johnson and Raphael (2012), based on more recent data than Levitt (1996), are between −0.06 and −0.11 for violent crime and between −0.15 and −0.21 for property crime. Their IV estimates for the earlier time period suggest much larger crime-prison effects, more in line with Levitt's (1996) elasticities of −0.38 for violent crimes, and −0.26 for property crimes, with decreasing marginal returns to incarceration being the most obvious explanation for these differences.

types of crimes or a different number of crimes than nonarrested ones. It might also not properly capture congestion and replacement effects that we discuss later in our section on identification.[11]

Buonanno and Raphael (2013) use region-level monthly data around the 2006 pardon to estimate the incapacitation effect. Disregarding deterrence, which they argue should be modest given the high frequency of their data, the number of saved crimes ranges between 17 and 21 crimes per prison year served when the authors exploit the discontinuity in their experiment, and ranges between 22 and 46 crimes when exploiting the dynamic adjustment path for incarceration and crime that is induced by the one-time shock provided by the pardon. Our estimated elasticities imply that a prison year saves around 22 crimes.

Studies That Isolate Deterrence.—More research has tried to isolate deterrence. One of the first studies, Kessler and Levitt (1999), exploits California's Proposition 8 sentence enhancements. In the short run these would just add additional time in jail to already long sentences, generating deterrence due to increased severity of punishments without generating incapacitation. Kessler and Levitt (1999) find strong evidence of deterrence, though these results have been challenged by Webster, Doob, and Zimring (2006). Kessler and Levitt (1999) examined data from every other year. The effects are much less evident when data on all years are used (see Webster, Doob, and Zimring 2006; Raphael 2006).

Helland and Tabarrok (2007) use the deterrent effect of California's "three-strike" law, to isolate deterrence. They find significantly lower recidivism for individuals convicted of two previous strike-eligible offenses than for individuals who had been convicted of only one strike-eligible offense but who, in addition, had been tried for a second strike-eligible offense and ultimately were convicted of a nonstrike-eligible offense. Weisburd, Einat, and Kowalski (2008) use a randomized field trial of alternative strategies for incentivising the payment of court ordered fines to estimate deterrence. Drago, Galbiati, and Vertova (2009) exploit random variation in sentence length due to a recent Italian collective pardon, and based on detailed microdata on recidivism, isolate and find strong evidence of deterrence.[12] Levitt (1998) measures changes in annual crime rates at the age of majority, where a discontinuity in punitiveness occurs. There seems to be large changes in deterrence, though at annual frequencies, the estimated effect might reflect both deterrence and incapacitation (Durlauf and Nagin 2011). This might explain the contrasting results of Lee and McCrary (2005). They examine a longitudinal database of individual level arrest records in Florida, taking advantage of data on the exact date of birth of arrestees and look for discontinuous changes in recidivism using a regression discontinuity design at the age of 18 when prospective offenders face more severe punishments but find no sizable changes in criminal behavior.

[11] A special issue of the *Journal of Quantitative Criminology* contains a thorough overview of studies on "incapacitation" by Piquero and Blumstein (2007), though they conflate it with deterrence.

[12] More specifically, in cases of recidivism the pardoned sentence is added to the new one, and this is shown to increase deterrence. While, due to data limitations, the 2006 pardon is not included in our sample, such pardon features the same general provisions, including the increased sentence, contained in all past pardons.

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 2007) and release small numbers of inmates. Despite this, Mocan and Gittings (2001) estimate the deterrence effect of gubernatorial pardons of persons on death row, finding that three additional pardons generate 1 to 1.5 additional homicides. Indeed, Donohue and Wolfers (2006) show that Mocan and Gittings' (2001) results are not robust to small and reasonable deviations to the empirical specifications. Kuziemko (2013) 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 who 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.[13]

I. Italy's Collective Pardons and Prison Population

In this section we provide the legal definition of pardons and amnesties, and describe the characteristics that are relevant in our quasi-natural experiment. We also review some additional facts for which information is available only for the pardon passed in 2006 but that we think also characterize the earlier pardons that we analyze.

Legal Definition.—Until 1992 the Italian president could issue pardons or amnesties after they had been mandated by a simple parliamentary majority.[14,15] Regions, instead, do not have legislative powers on criminal matters.

The main difference between amnesties and pardons is that amnesties eliminate both the sentence and the crime, as if they had 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

[13] 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.

[14] After that year 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).

[15] Appendix A.A1 contains a brief history of Italian pardons.

followed by amnesties.[16] 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. Amnesties, instead, are permanent.[17,18]

Structure of Recent Pardons and Amnesties.—Pardon and amnesty laws passed between 1963 and 2006 had the following structure:[19] (i) for pardons they would specify the number of years of the total sentence that would be pardoned (usually 1, 2, or 3 years), while for amnesties they would specify a threshold for the statutory maximum sentence, such that every criminal whose statutory maximum sentence was below such thresholds would be released (usually 3, 4, or 5 years). Between 1963 and 1986 often one year would be added to these thresholds when the inmate's age was below 18 or above 65. (ii) Sometimes the law would specify that the pardon or amnesty provisions would only apply to certain crimes (often nonfinancial crimes), or it would specify that for a few minor crimes such threshold would not apply (tax crimes, crimes related to student demonstrations, etc.). (iii) The enacting laws would always contain a list of crimes that would not be allowed to be pardoned, though the most common crimes would typically not be part of this list. (iv) In an attempt to lower the effects on crime, up until 1986 the laws would also explicitly exclude recurrent offenders from such clemency acts. In Section IIIB, we show that excluding pardons and amnesties in which recurrent criminals are not released leads to smaller incapacitation effects.

Once a pardon or an amnesty is passed a judge needs to check whether an inmate fulfills all the requirements that are specified by the law, and if so, mandates his immediate release. Prison authorities and regional governments cannot influence such decision, which can only be based on factual evidence.

Dynamics of Recent Pardons and Prison Population.—The left panel of Figure 1 shows the official prison capacity, and the total crime rate, as well as how prison population evolved between 1962 and 1995. The sawtooth shaped pattern in prison population is driven by pardons and amnesties. Figure 2 shows the log changes in prison population and the fraction of pardoned prisoners.[20] 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 some regions. But the effect of pardons on prison population

[16] Otherwise, prosecutors would have to spend time and effort investigating pardoned crimes, even if it is impossible to actually punish the perpetrators.

[17] 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).

[18] 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. Recidivism rates for these selected individuals appear to be significantly lower than those for prisoners (Santoro and Tucci 2006) and that some of these individuals might commit crimes even while subject to these alternative measures. Nevertheless, changes in crime might be due in part to these additional pardoned individuals.

[19] See online Appendix Table A1 for a more detailed description of each pardon.

[20] The Italian National Statistical Institute (ISTAT) groups together pardoned and amnestied prisoners.

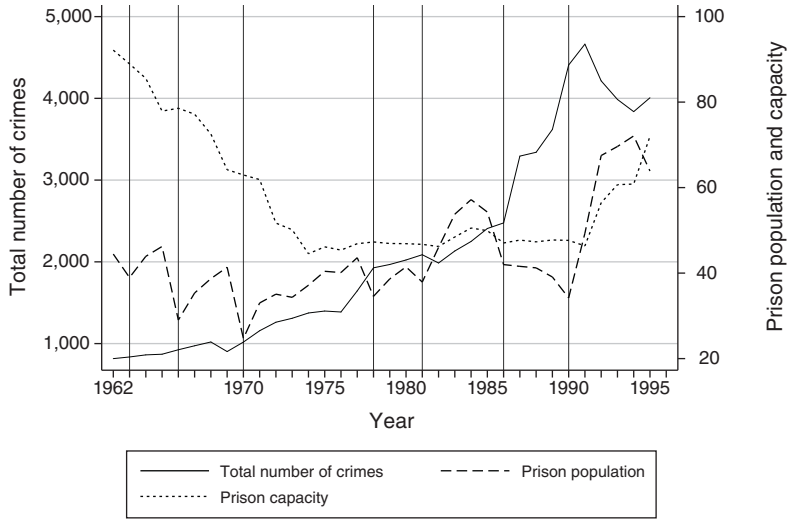

Figure 1. End of the Year Prison Population, Prison Capacity, and the Total Number of Crimes

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

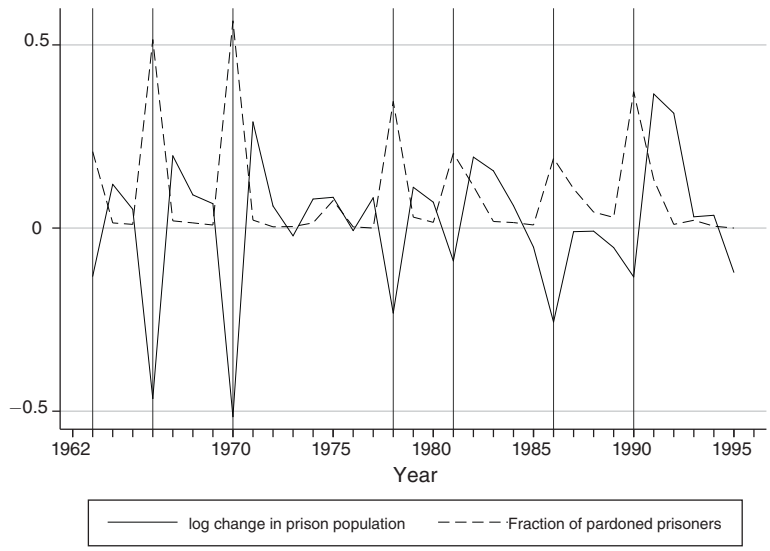

Figure 2. Change in Prison Population and Pardoned Prisoners

Notes: Vertical lines represent years in which pardons or amnesties have been passed. The regression line indicates an OLS prediction.

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.

Table 1—Fraction of the Prison Population That Is Pardoned

	1963	1966	1968	1970	1978	1981	1986	1990
Abruzzo & Molise	0.30	0.85	0.01	0.74	0.42	0.18	0.27	0.46
Basilicata	0.29	0.65	0.01	0.45	0.29	0.12	0.15	0.35
Calabria	0.25	0.38	0.02	0.38	0.31	0.15	0.14	0.34
Campania	0.17	0.46	0.01	0.70	0.38	0.18	0.21	0.36
Emilia Romagna	0.22	0.62	0.00	0.67	0.32	0.23	0.20	0.43
Friuli-Venezia Giulia	0.28	0.62	0.00	0.71	0.43	0.29	0.33	0.52
Lazio	0.20	0.42	0.04	0.32	0.31	0.21	0.14	0.28
Liguria	0.19	0.58	0.01	0.71	0.39	0.24	0.24	0.37
Lombardia	0.22	0.55	0.03	0.61	0.34	0.21	0.16	0.37
Marche	0.20	0.75	0.03	0.70	0.42	0.15	0.12	0.34
Piemonte & Valle d'Aosta	0.23	0.55	0.01	0.67	0.27	0.15	0.17	0.43
Puglia	0.22	0.51	0.01	0.51	0.40	0.25	0.28	0.40
Sardegna	0.13	0.39	0.00	0.39	0.27	0.20	0.20	0.24
Sicilia	0.19	0.45	0.01	0.50	0.37	0.20	0.10	0.42
Toscana	0.22	0.69	0.01	0.58	0.36	0.24	0.26	0.28
Trentino-Alto Adige	0.25	0.59	0.09	0.77	0.64	0.32	0.41	0.50
Umbria	0.17	0.39	0.00	0.57	0.42	0.21	0.47	0.32
Veneto	0.25	0.62	0.01	0.55	0.37	0.19	0.29	0.46

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.

Regional Variation of Recent Pardons and Amnesties.—There is also variation in the number and in the fraction of pardoned prisoners across regions (Table 1). For example, 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.[21]

Selection of Released Inmates by Recent Pardons and Amnesties.—It is important to understand whether the pardoned inmates represent the whole prison population. While for the pardons that happened before 2006 prison population is not available by crime type, such information is available for the 2006 pardon. Table 2 shows that the distribution of inmates by crime category is essentially unchanged before (July 2006) and after (September 2006) the 2006 pardon. The last column shows that 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

[21] The last pardon in our sample happened in 1990, as the judicial data about the 2006 pardon are not available yet.

Table 2—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 percent change represents the percentage change in the number of prisoners by main crime typology.

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 who have committed crimes against wealth, but the difference is 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 (see online Appendix Table A1) shows that historically very few crime categories have been excluded from such clemency bills, suggesting that differences between pardons are likely to be negligible. As a result, 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. In addition, given that recurrent criminals were not released until the 1986 pardon, inmates released before that date should be less criminogenic, which likely bias our results toward finding no effects.

Link between Regional Variation in Crime and Prison Population.—Regional prison population and the regional crime rate are tightly connected due to laws 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 and might only later be transferred to a prison that is closest to where the criminal's family resides.[22]

II. Data

Data Sources.—The Italian National Statistical Institute (ISTAT) publishes a yearly statistical supplement about the Italian judicial system. From these supplements, we collected information about the evolution of the prison population

[22] These provisions are contained in the Article 8 of the Codice di Procedura Penale (competenza per il territorio) and Article 42 of the 26 of July 1975 n. 354 law. Also notice that each region might have one or more jurisdictions.

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.[23]

In summary, our sample contains yearly data on (aggregate) crime rates and inmate population by region for the years 1962 to 1995. This sample spans eight pardons/amnesties that happened in the years 1963, 1966, 1968, 1970, 1978, 1981, 1986, 1990. In addition the sample contains also yearly disaggregate data by category of crime by region for the years 1985 to 1995. We also exploit for some results the availability of monthly aggregate crime rates for the years 1962 to 1982.

Summary Statistics.—Table 3 shows the summary statistics of the variables 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 inmates per 100,000 residents, 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.[24] 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, for which the definition is clear, and 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. 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

[23] 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).

[24] ISTAT does not provide statistics for several types of crime, which is why the sum of individual crimes for which ISTAT provides statistics is smaller than the total number of crimes.

Table 3—Summary Statistics

Variable	Mean	SD	Min.	Max.	Observations
			Judiciary data		
Monthly average sentence					
Thefts	7.286	3.127	2.747	25.313	468
Attempted and committed intentional homicide	126.966	41.941	0	360	468
Robberies, extortions, and kidnappings	32.244	16.079	0	139.13	468
Frauds	7.515	2.198	2.667	18.557	468
Total	12.016	4.14	5.044	26.781	468
Number of recorded crimes					
Thefts	2,072.664	1,160.16	238.676	8,078.645	468
Attempted and committed intentional homicide	3.61	3.32	0.257	23.585	468
Robberies, extortions, and kidnappings	43.946	54.326	0.995	306.061	468
Frauds	43.942	30.609	11.367	298.439	468
Total	3,283.956	1,554.405	788.667	11,623.533	468
Other					
Fraction of known perpetrators (in %)	23.539	16.925	0	73.915	612
			Police data		
Number of recorded crimes					
Mafia murders	0.473	1.209	0	7.971	234
Sexual assaults	1.488	0.501	0.491	3.605	234
Kidnappings	1.202	0.445	0.164	2.578	234
Drug-related crimes	46.346	29.392	1.966	159.845	234
Larceny	211.039	187.982	8.356	1,073.249	234
Burglary	276.073	112.222	11.155	754.677	234
Motor vehicle theft	331.375	264.212	48.011	1,174.157	234
Bank robberies	3.456	2.071	0.495	12.75	234
Total	1,983.47	1,297.925	536.903	7,696.002	612
Other					
Number of police forces	439.842	180.384	112.932	1,008.553	288
Number of police controls	52,970.296	28,174.884	0	125,819.99	255
			Prison data		
Prison population	42.434	17.212	7.504	100.916	612
Pardoned prisoners	3.575	6.072	0	35.552	612
Fraction in dormitories (in %)	12.166	5.708	0	36.113	611
			Other data		
GDP per capita (/1,000)	13.681	3.493	7.273	21.515	288
Consumption per capita (/1,000)	11.202	2.018	7.325	17.361	288
Unemployment rate	8.847	4.043	3.189	24.137	288
Population between age 15 and 35	0.3	0.133	0	0.641	288
Fraction with high school degree	0.156	0.054	0.076	0.408	288
Fraction with university degree	0.033	0.012	0.015	0.084	288

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

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 (the year in which they can potentially commit crimes for the whole year).[25]

More generally, based on the day of the year, d, on which the pardon becomes active, full-year equivalent pardoned prisoners are $PAR^*_{t,r} = \frac{365-d}{365} PAR_{t,r}$ in the year of the pardon and $PAR^*_{t,r} = \frac{d}{365} PAR_{t,r-1} + PAR_{t,r}$ in the year after the pardon, and $PAR^*_{t,r} = PAR_{t,r}$ in all other years. We also adjust the prison population accordingly. This adjustment assumes that the effect of a pardon is short-lived and that the effect lasts at most one year and is evenly distributed over 12 months. Since the evidence based on monthly data shows that most of the effects is concentrated in the very first months from the releases these turn out to be conservative assumptions. In the robustness section we experiment with several alternative adjustments and anticipate here that the effects are larger when incapacitation is (i) assumed to end on December 31, or is (ii) assumed to follow a seasonal component, or finally (iii) is assumed to be decreasing over time.

III. The Estimated Incapacitation Effect

A. *Identification Using Yearly Panel Data*

Collective pardons trigger simultaneous regional variations in crime that we assume to be conditionally exogenous, meaning exogenous after controlling for time effects and time-varying covariates. 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.[26]

Our identification strategy instruments changes in regional prison population with the number of pardoned prisoners released in the region. The inclusion of time effects coupled with the nature of our experiment allows us to kill two birds with one stone: (i) time controls purify our estimates from negative deterrence effects (including those that work through criminals' expectations) given the homogeneity of pardons (and expectations) across regions and (ii) they neutralize the possibility that criminals' expectations about pardons render these policies endogenous. In practice there might be deterrence effects that we cannot control for (Durlauf and Nagin 2010). In the rest of this section we offer a taxonomy of deterrence for our experiment highlighting the various difficulties that we face in our identification strategy and the remedies we propose.

[25] 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).

[26] Such variation comes from two sources: (i) 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 (Anderson, Kling, and Stith 1999); (ii) 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.

Long-Term Deterrence Effect.—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.

Prepardon 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. 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.

Prepardon 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. There is an endless sequence of pardon bills on the Parliament floor which is likely to be the prime source of information to predict pardons. Table A2 (in the online Appendix) shows that there were so many proposals that never became law that criminals would have a hard time predicting the timing of a new pardon (including the cutoff date).

Postpardon 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.[27] 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.

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 $p_{t,r}$. However, 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.[28] 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

[27] Drago, Galbiati, and Vertova (2009) use this rule to isolate deterrence effects.

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

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 test for these additional effects that depend on the size of the released prison population and generate nonlinearities between crime and prison population.

Endogeneity of the Instrument.—A 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).[29] Monthly data allow us to try to predict the implementation of a pardon using the information on crime available until right before it is passed. In particular Table 4 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 pardons are passed depending on recent patterns of crime. In addition, since pardons are unlikely to depend on year-over-year changes in crime we adopt the precaution of differencing the data, working with changes in crime instead of levels.[30]

Regional Composition Bias.—It might be the case that regions that had higher crime rates in the past release more prisoners. The fraction of pardoned prisoners in a region might thus 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 5 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, so that a compositional bias is unlikely to arise.

Average Effects and Local Effects.—While we showed that most types of criminals get released (see Table 2), variation in the distribution of crime seriousness across regions and over time might bias our estimates. 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 that are on average less serious. The opposite would be true if criminals who are caught

[29] 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. The 1986 pardon was the first one to solve a situation of overcrowding. The online Appendix discusses more in detail overcrowding and prison capacity.

[30] Differencing the data is also important in case crime levels and prison population are nonstationary.

Table 4—Probability That in a Given Month a Pardon or an Amnesty Is Passed

	Probability in percent that during the month a pardon was passed								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Total crimes	−0.217	−0.205	−0.217						
	(0.174)	(0.178)	(0.237)						
Total crimes $(t-1)$	0.076	0.077	0.083						
	(0.200)	(0.202)	(0.280)						
Total crimes $(t-2)$	−0.186	−0.189	0.083						
	(0.201)	(0.203)	(0.281)						
Total crimes $(t-3)$	0.328*	0.316*	0.041						
	(0.174)	(0.181)	(0.241)						
Average crimes during the last 6 months				0.000	−0.019	−0.006			
				(0.025)	(0.124)	(0.125)			
Average crimes during the last year							0.000	−0.009	−0.016
							(0.026)	(0.144)	(0.144)
Cubic in time	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Month FE	No	No	Yes	No	No	Yes	No	No	Yes
Observations	249	249	249	247	247	247	241	241	241
R^2	0.020	0.023	0.058	0.000	0.006	0.054	0.000	0.008	0.059

Notes: Monthly nationwide time-series ranging from January 1962 to December 1982. The probability is measured in percent and the regression is estimated using a linear probability model. Standard errors in parentheses.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Table 5—Testing the Endogeneity of Pardons

	Fraction of pardoned inmates (adj.)					
	1 percent sample		5 percent sample		10 percent sample	
	(1)	(2)	(3)	(4)	(5)	(6)
Crime $(t-1)$	−0.013	−0.011	−0.010	−0.002	0.033	0.042
	(0.013)	(0.015)	(0.018)	(0.024)	(0.020)	(0.026)
Region FE	No	Yes	No	Yes	No	Yes
Observations	324	324	213	213	189	189
R^2	0.002	0.024	0.002	0.088	0.022	0.148

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 the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

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.[31] We exploit the regional

[31] 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

variation given that approximately 90 percent of inmates get arrested in the region they reside (ISTAT 1961–1995).[32]

The Behavioral Model.—Let us introduce a simple model of criminal behavior to discipline 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's (1999) model, can be viewed as a reduced form of the search model of crime developed in Lee and McCrary (2005) and McCrary (2010). 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:

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

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

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

lengths contained a judge-specific fixed effect, though we do not have data to test for the existence of these fixed effects.

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

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:

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

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.

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 needed for identification when some years of data are unavailable such as when we look into crimes by category. For this reason we introduce two alternative ways to control for time effects. We believe that these controls approximate adequately 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 3. 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). That said, unlike for most release policies any residual deterrence would bias our results toward finding no incapacitation effect.

B. *Results*

Results for Total Crime.—Panel A of Table 6 shows the results of a first-stage regression of the change in prison population on the number of pardoned prisoners. Only when time controls, $f(t)$, are estimated using time fixed effects (column 3) the fraction of pardoned prisoners loses significant predictive power. When we control for year fixed effects, absorbing the nationwide variation in the number of pardoned

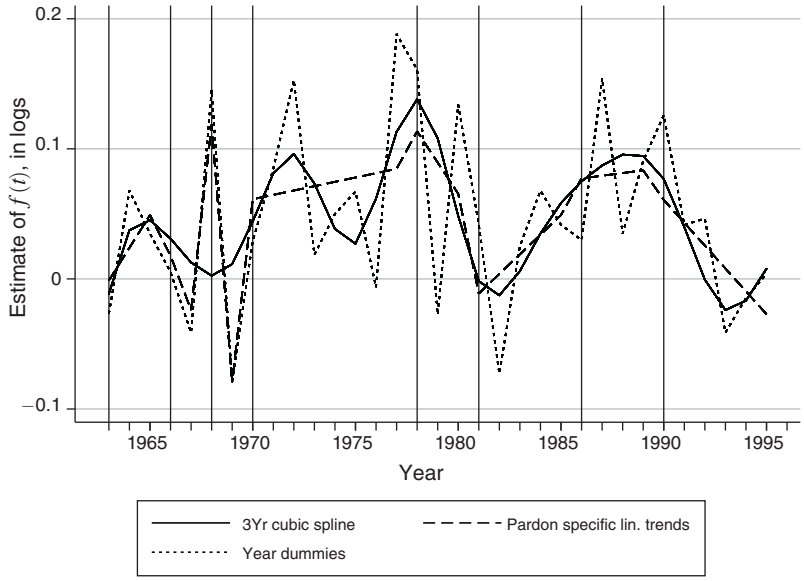

Figure 3. Estimated Time Effects of the log of Total Crime ($f(t)$)

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

prisoners, the F-statistic drops from around 200 to 17, which, however, is above the rule-of-thumb threshold level of 10 (Staiger and Stock 1997).

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.[33] The reduced form regressions are consistent with the first stage results. The estimated elasticity between crime and the fraction of pardon prisoners is close to 20 percent with the exception of the estimates using time fixed effects that is approximately 50 percent lower. The IV estimates, which correspond to the ratio of the reduced form elasticities on the first stage elasticities, tell us that a 10 percent reduction in prison population increases the estimated number of crimes by between 1.53 percent and 2.23 percent. The estimated elasticities are smaller when we don't control for time using time fixed effects; time fixed effects are in fact the "correct" way of controlling for deterrence and we confirm empirically that our estimates of incapacitation are contaminated by a negative postpardon deterrence effect when we do not control for time; we also notice that the use of splines or pardon-specific time trends do not adequately

[33] A special event took place in Italy in July 1990: the World Cup soccer tournament. In the 12 regions that hosted at least one 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), which is consistent with what was found by Campaniello (2013). 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. 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.

Table 6—(log) Changes in Crime on (log) Changes in Prison Population, 1963–1995

	(1)	(2)	(3)	(4)
Panel A. Δ log prison population				
FIRST STAGE				
Pardoned prisoners	−1.393***	−1.360***	−0.513***	−1.202***
	(0.0973)	(0.105)	(0.125)	(0.0743)
R^2	0.482	0.503	0.686	0.358
F-stat (excluded IV)	204.8	169.1	16.95	261.5
Partial R^2 (ex. IV)	0.416	0.337	0.028	0.355
Panel B. Δ crime				
REDUCED FORM				
Pardoned prisoners	0.213***	0.236***	0.115*	0.203***
	(0.0268)	(0.0374)	(0.0630)	(0.0296)
R^2	0.293	0.275	0.330	0.079
IV				
Change in prison population	−0.153***	−0.174***	−0.223*	−0.169***
	(0.0208)	(0.0286)	(0.124)	(0.0225)
R^2	0.247	0.246	0.252	0.050
OLS				
Change in prison population	−0.0686***	−0.0912***	−0.000950	−0.0967***
	(0.0181)	(0.0202)	(0.0271)	(0.0156)
R^2	0.274	0.268	0.328	0.075
Year controls	spline	time trends	dummies	none
Observations	594	594	594	594

Notes: All regressions 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 the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

control for deterrence. Nonetheless the price one has to pay in order to use year fixed effects is a considerable loss of precision. In fact, when we use year dummies the p-value of the elasticity of incapacitation is 5.6 percent, considerably higher than when using the other functional forms to control for time. However, due to data limitations, most of the analysis that follows uses a smaller sample (fewer years), making identification using year dummies often impractical due to lack of power. Yet, as discussed, even though postpardon deterrence biases the estimates toward zero, we are satisfied with less precise time controls since we most likely identify a very useful lower bound to incapacitation when using pardon-specific time trends. Notice, finally, that in line with Levitt (1996) and Johnson and Raphael (2012) and because of the simultaneity between crime and prison population, OLS estimates are biased toward zero.

Robustness Checks.—Table 7 performs a battery of robustness checks. Each line corresponds to a different regression. Regressions (1) and (9) are just replica of the ones shown in Table 6 and are shown for reference. Column 1 represents the regression with pardon-specific trends, column 2 the one with year dummies. Regressions (2) to (8) in panel A address the issue of heterogeneity of the effects via weighted

Table 7—(log) Changes in Crime on (log) Changes in Prison Population, 1963–1995

		Coefficient	SE	R^2	Observations
(1)	Baseline; pardon specific trends	−0.172***	(0.029)	0.158	594
Panel A. Different weighting					
(2)	Weighted by resident population	−0.156***	(0.027)	0.179	594
(3)	Weighted by per capita jail population	−0.195***	(0.030)	0.154	594
(4)	Weighted by total jail population	−0.166***	(0.028)	0.181	594
(5)	Weighted by the variance of Δ crime	−0.201***	(0.039)	0.165	594
(6)	Weighted by the variance of crime	−0.188***	(0.046)	0.174	594
(7)	Weighted by the variance of Δ prison population	−0.198***	(0.030)	0.142	594
(8)	Weighted by the variance of prison population	−0.177***	(0.028)	0.155	594
Panel B. Additional fixed effects and different clustering					
(9)	With year dummies	−0.223*	(0.124)	0.252	594
(10)	With region dummies	−0.173***	(0.029)	0.159	594
(11)	SE clustered by year	−0.172***	(0.067)	0.158	594
Panel C. Different adjustments for the exact timing of the pardon					
(12)	No adjustment	−0.119***	(0.033)	0.117	594
(13)	Only for the pardon years	−0.323***	(0.091)	0.035	594
(14)	Only the IV for the pardon years	−0.200***	(0.066)	0.053	594
(15)	Based on the monthly distribution of crimes	−0.183***	(0.030)	0.151	594
(16)	Based on a decreasing linear function	−0.201***	(0.032)	0.144	594
Panel D. Nonlinearities and lagged variables					
(17)	Adding a squared polynomial of prison population	−0.145***	(0.043)	0.149	594
(18)	Adding a lagged change in prison population	−0.182***	(0.030)	0.158	576
(19)	Adding a lagged change of crime	−0.181***	(0.029)	0.160	576
Panel E. Additional robustness checks					
(20)	In levels	−15.238***	(2.741)	0.256	594
(21)	In levels with year fixed effects	−21.500*	(12.616)	0.296	594
(22)	Without the Umbria 1991 dummy	−0.160***	(0.029)	0.163	594

Notes: All 2SLS regressions 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, unless otherwise specified. Standard errors clustered by region in parentheses.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

regressions.[34] Changing results when using different weights would signal the presence of such heterogeneity.

Regressions (2) to (4) weight the data based on resident population, per capita jail population, and total jail population, respectively. These regressions give more weight to regions with more resident population, or more prison population (in levels or in relative terms). These estimates are all close to 17.4 percent, the unweighted estimate.

Regressions (5) to (8) address the potential heterogeneity in deterrence across regions. Recall equation (1) in our behavioral model in which a product of two terms captures the change in deterrence: $\left(\frac{p_t J_t}{B} - \frac{p_t \tilde{J}_t}{B}\right) \times \left(1 - \sum_{s=1}^{S_{\max}} p_{t-s,r} C_{t-s,r}\right)$. Even if the change in the perceived severity (the first factor) varies uniformly across

[34] Online Appendix Table A3 shows the distribution of the weights across regions.

regions, regions might differ in the potential criminal population (the second factor). While such potential criminal population is likely to be proportional to either the resident population or the prison population, there might be residual heterogeneity that is difficult to account for. For instance, regions might differ in the fraction of residents who are at the margin between committing and not committing crimes. If such margins were correlated with changes in prison population they might bias our estimates of incapacitation. Assuming that for a given region the fraction of such potential offenders tends to vary little over time one would expect regions with more "marginal" criminals to exhibit large variations in crime and, as a consequence, in prison population. We weight our regressions based on such variances in (5) to (8), giving more weight to regions with higher variances in (i) crime rates, (ii) changes in crime rates, (iii) prison population, and (iv) changes in prison population; we find that the elasticities are indeed more negative, but not by much. Later, in Table 8 we produce a more direct test of heterogeneity interacting the variables used in weighting our regressions with the changes in prison population.

Panel B illustrates the robustness of the results when we add region fixed effects. Since the model is in first differences, these fixed effects capture differential linear time trends across regions. This addition does not alter the estimated elasticity.

Standard errors are always clustered at the region level. But there are only 18 regions, and standard errors would be underestimated if within-cluster correlation of the regressor and of the errors were large. Since we first-difference the data and regressors vary both with time and group, the serial correlation is likely to be small. Cameron, Gelbach, and Miller (2008) show that both special small-sample corrections and jackknife estimates represent two effective ways to correct for such bias.[35] When we use small sample adjustments the standard errors of the baseline estimate increase from 0.0286 to 0.0299, a 4 percent increase. When we use jackknife corrected standard errors they are actually 3 percent smaller (0.0278).

Regression (11) clusters the standard errors by year and not by regions producing higher standard errors, although the p-value is still below 1 percent.[36]

Panel C shows what happens when we change the assumptions on the timing of the pardons changing the construction of year-equivalent figures. Recall that our data measure prison population and pardoned prisoners by the end of the year. Our adjustment in our baseline regression assigns pardoned inmates to the pardon year in proportion to the fraction of time left between the date of the pardon and year-end (see equation (2)) assuming that the remaining pardoned inmates are criminally active the following year. Although we find our adjustment plausible, it has been pointed out that the most active offenders might indeed be rearrested shortly after the pardon so that most of the pardon effects on crime might be concentrated right after the release.

[35] The small sample adjustment inflates the variance of the estimator by $(N-1)/(N-K) \times M/(M-1)$, where N, K, and M stand for the number of observations, regressors, and regions or clusters, and jackknife estimates (the jackknife drops a region at the time, computes the leave-one-out estimate and then uses the 18 estimates to compute the variance) represent two valid ways to adjust such bias.

[36] The standard error when clustering by regions *and* year is equal to 0.0656, with a p-value that is still below 1 percent (Cameron, Gelbach, and Miller 2011).

Table 8—Heterogeneity of the Effects

	Δ log crime					
	(1)	(2)	(3)	(4)	(5)	(6)
Δ log-prison population (adj.)	−0.175***	−0.172***	−0.170***	−0.179***	−0.175***	−0.177***
	(0.029)	(0.030)	(0.028)	(0.029)	(0.028)	(0.028)
log inmates/population	0.009					
	(0.010)					
log inmates		0.002				
		(0.002)				
log V(Δ crime)			0.002			
			(0.001)			
log V(crime)				0.002		
				(0.001)		
log V(inmates)					−0.004**	
					(0.002)	
log V(Δ inmates)						−0.003**
						(0.001)
Interaction	0.008	0.006	−0.058**	−0.025	−0.019	−0.003
	(0.049)	(0.024)	(0.023)	(0.035)	(0.040)	(0.032)
Observations	594	594	594	594	594	594
R^2	0.155	0.155	0.161	0.156	0.156	0.155

Notes: All 2SLS regressions 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. All the interacted variables have been demeaned. "V" stands for the within-region variance. Standard errors clustered by region in parentheses.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Regression (12) shows that by not making any adjustments for the exact timing of the pardon produces a substantially lower elasticity. This is clearly due to severe measurement error that biases the results toward 0. Whenever a pardon happens at the end of the year there isn't enough time for offenders to commit crimes. The misclassification error is indeed 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) and consistent with this result the estimated elasticity in regression (12) is approximately half the size of the estimated elasticity in regression (1).

In regression (13) we still adjust prison population and the number of pardoned prisoners proportionally to the fraction of time left between the date of the pardon and year-end, but we do not implement any adjustment for the following year. This is like assuming that criminals commit crimes only in the first year. This adjustment has the effect of inflating the pardon effect with respect to regression (1). In regression (14) we adjust the number of pardoned inmates but not the prison population, and just for the years when the pardons get passed. This is like treating changes in prison population as if they were affected by classical measurement error that could be fixed using IV. The estimated elasticity is −25.4 percent. Regression (15) relaxes our baseline assumption that the distribution of criminal activity is uniform over 12 months by assigning pardoned inmates based on the monthly distribution of crimes over the whole period and across all regions. The resulting estimated elasticity is

slightly more negative than in the baseline case. Regression (16) assigns pardoned inmates over time using a triangular density with mode in the first month after the pardon. This assumption is meant to capture that pardoned inmates commit most of the crimes right after their release and that their activity tapers off linearly over the next 12 months. The resulting estimated elasticity is -0.201. On net, results in regressions (12) to (16) lead us to conclude that alternative adjustments of pardoned inmates over time in an effort to better capture the true timing of pardons produces estimated elasticities in line or even more negative than under our baseline assumption.

In panel D we test for nonlinearities and see whether the results are robust to the inclusion of lagged variables. In regression (17) we test for nonlinearities that might be driven by spillovers (congestion or replacement effects). If criminals in regions with larger reductions in prison population have, because of congestion, a smaller probability of detection, we expect the estimated elasticities to be more negative the larger the reduction in prison population. If instead a larger release of prisoners emphasizes competition between criminals we expect the opposite to happen. Adding the (demeaned) squared (log) change in prison population (instrumented using the squared fraction of pardoned prisoners) does not lead to large changes in the coefficient on the linear terms and the coefficient of the squared term is 0.197 ($SE = 0.267$), which would be consistent with replacement effects (the larger the changes in prison population the smaller the change in crime), but it is not statistically significant.

Regressions (18) and (19) control for the lagged change in prison population and the lagged change in crime, respectively. If pardons were passed after crime rates have been particularly high, leading to overcrowding, the elasticity estimated in regressions (1) to (17) 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.

Regression (20) shows that using variables in levels instead of logs, the estimated number of additional crimes is 15.25 without year dummies and equal to 21.5 with year dummies. The corresponding elasticities evaluated at the average change in prison population and at the average crime rate are close to 10 and 14 percent, slightly lower than when logs are used. Finally, regression (22) shows that the Umbria 1991 dummy does not alter the results, though it does when using year fixed effects.

In Table 8 we address the issue of heterogeneity using a more conventional approach, interacting the log change in prison population with variables that potentially measure heterogeneity, the same variables we used in panel A of Table 7 to produce our weights. These variables are in logs and demeaned for a straightforward interpretation. The direct elasticities of all these variables are close to zero and are only significant for the demeaned (log) variances of inmates and changes in inmates. More importantly, all the interactions are close to zero and only the variance in crime seems to signal some significant heterogeneity. Doubling the variance of crime (see online Appendix Table A3 for the distribution of variances across regions) reduces the elasticity from -17 percent to -22.8 percent, suggesting that

deterrence might indeed vary slightly across regions, due to different sizes of the inframarginal criminal population. However, we cannot rule out that our measures for the size of the inframarginal criminal population are just too noisy to precisely quantify heterogeneity.

To make sure that our results are not driven by a single region or a single decade we estimate the elasticity of incapacitation excluding single regions or single decades one at the time. Online Appendix Tables A4 and A5 show that there is no single region that drives the results. The analysis that excludes one decade at the time suggests that the elasticities were particularly large in the 90s, which is consistent with the fact that for the first time in the 1990 pardon and amnesty even recurrent criminals were released.

Results for Total Crime Conditional on Additional Covariates.—In Table 9, we report results obtained when controlling for additional time-varying covariates. Since some of the additional controls are available only for the years 1985–1995, the sample size drops from 594 to 198 observations. For this reason we use pardon-specific time trends instead of year dummies to gain precision. Despite the smaller sample size the elasticities are estimated quite precisely and are larger than the elasticities estimated before, suggesting that incapacitation might have increased over time. The elasticity drops from -17.4 to -26.8 percent. Less punitive amendments against recurrent and professional criminals during the 1986 and 1990 pardons are likely to be the main reason for these findings (see online Appendix Table A1).

Changes in the probability that the perpetrator of a crime has been identified by the 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 also control for the change in the fraction of population aged 15 to 35, the change in the population with a high school degree and the change in the population with a university degree. These additional controls do not change our estimated elasticities.

Police enforcement might respond strategically to the legislatures's pardons. Police officers might either increase or decrease their efforts to apprehend criminals depending on their objective function. 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 postpardon 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

Table 9—The Incapacitation Elasticity after Controlling for Additional Factors

	log change in crime, reduced form				
	(1)	(2)	(3)	(4)	(5)
log change in prison population (adj.)	−0.268***	−0.271***	−0.274***	−0.271***	−0.240***
	(0.052)	(0.053)	(0.053)	(0.050)	(0.048)
log sentence length		0.031*	0.034*	0.037**	0.027
		(0.016)	(0.018)	(0.018)	(0.018)
log change in probability perpetrator is known		0.009	−0.003	−0.002	−0.016
		(0.031)	(0.028)	(0.030)	(0.033)
log change in GDP			−0.259	−0.228	−0.121
			(0.404)	(0.405)	(0.332)
log change in consumption			0.206	0.081	−0.213
			(0.484)	(0.503)	(0.514)
log change in unemployment rate			−0.173**	−0.171**	−0.114**
			(0.074)	(0.073)	(0.055)
log change in population 15–35			2.252	2.069	1.206
			(1.902)	(1.788)	(1.714)
log change in population with high school degree			−0.243	−0.257	−0.259*
			(0.179)	(0.176)	(0.149)
log change in population with university degree			−0.080	−0.078	−0.071
			(0.104)	(0.107)	(0.100)
log change in police officers				−0.002	0.021
				(0.063)	(0.062)
log change in number of people controlled				0.061	0.046
				(0.056)	(0.055)
log change in the fraction of inmates staying in dormitories					−0.023
					(0.032)
log change in overcrowding					0.100***
					(0.019)
Observations	198	198	198	198	198
R^2	0.507	0.510	0.543	0.547	0.591

Notes: All 2SLS regressions 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 the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

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 we add these controls is that changes in prison quality might have a deterrence effect (Katz, Levitt, and Shustorovich 2003). Although the change in the rate of overcrowding captures part of the variability that is due to the pardons, we do not find significant changes to our estimated elasticities also suggesting that pardons can be credibly treated as exogenous.

Results for Different Crime Categories.—The results based on total crimes might ignore significant heterogeneity across crime types. To uncover this heterogeneity

Table 10—The Incapacitation Effect for Different Types of Crime for the Years 1985–1995

	Dependent variable	Coefficient	SE	R^2	Coefficient	SE	R^2	Observations
Police data 1985–1995								
		In logs			In levels			
(1)	Mafia homicides	−0.801*	(0.434)	0.194	−0.011**	(0.005)	0.101	216
(2)	Sexual assaults	−0.135	(0.226)	0.036	−0.006	(0.007)	0.021	216
(3)	Kidnappings	−0.004	(0.299)	0.014	−0.002	(0.007)	0.026	216
(4)	Drug deals	−0.477***	(0.082)	0.137	−0.550***	(0.124)	−0.020	216
(5)	Larcenies	0.012	(0.175)	0.056	0.226	(0.875)	0.114	216
(6)	Burglaries	−0.151***	(0.041)	0.107	−1.143***	(0.326)	0.290	216
(7)	MV thefts	−0.214***	(0.054)	0.398	−3.702***	(0.765)	0.396	216
(8)	Bank robberies	−0.410*	(0.211)	0.067	−0.037***	(0.012)	0.075	216
(9)	Total crimes	−0.306***	(0.038)	0.438	−27.456***	(4.333)	0.455	216
Judiciary data 1970–1995								
(10)	Thefts	−0.353***	(0.070)	0.222	−27.456***	(4.495)	0.105	450
(11)	Homicides	−0.324***	(0.087)	0.056	−0.041***	(0.014)	0.089	450
(12)	Robberies	−0.122	(0.083)	0.202	−0.404**	(0.173)	0.071	450
(13)	Frauds	−0.249**	(0.116)	0.193	−0.432*	(0.241)	0.134	450
(14)	Total crimes (judiciary)	−0.269***	(0.051)	0.165	−28.411***	(5.208)	0.099	450
(15)	Total crimes (police)	−0.195***	(0.031)	0.243	−17.077***	(2.989)	0.303	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 the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

we run our regressions by crime type. Unfortunately we do not have regional level data on changes in prison population by crime type, thus in the regressions by crime type the independent variable is still the total change in prison population.

To understand our results it is important to notice that even if a criminal is convicted for a nonpardonable crime, he/she might still have committed pardonable crimes as well. This is why Table 2 shows that during the last pardon even mafia members were released from prison. In addition, even if none of the criminals that committed nonpardonable crimes are released, pardons might still have an effect on nonpardonable crimes if criminals do not fully specialize in given crimes. Excluded crimes are thus not a perfect placebo test. Table 10 shows that between 1984 and 1995 the estimated elasticities on the types of crime that were explicitly excluded from pardons, like mafia murders, kidnappings, and sexual assaults, tend to be less precisely estimated and, with the exception of mafia murders, have estimated coefficients that are close to zero. In the judiciary data robberies, extortions and kidnappings are all under one category, and the estimated elasticity is also small, −12.2 percent, and not significantly different from zero.

Table 10 shows that there is considerable variability in the effects across crimes. This might in part be driven by the fact that all regressions use the same incarceration variables, introducing additional noise (see Spelman 2000). Despite these shortcomings and in the absence of better options we will be constrained to use crime-specific estimates for the cost-benefit analysis.

The elasticities on larcenies are not significantly different from zero but Italian victimization surveys show that only around half of those crimes are reported to the police (Muratore et al. 2004). Underreporting inflates the standard errors of the

estimated elasticities. Consistent with our hypothesis, motor vehicle thefts 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 21 percent. Row 14 and 15 of Table 10 show that using judicial crime data instead of police data strengthens the overall incapacitation effect (27 percent versus 20 percent). This result is likely due to (i) an increased precision in the measurement of crime given that "judges for the initial investigation" (giudice delle indagini preliminari) are supposed to dismiss all irrelevant cases before reporting a crime; and (ii) potentially longer criminal records of pardoned prisoners compared to first time offenders, which might increase the likelihood that formal charges will be filed.

Consistent with this, the elasticity for all thefts, which include larcenies and burglaries, is estimated to be 35.3 percent. Frauds have an estimated elasticity of 24.9 percent, and even the coefficient for homicides (murder and attempted murder) is significantly different from zero (32.4 percent). Also mafia homicides show a negative and significant effect. The estimated elasticity suggest that almost all released mafia mobsters re-offend, or lead others to offend. This is even more true if we consider that mafia criminals are less likely to be released.

Using judiciary data each released prisoner is estimated to lead to 28 crimes, though the grand majority of the effect is driven by simple thefts (27). Using police data, bank robberies, motor-vehicle (MV) thefts, burglaries, and drug-related crimes produces estimated elasticities that range between 15 and 48 percent.

In Section IIIA we mentioned that regions whose prisoners on average serve 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 11 we rerun the same regression as in Table 10 with the addition of demeaned average log sentence length by 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 at work 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 the trial.

IV. Policy Implications

The previous section has shown that the release of the marginal Italian prisoner increases 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 warrant such a release. It is important to note that even if the social benefits are larger than the social cost, there might still be alternative sanctions that dominate incarceration.

The Marginal Cost of Incarceration.—We estimate the cost of incarceration by regressing the total budgetary cost of the penitentiary administration (in 2004 euros)

Table 11—The Incapacitation Effect for Different Types of Crime for the Years 1970–1995
(*Controlling for selection*)

	Thefts (1)	Homicides (2)	Robberies (3)	Frauds (4)	All (5)
log change in prison population	−0.337***	−0.336***	−0.116	−0.252**	−0.275***
	(0.080)	(0.074)	(0.094)	(0.122)	(0.051)
log sentence	0.239	0.139	0.070	0.449	−0.086
	(0.189)	(0.470)	(0.139)	(0.450)	(0.105)
Interaction	0.014	0.092	−0.017	−0.046	−0.022
	(0.022)	(0.090)	(0.029)	(0.046)	(0.018)
R^2	450	440	438	438	438
Observations	0.219	0.067	0.205	0.185	0.175

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 the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

on prison population over the past 17 years, and 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).[37] 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,[38] 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 data sources and several assumptions. Table 12 reports the estimated elasticity (ϵ), the probability of reporting (p), the marginal effect of incarceration $\left(\beta = \frac{\epsilon}{p} \times \frac{\overline{crimes}}{\overline{prison - pop}}\right)$, the direct cost per crime (c), and the direct social cost ($s = \beta \times c$).[39]

We limit our baseline computations to direct costs, mainly direct property losses and quality of life losses, thus obtaining very conservative estimates. We are aware

[37] 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 per day) and 6.8 in Alabama (with an average cost of $22 per day, www.ojp.usdoj.gov).

[38] 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.

[39] 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.

Table 12—Social Benefit from Incarceration

	Total	Per 100,000 residents	Elasticity	Reporting probability	Marginal effect	Cost per crime	Social cost
Against the person	324,860	556					
Mafia-related murder	299	1	0.80	1.00	0.004	2,679,690	11,662
Nonmafia-related murder	1,249	2	0.32	1.00	0.007	2,679,690	19,713
Attempted murder	1,542	3	0.32	1.00	0.009	?	—
Assault	158,233	271	0.32	0.22	4.22	15,622	65,974
Sexual assault	4,571	8	0 ?	?	0 ?	?	—
Other (menacing, battery, pornography, etc.)	158,966	272	0 ?	?	0 ?	?	?
Against the family, the morale, the animals	18,180	31	0 ?		0 ?	?	—
Against property	2,174,810	3,720					
Motor vehicle theft (motorbikes)	80,494	138	0.21	0.95	0.33	2,156	715
Motor vehicle theft (cars)	182,470	312	0.21	0.87	0.82	7,145	5,864
Other thefts	1,252,117	2,142	0.35	0.54	14.77	326	4,816
Bank robbery	2,683	5	0.41	1.00	0.02	21,003	420
Other robberies	47,046	80	0.41	0.50	0.71	1,804	1,276
Extorsion	8,024	14	?	?	?	?	?
Kidnappings	196	0.34	0.00	?	—	?	—
Harm to things, animals, property, etc.	300,352	514	?	?	?	?	?
Fraud	301,428	516	0.25	1.00	1.36	9,953	13,582
Against the economy and the public trust	235,095	402					
Commercial fraud	8,583	15	0.25	1.00	0.04	9,953	387
Drug-related crimes	33,417	57	0.48	1.00	0.29	?	?
Other (forged currency, counterfeit)	193,095	330	0.25	?	?	?	?
Against the state and the public order	74,610	128	0 ?	?	0 ?	?	0 ?
Other crimes	294,917	504	0 ?	0.69	0 ?	?	0 ?
Total	2,968,594		0.32		22.59	5,766	124,409

Note: See Section IV for the list of sources and assumptions used.

that in so doing we are disregarding important costs related to criminal acts such as medical and mental health care, victim services, lost workdays or school days and indirect costs of victimization such as avoidance behavior, fear, or expenditures on moving, alarms, or guard dogs. Some of these costs are tangible (medical bills, expenditures, etc.) while others are clearly intangible and harder to measure. The few studies that analyse intangible costs consistently find that they likely outweigh tangible costs (Donohue 2009). For this reason we also try to assess the importance of such indirect and intangible costs in alternative computations that use (Donohue's 2009) high cost estimate.

The marginal effects of incarceration are based on the average crime rates in 2004, which is the last year for which published crime statistics are available. Notice that these social costs are based on the incapacitation effect only and might be larger or smaller depending on the additional deterrence effect from releasing inmates. Pardons, instead, would generate a deterrence that would lower crime and the related costs.

Most cost-per-crime estimates and the probabilities of reporting a crime come from ISTAT's 2002 victimization study (Muratore et al. 2004) and are in line with the low cost estimates shown in Donohue's (2009) review (the high cost estimates

are about five times higher). Italy's Value of Statistical Life (VSL), used to value a lost life due to intentional homicide, is comparable to estimates arising from studies done in the United States.[40] The social cost of frauds comes from a study by the Italian association of retailers (Confesercenti 2007).[41,42] For drug-related crimes there is no direct victim and so we assume there are no direct costs (Donohue 2009). For attempted murder and assaults, for which we estimate a positive elasticity, we use the conservative cost estimate of 15,000 euro used in Donohue (2009).[43]

When computing the total social cost in Table 12, elasticities marked with question marks are treated as zeros, a conservative approach.

Cost-Benefit Comparison.—Given our assumptions, we estimate a total social cost of crime of 124,409 euros, a value that is considerably higher than the marginal cost of incarceration.[44,45] We also estimate that the most socially costly crimes after a pardon are assaults (66,000 euros), nonmafia-related murders (19,700 euros) and frauds (13,500 euros). Our results suggest that the Italian prison population is below its optimal level. It also suggests that pardons are not selective enough since the cost associated with the ensuing increase in crimes far outweigh the cost of keeping those criminals in jail.

Discussion of Our Assumptions.—Given that incarceration is a complex phenomenon that involves a number of parties (offenders and their families, victims, potential victims, law enforcement, etc.) our cost-benefit analysis is necessarily based on a number of assumptions. We now highlight and discuss the assumptions that have led to our results and try to assess their relative weight in our conclusions.

Direct Costs and Indirect Costs.—As commented earlier indirect and intangible costs can be high. For this reason we compute our social costs also using Donohue's (2009) high cost estimates of crimes that include indirect and intangible costs and come up with an estimated total social cost of 655,000 euro, an order of magnitude higher than the cost of incarceration.[46]

[40] 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 (2004a, b) for an overview of recent estimates of the VSL.

[41] 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).

[42] We could not find enough information to estimate some elasticities and we marked them with a question mark in Table 12. We also assign a zero to elasticities of some crimes, based on institutional details of the pardons, and we mark them with a zero followed by a question mark in Table 12.

[43] We don't have a direct estimate for the elasticity of assaults and use the one that appears to be the closest in spirit, meaning the one on attempted murders and murders.

[44] 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, the social cost is still above the marginal cost of incarceration.

[45] 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 it in the cost-benefit analysis is likely to reduce the marginal cost of imprisonment, thereby making the case against pardons and amnesties even stronger.

[46] Notice that we are implicitly assuming a linear social function. If by contrast individuals are risk averse they 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.

Disutility of Incarceration.—Incarceration does not only bear monetary costs. Intangible or difficult to measure costs such as lost wages, and productivity of inmates, the value of the inmate's lost freedom, the psychological cost on the family of the incarcerated, potential postincarceration costs, like an increase in crime from prison-hardened criminals are often disregarded. Donohue (2009) proxies productivity losses using wage losses of the offenders in the order of $25,000 per year of incarceration, assuming they are male with high school diplomas but no college education, and assuming that 75 percent of offenders were employed before prison. Given higher unemployment rate and lower wages, productivity losses are likely to be even smaller for Italy and would hardly change our results. The value of the inmate's lost freedom is much more difficult to establish. Mastrobuoni (2011) uses the trade-off between the size of the haul and time spent inside a bank that robbers face when robbing a bank (more time brings more money but at a greater risk of being caught) to estimate such disutility (which includes lost productivity as well). He estimates a yearly average disutility of around 60,000 euros, quite a large sum that is due to the presence of some very high ability robbers (the median is closer to 30,000 euro). Even assuming that the average criminal is as able as bank robbers, adding the average disutility estimate to the cost of incarceration, keeps such costs below the social benefit of incarceration. In addition criminals might not take the psychological cost of their own family into account, but we have no estimate of such cost. Incarceration might also harden criminals, generating an additional cost from keeping them in prison. Chen and Shapiro (2007) find that inmates housed in high security cells are significantly more likely to recidivate although it is not clear whether such effects are due to prison experience or to peer effects and whether such effects are also operating at the intensive margin (an additional year in jail). It is possible that such effects might induce policymakers to shorten the optimal duration of incarceration or opt for alternative sanctions. Finally, Italian prisons in recent years have witnessed an intolerable level of overcrowding on the brink of violating human rights. Unfortunately we do not have data to measure how the disutility of criminals is affected by such harsh conditions of detention but we acknowledge the urgent need of an expansion in prison capacity.

Transfers from Victims to Criminals.—Cook (1983) argues for including the criminals' utility in calculations of the society's well-being, other authors argue against its inclusion (see Ludwig 2006; Cohen 2005; Trumbull 1990). For example, according to Cohen (2005) the value of stolen goods should be included in cost-benefit analysis since theft imposes private wealth reduction, and as such cannot be regarded as a simple transfer.

One way to decide how to solve this issue might be to follow what society values (see Trumbull 1990). While there is no direct evidence about such values despite intolerable levels of overcrowding, only 14 percent of Italians supported the 2006 pardon (EURISPES 2007),[47] indicating that only few take the inmates well-being into consideration.

[47] And more than 70 percent of them believed that the pardon had led to an increase in crime.

To understand how our policy implications depend on these transfers we can compute the marginal social cost excluding all crimes that involve a transfer of property (we disregard transfers of utility due to intangible factors). Such total social cost is 97,350 and still larger than the cost of incarceration.[48]

As a final note: (i) our results do not depend on the social cost of any particular crime, so that excluding any single crime from the analysis would still lead to preferring incarceration to early releases; (ii) the estimated social cost using variables in levels instead of logs would give similar results (see the last line of Table 12 and Table 10).

V. Conclusions

We use an atypical judicial policy—Italy's collective pardon—to isolate the causal effect of incapacitation on crime. First we exploit the structure of pardons to rule out the possibility that they might generate endogenous responses by criminals, an element to take into consideration when criminals are allowed to form expectations about sentence-reducing policies before they are enacted. Then we show with a simple model that, in principle, if pardons and amnesties are nationwide policies, the incapacitation effect can be identified separately from the deterrence effect. We next set up an empirical model that controls for a host of deterrence effects and has the potential to produce estimates of a pure incapacitation effect. Certainly our experiment might generate many forms of deterrence and controlling for them all can be a daunting task. Yet it has been shown that the structure of Italian pardons is likely to produce an increase in deterrence that we dub "postpardon deterrence." This implies that if the controls for deterrence that we use are insufficient we are still able to isolate a very informative lower bound to incapacitation. Consistent with the elasticities found in Levitt (1996), who uses the status of overcrowding litigation in US states as an instrument and estimates the sum of incapacitation and deterrence, our elasticities of pure incapacitation are indeed smaller.

In principle, collective pardons could represent a more cost-efficient screening device than individual parole boards. Pardon and amnesty laws do contain some screening provisions. For instance, habitual criminals have been typically excluded from pardons, and elderly prisoners, believed to have lower recidivism rates, sometimes have been granted larger sentence reductions. Despite this possibility, our cost-benefit analysis suggests that the social cost of releasing an extra inmate through a typical pardon is significantly larger than the cost of incarceration. This suggests that the Italian prison population is below its optimal level. Since pardons are shown to have a large direct effect on crime it suggests that they are not selective enough as the cost associated with the ensuing increase in crimes far outweighs the cost of keeping those criminals in jail.

[48] This difference in social costs is perfectly in line with Anderson (1999), who estimates that transfers account for roughly one-third of the overall costs of crime.

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