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ABSTRACT

Indirect Effects of a Policy Altering Criminal Behaviour: Evidence from the Italian Prison Experiment*

We exploit the Collective Clemency Bill passed by the Italian Parliament in July 2006 to evaluate the indirect effects of a policy that randomly commutes actual sentences to expected sentences for 40 percent of the Italian prison population. We estimate the direct and indirect impact of the residual sentence – corresponding to a month less time served in prison associated with a month of expected sentence – at the date of release on individual recidivism. Using prison, nationality and region of residence to construct reference groups of former inmates, we find large indirect effects of this policy. In particular, we find that the reduction in the individuals' recidivism due to an increase in their peers' residual sentence is at least as large as their response to an increase in their own residual sentence. From this result we estimate a social multiplier in crime of 2.

JEL Classification: K00, C90

Keywords: crime, social interactions, indirect effects

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Introduction

Despite governments spending large amounts of resources in fighting crime, we still do not know much about the effectiveness of different approaches to combating crime. Crime control policies typically reduce crime through three channels: by removing criminals from society (*incapacitation*), by modifying individuals' preferences and expectations about the consequences of their actions (*rehabilitation*), and by manipulating the opportunity costs of committing a crime (*deterrence*). While incapacitation and rehabilitation are peculiar to incarceration, a number of policies may rely on the third channel only.

Prisons account for a significant part of the expenditure on the functioning of the justice system.[1] Hence, it is relevant to understand how incarceration impacts on crime and whether alternative policies that reduce crime but are not based on incapacitation could be more effective than incarceration, which is very costly (Donohue and Siegleman, 1998).[2] For example, electronic monitoring, transfer and housing mobility programs are not associated with incapacitation but modify the incentives not to commit a crime and recent evidence suggests these approaches can be very effective (Di Tella and Schargrodwsky, 2009; Ludwig, Duncan and Hirschfield, 2001; Kling, Ludwig and Katz, 2005; Cullen, Jacob and Levitt, 2006). A necessary condition for correctly evaluating the impact of both incarceration and alternative policies is to identify and evaluate whether these crime control policies have indirect effects. Indirect effects may be very large if crime is contagious, meaning that the criminal activity of an individual increases the

[1] For the fiscal year 2006, the Bureau of Justice in the US estimated that federal, state and local governments spent an estimated $68 billion on corrections, that is roughly half of the total expenditure on police and judicial and legal activities.

[2] Donohue and Siegleman (1998) discuss the cost and crime-reducing potential of imprisonment and social spending such as preschool enrichment programs and family interventions that alter future criminal behaviour. They argue that it can be optimal to shift resources from policies that increase incapacitation (prisons) to social programmes.

propensity to commit a crime of other individuals. In this case, a policy altering incentives to commit a crime may have an impact also on individuals not targeted by the policy. Thus, ignoring such indirect effects would lead to underestimating the effect of any intervention altering criminal behaviour and would hamper the correct evaluation of both the effectiveness of the policy and its relative costs and benefits with respect to alternatives.

In this paper we use the recent Italian prison experiment of August 2006 to study the indirect effects of a policy dramatically changing the incentives to commit a crime, and from the evaluation of these indirect effects we try to infer whether the criminal activity of the former inmates in our sample is in fact contagious. The Italian prison experiment involved more than 20,000 inmates, i.e. about 40 percent of the Italian prison population at that time. It provides us with an ideal experimental design where different treatments altering individual criminal behaviour for groups of potential criminals were administered in a random fashion. Hence, the experimental design enables us to estimate how individuals respond to the treatment received and to the treatment received by other members of a group.

The Italian prison experiment is provided by the Collective Clemency Bill, passed by the Italian Parliament in July 2006. The collective clemency provided for an immediate 3-year reduction in detention for all inmates who had committed a crime before May 2, 2006. On approval of the bill, almost 22,000 inmates were released from Italian prisons in August 2006. Most importantly for our objective, the bill provides that if a former inmate recommits a crime within 5 years of 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.

Thus, the policy commutes 1 month of the original sentence into 1 month more expected sentence for future crimes at the individual level.

Our main variable of interest is the residual sentence at the date of release, which varies at the individual level. Conditional on inmates' original sentences, the variation in the residual sentence at the date of the pardon (and hence in the expected sentence for any crime) depends only on the date of an inmate's entry into prison, which is plausibly exogenous.[3] Moreover, as reported in Drago, Galbiati and Vertova, (2009), this policy directly affects individual criminal behaviour: a month more residual sentence has a large effect on individual recidivism, as it decreases the probability of being re-arrested by about 0.18 percentage points. As a first step, we try to understand whether former inmates' decisions to reoffend or not are influenced by the residual sentences of those they are more likely to be in contact with. To answer this question, we define groups of inmates based on prison and nationality for foreign inmates (who represent 40 percent of the sample) and prison and region of residence for Italian inmates, so that inmates released from the same prison and of the same geographical origin belong to the same group. The data support the notion that the residual sentences of the individual's peers are as good as randomly assigned (these are uncorrelated to individual characteristics, conditional on peers' original sentences). We note that the residual sentences of the individual's peers have a large impact on individual recidivism. The estimated impact of the average residual sentence in the group (excluding the individual himself) is comparable to the direct effect of the individual residual sentence. In particular, 1 month more average residual sentence decreases the probability of being re-arrested by 0.16 percentage points. As supporting evidence that we generate sensible

[3] As supportive evidence for this hypothesis, we find that conditional on the original sentence length, inmates' observable characteristics are balanced for individuals below and above the median of the remaining sentence.

peer groups, we provide several falsification tests where nationalities and regions within the same prison or prisons for the same nationality and region are randomly generated. When we construct the peer groups according to randomly generated prisons or nationality and region identifiers, the indirect effects of the policy are essentially zero.

The natural experimental design enables us to understand if the source of these indirect effects is peers' criminal behaviour. Considering groups composed of inmates with the same length of stay (and who were released from the same prison and who are from the same national group or region for Italians), we still find a large negative effect of the average residual sentence on individual recidivism. Given that one month of residual sentence is one month more of expected sentence associated with a month less of time served, finding no indirect effects in this case where all inmates in a group served the same amount of time would have implied that peers' time served rather than peers' expected sentences reduces individual criminal behaviour.[4] This result supports the idea that the effect of average residual sentences comes from the incentive effect of average peers' expected sentences and that recidivism is in fact contagious. The unique Italian prison experiment makes it possible to estimate a social multiplier of crime for this large sample of former inmates. Under the assumption that average peers' residual sentences impact individual behaviour only through peers' criminal behaviour, the basic results in this paper imply a social multiplier of about 2.[5] This means that a shock decreasing individual recidivism of 1 percent implies a 2 percent reduction in aggregate recidivism in equilibrium for the universe of individuals released pursuant to the Bill.

[4] Peers' time served may reduce criminal behaviour, for example, because the peers' length of stay is associated with individual knowledge about specific crime rates that may be transmitted by peers in prison (Bayer et al. 2009).
[5] The fact that the average incentive has an effect on criminal behaviour that is at least as large as the individual incentive implies that by increasing the residual sentence for all inmates by one month, we obtain a total effect that is double the direct effect.

This paper contributes to the literature on policy evaluation and social interactions in crime in several ways. By exploiting the quasi-natural variation in the data and the large scale of the experiment, we provide evidence for large indirect effects of a broad policy intervention that affected 40 percent of the prison population in Italy at a given point in time. An important aspect of our work is that policymakers can manipulate the variable (i.e. residual sentence) under scrutiny (e.g. probation is similar to the policy we exploit in that it commutes actual sentences into expected sentences). To this extent, this study has policy relevance given the large applicability of such interventions. Moreover, the unique Italian Collective Clemency Bill makes it possible to extrapolate a social multiplier. Given the difficulty of identifying social interactions (Manski, 1993), this is one of the first estimates of a social multiplier in crime in an experimental setting.

The paper develops as follows. In the next section we discuss the related literature and in section III we briefly describe the institutional setting. Section IV illustrates the empirical strategy and section V the data and the empirical results. The last section concludes.

II. Related Literature

Our study is related to two strands of literature analyzing effective approaches to crime reduction through incentives that alter criminal behaviour. The first one exploits aggregate data and provides estimates of policy parameters that comprise the direct and the indirect effect of interventions reducing crime. For example, Levitt (1996) estimates the effect of the prison population on crime, which includes both deterrence and incapacitation effects; Owens (2009) estimates incapacitation effects by exploiting sentence enhancements; Levitt (1997), Corman and

Mocan (2000) and Di Tella and Schargrodsky (2004) estimate the crime-reducing effects of enlarging police forces. While these studies are able to capture the overall effect of an intervention (an increase in police or prison population), from these papers we cannot understand how much of the overall effects can be accounted for by the externalities generated by the intervention itself. In particular, it is difficult to use aggregate data to make inferences about individual behaviour in the presence of externalities (Glaeser et al. 2003). The second strand of literature studies how potential criminals respond to interventions altering criminal behaviour using the individual as the unit of observation (Drago, et al., 2009: Kuziemko, 2007; Lee and McCrary, 2009; Kling et al., 2005; Cullen et al., 2006; Lochner and Moretti 2006). These papers estimate the direct response of individuals to the intervention. However, if externalities are present, these papers do not estimate a relevant part of the policy intervention and fail to estimate the "true" average treatment effect on the treated, which is the sum of the direct effects and any indirect effects of the policy.

The importance of exploring indirect effects is not confined to crime and it is related to the design and evaluation of controlled experiments. Randomized experiments solve the obvious problems of self-selection of individuals into particular treatments. However, in controlled experiments, in order to have a correct evaluation of the treatment effects one has to assume that treatment for untreated subjects has no effect (i.e. the stable unit treatment value assumption, SUTVA) (Imbens and Woolridge, 2009).[6] Some recent papers have shown that in some contexts,

[6] The absence of general equilibrium effects is also crucial in many applications of programme evaluation. For example, when randomization does not take place at the individual but rather at a more aggregate level (school, block, group of individuals) and there is not perfect compliance, using assignment to a treatment group as an instrumental variable for actual treatment leads to a failure of the exclusion restriction in the presence of externalities within groups (the assignment to treatment influences individual behaviour not only via the probability of receiving treatment but also through externalities).

in fact, indirect effects exist. Most notably, Miguel and Kremer (2004) estimate externalities of a deworming treatment in Kenya, Duflo and Saez (2003) provide evidence of informational spillovers in retirement plan decisions, and Angelucci and De Giorgi (2009) show that cash transfers in treatment villages of the Progresa program in Mexico increase the food consumption of ineligible households relative to the average consumption in control villages. More related to our paper, using field data and snowfall as an instrument for local inspections, Rincke and Traxler (2010) is the first work documenting large enforcement spillovers in compliance with TV licence fees payment in Austria.

Our paper adds to this literature by documenting large indirect effects of a policy that affects the incentive to reoffend for a large sample of former inmates. With respect to most of the relevant literature, our design has two key features. First, we have a continuous treatment (the residual sentences) directly affecting criminal behaviour. Second, this treatment varies at the individual level (i.e. we have within-group variation). These important features allow us, under an exclusion restriction, to translate the estimated indirect effects of the policy into the effects of group criminal behaviour on individual behaviour.

Our paper is also related to the literature on social interactions and crime. The direct effects of policies altering individual behaviour are amplified by the presence of social interactions. However, as Manski (1993) pointed out, it is crucial to distinguish exogenous social interactions from endogenous ones. When the characteristics of a group influence the average behaviour in a group, we are in the presence of exogenous social interactions. In this case, we do not have indirect effects, in the sense that any exogenous average behaviour's shock in a group does not result in a change in individual behaviour. Instead, in the presence of endogenous social interactions, individual behaviour is influenced by the behaviour of other group members and the effect of any intervention reducing individual criminal behaviour is amplified by social interactions. It is difficult to identify endogenous social interaction in the absence of an exogenous source of variation in the criminal peer group behaviour (Manski, 1993; Brock and Durlauf, 2001; Mofitt, 2001). Glaeser et al. (1996) and Ludwig and Kling (2007) circumvent the identification problems by using aggregate data.[7] They find positive peer effects in criminal behaviour only for less serious crimes. In a recent paper, Bayer et al. (2009) estimate network crime effects for former inmates in the U.S. They study the impact of peer characteristics on individual recidivism and find that individual exposure to peers in the same crime category increases individual recidivism. As the identification strategy allows controlling for non-random assignment of inmates into facilities but does not allow estimation of the effect of group behaviour on individual behaviour, Bayer et al. (2009) assume that the estimated social effects are mediated only by exogenous social interactions. The main difference of our study relative to Bayer et al. (2009) is that we document the externalities of an intervention that directly affects incentives to reoffend and therefore our estimates are more easily interpretable in the sense of the effect of group behaviour on individual behaviour. Moreover, as our treatment variable is peers' residual sentences – which we assume to be orthogonal to individual and peer characteristics – rather than peer characteristics, we can allow for self-selection of individuals into peer groups.

[7] Glaeser et al. (1996) observe that when the cross-group variation in individual behaviour exceeds that which can be accounted for by fundamentals (observables and unobservables), one is left with "excess variance," which can be used to identify social interactions. Kling and Ludwig (2007) use data from the Moving to Opportunity (MTO) randomized housing-mobility experiment. They exploit variation in neighbourhood changes across cities induced by treatment group assignment as a source of exogenous variation in crime rates across neighbourhoods.

III. The Italian prison experiment

Here we briefly describe the motivations for and the provisions of the collective pardon law approved by the Italian Parliament in July 2006.[8]

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 55,000 compared to a total of 42,000 available places; the average overcrowding index was 131 inmates to 100 prison places. As a consequence of this emergency situation, the Italian Parliament passed a collective pardon on July 30th 2006 (Law 241/2006). This legislative measure is to be considered of exceptional nature. According to the Italian Constitution, any law providing for the implementation of an amnesty or a collective pardon must be approved by both Chambers of Parliament with a majority of two thirds of the votes in favour of each article of the law (Section II, Art.79 of the Italian Constitution). These conditions are the same as those for approval of a constitutional reform (art.138).

The bill provides for a reduction in the length of detention for those who committed a crime before May 2nd 2006. This backdating of the collective pardon, which was announced immediately after the 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 legislation 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

[8] See Drago et al. (2009) for a more complete description of the Italian criminal justice system and the background details concerning the design and approval of the collective clemency.

types of crime are excluded from the collective pardon, in particular those related to the 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 provide 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 is less than three years. As a consequence of the collective pardon, from a total of 60,710 individuals on July 31st 2006, the prison population dropped to 38,847 on August 1st 2006.

As far as our research question is concerned, the crucial consequence of the bill is the variation in prison sentences at the individual level. The bill provides that all those re-committing a crime within the five years following July 31st 2006 and receiving a further sentence greater than two years lose the benefit of the clemency. This means that within the five years following their release from prison as a result of the collective pardon, former inmates face an additional expected sanction equal to the residual sentence pardoned by the bill. Take for instance two criminals convicted with the same sentence and having a residual sentence of less than three years on August 1st 2006. They are both released from prison on August 1st 2006. Suppose that the first individual entered prison one year before the other and has a pardoned 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).

IV. Empirical Strategy and Identification

In this section we present our empirical strategy and we discuss the source of variation for identification. Denote with y_{ijk} the post-release outcome of an individual i of nationality k (region of residence k if Italian) who served his former sentence in prison j (y_{ijk} takes value 1 if the individual was re-arrested in the period under consideration and 0 otherwise). As we will explain in detail in the next section, prison and nationality (region of residence for Italians) define a reference group. Moreover, denote with $sentres_{ijk}$ and $sentence_{ijk}$ his residual sentence (pardoned) and original sentence.[9] The basic regression model we use in this paper is:

$$y_{ijk} = \beta_0 + \beta_1\, sentence_{ijk} + \beta_2 sentres_{ijk} + \beta_3 avgentence_{(-i)jk} + \beta_4 avgsentres_{(-i)jk} + X'_{ijk}\, \phi$$

$$+ avgX'_{(-i)jk}\, \varphi + \varepsilon_{ijk}\,, \qquad (1)$$

where $avgsentres_{(-i)jk}$ and $avgentence_{(-i)jk}$ are individual-level variables; specifically, the average residual sentence and the average original sentence in the group of individuals of nationality (region of residence for Italians) k in prison j, excluding individual i, respectively. In other words $avgsentres_{(-i)jk}$ represents individual i's peers' average residual sentence, which according to the design of the collective pardon is part of the individual i's peers' incentives to recidivate. With X_{ijk} we denote a set of individual-level control variables and $avgX_{(-i)jk}$ their averages in the group, excluding the individual i.

[9] Throughout the analysis both original sentence and residual sentence and their averages are expressed in months.

The estimated coefficient of interest is β_4, that is the response of individual *i* to an additional month in the average residual sentence in his group, i.e. the indirect response to the policy. The coefficient β_2 is the direct response to the policy that commutes one month of actual sentence into one month of expected sentence for individual *i*. It is worth remarking that unlike other papers such as Bayer et al. (2009) interested in the effect of peers' characteristics on individual behaviour, here our treatment is the peers' residual sentence rather than peers' characteristics. Therefore, in our framework we do not need to assume that selection of individuals into groups (and hence also in a given prison) is random. The assumption needed is that the peers' residual sentences are orthogonal to individual and peer characteristics. Hence, to obtain a consistent estimate of β_4, the conditional independence assumption is that once we control for the individual sentence, residual sentence and average sentence, the average residual sentence is orthogonal to unobservables. Namely, the assumption is $cov\ (avgsentres,\ \varepsilon \mid sentence,\ sentres,\ avgsentence) = 0$. Obviously, we cannot directly test this hypothesis but in the next session we provide evidence consistent with it based on different tests on observables.

In order to provide some intuition about the source of variation exploited in our empirical work, here we present three extremely simplified examples. Once we condition on the individual original sentence, individual residual sentence, and average original sentence, there are three scenarios leading to variation in the variable of interest (average peers' residual sentence). Consider a group composed of two individuals only, *A* and *B*. In the first scenario, individuals *A* and *B* entered prison in the same month but with different original sentences. For example, individual *A* entered with an original sentence of 18 months and individual *B* with an original sentence of 9. Both entered and are released together three months later. Hence, *A*'s peer's *average* residual sentence is equal to 6 months (the residual sentence of *B*) and *B*'s peer's *average* residual sentence is of 15 months (the residual sentence of *A*). This scenario is reported in the first column of Table I. Second, individuals *A* and *B* entered prison with the same original sentence but in different months (second column of Table I). The third scenario is the one in which the two scenarios 1-2 above are combined, with the individuals entering with different original sentences and in different months (third column). Unlike the third one, in the first two examples there is perfect collinearity among the four variables. Whenever an additional individual breaks this collinearity, we have variation in the average peers' residual sentence that is exploited to estimate the parameter β_4. These simplified examples clarify the fact that identification of the parameter of interest is obtained from groups of individuals for which once we condition on individual original sentence, individual residual sentence and average original sentence, there is enough variability in the average residual sentence.

A final issue is related to the dependent variable. Clearly, we do not observe the criminal activity of former inmates, but only whether they are rearrested. Given that the number of individuals who commit a criminal act but are not arrested is likely to be much higher than the number of individuals who are arrested but have not committed a criminal act, the estimated impact of the residual sentence and of the average residual sentence should be interpreted as lower bounds of the true effects.

V. Data, Peer Groups and Evidence on the Identification Assumption

A. Individual-Level Data

The 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 all the individuals released as a result of the collective pardon law between August 1st 2006 and February 28th 2007. The full sample includes 25,813 individuals; 81 percent were released on August 1st 2006. For each individual, the data provide information on whether or not he or she re-offends within the period between release from prison and February 28th 2007. This means that for most of the individuals the data report 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, sex, level of education, marital status, nationality, province of residence, nationality and employment status before being sentenced to prison. 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 their sentence in prison, i.e. we exclude from the analysis individuals convicted to serve a sentence in a penal mental hospital (98 individuals). Moreover, we exclude from the sample any individual with a residual sentence higher than 36 months. This is the case of individuals accumulating different charges with a sentence for at least one, but awaiting verdicts on others. We do not consider individuals for whom sentence data are missing. Because we want to perform the empirical analysis with a sample that is homogenous along both the date of release and the length of window (7 months), we exclude individuals with a residual sentence equal to 36 months. We do not know the exact date of release of each inmate but we know that any other inmate released after August 1st 2006 necessarily had a residual sentence of 36 months. The final sample used in the empirical investigation is made up of 20,950 individual-level observations.

B. Reference Groups

Using the individual-level data, we construct reference groups as groups of inmates of the same nationality in the same facility. For Italians (which are the majority of our sample) we refer to those from the same region who pass their sentence in the same facility.[10] The underlying assumption is that speaking the same language and sharing similar characteristics and values are all factors that facilitate interaction, especially in an isolated environment such as a prison. In fact, Italians from the same region are very likely to share the same values and cultural background (see Guiso, Sapienza and Zingales 2008). Bertrand et al. (2000) and Aizner and Currie (2004) treat language as the characteristic defining a reference group in the context of welfare use participation. Nonetheless, in the context of our study we feel it is more appropriate to resort to nationality as the feature defining a group. Italy is a country of very recent immigration and it is likely that non-Italian speakers have not been living in Italy for a long time. If this is the case, the literature on migration studies (Boyd, 1989) shows that it is common for recent immigrants to keep strong ties with people of the same nationality in the country of destination. For instance, by resorting to language as a variable to construct reference groups, we

[10] Note that women and men always belong to different groups, as they spend their sentences in different prisons or different branches of the same prison.

would treat Mozambicans and Brazilians as belonging to the same group. Relative to nationality, this would be a noisier proxy for identifying cultural and contextual differences. However, in section F we explore how results change when we change this definition of the reference group. Finally, unlike previous works which assume that interactions among individuals take place at the district or zip code level, by the very nature of prisons the assumption that exposure to peers is at the prison level seems less problematic.

C. Summary Statistics and Evidence on the Identification Assumption

In column 1 of Table II descriptive statistics on the individual-level data are reported. Those re-offending constitute 11.5 percent of the sample. Most of the sample is composed of males (95 percent) and Italians (62 percent). Only 28 percent are married and 34 percent were permanently employed before entering prison; 90 percent had attended compulsory schooling. The average residual sentence – varying between 1 and 35 months – is equal to 14.51 months, while the average original sentence is about 39 months. The variation in the original sentence length is large. There are individuals with a sentence longer than 360 months who were convicted for violent crimes (e.g. murder) as well as individuals with very short sentence lengths. The majority of the sample comprises individuals convicted for crimes against property or offences related to the drug law. 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.

Excluding groups composed of one individual, in our sample we have groups of Italians from 20 regions (the total number of Italian regions) and groups of foreign inmates from 129 nationalities. The number of groups defined on the basis of nationality (region of residence for

Italians) and prison facility is 1,670. These statistics refer to groups with more than one individual (regression analysis uses only groups with more than one observation). The average size of the group is 11.27. Most groups are foreign (61.38 percent), although the average size of foreign groups is 6.46, as opposed to 19.20 for Italians. Where females and males are registered in the same prison, we separate groups according to sex, so that females from the same prison and nationality do not belong to the groups of males. We have 71 groups of females (25 groups of Italians and 46 of foreigners). In column 2 we report the descriptive statistics for individuals belonging to groups with more than one inmate, which we exploit to estimate the basic regression model.

To provide evidence consistent with the identifying assumption we resort to several tests. Our basic assumption is that peers' residual sentence is orthogonal to unobservables once we condition on peers' original sentence (and individual original and residual sentences). This is equivalent to saying that for individual *i*, once we fix the average original sentence of his peers and his original and residual sentence, the average residual sentence determined by the date of entry into prison of his peers is as good as random.

As a first step, in columns 3-4 of Table II, we report the averages of the observed characteristics for those observations where the average residual sentence of other inmates in the same group is above the median for that average original sentence length, and those observations where the average residual sentence of those sharing the same reference group is below the median for that average original sentence length.[11] In column 5, differences in the means are reported. This is

[11] As the average original sentence is a continuous variable, for each individual observation we condition on the

equivalent to a test of observables being balanced for individuals with average residual sentence below and above the median, conditional on the original sentence. This test is non-parametric in that it tests the equality of means between two groups without imposing any assumption on the relationship between observables and average residual sentence. As shown in column 5, in nearly all cases there is no significant relationship between the demographic variables and the average residual sentences. The few point estimates that are statistically different from zero reveal extremely small differences and well below 5 percent of a standard deviation from the mean. These results support the idea that the average residual sentence in a group is a variable that is uncorrelated to unobservables once we condition on the average original sentence. In column 6, we perform the same test but we impose a parametric structure on this, presenting the point estimates of the OLS regressions of each individual characteristic on average residual sentence and average original sentence. In column 7, we present the estimate of the same OLS regression conditioning also on individual original and residual sentences. Compared to the previous test, the OLS weighting scheme tends to overestimate some differences in observables between individuals with low and high average residual sentence.[12] There are many reasons why the results from a parametric test may be different from the results of a linear regression, including the distribution of regressors and the amount of heterogeneity in the relationship of interest (Yitzhaki, 1986 and Angrist and Kruger, 1999). While for the sake of completeness we also report the tests from the linear regressions, we argue that the most informative test is the first one reported in columns 3-5. Our treatment variable is continuous and the crucial assumption is that the treatment is caused by random fluctuations in the peers' dates of entry into prison. The most transparent and sensible way to support this assumption is to show that observables are balanced

closest higher integer (e.g. for average group original sentences between 1 and 2 months we condition on the value 2, for average group original sentences between 2 and 3 months we condition on the value 3, and so on and so forth.)

[12] Also in this case, however, the few point estimates that are precisely estimated are very small.

without imposing a linear relationship between observables and the date of entry into prison.[13]

Finally, in the regression analysis, when we look for differential effects and we check whether the coefficient on the average residual sentence varies with observables, we still obtain a significant effect of the average residual sentence.

VI. Results

A. Graphical Evidence

Before presenting our main econometric specification, we provide some graphical evidence showing whether former inmates respond to the expected sentences of those sharing their same reference group or not. To give an idea of the relevance of the indirect effect of an incentive not to engage in crime, in Figure 1 we report both individuals' responses to their own expected sentences and to their peers' expected sentences. Figure 1-a reports the recidivism rate for each original sentence for former inmates with residual sentences both above and below the median for that original sentence length, thus providing graphical representation of the direct effect of the residual sentence on individual recidivism. Here we report only sentence groups between 20 and 50 months, which is the range of sentences to which most individuals are convicted. As is clear from Figure 1, the recidivism rate for individuals with residual sentences above the median is systematically lower for each sentence. In particular, for this group of inmates, the means of residual sentences are equal to 9.69 and 23.55 months for inmates with residual sentence below

[13] In fact, when we regress, for example, the fraction of Italians on the average residual sentence and average original sentence as in column 6, we ask if, conditioning on the average original sentence, an increasing fraction of Italians' peers enter prison earlier or later than foreigners, by testing whether a monotonic relationship exists between the date of entry in prison and the share of Italians over the entire range of the dates of entry into prison.

and above the median, respectively. The average recidivism of the former group is 12.9 percent while that of the other group is 10.2 percent.

Figure 1-b reports the recidivism rates for former inmates whose peers' residual sentences are above and below the median, conditional on peers' original sentence.[14] The emerging picture is one of higher recidivism for former inmates whose peers' expected sentence is lower than the median conditional on peers' original sentence. For the range of original average sentences that we report, the average peers' residual sentences are equal to 12.4 and 16.2 months for inmates with residual sentence below and above the median, respectively. The average recidivism of the former group is 11.3 percent while that of the latter is 12.4 percent. Figure 1-b suggests that the individuals' response to their peers' incentives (i.e. the indirect effect of residual sentences) has a relevant size. In the reported interval, an increase of about 4 months in average residual peer sentence reflects in a reduction of average recidivism of about 9.5%. Overall, Figure 1 shows preliminary evidence that a policy manipulating incentives not to recommit a crime has sizeable effects, both direct and indirect.

B. Results

Table III reports the baseline results of variations in model (1). Standard errors are clustered at the group level. In the first column we present the results for a specification of the model including only the individual original sentence ($sentence_{ijk}$), the average original sentence of the group excluding the individual i ($sentence_{(-i)jk}$), the individual residual sentence ($sentres_{ijk}$) and the average residual sentence in the group excluding individual i ($avgsentres_{(-i)jk}$). The coefficient

[14] In this case, as the average original sentence is a continuous variable, for each individual observation, we condition on the closest higher integer.

β_2 is negative and precisely estimated: an additional month in the residual sentence decreases the probability of recidivism by 0.16 percentage points. The coefficient β_4 on the average residual sentence is also negative and precisely estimated. It appears that the average effect of peers' residual sentence is at least as important as the individual residual sentence. The results suggest that by increasing the residual sentence of an inmate's peers by one month, the probability of recidivism reduces by 0.20 percentage points. In columns 2-3 we include a set of individual characteristics – age, sex, nationality, education, marital status, employment dummy and the type of crime committed before release both for the individual (column 2) and for other group members (column 3). We do not observe statistically significant differences between the various specifications, though in columns 2-3 the indirect effects are slightly smaller. Overall, these results show large indirect effects of the policy commuting actual sentences into expected sentences. The reduction in recidivism by an additional month in the residual sentence is about 1.5 percent if we consider the direct response of individuals to the policy. Considering the results in columns 2-3, we have a 1.3 percent reduction in recidivism that is caused by indirect effects. Overall, the reduction in recidivism from an increase of one month for all individuals in a group is about 2.8 percent.

If the average residual sentence is orthogonal to group characteristics then it should be orthogonal also to prison characteristics, which can have an effect on recidivism after release (Chen and Shapiro, 2007 and Drago, Galbiati and Vertova 2010). Although the potential non-random selection of groups of inmates into prisons is not an issue, we can include in our specification prison fixed effects. These fixed effects control for any non-random selection of inmates into prison and for any fixed differences of prison affecting recidivism rates. Results from this specification are reported in column 4 and suggest that the effect of the average residual sentence remains essentially unchanged.

In model (1) we assume that peers' residual sentence affects individual recidivism of different potential criminals in the same way. In the presence of heterogeneous treatment effects, if peers' residual sentence is orthogonal to treatment response heterogeneity, the estimated coefficient β_4 from model (1) with no heterogeneity provides a consistent estimate of the average treatment effect. The assumption that the peers' residual sentence is orthogonal to treatment response heterogeneity is supported by the fact that this peers' residual sentence is orthogonal to observables, as is shown in Table II. In Table IV we report the results of more flexible linear probability models allowing the effect of peers' residual sentence to vary with individual original sentence, individual residual sentence, nationality (Italian or foreign), type of offence before release (drug and property offence), and size of the group. Each row in the table represents a different model in which we include the full set of controls. In column 1 we report the coefficient on the average residual sentence and in column 2 the coefficient on the interaction term between the average residual sentence and the variable identified in the row heading. It appears from column 2 that the data do not support heterogeneity. Most of the interaction terms are close to zero and are never precisely estimated. There is some indication that the effect of the average residual sentence is different for Italians, although the interaction term is not statistically significant at conventional levels. The effect of the average residual sentence does not change with the size of the group. In the last four rows of Table IV, we interact the average residual sentence with four dummies corresponding to the four quartiles of the size of the group in our sample, but also in this case we do not find any evidence supporting heterogeneity.

C. How results change when we vary the reference groups

In this section we explore the boundaries of the indirect effect estimated above by changing the definition of the reference group adopted so far. This analysis also gives empirical support for the definition of the reference group adopted. As a first step, we investigate how the results change if we redefine the reference groups as the groups of those serving sentences in the same facility but letting the nationality vary randomly. We proceed with a falsification test as follows. Recall that we have 129 nationalities of foreign inmates and 20 Italian regions of residence for Italian inmates. Within each reference group defined by prison and nationality (and region of residence for Italians) we randomly assign a number between 1 and 129 for foreign and Italian inmates. This number reflects the new identification number of the "false" nationality. Then, we again create the reference groups defined by prison and (false) nationality. With this procedure we obtain randomly generated groups of inmates who served their sentence in the same prison. However, now we have people from different nationalities belonging to the same group. In particular, we allow Italians and foreigners to belong to the same group. In column 1 of Table V we report the results from the specification reported in column 3 of Table III. The number of observations is reduced because with this procedure we have many more groups composed of only one inmate.[15] The coefficient on the average residual sentence is now -0.000001 and is not statistically significant.

As a second step, for each group of Italians we randomly generate a number between 1 and 20 (the regions of residence). We do the same with foreign inmates, randomly generating a number between 1 and 129. Then, we again create the reference groups defined by the same prison and

[15] We also experimented by randomly assigning less than 129 nationalities, thereby reducing the number of groups composed of only one individual, obtaining very similar results.

nationality for foreigners and prison and region of residence for Italians. With this procedure we obtain groups of inmates who served their sentence in the same prison. However, now we have Italians that belong to the same group even though they are not from the same region and groups of foreign inmates that belong to the same group even though they are from different nationalities. Unlike the previous falsification test, we allow Italians to belong only to groups of Italians (although from different regions) and foreigners to belong only to groups of foreigners (although from different nationalities). In column 2 of Table V we report the results from the specification reported in column 3 of Table III. The coefficient on the average residual sentence is now -0.0003, with a t-statistic of -0.77. In column 3 of Table V, we do the same exercise randomly assigning 20 nationalities instead of 129, thereby increasing the number of observations and reducing the number of groups. Neither in this case do we find evidence of indirect effects.

In another falsification test we try to focus on the role of nationality by letting the prison where inmates served their original sentences vary randomly. For each group of inmates we randomly generate a number between 1 and 199 (the number of prisons in our sample). Then we group inmates on the basis of their real nationality or region of residence for Italians and on the basis of this randomly generated prison identification number. In this way, we have inmates grouped together with inmates from the same nationality and region of residence but who served their sentences in different prisons. Column 4 of Table V reports the results. The coefficient on average residual sentence is -0.00011 with a t-statistic of -0.20. This further test supports our initial definition of the peer group by showing that interaction amongst people of the same nationality is much stronger between those who spent their original sentences in the same facility.

D. Understanding the effects of the average residual sentence

If the date of incarceration (which determines the residual sentence) itself provides a good candidate for the set of variables defining reference groups, the interpretation of our results requires a further effort. In this case it is difficult to understand if the effect of the average residual sentence comes from the average incentive (expected sentences) at the group level or from the fact that inmates in the same group served more or less time together. In other words, it is difficult to tell apart the effect of incentives on one's peers from the effect of the time served together in the correctional facility. In fact, 1 month of residual sentence corresponds to an additional month in the expected sentence and 1 month less time served in prison.[16] Given this, inmates having peers with longer residual sentences also have peers with whom they served less time. Thus, the observed negative effect on individual recidivism can be due to the fact that an inmate served less time with their peers. As Bayer et al. (2009) show, inmates in prison build criminal capital and this mechanism operates through social interactions. The other interpretation is that a longer average expected sentence of one's peers has a negative effect on one's recidivism because peers face higher costs from committing a crime. In this interpretation, because there are peer effects in crime after release, longer expected sentences of one's peers reduce one's criminal behaviour. Note that in both interpretations, we still consistently identify the indirect effects of a policy commuting actual sentences into expected sentences. Nevertheless, it is important to understand the source of these indirect effects.

[16] See Nagin et al. (2009) for an in-depth discussion of the effects of the prison experience and time spent in correctional facilities.

To understand whether the effect documented above is in fact due to longer or shorter interactions in prison or to incentives after release, we proceed as follows. We construct reference groups as before (prison and nationality or region if Italians) but we add another variable defining groups. To fix the time of interaction in prison, we define a reference group as inmates released from the same prison, from the same national group (or region if Italians) and who entered prison in the same month. With this procedure, we obtain groups with a smaller average size. In particular, with this procedure we have many more groups (2888) composed of many fewer individuals (about 3 on average), with a total number of observations of less than 10,000. Indeed, in each prison facility, there are not many individuals of the same nationality entering on the same date.

Differences in individual residual sentence within a group now come only from differences in individual original sentences. Because we fix the time served in each group, we cannot run a regression like model (1). In this case, individual residual sentence, individual original sentence, average original sentence and average residual sentence would be collinear (as in column 1 in Table I). To understand the effect of the average residual sentence, we adopt the following strategy. Excluding the average original sentence, the coefficient on the average residual sentence captures the joint effect of average residual sentence and average original sentence (which is excluded). Note that longer average residual sentences are associated with longer average original sentences. Therefore, if we still find a negative coefficient on the average residual sentence, this negative coefficient should be a lower bound estimate of the peers' residual sentences. The logic here is that the original sentence should capture the dangerousness of an inmate. Hence, if the peers' average original sentence has any effect on individual recidivism, this effect should be positive. To be clear, consider the following example. Two individuals from Morocco, A and B, belong to the same group because they were released from a given prison in Rome and entered prison in the same month, exactly 40 months before release in August 2006. Individual A had an original sentence of 50 months, hence he has a residual sentence on release of 10, while individual B, having an original sentence of 70 months, has a residual sentence on release of 30. In running the regression model (1) excluding the average original sentence (excluding the individual himself), we estimate the effect on A's recidivism of the residual sentence of B, controlling for A's original sentence and residual sentence (50 and 10 months, respectively).[17] It is clear that the residual sentence of B also incorporates information about a longer original sentence of B. However, if we find that the residual sentence of B (30 months) has a negative effect on A's recidivism, we expect that this negative coefficient comes from the residual sentence rather than from his original sentence. If anything, B was convicted to a longer sentence and he should positively influence A's recidivism.

Table VI illustrates the results. We observe a negative coefficient on the average residual sentence between -0.0008 and -0.0010. This is lower than the coefficient in Table III but it still reveals a sizeable effect of the average residual sentence compared to the individual residual sentence. Because reference groups are constructed in a way that peers served the same amount of time in prison, the effect on the key variable cannot be attributed to the fact that peers with longer residual sentences served less time in prison with any one inmate. The results in this table suggest that the effect of the average residual sentence also develops through peers' incentives after release. While we cannot exclude that interactions in prison play a role in the determination of the main results (Table III), Table VI indicates that interactions in prison (time served) do not

[17] Clearly, in running such a model we also estimate the effect on B's recidivism of the residual sentence of A.

entirely drive these results and that a substantial part of the effect develops through peers' incentives.

E. Interpreting the indirect effects as equilibrium effects: the social multiplier of crime

In this section we ask what the equilibrium effects of a policy manipulating incentives to recidivate in a way similar to the policy under exam are. Regressing average recidivism on average residual sentence (without excluding individual *i*), we find a coefficient on the average residual sentence that is double the coefficient on the individual residual sentence from the individual recidivism regression in which we regress individual recidivism on the individual residual sentence. Under the assumption that the average residual sentence of an individual's peers influences his recidivism only through the effect that the average residual sentence has on his peers' recidivism, this implies a social multiplier of recidivism of about 2: an exogenous shock decreasing individual recidivism by 1 percent implies a 2 percent reduction in aggregate recidivism in equilibrium (Glaeser et al. 2003).[18] It is important to note that whether the average residual sentence of one's peers affects one's behaviour through their expected sentence or their time served (or both) does not matter for this exercise. The fundamental assumption is that the average residual sentence in a group does not affect individual behaviour through channels other than group behaviour. We would obtain the same social multiplier from a regression of individual recidivism on average recidivism (excluding individual *i*) and instrumenting the latter with the average residual sentence (excluding individual *i*). Specifically, from such a regression we obtain a coefficient on average recidivism of about 0.5 which implies a social multiplier of 2 (Glaeser et al. 2003).

[18] The same results come straight from Table III. If we could manipulate every inmate's sentence by commuting one month of actual sentence into one month of expected sentence we would obtain in equilibrium a result double the direct effect.

While we cannot compare this result with other findings in the literature on crime, we observe that a social multiplier larger than 2 is found in the literature in other contexts. For example, in similar design to our paper, Cattaneo and Lalive (2009) estimate from a cash subsidy encouraging schooling attendance a social multiplier of 2 in schooling decisions; Glaeser et al. (2003) report a social multiplier of about 2 in social group membership among students in Darthmouth College dorms and Maurin and Moschion (2009) report a strong social multiplier too in mothers' labour supply by exploiting exogenous variation in the sex of siblings.

Taken together, these results suggest that crime is an inherently social activity. Interdependencies between subjects are such that in order to correctly evaluate crime control policies we should take into account indirect effects and consider that the direct effects of the policies will be amplified by the behavioural responses of peers and neighbours, even when they are not necessarily directly affected by the policy itself.

VII. Concluding Remarks

In this paper we have exploited a unique quasi-experimental dataset to document large externalities of a policy that manipulates individual incentives to reoffend by commuting inmates’ actual residual sentences into expected sentences. Our identification strategy is based on a unique feature of the collective clemency bill passed by the Italian parliament in July 2006. As we have shown in the paper, the collective clemency bill commuted actual sentences into future expected sentences for about 25,000 former inmates – 40 percent of the prison population in Italy as of July 2006. As at the date of release inmates had different remaining sentences to serve, the institutional design implies an exogenous variation in prison sentences at the individual level. Our estimates suggest that the indirect effect of average incentives to peers not to commit a crime is at least as important as the direct effect of individual incentives to reoffend. This result has important implications for the evaluation of policies affecting criminal behaviour. As Glaeser et al. (2003) argue, in the presence of social interactions, the use of aggregate data to estimate aggregate relationships will overstate individual elasticities. On the other hand, the use of individual-level data will lead to an incorrect evaluation of the effect of any policy if we ignore potential indirect effects deriving from social interactions. The main result in this paper also has implications for the design of randomized controlled experiments in the contexts of illegal behaviour. The choice of the unit of treatment to randomize is crucial and we should consider social interactions if we want to perform a correct evaluation of a programme.

After documenting the large spillover effects of the policy that affects incentives to reoffend, we have exploited the source of exogenous variation in individual behaviour to further investigate the contagious effect of crime (i.e. if and how the criminal behaviour of a member affects the behaviour of other group members). Under the hypothesis that social interactions are the channel of the effect of a variation in incentives to peers on an individual’s criminal behaviour, we have estimated a social multiplier in reoffending behaviour between former Italian inmates of about 2. The credibility of this estimate and the main results rely on the large-scale nature of the experiment in which 40 percent of the prison population was released in August 2006.

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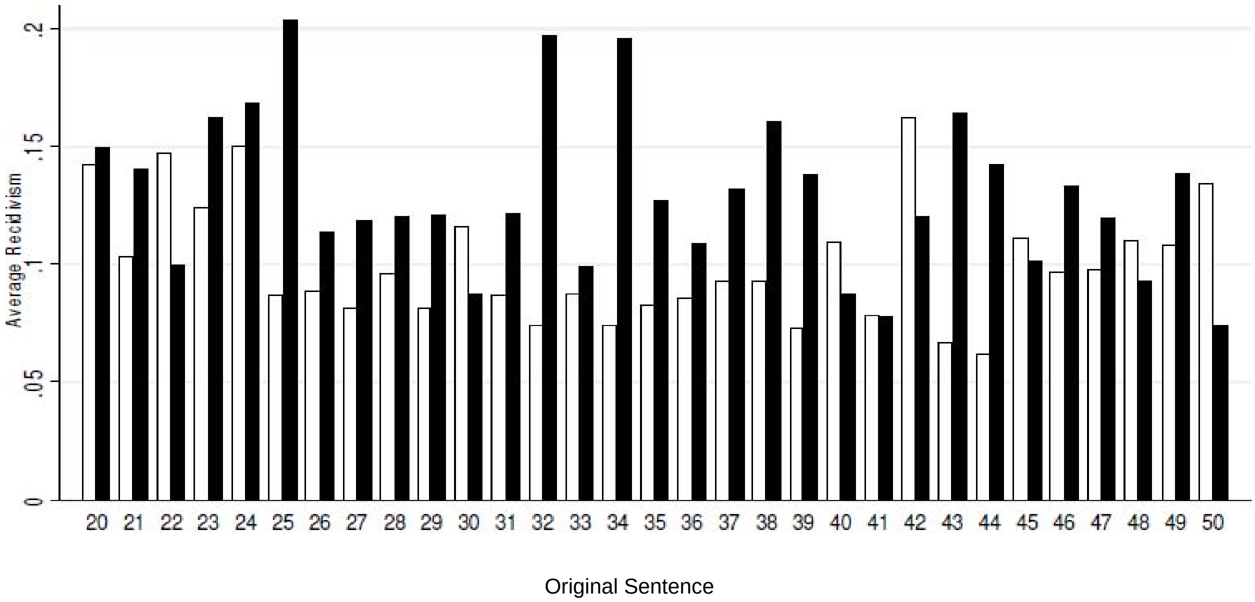
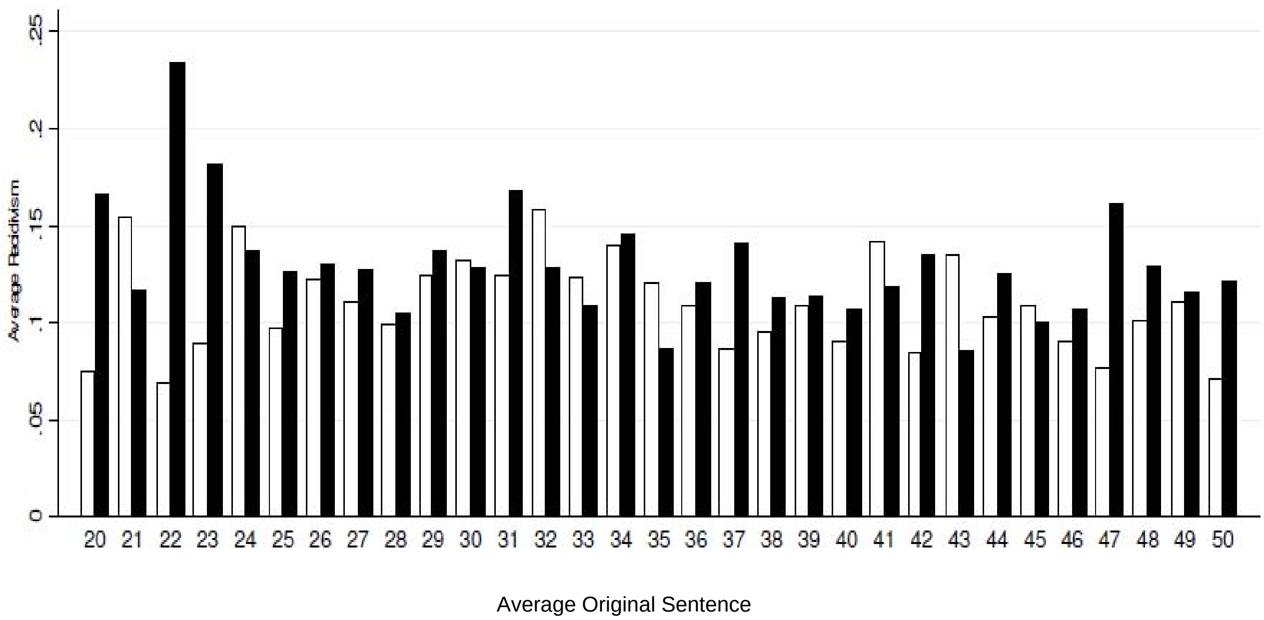
Figure 1. Direct and Indirect Effect of Residual Sentence on Recidivism

1.a



1.b



Notes: Fig.1.a Black bars represent average recidivism for individuals with residual sentences below the median conditional on original sentence, and white bars average recidivism for individuals with residual sentences above the median conditional on original sentence.
Fig. 1.b a) Black bars represent average recidivism for those observations where the average residual sentence of other inmates in the same group (excluding the individual herself) is below the median, conditional on the group average original sentence. White bars refer to recidivism in those observations where the average residual sentence of other inmates in the same group (excluding the individual herself) is below the median, conditional on the group average original sentence.
b) The average original sentence is a continuous variable, hence, for each individual observation, we condition on the closest higher integer.

Table I – Different scenarios for variation in the average residual sentence

	First scenario: A and B enter together with *different* original sentences and are released 3 months later.	Second scenario: A and B enter with the *same* original sentence. A and B are released 6 and 3 months after their entry, respectively.	Third scenario: A and B enter with *different* original sentences. A and B are released 6 and 3 months after their entry, respectively.
A’s original sentence	18	18	18
A’s residual sentence	15	12	12
B’s original sentence	9	18	9
B’s residual sentence	6	15	6
A’s average peer’s original sentence	9	18	9
A’s average peer’s residual sentence	6	15	6
B’s average peer’s original sentence	18	18	18
B’s average peer’s residual sentence	15	12	12

TABLE II

INDIVIDUAL CHARACTERISTICS FOR AVERAGE RESIDUAL SENTENCES ABOVE AND BELOW THE MEDIAN

		Whole Sample	Whole Sample, Individuals belonging to groups with at least 2 individuals	Average Residual Sentence below the Median	Average Residual Sentence above the Median	Difference	OLS	OLS
		(1)	(2)	(3)	(4)	(5)	(6)	(7)
Other Group Members' Average Original Sentence (in months)			38.697	38.336	39.046	-0.709		
			(.121)	(.153)	(.187)	(.739)		
Other Group Members' Average Residual Sentence (in months)			14.474	12.409	16.470	-4.061		
			(.032)	(.035)	(.045)	(.249)		
Individual Recidivism		0.115	0.117	0.123	0.112	-0.115		
		(0.002)	(.002)	(.003)	(.003)	(.005)		
Individual Original Sentence (in months)		38.982	38.681	39.2	38.18	0.990		
		(0.225)	(.235)	(.334)	(.330)	(1.212)		
Individual Residual Sentence (in months)		14.511	14.471	14.834	14.121	0.710		
		(0.070)	(.073)	(.105)	(.102)	(.664)		
Age on Exit		38.764	38.734	38.574	38.889	-0.303	0.070	0.087
		(0.069)	(0.073)	(.109)	(.109)	(.322)	(.033)	(.031)
Married		0.284	0.286	0.294	0.279	0.014	0.000	0.000
		(0.003)	(.003)	(.005)	(.005)	(0.013)	(0.001)	(0.001)
Permanenetly Employed		0.339	0.341	0.337	0.345	-0.008	0.001	0.001
		(0.005)	(.005)	(.007)	(.007)	(.016)	(.002)	(.002)
Percentage of Males		0.954	0.962	0.976	0.948	0.029	-0.003	-0.003
		(0.001)	(.001)	(.002)	(.002)	(.011)	(.001)	(.001)
Share of Italians		0.621	0.657	0.645	0.669	0.023	0.009	0.009
		(0.003)	(.003)	(.005)	(.005)	(.038)	(.003)	(.003)
Area of Residence:								
	North	0.425	0.415	0.416	0.415	-0.001	-0.004	-0.004
		(0.003)	(.004)	(.005)	(.005)	(.046)	(.003)	(.003)
	Center	0.185	0.179	0.147	0.21	-0.064	0.004	0.004
		(0.003)	(.003)	(.004)	(.004)	(.042)	(.003)	(.003)
	South	0.378	0.395	0.431	0.36	0.07	0.000	0.000
		(0.003)	(.004)	(.005)	(.005)	(.047)	(.004)	(.004)
Education:								
	Compulsory	0.901	0.909	0.916	0.903	0.017	-0.001	-0.001
		(0.003)	(0.003)	(.004)	(.004)	(.006)	(.001)	(.001)
	High School	0.079	0.074	0.067	0.08	-0.012	0.001	0.001
		(0.002)	(.002)	(.003)	(.003)	(.005)	(.001)	(.001)
	College (Degree or equivalent)	0.009	0.009	0.009	0.009	0	0.000	0.000
		(0.001)	(.001)	(.001)	(.001)	(.001)	(.000)	(.000)
Kind of offence:	Drugs Offences	0.404	0.401	0.403	0.398	0.005	0.004	0.003
		(0.003)	(.004)	(.005)	(.005)	(.014)	(.002)	(.002)
	Crime against Property	0.412	0.417	0.412	0.421	-0.008	-0.001	-0.001
		(0.003)	(.004)	(.005)	(.005)	(.013)	(.001)	(.001)
	Violent Crimes	0.005	0.095	0.098	0.092	0.006	-0.001	-0.001
		(0.000)	(.002)	(.003)	(.003)	(.005)	(.001)	(.001)
	Immigration bill	0.012	0.026	0.027	0.025	0.001	-0.002	-0.001
		(0.001)	(.001)	(.002)	(.002)	(.003)	(.001)	(.001)
	Crime against Public Safety	0.029	0.005	0.004	0.006	-0.002	0.000	0.000
		(0.001)	(.001)	(.001)	(.001)	(.001)	(.000)	(.000)
	Gun Law	0.094	0.012	0.013	0.012	0.001	0.000	0.000
		(0.002)	(.001)	(.001)	(.001)	(.002)	(.000)	(.000)
Total Number of Obsesvations		20950	18872					

Notes: Standard errors in parenthesis. Robust standard errors in columns (5, 6, 7) clustered by group indicator. Column (1) reports summary statistics for the whole sample. Column (2) reports summary statistics for individuals belonging to groups with at least two observations . Columns (3)-(4) report summary statistics for the sample divided in evenly sized groups as follows. For each group of inmates whose peers' (i.e. other group members) average original sentence length falls in the same number of months interval, the median of the average peers' residual sentence is calculated. Column (3) reports summary statistics for those observations where the average peers' residual sentence is below the median for that original sentence length and column (4) reports summary statistics for those observations where the average peers' residual sentence is above the median for that original sentence length. Column (5) reports the point estimates of the differences between the means in columns (3)-(4). Column (6) reports coefficients on average peers' residual sentences from regressions with individual level observables as dependent variables controlling for peers' residual and original sentence. Column (7) reports coefficients on average peers' residual sentences from regressions with individual level observables as dependent variables controlling for peers' original sentence, individual residual sentence and original sentences .

TABLE III
BASELINE RESULTS

	(1)	(2)	(3)	(4)
Individual residual sentence	-0.0016	-0.0018	-0.0018	-0.0018
	(-6.02)	(-6.36)	(-6.37)	(-6.26)
Average peers' residual sentence	-0.0020	-0.0015	-0.0016	-0.0015
	(-3.15)	(-2.45)	(-2.52)	(-2.22)
Individual original sentence	-0.0001	0.0003	0.0003	0.0003
	(-1.20)	(3.27)	(3.29)	(3.27)
Average peers' original sentence	0.0001	-0.0000	-0.0001	-0.0001
	(0.33)	(-0.13)	(-0.48)	(-0.59)
Individual characteristics	NO	YES	YES	YES
Average peers characteristics	NO	NO	YES	YES
Idividual type of crime	NO	YES	YES	YES
Peers' averages of type of crime	NO	NO	YES	YES
Prison Fixed Effects	NO	NO	NO	YES
R-squared	0.004	0.021	0.024	0.038
Number of groups	1670	1663	1572	1572
Observations	18836	17399	17296	17296

Notes.- OLS estimates are reported. The dependent variable is equal to one if the individual returned to prison after release and zero otherwise. Robust t-statistics in parenthesis. Standard errors are clustered at the group level. Individual variables include education levels, age at the date of release, a dummy indicating marital status, nationality, and employment condition before imprisonment. Values of average groups' characterisitics are constructed starting from the individual values of the same variables.

TABLE IV
INTERACTION TERMS

	Average residual sentence	Interaction of average residual sentence with row variable
	(1)	(2)
No interaction	-0.00165	-
	(-2.52)	
Individual original sentence	-0.00180	0.00000
	(-1.91)	(0.23)
Individual residual sentence	-0.00143	-0.00001
	(-1.33)	(-0.31)
Dummy on Italians	-0.00222	0.00147
	(-2.75)	(1.25)
Drug offence	-0.00135	-0.00072
	(-1.57)	(-0.64)
Crime against property	-0.00187	-0.00055
	(-2.48)	(-0.46)
Size of the group	-0.00154	-0.00000
	(-2.24)	(-0.75)
Dummy if group<=6	-0.00229	0.00072
	(-2.58)	(1.11)
Dummy if 6<group<=27	-0.00229	0.00041
	(-2.58)	(0.68)
Dummy if 27<group<=80	-0.00229	0.00027
	(-2.58)	(0.51)
Dummy if group>80	-0.00229	-
	(-2.58)	

Notes.- OLS estimates are reported. The dependent variable is equal to one if the individual returned to prison after release and zero otherwise. Robust t-statistics in parenthesis. Standard errors clustered at the group level. Each row but the last four rows represents a separate model, with average residual sentence included as a main effect (coefficient reported in column 1) and interacted with the variable indicated in the row heading (coefficient reported in column 2). All models include the same controls used in column 3 of Table III.

TABLE V
FALSIFICATION TESTS

	Nationalities (129) randomly assigned (1)	Regions (20) and nationalities (129) randomly assigned (2)	Regions (20) and nationalities (20) randomly assigned (3)	Prisons (199) randomly assigned (4)
Average peers' residual sentence	0.0000	-0.0003	-0.0000	-0.0001
	(0.10)	(-0.77)	(-0.14)	(-0.20)
R-squared	0.024	0.023	0.022	0.022
Number of groups	3184	3056	2430	2709
Observations	12659	13928	17274	16674

Notes.- OLS estimates are reported. The dependent variable is equal to one if the individual returned to prison after release and zero otherwise. Robust t-statistics in parenthesis. Standard errors are robust. The specifications adopted are the same as in column 3 of Table III. The reference groups of these falsification tests are constructed as follows. See the text for the procedure to construct the reference groups

TABLE VI

BASELINE RESULTS WITH REFERENCE GROUPS DEFINED ALSO BY THE EXACT MONTH OF ENTRY IN PRISON

	(1)	(2)	(3)	(4)
Individual residual sentence	-0.0018	-0.0018	-0.0017	-0.0017
	(-4.34)	(-4.20)	(-3.91)	(-3.78)
Average peers' residual sentence	-0.0008	-0.0010	-0.0010	-0.0008
	(-1.86)	(-2.19)	(-2.06)	(-1.60)
Individual original sentence	0.0003	0.0005	0.0003	0.0005
	(1.37)	(2.14)	(1.58)	(1.98)
Individual characteristics	NO	YES	YES	YES
Average peers characteristics	NO	NO	YES	YES
Idividual type of crime	NO	YES	YES	YES
Peers' averages of type of crime	NO	NO	YES	YES
Prison Fixed Effects	NO	NO	NO	YES
R-squared	0.002	0.021	0.021	0.043
Number of groups	2888	2849	2652	2652
Observations	9401	8598	8401	8401

Notes.- OLS estimates are reported. The dependent variable is equal to one if the individual returned to prison after release and zero otherwise. Robust t-statistics in parenthesis. Standard errors are clustered at the group level, where groups are defined by nationality (region if Italians), prison and the exact month of entry into prison. The specifications adopted are the same as in column 3 of Table III but here the average original sentence is excluded from the regressions. Individual variables include education levels, age at the date of release, a dummy indicating marital status, nationality, and employment condition before imprisonment. Values of average groups' characterisitics are constructed starting from the individual values of the same variables.