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Francesco Drago
Roberto Galbiati
Pietro Vertova

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**Francesco Drago**
*University of Naples Parthenope*
*and IZA*

**Roberto Galbiati**
*European University Institute*

**Pietro Vertova**
*University of Bergamo*
*and Bocconi University*

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IZA

P.O. Box 7240
53072 Bonn
Germany

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

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# ABSTRACT

# The Deterrent Effects of Prison: Evidence from a Natural Experiment*

In this paper we test for the theory of deterrence. We exploit the natural experiment provided by the Collective Clemency Bill passed by the Italian Parliament in July 2006. As a consequence of the provisions of the bill, expected punishment to former inmates recommitting a crime can be considered as good as randomly assigned. Based on a unique data set on post-release behaviour of former inmates, we find that an additional month in expected sentence reduces the propensity to recommit a crime by 1.24 percent: this corroborates the general deterrence hypothesis. However, this effect depends on the time previously served in prison: the behavioural response to an additional month of expected sentence decreases with the length of the prison spell. This second result can be hardly reconciled with the specific deterrence hypothesis according to which a stronger past experience of punishment should increase the sensitivity to future expected sanctions.

JEL Classification: K42

Keywords: crime, deterrence, natural experiment, recidivism

Corresponding author:

Francesco Drago
Departimento di Studi Economici
Università di Napoli, Parthenope
Via Medina 40
80133 Napoli
Italy
E-mail: fdrago@uniparthenope.it

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## 1. Introduction

Many factors may influence the decision to commit a crime. Among these factors, public law enforcement and sanctioning activity play a crucial role. A first task for social scientists interested in the empirical study of criminal punishment is to understand how potential criminals react to an increase in expected sanctions. The theory of deterrence suggests that a marginal increase in expected punishment *ceteris paribus* reduces the propensity to commit criminal acts (*general deterrence*) .[1]

Empirical assessment of the deterrence hypothesis presents a fundamental obstacle: it is very challenging to observe in reality an exogenous variation in expected punishment[2]. As a last resort to tackle this challenge, a natural setting where different expected sanctions are randomly assigned to individuals is required. In creating these conditions, the Collective Clemency Bill[3] passed by the Italian Parliament in July 2006 represents a suitable natural experiment to test for the theory of deterrence. This law, enacted to address the widespread situation of overcrowding in Italian prisons, provided for an immediate three-year reduction in detention for all inmates who had committed a crime before May 2nd 2006. Upon the approval of the bill, almost 22000 inmates were released from Italian prisons on August 1st 2006. The bill states that if a former inmate recommits a crime within five years following his release from prison, he will be required to serve the residual sentence suspended by the pardon (varying between 1 and 36 months) in addition to the sentence given for the new crime.[4] Therefore, former inmates face different expected sentences for any potential crime because of different residual sentences at the time of the release. Conditional on the sentence inmates were convicted to, the variation in the residual sentence among released inmates depends only on the date of entry into prison of inmates, which can be considered as good as random.

We were granted access to the Italian Department of Prison Administration (DAP) database records on the universe of individuals released as a result of the collective pardon law between August 1st 2006 and February 28th 2008. The dataset includes 25813 individuals, 85% of which is composed of individuals released on August 1st 2006. For each individual, in addition to a large set of variables at the individual level, these data provide information on whether or not the former inmate re-offended within the period between his release from prison and February 28th 2007.

[1] Becker (1968).
[2] Levitt and Miles (2004) have recently surveyed the relevant empirical literature and remarked on many of the critical issues hampering the identification of the effects of sanctions.
[3] See Law 241/2006, in the *Gazzetta Ufficiale* of the Italian Republic, July 31,2006.
[4] Consider an individual who, having a residual sentence of two years to serve on July 31, 2006, is released from prison as a consequence of the collective Clemency Bill. If he recommits a crime within the five years following July 31, 2006 her expected sanction is equal to the sanction for the new crime plus an additional sentence of two years of prison.

By using this data set, in this paper we test for *general deterrence*: we exploit the exogenous variation in residual sentence to identify the deterrent effect of an additional month of expected sentence on individual post-release criminal behaviour. The results corroborate the general deterrence hypothesis: a marginal increase in expected sanctions reduces the probability of recidivism of former inmates. We calculate that an additional month of expected sentence reduces the propensity to recommit a crime by 1.24 percent.

Our data set also allows us to investigate another crucial issue on deterrence. Indeed, it is of theoretical and policy relevance to understand how previous punishment affects the behavioural response to expected punishment. By exploiting exogenous variation in the levels of both time served and expected punishment for the same group of sentences, we can determine the effect of previous prison spells on the deterrent effect of expected sentences. Our results show that a longer period served in prison tends to decrease the deterrent effect of an additional month of expected sanction. This evidence is hard to reconcile with the specific deterrence hypothesis according to which a stronger experience of punishment should increase the sensitivity to future expected punishment.[5]

Our paper contributes to the literature by providing credible evidence about deterrence in a natural experiment setting where we can observe exogenous variation in expected sanctions. This allows us to solve some fundamental problems in identifying individuals' response to a variation in the severity of punishment. In the empirical literature, general deterrence is typically tested by analyzing how crime rates are affected by an increase in criminal sanctions. However, when we register a drop in crime rates following an increase in criminal sanctions, two competing explanations are at stake: discouragement of criminal behaviour is induced by the increase in its relative price (*the general deterrent effect*); the reduction in crime is mechanically due to the removal of criminals from the community (*the incapacitation effect*) (Levitt, 1996 and Kessler and Levitt, 1999). Moreover, the identification of the deterrent effect of an increase in expected sanctions is hampered by the fact that criminal sanctions may be endogenously determined. For example, state and local governments may respond to high crime rates by hardening criminal sanctions (Ehrlich, 1973; Levitt, 1998; Levitt, 2004). By exploiting the exogenous variation in expected sentence generated by the natural

[5] In this respect, our paper is related to the recent literature on the effects of prison treatment (Chen and Shapiro, 2006; Kuziemko, 2006, Pintoff, 2006, Kling, *forthcoming*). This literature examines whether the type and the conditions of punishment affect the post-release behaviour of inmates. Chen and Shapiro (2006) estimate the effect of prison conditions on recidivism rates by exploiting a discontinuity in the assignment of federal prisoners to security levels. Pintoff (2006) capitalizes on discontinuities in punishment that arise in Washington State's juvenile sentencing guidelines to identify the effect of incarceration on the post-release criminal behaviour of juveniles. Kling (*forthcoming*) uses a variety of research designs to estimate the effect of increases in incarceration length on the employment and earnings prospects of individuals, finding no significant effects. Kuziemko (2006) compares the parole system with a fixed-sentences regime by exploiting policy shocks and institutional features in Georgia. She provides evidences that the abolition of the parole system has increased both per-prisoner costs and recidivism.

experiment, we can identify the general deterrent effects of expected sanctions without any possible bias connected to the incapacitation effect and the endogenous response of policy makers. Finally, the paper contributes to the literature about the effects of past experienced punishment on criminal choices by showing that having experienced longer prison spells, instead of strengthening the behavioural response to the threat of sanctions, weakens the deterrent power of an increase in expected sentences.

The paper develops as follows. Section 2 presents the historical and political background of the Clemency Bill approved in Italy in July 2006 and describes the provisions of the bill in detail. Section 3 describes the empirical strategy. Section 4 reports the empirical results. Finally, in Section 5, we provide a more detailed interpretation of the results and we make some concluding remarks.

## 2. Collective Pardon in Italy: the Case for a Natural Experiment

### 2.1 Motivation for the Legislative Measure and Institutional Background

In order to identify the effects of an increase in expected sanctions on former inmates' propensity to engage into criminal activity, we exploit the natural experiment provided by the Collective Clemency Bill approved by the Italian Parliament on July 30th 2006. Hereafter we provide a summary description of the institutional background that led to the bill's approval and we briefly describe its provisions.

In recent years the Italian prison system has been characterized by harsh conditions of overcrowding. At the end of the 1990s, the total number of inmates was 55000 with a total number of available places of 42000; the average overcrowding index was 131 inmates to 100 places in prison.[6] This situation became clear to the eyes of public opinion in 2000, in particular after a campaign promoted by the Catholic Church which started with the visit of Pope John Paul II to Regina Coeli, one of the criminal residential facilities in Rome. In the following months there was a huge debate in the media and several deputies in the *Camera dei Deputati* (one of the two Chambers of the Italian Parliament) tabled a bill proposing an amnesty[7] and a collective pardon.[8] The public debate did not conclude with the bill being passed, but the harsh situation in the prison system remained under the media spotlight. The political debate gained new strength after the official visit of Pope John Paul II to the Italian

[6] See: Italian Department of Prison Administration, Statistics Office: http://www.giustizia.it/statistiche/statistiche_dap/organigramma_01.htm.

[7] The Italian juridical system makes a distinction between amnesty and collective pardon. An amnesty extinguishes both the criminal record and the sanction. The collective pardon shortens or eliminates sanctions but does not extinguish an individual criminal record.

[8] http://www.camera.it/_dati/leg13/lavori/stampati/sk7500/articola/7086.htm.

Parliament.[9] In his official speech he put great emphasis on the situation of inmates in the Italian prison system and suggested the opportunity of an amnesty. Despite this widespread attention, the Italian Parliament passed the collective pardon bill only four years later on July 30th 2006. The reasons for this delay find their roots in the exceptionality of such a legislative measure. According to the Italian Constitution,[10] any law providing for the implementation of an amnesty or a collective pardon must be approved by both the Chambers of the Parliament with a majority of two thirds of the votes, regarding each article of the law. These conditions are the same as those for the approval of a constitutional reform (art.138). In the following paragraph we describe the provisions of the collective pardon bill in greater detail.

### 2.2 Law 241/06, Collective Clemency Bill

The bill provides for a reduction in the length of detention for those people who committed a crime before May 2nd 2006. This backdating of the collective pardon, which was announced immediately when Parliament began to debate the bill, rules out any possible effect of the collective pardon on crime rates during the months before the approval of the measure. The legislative measure reduces prison sentences by three years for a large number of inmates, but does not extinguish the offence. As a consequence, on August 1st 2006 all those with a residual prison sentence of less than three years were immediately released from residential facilities. Some types of crimes are excluded from the collective pardon, in particular those related to mafia, terrorism, armed gangs, massacres, devastation and sacking, usury, felony sex crimes (in particular against juveniles), kidnapping, and the exploitation of prostitution.

The provisions of the bill concerning the reduction of incarceration length implies that every inmate convicted of a crime (other than those listed above) committed before May 2nd 2006 is eligible for immediate release from prison as soon as his residual sentence becomes less than three years. Notice that the effects of the collective pardon will persist for many years. For example, of inmates who had committed a crime before May 2nd 2006, those who had three years (or less) of detention in prison to serve were immediately released on August 1st 2006; those who had three years and 1 month to serve were released on September 1st 2006; those who had to serve exactly twenty years of further detention will be released on August 1st 2023 instead of August 1st 2026.

As far as our research question is concerned, the crucial consequence of the bill is a randomization in expected sentences for future crimes. The bill provides that all those re-committing a crime within the

[9] This official visit of the Pope to the Parliament gained a big media attention. This was the first visit of a Pope to the Parliament in the history of Italian Republic.
[10] Costituzione della Repubblica Italiana, Section II, Art.79.

five years following July, 31st 2006 and receiving for such a criminal act a sentence greater than two years lose the benefit of the clemency. This means that within the five years following their release from prison resulting from the collective pardon, former inmates face an additional expected sanction equal to the residual sentence pardoned pursuant to the approval of the bill. Under the assumption that, conditional on the original sentence, residual pardoned sentences are orthogonal with respect to unobservables, it is clear that this provision of the bill implies exogenous variation in expected sentences that constitutes a unique opportunity to test for the deterrent effect of sanctions. Take for instance two criminals convicted with the same sentence and having a residual sentence of less than three years on 1st August 2006. They are both released from prison on August 1st 2006. Suppose that the first individual entered prison one year before the other: he has a pardoned residual sentence of one year, while the second has a pardoned residual sentence of two years. In the following five years, for any possible kind of crime, they face a difference in expected sentence of one year. For a robbery with a maximum official expected sentence of ten years the first individual expects a sentence of eleven years (ten years for the robbery plus one year residual sentence pardoned by the Collective Clemency Bill), while the second expects a sentence of twelve years (ten years plus two years of residual sentence).

As a consequence of the collective pardon, almost 37% of the inmates in Italian prisons were released in the first 2 months: from 60710 individuals on July 31st 2006, the total prison population dropped to 38847 on August 1st 2006 (see figure 1). Notice that the number of beneficiaries of the collective pardon is decreasing over time. Indeed, on June 30th, 19.2% of inmates with a definitive sentence had to serve from three to six years in prison, 7,6% from 6 to ten years, 6% from 10 to 20, 4,5% more than twenty years or life imprisonment. Most of the released individuals are prisoners with a final sentence. Among the prisoners who had been released because of the collective pardon by October 25th 2006, 17298 individuals had exclusively final sentences; 4343 individuals had final sentences for some crimes but were waiting for final sentences for other accusations; 2615 individuals had no final sentence (2615 appellants and 416 individuals waiting for a first verdict) but they were accused of crimes whose expected maximum punishment was shorter than three years.

*Figure 1 about here*

*Figure 2 about here*

## 3. Empirical Analysis

### 3.1 Data

The primary source of data for this study is an internal database that the Italian Department of Prison Administration (DAP) maintains on offenders under its care. We were granted access to the DAP database records on the universe of individuals released pursuant to the collective pardon law between August 1st 2006 and February 28th 2007 (25813 individuals). The 81 percent of these individuals was released on August 1st 2006. For each former inmate the data provide information on whether or not he has recidivated within the period between release from prison and February 28th 2007. This means that for 85% of individuals the data report if there has been recidivism in the first 7 months after release from prison. Moreover, the data set contains information concerning a large set of variables at the individual and facility level. For each individual, information is reported on: the facility where the sentence was served, the official length of the sentence, the actual time served in the facility, the kind of crime committed (i.e. the last crime committed in the individual's criminal history), age, level of education, marital status, nationality, province of residence, employment status before being sentenced to prison, and whether the individual had a final sentence or was waiting for the first verdict or for the results of an appeal at the date of release. As data on subsequent convictions are not available, we use a subsequent criminal charge and imprisonment as the measure for recidivism.

Our analysis is restricted to people serving the sentence in prison, i.e. we exclude from the analysis individuals convicted to serve a sentence in a judiciary mental hospital (98 individuals). To avoid confounding elements related to sex, we concentrate our analysis on males (we do not consider data about 1274 female). Moreover, we exclude from the sample any individual with residual sentences greater than 36 months. This is the case of individuals cumulating different charges with a sentence for at least one, but awaiting verdicts on others. Finally we do not consider individuals for whom sentence data are missing. The final sample we use in the empirical investigation is made up of 20452 individual-level observations. Table I reports descriptive statistics on the individual-level data both for the all sample of released individuals and the sub-sample of those who were rearrested in the period of observation. Those recidivating constitute the 12% of the final sample. As an effect of the collective clemency we observe that on average inmates benefit from a reduction of 14.98 months (13.31 for those rearrested) of time actually spent in jail with respect to the official sentence with a standard deviation of 10.46 (9.88 for those rearrested). This implies that the average adjunctive sanction for those recommitting a criminal act after the release pursuant to the collective pardon is 14.98 months, varying between 1 and 36 months. Appendix 1 provides a description of the crimes included in the different categories. Some crime categories (e.g. mafia, terrorism and felony sex crimes) are missing from our sample as they were excluded from the collective pardon. The majority of people in the

sample were convicted for having committed crimes against property or offences related to the drug law. The average age of former inmates in our sample is nearly 37; 35% of them were in stable employment before being sentenced to prison and 47% were unemployed.

*Table* I *about here*

### 3.2. The Basic Regression Model

The focus of the empirical analysis is on inmates' post-release behaviour (being re-arrested or not). The subjects of our sample satisfied the eligibility condition stated by the Collective Clemency Bill passed by the Italian Parliament in 2006, i.e. having a residual prison sentence to serve of less than 36 months. As explained above, each inmate faces an expected sentence for any type of crime which is enhanced by the residual sentence at the date of release. Hence, we use variation in the residual sentence to study the deterrent effect of an additional month of expected sentence for any type of crime. Denote with $y_i$ the post-release outcome of an individual *i*, with *sentence* the initial sentence and with *sentres* the residual sentence. The post-release outcome we observe is whether or not former inmates recidivate ($y_i$ takes value 1 if the individual was rearrested in the period under consideration and 0 otherwise). The basic regression model is:

$$y_i = \alpha + \beta_0 sentence_i + \beta_1 sentres_i + \varepsilon_i . \quad (1)$$

The collective pardon provides us with a natural experiment because, for each sentence, variation in *sentres* depends only on the date of entry in prison, which is as good as random. The identifying assumption needed to obtain a consistent estimate of the coefficient of interest $\beta_1$ is that, conditional on sentence length, variation in the residual sentence is not systematically correlated to variation in unobservables that affect the post-released outcome, i.e. $\mathrm{cov}(sentres_i, \varepsilon_i \mid sentence) = 0$. This assumption can also be stated by saying that the determinants that led individuals with a sentence equal to T months to enter the prison in any month in the interval [T-36, T] before August 2006 are orthogonal to the probability of recidivism. We corroborate indirectly the hypothesis of randomness of residual sentence by regressing, for each sentence group, residual sentence separately on each observable characteristic. The results of these regressions are presented and discussed in Appendix 2. Given that not all criminal acts result in arrests, as we will discuss in the next section, an estimated

$\beta_1 < 0$ should be interpreted as a lower bound of the deterrent effect of expected sentences on criminal activity.

To check the robustness of our results, we also include in the basic regression model a set of individual characteristics such as age, type of crime committed before release, nationality, education, marital status, employment dummy, and juridical status. In addition, we condition on prison and police jurisdiction fixed effects. All these are potential factors relevant to explain recidivism. While the results are consistent with the hypothesis that *sentres* is a variable randomly distributed also within prisons and police jurisdictions (see the coefficients on *sentres* in Table II), this might not be the case for other individual variables (e.g., there might be prisons with the most dangerous individuals having low levels of education). Hence, we present the main results by including prison and police jurisdiction fixed effects in the specifications.

### 3.3. Distinguishing between General Deterrence and Time Served

By construction, the residual sentence is equal to the sentence minus the number of months served in prison, denoted as *timeserved*. Given this identity, the inclusion of all three variables (*sentres*, *timeserved* and *sentence*) in (1) would generate perfect collinearity. Omitting *timeserved* from (1) allows us to identify the effect of *sentres* without any strong restrictions on the parameters of the model. Once we condition on sentence, the randomness of *sentres* implies that *timeserved* is random too. In other words, omitting *timeserved* does not induce any bias in the coefficient on the other variables.

However, this strategy is not without cost. The problem here is that the interpretation of the effect of *sentres* as a behavioural response to an additional month in expected sentence is confounded by the effect of the time served in prison. Given the identity *sentres* = *sentence* - *timeserved*, for each sentence, an additional month of *sentres* also means a month less served in prison, so that we cannot exclude *a priori* that the coefficient on *sentres* is the composite effect of the incentive to recommit a crime (the deterrent effect) and the possible effect of an additional month spent in prison.

In order to provide evidence that the deterrent effect of *sentres* is not confounded by the direct effect of the time served in the prison, we adopt two strategies. First, we exploit another provision of the Collective Clemency Bill. The bill explicitly states that the rule according to which inmates re-entering prison also have to serve the residual sentence, does not hold for inmates convicted with a new sentence of two years or less. Given the evidence that individuals tend to recidivate in crime categories where they already have some experience (Bayer et al., 2007), inmates with an original sentence shorter than or equal to 24 months are less likely to commit a crime associated with a

sentence longer than two years. Finding a significant effect of *sentres* for individuals with original sentences of less than 24 months would cast doubts on the interpretation that for all other individuals the effect of *sentres* is an effect due to deterrence. For the sub-sample with an original sentence shorter than 24 months, *sentres* should not reflect deterrence. In fact, we find that for this sub-sample *sentres* has no effect; this reassures us that the effect of *sentres* is due to the pure deterrent effect (results in the next sub-sections). In the second strategy we try to estimate model (1) by controlling simultaneously for *sentence*, *sentres* and *timeserved*. We circumvent this problem of the perfect collinearity of the three variables by estimating the following regression model:

$$y_i = \alpha + \beta_0 sentencefixeffects_i + \beta_1 sentres_i + \beta_2 timeserved_i + \varepsilon_i, \quad (2)$$

where *sentencefixeffects* is a set of dummy equal to one for each sentence group multiple of twelve. This approach does not entirely solve the problem of identification of the three effects separately but it provides evidence on how *timeserved* affects recidivism. For a given *sentres*, we can obtain an additional month in time served by increasing the individual sentence. Given that in (2) we control for sentences grouped in multiple of twelve, to interpret the estimated $\beta_2$ as the effect of an additional month of time served on recidivism without the confounding element of an additional month in sentence, it is sufficient to assume that any potential effect of sentence on recidivism is monotonic in the entire interval of sentences. Again, finding a significant impact of *timeserved* from model (2) would cast doubts on the interpretation of the effect of *sentres* as a pure deterrent effect. However, from this analysis we find that the impact of *sentres* is very similar to that found from regression model (1) and the effect of *timeserved* is very small and statistically not significantly different from zero (results in the next sub-sections).

## 4. Results

### 4.1. Baseline Regressions

Before presenting results from baseline regression, we offer some graphical evidence on how the residual sentence affects recidivism. In Figure 3 we report the recidivism rate for each sentence for former inmates with residual sentences both above and below the median (we report only sentence groups between 25 and 44 months, which is the range of sentences to which most individuals are convicted). As it is clear from Figure 3, the recidivism rate of individuals with residual sentences above the median is systematically lower for each sentence (except for two sentences). The difference

in the recidivism rates among individuals with residual sentence above and below the median varies between about 14 and 1 percentage points (excluding the two sentences for which the difference is negative). Given that the additional expected sentence, captured by the residual sentence, is a variable that is not likely to be correlated to observables and unobservables, Figure 3 shows preliminary, suggestive, evidence that, on average, former inmates respond to higher expected sentences by reducing their criminal activity.

*Figure 3 about here*

We now present the results from regression model (1). The dependent variable is equal to 1 if the inmate has recommitted a crime by February 28, 2007 and 0 otherwise. Table II reports the results. Standard errors are adjusted for clustering at the prison level (province of residence level) to allow for any arbitrary autocorrelation of the errors in each prison (in each police jurisdiction). In the first column of Table II we report the coefficient on *sentres* from a logit regression that includes only *sentence* and *sentres*. The coefficient on the variable of interest is negative (-.01588) with a z-robust statistic equal to -5.89. For each sentence a higher *sentres*, i.e. a higher expected sentence, decreases the probability of recidivism. In the second column we present the results from a logit regression that also includes individual controls as explanatory variables (age, education, marital status, employment status, juridical status, and a dummy for geographical origin are included as covariates). The coefficient on the residual sentence is now equal to -.01494 with a z-robust statistic equal to -5.31. The fact that from column 1 to column 2 the coefficient on the residual sentence does not change significantly is not surprising given that the collective pardon induces a variation in *sentres* that is essentially uncorrelated to unobservables and observables. In the next column of Table II we also include the type of crime for which the inmates were arrested when they entered the prison before release. In column 4 we report the coefficient from a conditional logit model, grouped by prison, to take account of differences across prison that drive criminal behavior after release. In the last column, we present the results from a conditional logit model grouped by province of residence to take account of differences across police jursdictions that influence the probability to be re-arrested. Unsurprisingly, the coeffcient on *sentres* from the last two columns is essentially identical to the one reported in columns 1-3.

*Table III about here*

The average effect of an additional month of residual sentence on the probability of recommitting a crime after release is negative and robust across several specifications. Given that the number of individuals that commit a crime act but are not arrested is likely to be much higher than the number of individuals that are arrested but have not committed a crime act, the estimated impact of *sentres* should be interpreted as a lower bound of the deterrent effect of expected sanctions.[11]

In Table A2 in the Appendix we report the results of the coefficients on individual variables. As expected, age, being married and employment status have a negative impact on recidivism. The sign of the coefficient on education goes in the same direction, though it is not significant at conventional levels.

### 4.2. Deterrence and the Direct Effect of Time Served

An additional month of residual sentence not only means an additional month of expected sentence but also a month less served in prison. In order to draw a robust interpretation of the empirical findings, it is important to provide evidence of how the time served in prison directly affects recidivism. In particular, to assess the validity of the interpretation that the impact of *sentres* reflects a deterrence effect not confounded by the impact of time served, we adopt the two strategies illustrated in sub-section 3.3.

We first exploit the provision of the Collective Clemency Bill which implies that for inmates recommitting a crime associated with sentences of less than two years[12] the deterrent effect of the residual sentence should not exist. Inmates with original sentences below 24 months are likely to be subject to the risk of recommitting similar crimes to those associated with their original sentences. For these individuals, the effect of *sentres* provides information on whether there are reasons to believe that the time served directly affects recidivism, and, as a consequence, whether the amount of time served is a confounding element in our analysis. The number of individuals with sentences less than 24 months is very large, i.e. 7312, and the variation in the residual sentence is between 1 and 24 months. From the first three columns of Table III we notice that the coefficient on *sentres* for the sub-sample with sentences in the interval 1-24 is very small and statistically not different from zero. These findings suggest that the effect of *sentres* reflects a pure deterrent effect.

[11] We do not have official data on the fraction of arrested individuals that are declared innocents. Note that in Italy the incarceration before conviction is an extreme measure that generally occurs when there is the evident risk of re-iteration of crime and the possibility of counterfeiting of evidence. To give an idea of how many crime acts do not result in arrests in Italy, in 2006 for the 78 percent of crimes acts reported, police did not find the offender (*Relazione sull'attività giudiziaria nell'anno 2006*, p. 22; Data from the Italian Supreme Court.).

[12] It is important to note that the Italian sentencing system is quite rigid and minimum compulsory sentences for each crime are fixed by law in the Penal Code.

*Table III about here*

To provide additional evidence on this point, we estimate the regression model (2). Results are reported in column 4, 5 and 6 of Table III. The coefficient on *timeserved* is very small and always insignificant across different specifications. Unlike *timeserved*, the effect of *sentres* is negative and precisely estimated. Note that for a given *sentres*, we can obtain an additional month of time served by increasing the individual sentence. Moreover, in estimating the regression model (2) we condition on *sentencefixeffects*, that is a set of dummy equal to one for each group of sentence multiple of twelve (the groups are defined on the sentence interval 1-12, 13-24, 25-36, 37-48, 49-60, 61-72, 73-369). The coefficient on *timeserved* would pick up the effect of sentence within each group of sentences if the potential effect of sentence on recidivism were the same within each group. In column 5 of Table III we report the results from the regression model (2) by redefining the sentences intervals: 0-18, 18-30, 31-42 and so on**.** The results are essentially unchanged. In addition, as it is reported in the last column of Table III, when allowing for an interaction between *sentencefixedeffects* and type of crime, the results are very similar to those found above.[13] On the whole, from this analysis we conclude that it is appropriate to interpret the coefficient on *timeserved* as the effect of an additional month spent in prison on recidivism. To the extent that time served does not seem to have a direct effect on the probability of re-offending, the effect of *sentres* on recidivism found in the previous analysis can be attributed to deterrence.

*Table IV about here*

**4.3. How Large is the Deterrent Effect?**

To shed light on the magnitude of our estimated effects, we present the results from the logit regression of how an increase of an additional month in the residual sentence (i.e. a month's increase in the additional sentence for a given type of crime) decreases the probability of recommitting a crime evaluated at the mean of the explanatory variables. Taking the specification of column 3 in Table II, this result shows that an additional month in *sentres* leads to a reduction in the probability of re-offending equal to 0.0014, evaluated at the mean level of the explanatory variables. Given that for this

[13] Within each group of sentences, types of crime are correlated to individual sentence. By allowing interaction between *sentencefixedeffects* and type of crime, we try to account for part of the hypothetical effect of sentence - *within* each group of sentences - that is correlated to *timserved*.

population the fraction of those who recommit a crime is 0.1177, this effect is large. This basic result indicates that by increasing the expected sentence by 1 month, the probability of recommitting a crime should decrease to about 0.1163, with a drop of 1.24 percent.

It is worth comparing these results with the previous findings in the literature. Kessler and Levitt (1999) is one of the few studies that estimate a deterrent effect of sentence enhancements, making a clean distinction between deterrence and incapacitation effects. In their study a change of law leads to sentence enhancement targeting the most frequent and dangerous criminals. They exploit the fact that in the short run there should not be an incapacitation effect after the law changes, and find that in California some crime rates fall by 4 percent after sentence enhancement, which, for example, increased the sentence for any "serious" felony offender by five years. Our paper suggests a larger deterrent effect. However, one should be cautious in comparing the two magnitudes. Our sample is composed of former inmates only and concerns all types of crime in Italy; we estimate the effect of deterrence on recidivism while Kessler and Levitt (1999) focus on crime rates. At most, such a comparison could suggest that the concerns both of Kessler and Levitt and others (e.g. Owens, 2006) that the deterrent effect could have been overestimated, do not find empirical support in our paper.

### 4.4. Differential Effects and Specific Deterrence

We now investigate whether the deterrent effect varies for different levels of sentences longer than 24 months. For example, is the effect of one additional month of expected sentence the same for two individuals who have sentences equal to 36 months and 70 months respectively? A natural approach to address this question is to estimate the basic regression model by allowing the effect of *sentres* to be different across different sub-samples defined by different sentences intervals. In Table IV we present the results of this analysis. The sub-samples are defined for the following sentence intervals: 25-48 months, 49-72 months, and 73-369 months (we do not present here the results for the interval 1-24, already shown in Table III), with 7891, 3074 and 2175 inmates for each group respectively. For the sake of the reliability of the results, it is worth noting that in the last sub-sample the number of inmates with very long sentences is very small, e.g. only about 5 percent of the last sub-sample have sentences longer than 200 months.[14] Moreover, the average age in the three groups is 36, 36 and 40 respectively.

The results in the Panel A of Table IV show that for the first two sub-samples the effect of the residual sentence is higher in absolute value than that found above for the entire sample (in the same specification adopted in Table II the coefficient was -.01532). In addition, the coefficient is essentially

[14] The results remain unchanged if we exclude from the last sub-sample individuals with very high sentences (e.g. higher than 200 months).

the same both in the first and in the second group. From Table IV, the results indicate that deterrence is not effective for sentences higher than 72 months.[15]

It is important to analyze why we obtain different results for different sentence groups. Can these differences be explained only by an unobserved heterogeneity between individuals in the first two sub-samples (sentences between 24 and 48 months and 48-72 months) and in the last sub-sample (with sentences longer than 72 months)? Individuals in the sentence interval 48-72 months may be intrinsically different from those in the last sentence interval and this could be the reason why they have different behavioural responses to expected sentences. For example, unlike those convicted with shorter sentences, those with long sentences could suffer from serious problems of self-control and be unable to respond as they would wish to the incentive provided by the residual sentence. Also, it is possible that individuals with long sentences are likely to commit crimes similar to their previous ones. As they would be given long sentences and stay in prison for most of their lives, their behavioural response to deterrence could be zero. While all these are plausible explanations, they might not be the only ones at work.

The groups defined by different sentence intervals differ in the average time served in prison. In particular, former inmates with longer sentences have on average longer spells in prison. Hence, the time served might play a role in determining the difference in behavioural response to residual sentences between former inmates with long and short sentences. The natural experiment provided by the collective pardon allows a careful investigation of this hypothesis. In particular, we can exploit the fact that for the same group of sentences we observe different levels of *timeserved* and *sentres*. For sentences higher than 24 months, we estimate regression model (2), which also includes an interaction term between *timeserved* and *sentres.* We report these results in the panel B of Table IV. We obtain a positive and statistically significant coefficient on the interaction term.[16] Moreover, by allowing interaction between *sentres* and *timeserved*, the intercept on *sentres* becomes more negative (see Table III, columns 2 and 3). In other words, variation in time served contributes to explain the differences in behavioural response to residual sentence between inmates with shorter and longer sentences. These results show that *sentres* (i.e. an increase in the expected sanction for a certain kind of crime) has a strong negative impact for a relatively low number of months served and decreases as *timeserved* increases. This is a robust finding even after including a wide set of interactions in the regressions between *sentres* and individual characteristics and types of crime (results not reported).

[15] By splitting the sample according to the group of sentences, we observe that the coefficient on *sentres* is very small and statistically not different from zero for former inmates with sentences longer than 72 months.

[16] We also estimated a linear probability model. The interaction term is positive and statistically significant at 5 per cent level across the different specifications. In addition, the interaction term is positive and precisely estimated when we use the formula given in Norton et al. (2004) for interaction terms in non-linear models.

## 5. Robustness and Alternative Explanations

### 5. 1. Is post-release behaviour after 7 months too short a time period?

A first concern is that we observe individuals only until February 28th 2007, 7 months after release. At this date the recidivism rate was 0.1172. We expect that in the future more inmates released under the collective pardon will re-enter prison. One issue is whether our results regarding deterrence would remain unchanged if we could observe inmates over the next few years. The main alternative explanation for our results, suggesting that our findings might change goes as follows. Suppose that all former inmates pursue some kind of criminal activity. However, those with longer residual sentences are less likely to be caught by the police because they served less time in prison and they did not forget how to avoid an arrest or a conviction. If this explanation is valid, over one or two years we would observe virtually no difference in recidivism rates between individuals released with long and short residual sentences (e.g., sooner or later the police arrest all former inmates pursuing criminal acts). The limitation of this explanation is that it implies a direct effect of time served on our dependent variable. But, as we report in sub-section 4.3, we do not find evidence for a direct effect of time served in prison on recidivism. Hence, we view this alternative explanation as not completely satisfactory.

### 5. 2. Police behaviour in targeting criminals

Another potential issue concerns how the police target released potential criminals. Suppose that the police believe that individuals with higher residual sentences are less likely to commit a crime. If in addition the police have an incentive to increase the number of arrests, they could direct their efforts towards individuals with lower residual sentences. In this case we would overestimate the deterrent effect of expected sanctions. While this is a plausible story, it is difficult to believe that the main result - that individuals have a lower propensity to commit a crime if they expect higher sentences - would be invalidated if it were true. First, the explanation requires that the police know the residual sentences of former inmates and discriminate between potential criminals based on this individual variable. Second, even if one is willing to assume that the police are well informed about former inmates and do discriminate, there is a general equilibrium effect induced by this potential police behaviour that should compensate for the difference in the probability of being arrested for individuals with long and short residual sentences. Indeed, those former inmates with long residual sentences would increase their criminal activity in response to a lower relative cost of crime (because, if the police target individuals with short residual sentences, those with long residual sentences face a lower probability of being arrested). Note that this effect goes in the opposite direction of what we find in the data.

### 5.3. Is being re-arrested a good measure to test for deterrence?

Another concern is whether our measure of criminal activity, being re-arrested or not, is a good measure for assessing the existence of deterrence. While it is very likely that a former inmate who is rearrested has committed a criminal act, we cannot be sure that all the others not arrested are not pursuing criminal activity. It is important to emphasize that this might be a critical issue for the interpretation of the results only if there are reasons to believe that, *conditional on pursuing criminal activity*, the probability of being arrested decreases with the residual sentence. In the two previous explanations we discussed the hypothesis of a lower probability of being arrested for individuals with long residual sentences as a consequence of costless individual behaviour. However, there is another potential channel through which individuals with high residual sentences might have lower probabilities of being arrested. In particular, suppose that an individual pursuing some criminal activity can decrease the probability of being arrested by investing time or resources. In this case, those facing higher expected sentences (i.e. higher costs from being caught by the police) would have more incentives to invest in decreasing the probability of being arrested (e.g., they could exercise much more precaution in committing criminal acts). Could our results be explained by the fact that actual criminals with higher residual sentences are less likely to be rearrested because they invest more in avoiding being caught by the police? We maintain that this might not be the case. Our exercise is an evaluation of the response of criminals to a marginal increase in the cost of committing an unlawful act. The results can be interpreted as the response to such a variation in expected sanctions by the "marginal criminal", i.e. one who would commit a crime in the absence of an exogenous increase in the expected cost of the criminal act. For such a criminal, an increase in the cost of committing a crime makes him no more indifferent between engaging into an unlawful activity or not. It follows that an increase in the expected sanction should not lead him to invest resources in decreasing the probability of being arrested (at the margin he should not find it convenient to pay even a small price).

## 6. Concluding Remarks

In this paper we contribute to the empirical literature on the study of criminal punishment by providing credible evidence about the deterrent effects of an increase in expected sentences. Our identification strategy exploits the natural experiment provided by the collective Clemency Bill passed by the Italian Parliament in July 2006. The institutional features of the bill imply, for the sample of individuals released upon the approval of the bill, an exogenous variation in expected sentence for any kind of crime recommitted after release. This experimental setting allows us to overcome issues hampering the identification of the deterrent effects of increasing expected sanctions. In particular, our design allows

us to identify the deterrent effect of an increase in sentences separately from its incapacitation effect and from the possible endogenous reactions of policy makers to crime.

Our findings provide credible evidence that a one month increase in expected punishment lowers the probability of committing a crime. This corroborates the theory of general deterrence. Moreover, we find that general deterrence works in different ways across groups of individuals with different prison time. Our findings suggest that specific deterrence does not hold: a longer period served in prison tends to weaken and not strengthen the deterrent effect of an additional month of expected sanction. In other words, more severe punishment does not imply a stronger sensitivity to the threat of future punishment but rather the opposite relationship. This suggests that the experience of punishment tends to neutralize the behavioural response to general deterrence. Further analysis is necessary in order to better interpret this result from a theoretical point of view.

Finally, the paper relates to recent studies that offer evidence that prospective young criminals might not be deterred because of myopia, or general problems in perceiving sanctions (Lee and McCray, 2005 and Jolls et al. 1998). In this respect, the evidence provided is mixed. On the one hand, the finding that former inmates with long sentences (perhaps the most dangerous) do not respond to the incentives provided by the enhancement of expected sanctions is consistent with the findings of Lee and McCray (2005), who focus on serious crimes which involve a victim such as murder, robbery and assault. However, the credible finding of a large deterrent effect for all other former inmates (convicted with shorter sentences) suggests that the results of this literature might not be extendable to the generality of criminals.

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**Appendix 1**- Description of the Categories of Crime

*Drug offences*: In this category are included all the violations of the law on the use and selling of drugs (Decree of the President of the Republic October 9th 1990- 309 and subsequent modifications and amendments).

*Crimes against property*: In this category are included: theft, larceny, robbery, bag-snatching and in general all the offences regulated by Book II Section XIII of the Italian Penal Code.

*Crimes against public administrations*: In this category are included crimes against the public interest and administration regulated by Book II Section II of the Italian Penal Code.

*Crimes against public safety*: In this category are included all crimes related to possible danger to the safety of people, things, public utilities, buildings. All the crimes under this category are included in Book II Section VI of the Italian Penal Code.

*Violation of gun law*: In this category are included all the violations of the law on using and carrying guns and other arms (Law 110/75 and subsequent modifications and amendments).

*Immigration law*: In this category are included all the violations of the law on the regulation of immigrations and the juridical status of foreign citizens (Legislative July 25th 1998 – 286 and following amendments and modifications).

*Various crimes against persons*: In this category are included: assault, homicide and in general all the offences regulated by Book II Section XII of the Italian Penal Code.

*Corruption and crimes against justice administrations*: In this category are included crimes against the correct functioning of the justice administration and police and in general all crimes regulated by Book II Section III of the Italian Penal Code.

**Appendix 2-** Testing for the Random Assignment of Residual Sentence

In order to provide evidence for our identifying assumption, we present the results of the tests for random assignment of residual sentence for given sentence. We split our sample in groups of released prisoners characterized by the same sentence. Then, for each group, we regress residual sentence separately on each observable variable at the individual level (employment conditions, age, grade of judgment, marital status, education, nationality).

Table A1 reports the results of these regressions for the interval of sentences (25-44) months, i.e. the interval of sentences around the median value of sentence (34). Most coefficients are not significant, suggesting that for a given sentence the date of entry to prison is essentially random. It is important to note that some coefficients are significant; in particular, for some sentences, age and nationality significantly predict the exact date of entry in prison. This is not surprising as we expect that, for particular reasons such as the arrest of criminal gangs or mass entries of immigrants, the sub-group of inmates obtaining a certain sentence at a specific time may be characterized by statistically significant differences in terms of some observables (in particular nationality and age[17]) with respect to sub-groups of inmates obtaining the same sentence at a previous or subsequent time. However, this does not threaten the reliability of our identification strategy. What is important for our identifying assumption to be valid is that there is not any systematic lag in the moment of entry to prison induced by individual or geographical characteristics. Results in Table A1 show that there is no such systematic pattern, i.e. residual sentence is not systematically predicted by observables. While this cannot be a direct proof of the fact that residual sentence is not systematically predicted by some unobservable, we can be confident that it is a plausibly solid indirect test.

[17] Notice that nationality and age are strongly correlated since immigrants are on average younger than Italians.

Figure 1. Prisoners in Italy

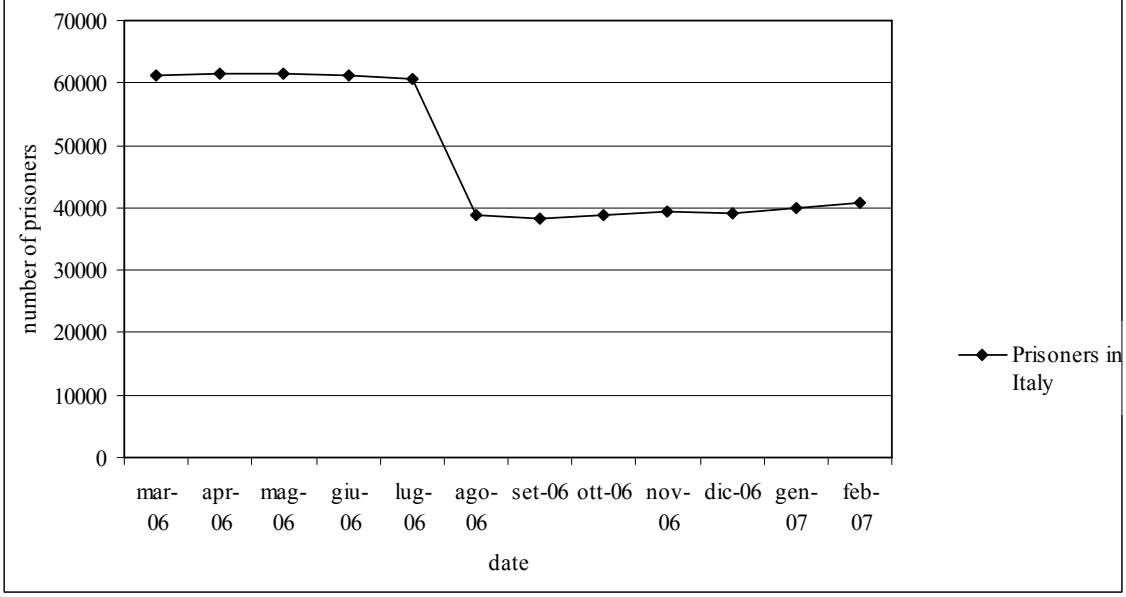


Figure 2. Released prisoners

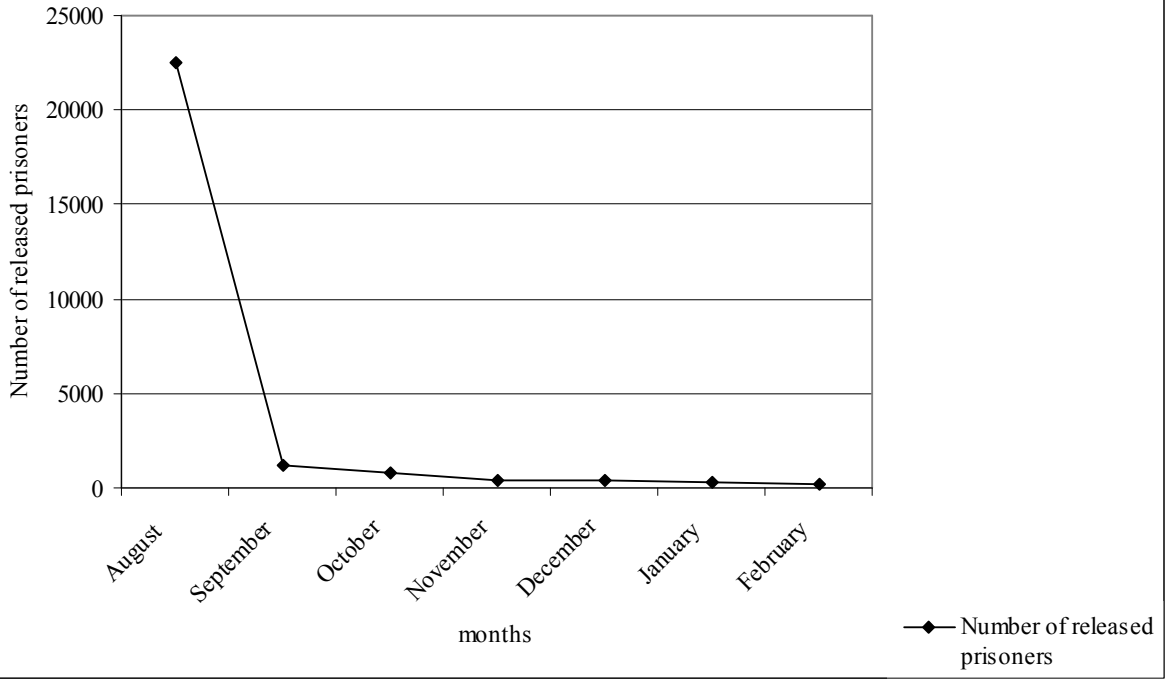

Figure 3. Residual sentence and recidivism

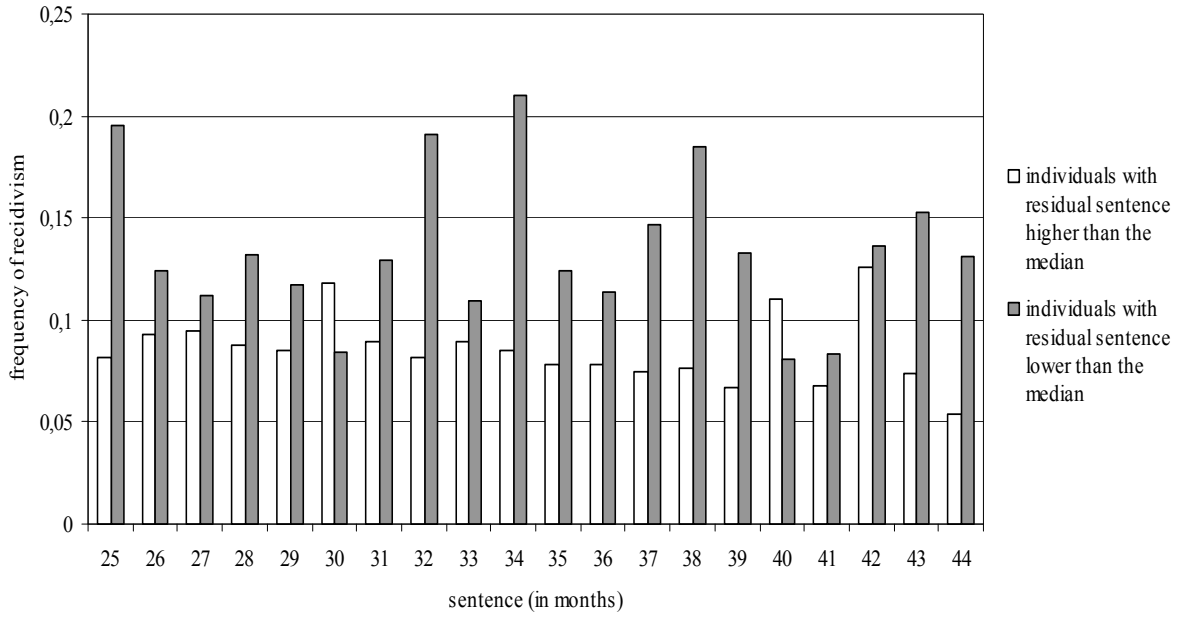

TABLE I: Descriptive Statistics

| | | Full Sample | | Criminals Recidivating | |
|---|---|---|---|---|---|
| *Number of Observations* | | 20452 | | 2397 | |
| *Individual Characteristics* | | | | | |
| | | *Mean* | *Standard deviation* | *Mean* | *Standard deviation* |
| Recidivism | | 0,12 | 0,32 | | |
| Age on Exit | | 36,71 | 9,93 | 34,40 | 8,63 |
| Time Served, in Months | | 24,81 | 29,94 | 23,03 | 26,41 |
| Length of Sentence | | 39,79 | 33,34 | 36,34 | 30,00 |
| Residual Sentence | | 14,98 | 10,46 | 13,31 | 9,88 |
| | | *Frequency* | | *Frequency* | |
| Nationality | | | | | |
| | Italian | 0,63 | | 0,62 | |
| | Non Italian | 0,37 | | 0,38 | |
| Marital Status | | | | | |
| | Married | 0,29 | | 0,20 | |
| | Unmarried | 0,58 | | 0,68 | |
| | Other | 0,13 | | 0,12 | |
| Education | | | | | |
| | Illiterate | 0,02 | | 0,02 | |
| | Primary | 0,31 | | 0,33 | |
| | Junior High | 0,52 | | 0,51 | |
| | High | 0,05 | | 0,04 | |
| | College (Degree or equivalent) | 0,01 | | 0,01 | |
| | Other | 0,09 | | 0,09 | |
| Employement | | | | | |
| | Permanently Employed | 0,35 | | 0,24 | |
| | Temporary Job | 0,08 | | 0,07 | |
| | Unemployed | 0,47 | | 0,58 | |
| | Student | 0,01 | | 0,01 | |
| | Retired | 0,01 | | 0,01 | |
| | Other | 0,08 | | 0,09 | |
| State of Judgement | | | | | |
| | Final Judgement Taken | 0,73 | | 0,66 | |
| | Mixed | 0,19 | | 0,24 | |
| | Appellant | 0,09 | | 0,08 | |
| | Other | 0,01 | | 0,02 | |
| Kind of Offence | | | | | |
| | Drugs offences | 0,39 | | 0,36 | |
| | Crime against property | 0,42 | | 0,50 | |
| | Crime against public admininstra | 0,02 | | 0,02 | |
| | Crime against public safety | 0,01 | | 0,01 | |
| | Violation of guns bill | 0,01 | | 0,01 | |
| | Immigration bill | 0,03 | | 0,02 | |
| | Various crimes against persons | 0,07 | | 0,05 | |
| | Curruption and crimes against ju | 0,01 | | 0,01 | |
| | Other | 0,04 | | 0,02 | |

TABLE II: Baseline Results

| | 1<br>Logit | 2<br>Logit | 3<br>Logit | 4<br>Conditional Logit | 5<br>Conditional Logit |
|---|---|---|---|---|---|
| sentres | -0,0158805 | -0,0149448 | -0,01532 | -0,0152899 | -0,0154041 |
| | (-5,89) | (-5,31) | (-5,34) | (-5,20) | (-4,95) |
| sentence | -0,0013755 | -0,0005516 | 0,0004011 | 0,0003765 | 0,0001152 |
| | (-1,90) | (-0,71) | (0,48) | (0,44) | (0,13) |
| individual characteristics | NO | YES | YES | YES | YES |
| type of crime | NO | NO | YES | YES | YES |
| Pseudo R-squared | 0,005 | 0,033 | 0,040 | 0,041 | 0,041 |
| Observations | 20452 | 18829 | 18823 | 18725 | 18815 |

Notes: Robust Z-statistics adjusted for clustering at the prison level (in column 5 at province level) in parenthesis. Individual variablesinclude education, age at the date of release, marital status and nationality dummies, juridical status and employment condition before imprisonnent. In column 4 (5) a conditional logit specificaion is estimated, grouped on prison (province), where the standard errors are clustered by prison (province)

TABLE III: The direct effect of time served

| | 1<br>sentence<=24 | 2<br>sentence>24 | 3<br>all sentences | 4<br>all sentences | 5<br>all sentences | 6<br>all sentences |
|---|---|---|---|---|---|---|
| sentres | -0,0066559 | -0,013445 | -0,0141372 | -0,0140196 | -0,0155668 | -0,01269 |
| | (-0,67) | (-4,39) | (-4,62) | (-4,41) | (-4,58) | (-3,88) |
| sentence | 0,0076087 | 0,0013051 | 0,0015024 | - | - | . |
| | (1,02) | (1,34) | (1,59) | | | |
| timeserved | - | - | - | -0,0010423 | -0,0012361 | 0,00240 |
| | | | | (-0,61) | (-0,69) | (0,14) |
| Dummy if sentence<=24 | - | - | 0,069332 | - | - | - |
| | | | (0,83) | | | |
| Sentres x dummy if sentence<=24 | - | - | 0,0118045 | - | - | - |
| | | | (1,56) | | | |
| Sentence fixed effects (a) | NO | NO | NO | YES | NO | YES |
| Sentence fixed effects (b) | NO | NO | NO | NO | YES | NO |
| individual characteristics | YES | YES | YES | YES | YES | YES |
| type of crime | YES | YES | YES | YES | YES | YES |
| interaction terms between type of crime and sentence fixed effects | NO | NO | NO | NO | NO | YES |
| Pseudo R-squared | 0,035 | 0,045 | 0,042 | 0,041 | 0,041 | 0,042 |
| Observations | 6209 | 12240 | 18725 | 18725 | 18725 | 18583 |

Notes: Conditional logit estimates grouped on prison in first five columns. Robust Z-statistics adjusted for clustering at the prison level in parenthesis. Individual variables include education, age at the date of release, marital status and nationality dummies, juridical status and employment condition before imprisonnent. (a) set of sentence dummies indicating the following interval of sentences (0-12) (13-24) (25-36) (37-48) (49-60) (61-72) (>72); (b)set of sentence dummies indicating the following interval of sentences (0-18) (19-30) (31-42) (43-54) (54-66) (66-78) (>78). The last column reports results from a logit model (the conditional logit model grouped by prison delivers a matrix not positive definite) .

TABLE IV: The indirect effect of time served

Panel A

| | 1 | 2 | 3 |
|---|---|---|---|
| sentres | -0,0198 | -0,01755 | -0,0161547 |
| | (-4,85) | (-3,86) | (-3,59) |
| sentence | -0,0031052 | -0,001799 | -0,0007247 |
| | (-1,92) | (-1,09) | (-0,45) |
| Sentres x dummy if 24<sentence<=48 | omitted | omitted | omitted |
| Sentres x dummy if 48<sentence<=72 | 0,0046059 | 0,001013 | -0,0004242 |
| | (0,59) | (0,10) | (-0,05) |
| Sentres x dummy if sentence>72 | 0,01940 | 0,01700 | 0,0153641 |
| | (2,69) | (2,26) | (2,03) |
| Individual characteristics | NO | YES | YES |
| Type of crime | NO | NO | YES |
| Pseudo R-squared | 0,005 | 0,034 | 0,045 |
| Observations | 12945 | 12240 | 12240 |

Panel B

| | 1 | 2 | 3 |
|---|---|---|---|
| sentres | -0,0256057 | -0,0255736 | -0,023344 |
| | (-4,77) | (-4,66) | (-4,47) |
| Timeserved x sentres | 0,0001829 | 0,0001916 | 0,0001669 |
| | (2,08) | (2,13) | (1,93) |
| sentence fixed effects (a) | YES | NO | YES |
| sentence fixed effects (b) | NO | YES | NO |
| Individual characteristics | YES | YES | YES |
| Type of crime | NO | NO | YES |
| Pseudo R-squared | 0,035 | 0,035 | 0,046 |
| Observations | 12240 | 12240 | 12240 |

Notes: Conditional logit estimates grouped on prison in Panel A and Panel B. Only individuals with *sentence*>24. Robust Z-statistics adjusted for clustering at the prison level in parenthesis. All specifications incldue individual variables: education, age at the date of release, marital status and nationality dummies, juridical status and employment condition before imprisonnent. In each each regression, for any interaction term between two variables included, we include both variables separately. (a) set of sentence dummies indicating the following interval of sentences (25-36) (37-48) (49-60) (61-72) (>72); (b)set of sentence dummies indicating the following interval of sentences (25-30) (31-42) (43-54) (54-66) (66-78) (>78).

TABLE A1: Test of Random Assignment for Given Sentence

| Sentence in Months | Employment Condition | Age | Grade of Judgement | Marital Status | Education | Nationality |
|---|---|---|---|---|---|---|
| 25 | 1,58 | 0,04 | 0,39 | 1,5 | -0,34 | 0,85 |
| | (1,47) | (0,96) | (0,38) | (1,37) | (-0,9) | (0,95) |
| 26 | -0,02 | 0,12 | 1,33 | 1,87 | -0,44 | 0,33 |
| | (-0,02) | (3,31) | (1,81) | (2,19) | (-1,3) | (0,46) |
| 27 | 0,49 | 0 | 1,64 | 1,17 | -0,46 | 1,27 |
| | (0,46) | (-0,1) | (1,83) | (1,24) | (-1,1) | (1,43) |
| 28 | -0,11 | 0,07 | 0,91 | 2,41 | 0,01 | 0,2 |
| | (-0,12) | (2,07) | (1,29) | (3,02) | (0,06) | (0,3) |
| 29 | 1,85 | 0,08 | 0,22 | 0,68 | -1,3 | 1,23 |
| | (1,32) | (1,33) | (0,19) | (0,53) | (-1,42) | (1,11) |
| 30 | 1,02 | 0,06 | -0,68 | -0,37 | -0,05 | 0,36 |
| | (1) | (1,6) | (-0,88) | (-0,42) | (-0,14) | (0,46) |
| 31 | 1,05 | 0,06 | 3,67 | 0,61 | -0,46 | 2,11 |
| | (0,8) | (1,13) | (2,9) | (0,48) | (-0,78) | (1,75) |
| 32 | 0,47 | 0,07 | -0,38 | 0,39 | -78 | -0,6 |
| | (0,39) | (1,58) | (-0,45) | (0,4) | (-1,83) | (-0,69) |
| 33 | -1,27 | 0,07 | 2,59 | 1,54 | -0,74 | 1,99 |
| | (-0,9) | (1,36) | (2,24) | (1,33) | (-1,41) | (1,76) |
| 34 | 0,34 | 0,02 | 1,29 | -1,72 | -0,24 | 0,44 |
| | (0,24) | (0,51) | (1,1) | (-1,22) | (-0,48) | (0,41) |
| 35 | -0,76 | 0,09 | 0,81 | -2,24 | -0,7 | 3,63 |
| | (-0,5) | (1,59) | (0,58) | (-1,68) | (-1,1) | (2,56) |
| 36 | -1,11 | 0,04 | -2,02 | 0,59 | -0,45 | -0,51 |
| | (-0,98) | (1,03) | (-2,67) | (0,64) | (-1,17) | (-0,68) |
| 37 | 2,51 | 0,09 | 2,49 | -0,26 | -0,45 | 4,4 |
| | (1,46) | (1,53) | (1,7) | (-0,19) | (-0,77) | (3,61) |
| 38 | -0,33 | 0,14 | 2,13 | 1,43 | -0,34 | 3,81 |
| | (-0,22) | (2,59) | (1,87) | (1,26) | (-0,62) | (3,74) |
| 39 | -1,09 | 0,17 | 2,78 | 1,3 | -0,09 | 5,96 |
| | (-0,72) | (3,02) | (1,8) | (0,89) | (-0,15) | (4,08) |
| 40 | -1,77 | 0,12 | -3,21 | 1,86 | -0,75 | 1,29 |
| | (-1,53) | (2,79) | (-3,37) | (1,7) | (-1,74) | (1,38) |
| 41 | -1,01 | 0,13 | 1,54 | 1,55 | -1,35 | 3,8 |
| | (-0,62) | (2,13) | (1,11) | (1,13) | (-2,34) | (3,22) |
| 42 | -2,75 | -0,02 | 0,94 | 0,77 | -1,6 | 2,22 |
| | (-1,85) | (-0,46) | (0,84) | (0,66) | (-3,24) | (2,16) |
| 43 | -2,23 | 0,06 | 0,89 | 1,02 | -85 | -0,45 |
| | (-1,59) | (1,1) | (0,72) | (0,81) | (-1,54) | (-0,41) |
| 44 | -0,21 | 0,02 | -1,64 | -0,73 | -0,44 | -0,37 |
| | (-0,2) | (0,65) | (-1,61) | (-0,69) | (-1,04) | (-0,41) |

Note: The entries in the table represent the coefficient of separate univariate regressions of the residual sentence on different individual background characteristics or given sentence. Robust t-statistics are reported in italic

TABLE A2: Baseline results all control variables

| | 1 All Conditional Logit | 2 Non italian Conditional Logit | 3 Italian Conditional Logit |
|---|---|---|---|
| sentres | -0,0152899 | -0,0168784 | -.012959 |
| | (-5,20) | (-3,26) | (-3.80) |
| sentence | 0,0003765 | -0,0012767 | .000339 |
| | (0,44) | (-0,61) | (0.32) |
| italian | -0,54171 | - | - |
| | (-2.17) | | |
| married | -0,4063575 | -0,5420136 | -.3609441 |
| | (-7.55) | (-4,6) | (-5.28) |
| final judgement | -0,3287168 | -0,1488793 | -.4439436 |
| | (-6.6) | (-1.93) | (-6.05) |
| age | -0,0223054 | -0,0057156 | -.0280586 |
| | (-7.82) | (-1.01) | (-8.00) |
| education | -0,0262654 | -0,0332867 | -.0232403 |
| | (-0.89) | (-0.77) | (-0.62) |
| employment status | -0,2977809 | -0,1131761 | -.3629402 |
| | (-4.51) | (-0.87) | (-4.75) |
| drug | 0,3251337 | 0,1856732 | .3531305 |
| | (1.78) | (0.54) | (1.68) |
| against property | 0,6888503 | 0,5128519 | .7307033 |
| | (3.71) | (1.4) | (3.56) |
| against public administration | 0,5841431 | -0,005523 | .8208356 |
| | (2.22) | (-0.01) | (2.76) |
| gun law | 0,4012971 | 0,4916572 | .4402429 |
| | (1.37) | (0.77) | (1.40) |
| breach of public faith | -0,2258299 | -0,1225789 | -.6220884 |
| | (-0.44) | (-0.17) | (-0.86) |
| public safety | 0,3219819 | 1,177146 | .2472627 |
| | (0.82) | (1.49) | (0.52) |
| immigration law | 0,1635728 | -0,0444501 | -13.81521 |
| | (0.67) | (-0.12) | (-28.92) |
| against competition and market functioni | -0,607101 | -12,22648 | -.4956709 |
| | (-0.9) | (-11.66) | (-0.70) |
| prostitution | 0,6065797 | 0,6907334 | .1838654 |
| | (1.21) | (1.06) | (0.17) |
| petty crimes | -0,6133598 | -12,96396 | -.4480134 |
| | (-0.59) | (-19.77) | (-0.43) |
| public order | -0,2160399 | -13,00726 | .1203736 |
| | (-0.26) | (-27.83) | (0.14) |
| against person violent crimes | 0,1508612 | -0,0397042 | .2526588 |
| | (0.71) | (-0.1) | (1.02) |
| against family | -0,1866572 | 0,3681381 | -.2344512 |
| | (-0.35) | (0.33) | (-0.40) |
| corruption of judges | -0,0719349 | 0,5017423 | -.0263271 |
| | (-0.23) | (0.51) | (-0.08) |
| against the State | 0,8835556 | -13,84958 | 1.610782 |
| | (1.09) | (-18.74) | (2.05) |
| other | 0,7496466 | -0,2311501 | .9046575 |
| | (2.78) | (-0.3) | (3.03) |
| Pseudo R-squared | 0,041 | 0,041 | 0,046 |
| Observations | 18725 | 6721 | 11774 |

Notes: Conditional logit estimates grouped on prison. Robust Z-statistics adjusted for clustering at the prison level in parenthesis.