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The Political Cost of Being Soft on Crime: Evidence from a Natural Experiment*

Francesco Drago† Roberto Galbiati‡ Francesco Sobbrio§

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Abstract

This study analyses voters' response to criminal justice policies by exploiting a natural experiment. The 2006 Italian Collective Pardon Bill, designed and promoted by the incumbent center-left (CL) coalition, unexpectedly released about one-third of the prison population, creating idiosyncratic incentives to recidivate across pardoned individuals. Municipalities where resident pardoned individuals had a higher incentive to recidivate experienced a higher recidivism rate. We show that in those municipalities voters "punished" the CL coalition in the 2008 parliamentary elections. A one standard deviation increase in the incentive to recidivate – corresponding to an increase of recidivism of 15.9 percent – led to a 3.06 percent increase in the margin of victory of the center-right (CR) coalition in the post-pardon national elections (2008) relative to the last election before the pardon (2006). We also provide evidence of newspapers being more likely to report crime news involving pardoned individuals and of voters hardening their views on the incumbent national government's ability to control crime. Our findings indicate that voters keep politicians accountable by conditioning their vote on the observed effects of public policies.

Keywords: Accountability, Retrospective Voting, Natural Experiment, Crime, Recidivism, Media.
JEL Classification: D72, K42.

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1 Introduction

Crime is perceived as a crucial social issue in most Western countries. In the Eurobarometer survey, for instance, crime ranks among the top five most important problems in several European countries (Mastrorocco and Minale, 2018).[1] Accordingly, there is a widespread belief that criminal justice policies have a significant impact on voting behavior.[2] In particular, elected officials seem to believe that being soft on crime does not pay off (Levitt, 1997; Huber and Gordon, 2004; Murakawa, 2014; Lim *et al.*, 2015). Nevertheless, despite the importance of this issue for potential voters and the observed behavior of elected officials, existing studies on the link between crime control policies and voters' behavior are mostly correlational and provide mixed evidence (Hall 2001; Krieger 2011). Thus, we know very little about whether voters respond to crime policies.

This paper exploits an original case study, based on a natural experiment, to examine how voters respond to criminal justice policies. To the best of our knowledge, this is the first study to provide evidence concerning voters' response to crime control policies in a quasi-experimental setting. More generally, our study is informative on how voters assess policymakers in the presence of incomplete information, a broader and fundamental question in political economy (Barro, 1973; Ferejohn, 1986; Fearon, 1999; Persson and Tabellini, 2002; Besley, 2006; Ashworth, 2012; Duggan and Martinelli, 2017).

In July 2006, the Italian Parliament unexpectedly passed a collective pardon that sanctioned the release of 37 percent of the total prison population. The pardon was (mainly) put forward by the parties of center-left (CL) coalition government elected in the April 2006. According to the pardon, all the inmates with a residual sentence of less than three years were released in August 2006. The policy was designed in such a way that released prisoners recidivating within a five-year period would serve an additional sentence equal to their residual sentence at the time of their release. Therefore, this provision of the bill had a bearing on pardoned individuals' incentives to recommit crime after their release from prison since it commuted each month from the (residual)

[1] Concerns regarding crime are shared by citizens on both sides of the Atlantic. According to Krisberg and Marchionna (2006), 74% of US citizens are somewhat or very concerned about the problem of crime in their communities, and 79% are concerned or fearful about the annual release of 700,000 prisoners. See also Enns (2014) for evidence on public support for tough crime policies.

[2] According to press accounts, if in 1994 Bill Clinton "hadn't embraced a 'tough on crime agenda' [he] might never have become—or remained—president"(*The Atlantic*, May, 2015). Similarly, Michael Dukakis' defeat in the 1988 US presidential elections is commonly attributed to his "soft on crime" record as Governor of Massachusetts (*The New York Times*, July 5, 1988 and the Willie Horton ad).

original sentence in an extra month of an eventual sentence for future crimes.

Drago *et al.* (2009) exploit this empirical design to show that individuals' propensity to recommit a crime is influenced by their remaining sentence at the date of release: a longer residual sentence strongly discourages future recidivism. The quasi-experimental nature of the variation is based on the fact that—conditional on the original sentence—the incentive to recidivate (henceforth ITR) determined by the residual sentence at the date of release in August 2006, depends only on the date of entry into prison which is plausibly random (Drago *et al.*, 2009).[3]

In this paper, we aggregate this individual heterogeneity in the residual sentences at the municipality level. As we have information on the municipality of residence for the former inmates that were released, we can rank municipalities according to their inmates' ITR (the higher the residual sentence the lower the ITR). We study whether voters punished the governing CL coalition (that designed and promoted the pardon) in the 2008 national elections, by exploiting municipal variation in the residual sentence and the resulting different effects of the policy at the local level. Restricting the sample to municipalities with at least one inmate released, 75 percent have less than four pardoned individuals and 50 percent have less than two, with bigger cities having a much larger number of pardoned individuals. Hence, we observe that there is sufficient meaningful variation in the ITR—which translates into variation in recidivism rates—across the more than 2,000 municipalities where released inmates reside. Overall, while voters were not aware of the ITR of pardoned individuals as the average residual sentence was not in the public sphere, they were exposed to different recidivism rates depending on the average residual sentence of released inmates residing in their municipality.

In estimating the voters' response to the local effects of the policy, the identifying assumption is that the ITR in a municipality is exogenous once we control for the average original sentence of pardoned inmates. In practice, once controlling for the length of the original sentence, the date of admission to prison (which determines the residual sentence at the date of release) should not be systematically correlated with voters' behavior or municipal level characteristics. If this assumption holds—as several placebo and balancing tests suggest—potential spatially correlated boom and busts cycles in prison entry should not be correlated with the cross-sectional municipal

[3] As an example, consider two inmates who committed a crime in 2002 and they were convicted with a sentence of five years. In August 2006, as they have a residual sentence of less than three years, they are released. Any difference in their residual sentence (that will sum up to an eventual sentence for a future crime) depends on the different date of admission in prison in 2002.

variation or pre-trends in voters' behavior.[4] Hence, this design allows us to exploit a margin of variation in the effects of the collective pardon which voters might directly relate to the public policy.

We show that a higher average ITR translates into a worse policy outcome at the municipal level (a higher recidivism and crime rate) and into a harsher electoral "punishment" of the CL coalition. In terms of voters' electoral response, the estimated political cost of being soft on crime is not negligible: a one standard deviation increase in the ITR at the municipal level— corresponding to an increase of recidivism of 15.9 percent—resulted in a 3.06 percent increase in the margin of victory of the center-right (CR) coalition in the post-pardon national elections of 2008 relative to the pre-pardon elections of 2006.

We also investigate the underlying mechanism linking public policies and electoral behavior (voters' information and beliefs). First, we assemble an original dataset of local news stories that refer to acts of crime committed by former inmates' and make explicit reference to the fact that they were released because of the pardon. Second, we match local news to the municipalities to create a municipal-level measure of exposure to the effects of the pardon. Consistent with the idea that ITR is a driver of information on the local effects of the policy, we show that newspapers were more likely to report news on recidivism by pardoned individuals in municipalities where their ITR was higher. This provides evidence that voters were most likely to receive a negative signal on the realized effects of the policy in such municipalities.

The higher recidivism rates induced by a higher ITR not only led to more (negative) news on the effects of the policy but also to an updating of voters' beliefs on the valence of the center-left coalition. By collecting data from two independent surveys, we show that voters were more likely to report a negative assessment on the CL coalition's ability to control crime in municipalities where the ITR was higher. Furthermore, in these municipalities voters held more negative views on the overall competence of the main CL party, the main political supporter of the pardon law.

To further investigate the link between voting behavior and the local effects of the pardon, we present evidence suggesting that the voters' inference and electoral response were specific to the policy at stake. We exploit the circumstance that some of the MPs belonging to the CR coalition

[4] In fact, we show that the ITR at the municipal level is not correlated with voters' behavior before the bill or with observable municipality characteristics, which is consistent with the identifying assumption. Importantly, the ITR is not correlated with measures of crime waves before the pardon at the municipal level.

voted in favor of the pardon in 2006. If voters are able to map the effects of the pardon onto MPs' political behavior, their response to the negative effects of the policy should be less favorable for the CR coalition in electoral districts where the percentage of its candidates who voted for the pardon was higher. We show that this was the case. In districts where a higher percentage of CR candidates voted in favor of the pardon, a higher ITR translated into a smaller increase in the CR's winning margin in the 2008 elections (in comparison to the 2006 elections).

Finally, we document that a higher ITR was *not* associated with individuals being more likely to perceive crime as the most important issue in Italy or in their local area. This suggests that individuals correctly associated the collective clemency bill with the recidivism of pardoned individuals and not with crime in general. In addition, we show that variation in the ITR is uncorrelated with voters' behavior in European elections. That is, the local effects of the collective pardon did not shift voters' ideological preferences more broadly or lead them to "punish" the CL coalition in domains where its competence in crime policies was not relevant.

Overall, besides providing evidence on voters' response to crime policies, our results are informative for the academic debate on voters' sophistication (Wolfers, 2002; Kendall *et al.*, 2014; Dal Bo *et al.*, 2018; Hopkins and Pettingill, 2018). Our findings suggest that voters respond to the observed effects of a public policy, both in terms of beliefs and behavior, in a way that is broadly consistent with retrospective voting models of electoral accountability.[5]

The paper is structured as follows. Section 2 provides background information regarding the 2006 Italian Collective Pardon Bill and its political relevance in the 2008 elections. Section 3 presents the data. Section 4 discusses the empirical strategy. Section 5 reports the effects of the policy on the recidivism of pardoned individuals at the municipal level and presents the main results on voters' electoral response to the effects of the Collective Pardon Bill. Section 6 presents evidence shedding light on the mechanism behind the main results. Section 7 discusses different potential interpretations of the empirical results. Section 8 concludes. The online Appendix presents a retrospective voting model providing a theoretical framework for the empirical analysis. In addition, it describes in details the database on crime-related news and contains additional tables and figures which are also discussed in the main text.

[5]See also Ansolabehere *et al.* (2014) for evidence that state unemployment in the US robustly correlates with evaluations of national economic conditions and presidential support.

2 The 2006 Italian Collective Pardon Bill

Our empirical analysis exploits variations in the incentives to commit a crime that resulted from the provisions of the collective pardon law approved by the Italian Parliament in July 2006 (Law 241/2006).[6] The policy was designed and promoted mainly by the parties of CL government coalition that was elected in April 2006. It is important to remark that the policy was not part of the political platform of the CL coalition during the 2006 electoral campaign. The pardon was approved by both chambers of parliament with a majority of two-thirds of the votes regarding each article of the law as required by the Italian Constitution for the implementation of an amnesty or a collective pardon (Sec. II, Art. 79). Hence, some members of the center-right (CR) coalition voted for the pardon bill—a circumstance that we exploit in the empirical section to test the accountability mechanism.[7] The CL coalition government designed this law and proposed it to the Italian parliament as one of its first policy measures because of a prison overcrowding emergency—a problem that is also faced by many goverments across the world, including California and France, which both recently enacted specific policy interventions to tackle this problem.[8] In the 1990s the incarceration rate in Italy increased steadily while the capacity of prisons remained substantially stable. Before the collective pardon, the average overcrowding index was 131 inmates to every 100 prison places. As a consequence, since the end of 90s the Catholic Church, leftist parties and civic associations have campaigned for laws that would alleviate the inhuman and degrading treatment of prisoners in overcrowded jails.

The bill was approved on July 31, 2006 with immediate effects. The main provisions of the Collective Pardon Bill were the following. It granted a three-years reduction in the length of detention for prisoners who were serving their sentence for a crime committed before May 2, 2006. The exclusion of crimes committed after May 2 was announced at the beginning of the parliamentary debate on the pardon bill, thus preventing strategic behaviors of potential criminals during the months leading up to the approval of the law. The sentence reduction applied to a large number of offenses, including property crime, violent crimes, drug trafficking and white-

[6] Drago *et al.* (2009) describe in detail the institutional background of the Italian criminal law system and the process that led to the approval of the bill.

[7] Some MPs affiliated to*Forza Italia*—one of the parties within the CR coalition—also signed one of the pardon proposals that was debated in parliament. We thank an anonymous referee for bringing this to our attention.

[8] See Lofstrom and Raphael (2013) for the case of California and Maurin and Ouss (2009) for the case of France.

collar crimes.[9] Thus, an inmate convicted for a crime committed before May 2, 2006 was eligible for immediate release from prison as long as his residual sentence was less than three years. As a result, the prison population dropped from a total of 60,710 individuals on July 31, 2006 to 38,847 on August, 2006.

However, the law did not erase the offense or the punishment: the sentence reduction was conditional on the inmate's post-release behavior. All those that benefited from the incarceration term reduction who recommitted a crime within five years, lost their right to pardon. In the five-year period following their release from prison, former inmates granted collective pardon faced an *additional* expected sentence equal to the residual sentence pardoned by the bill. Thus, as far as the residual pardoned sentence is as good as random, the conditional sentence suspension provided a random incentive to commit crime from the perspective of former inmates. The following example helps to clarify how individual incentives to re-offend are randomized by the law. Consider two criminals convicted of the same crime. Both inmates had a residual sentence of less than three years on July 31, 2006. As a consequence of the new law they are both released from prison on August, 2006. Suppose that the first individual had a residual sentence of one year and that the second individual entered in prison one year after the first individual. Hence, the second inmate had a pardoned residual sentence of two years. Over the following five years, for any crime category, they face a difference in expected sentence of one year. For example, if they commit a burglary that carries a sentence of three years, the first individual would be sentenced to four years in prison (there years for the burglary plus one year from the pardoned residual sentence), while the second individual would be sentenced to five years (three years plus two years of residual sentence). It is worth noting that the difference in the timing of incarceration comes both from whether or not a criminal entered prison directly upon the day of apprehension and on when the crime was committed. We address potential threats to identification in Section 4.

2.1 The Collective Pardon and the 2008 Election

The July 2006 Collective Pardon Bill put forward by the CL coalition proved to be a very prominent issue for Italian voters up to the next (early) national elections in April 2008. Figure 1 summarizes

[9] Mafia related crimes, children abuse and terrorism were excluded from the pardon.

the timing of elections and of the Collective Pardon Bill.[10]

The high relevance of this issue was the combined result of three main factors. First, the sharp drop in the incarceration rate created by this policy, as shown by Figure 2, was followed by an increase in the overall number of crimes committed, as shown by Figure 3 (a 12.4% increase in crimes between June and December 2006 compared with a 0.35% increase in the previous semester and with a 1.78% increase in the same semester of the previous year). Second, as illustrated by Table 1, the majority (51.3%) of the Italian population perceived the Collective Pardon Bill to had induced a large increase in crime. An additional 27% stated that they perceived the pardon to have created a positive, yet limited, increase in crime. At the same time, consistently with the rationale behind our empirical investigation, Table 1 shows a significant heterogeneity in the perceived effects of the pardon across individuals (even conditional on political ideology). Finally, as shown by Figure 4, the space devoted to the coverage of crime by national television channels substantially increased following the rise in crime induced by the collective pardon.[11] In short, the pardon was followed by a substantial increase in crime in the period 2006-2008, the majority of Italian voters perceived such an effect and, last but not least, news media kept the crime issue highly salient up to the April 2008 elections. Overall, the high relevance of the Collective Pardon Bill and of its perceived effects on crime might have been detrimental for the CL coalition for two primary reasons. As already stated, the fact that the CL coalition took the lead in the design of the law and was in government at the time of the policy's approval ensured that the CL was considered responsible for the effects of such a policy.

Secondly, the "crime issue" is typically *owned* by rightist parties, since they are the perceived by voters as the most competent parties in managing it (Petrocik, 1996; Puglisi, 2011). Accordingly, the CR coalition was the one most likely to gain from an increase in the relevance of crime (Belanger and Meguid, 2008; Aragones *et al.*, 2015). We discuss these alternative mechanisms in Section 7.

[10]Notice that, as pointed out Figure 1, the variation in the residual sentence of pardoned individuals exploited in our data comes exclusively from prisoners released in August 2006 (i.e., prisoners with a residual sentence lower or equal to 36 months).

[11]The observed decrease in the number of news on crime between the end of 2007 and June 2008 could be explained by two factors. First, the collapse of the incumbent government in January 2008 and the consequent early April 2008 elections increased the space devoted to political news by news programs crowding out news on other topics (see Eisensee and Strömberg, 2007 for empirical evidence on the crowding-out effects of *news pressure* by newsworthy events). Moreover, the observed drop in the number of news on crime in the first semester of 2008, might also be explained by a sharper decrease in the number of news on crime after the 2008 elections when the CR government took office, i.e., between April and June 2008 (Demos-Unipolis, 2009).

3 Data

The empirical analysis uses data from several different datasets. The first dataset provides information on the characteristics of the prisoners released following the 2006 Collective Pardon Bill. The data contains information on the municipality where each prisoner reside, the length of his residual sentence at the time of release, the length of his original sentence and the type of crime committed. The data contains information on the entire population of individuals pardoned by the bill. However, while the information on the municipality of residence of each Italian prisoner released is informative of his official residence, this is not the case for non-Italian prisoners. That is, the information on the residency of foreign prisoners is not a reliable proxy of their place of residence. Hence, we exclude non-Italian individuals from the sample. Accordingly, to reduce measurement error, we focus only on municipalities where there is at least one Italian pardoned inmates resident in the municipality. As a result, the final dataset on pardoned inmates is composed by 12,355 Italian individuals resident in 2,256 municipalities. That is, around one-third of the municipalities in Italy had at least one Italian pardoned individual resident in the municipality. The summary statistics are reported in Table 2A where we average-out the data on the characteristics of pardoned individuals at the municipality level. Figure 5 illustrates the geographical distribution of the (standardized) average ITR of pardoned individuals at the municipal level. This figure shows a substantial level of variation in the ITR that is not correlated with any regional pattern (for example higher in the south or in the north or in any particular region). In the following analysis, we show that conditional on the average original sentence the variation in the ITR is orthogonal to observable city characteristics. Notice that, if municipalities were to present a very large number of resident pardoned inmates, we would not have enough variation in our key variable. However, this is not the case. The 90 percent of municipalities have less than 9 pardoned individuals resident in the municipality, the 70 percent less than four and the 50 percent less than two.[12]

We then complement this dataset using electoral data on the 2006 and 2008 parliamentary elections by the Italian Minister of Internal Affairs, for all municipalities (even the ones with no pardoned Italian prisoners). This dataset reports information on the votes to political parties in the 2006 and 2008 elections. Both elections were subject to the same proportional electoral law.[13]

[12]Most of the variation in the ITR comes from cities with few released individuals and eligible voters. Figure A1 and A2 show the relationship between the number of voters and the standard deviation in the ITR.

[13]The 2006 and 2008 parliamentary elections employed a proportional system, 26 electoral districts, and a majority

As Table 2B shows, both the CR and CL coalitions lost votes in the 2008 elections (with respect to 2006), with the CR loosing less than the CL. The difference in the margin of victory between 2008 and 2006 was on average 0.07 percentage points. Table 2B summarizes the geographic, socio-economic and demographic characteristics at the municipal level used in the empirical analysis (which are provided by the Italian National Statistical Institute, ISTAT).

In order to analyze municipal-level variations in voters' information on the effects of the collective pardon, we extrapolated data—from the *Factiva* database—regarding news on criminal acts involving pardoned individuals between August 1, 2006 (i.e., when the pardon became effective) and March 30, 2008 (i.e., the 2008 elections). We then matched news with municipalities to create a measure of municipal-level exposure of voters to the effects of the pardon. The online Appendix provides detailed information on the construction of this dataset. In addition, we use survey data from the Italian National Elections Study Survey (ITANES) in order to gather information on voters' issue priority and on their evaluation of the incumbent CL government's policies on crime. We also use survey data from the IPSOS *Polimetro* in order to obtain additional information on voters' issue priority (both nationally and in their local area of residence) and on voters' overall evaluation of the main CL and CR parties.[14] The ITANES dataset is a post-election survey of around 2,800 individuals interviewed in the month after the 2008 elections. The IPSOS dataset is composed by several waves of weekly and monthly interviews (for a total of around 28,000 interviews) in the period April 2008-December 2008. The summary statistics for this data are reported in Table 2B.

4 Empirical Strategy: The Pardon Bill as a Natural Experiment

The empirical strategy exploits a unique feature of the Collective Pardon Bill providing that former inmates re-committing another crime will have to serve the residual sentence at the date of their release (August 2006) in addition to the new sentence. As we explained in the introduction, individuals with lower residual sentences are more likely to recidivate compared to individuals

premium granted to the electoral coalition that obtained the higher share of votes at the national level (for the lower chamber).

[14]ITANES is research project on electoral behavior of the "Istituto Carlo Cattaneo Research Foundation" (www.cattaneo.org). IPSOS is one of the largest public opinion polling companies in Italy (http://www.ipsos.it/).

with higher residual sentences (Drago *et al.*, 2009). Crucially for our study is the source of the variation in the residual sentence that we exploit in order to identify the effect of local variations in recidivism. For the same original sentence, inmates have different residual sentences depending on the date of entry into prison. The empirical analysis exploits the variation at the municipality level by aggregating this individual heterogeneity. In the next section we discuss the source of variation in the municipal ITR and the potential threats to identification.

The analysis uses the following main model to assess the impact of the pardon on voters' electoral response:

$$\Delta y_i = \alpha + \beta_1 ITR_i + \beta_2 original\ sentence_i + \beta_3 \mathcal{I}_i + \beta_4 X_i + \beta_5 Z_i + \epsilon_i \tag{1}$$

where *ITR* (incentive to recidivate) is defined as 36 minus the average residual sentence, where 36 is the maximum pardoned residual sentence according to the design of the bill.[15] The *ITR* variable measures the average incentive to recidivate of pardoned individuals residents in municipality i. It is standardized so that its standard deviation is equal to one. Δy is the variation in the political outcome of interest (the margin of victory of the CR coalition with respect to the CL coalition) between the national elections of 2008 and 2006. The variable *original sentence* indicates the average original sentence of former inmates resident in municipality i. $\mathcal{I}_i$ represents a dummy indicating whether there is at least one pardoned individual resident in the municipality.[16] X_i is a vector of controls at the municipal level that includes the average municipal pre-pardon (2004-2005) crime rate, the average taxable per capita income in 2008 and a set of municipal characteristics in Census year 2001 (see Table 2B). The last set of variables, Z_i, includes the number of pardoned individuals weighted by municipality population per 1,000 inhabitants and all other observable demographic and "criminal" characteristics of former inmates resident in municipality i averaged at the municipal level (i.e., percentage of former inmates that were unemployed, married, with a primary school degree, a secondary school degree and with a university degree; percentage of former inmates convicted for drug crimes, for crimes against property and for violent crimes). For all the municipalities where $\mathcal{I}_i = 0$, that is municipalities with no pardoned individuals, all

[15] According to this specification, an individual with one month of residual sentence upon release will have an ITR equal to 35, whereas an individual with a 35 months residual sentence has an ITR equal to one.

[16] In order to obtain a more homogeneous sample, since all municipalities with at least one pardoned individual resident in the municipality have 500 inhabitants or more, we exclude all municipalities with no pardoned individuals having less than 500 inhabitants.

variables in Z_i as well as the original sentence and the ITR variable are set equal to zero.

In specification (1) the estimated coefficient β_1 measures the impact of a one standard deviation increase in the average ITR (which corresponds to 8.2 fewer months of residual sentence) for former inmates resident in municipality i. The estimation of β_1 is obtained by exploiting the variation in the average residual sentence for all municipalities with at least one pardoned individual. We estimate the impact of ITR by both excluding and including municipalities with no pardoned prisoners (i.e., also those with $\mathcal{I}_i = 0$). The inclusion of these municipalities contribute to the estimation the residual variance of the set of variables X_i that, in turn, it is used to estimate our coefficient of interest.

Finally, while, later on, we show that our key variable of interest (the ITR) predicts recidivism at the municipal level (see Table 5), in the main analysis we adopt a conservative approach and present reduced form estimates instead of using it as an instrument for the crime rate at the local level since the exclusion restriction could fail. In fact, through general equilibrium effects the average residual sentence may impact the overall crime rate (e.g. through congestion effects, social interactions and spillover effects) - as we document later - thereby affecting voters' welfare and influencing electoral outcomes (Drago and Galbiati, 2012). This failure of the exclusion restriction may be exacerbated if the effect of the ITR on the overall crime rate is mediated by the news media and if this has an impact on the electoral outcome.[17] Hence, we consider β_1 as the voters' response to the effects of the policy implemented with the approval of the bill that includes the direct effect on recidivism and indirect effects mediated by the overall crime rate and news media.[18]

4.1 Identification, Balancing Tests and Pre-Trends

Specification (1) is a reduced form model that estimates the effects that the collective pardon had on electoral outcomes. Our key identifying assumption is that conditional on the average original sentence of pardoned inmates, the ITR is orthogonal to unobservable characteristics. The key source of variation is the the date of admission to prison. Indeed, as explained above, conditional on the original sentence, inmates have different residual sentences—and therefore different ITR—

[17] At the same time, for the interested reader, Table A13 in the online appendix reports the IV estimates.

[18] It is important to note that in the presence of inmates from municipality i at risk of recidivism in municipality j, our coefficient should be interpreted as a lower bound of the causal effect of the ITR on electoral outcomes. If the mobility patterns are not correlated with the observed average residual sentence, we can interpret this as a classical measurement error leading to downward biased estimates of the causal effect of the average residual sentence.

because they entered prison in different dates. Admission to prison is determined by the day of apprehension (provided that a court decided to hold them in prison) or by the day in which they were sentenced. At the individual level, the evidence shown in (Drago *et al.*, 2009) is consistent with the assumption that the date of admission into prison is orthogonal with respect to future acts of crime (conditional on the original sentence, the date of admission is as good as random). In our empirical design, since we aggregate the individuals' residual sentences at the municipal level, the question is whether or not the municipal level variation in ITR is endogenous to voting behavior.

The first threat to identification is whether this variation in the residual sentence at the municipal level reflects heterogeneity in the crime waves experienced by municipalities and if this heterogeneity is correlated with voting behavior for reasons that are unrelated the local effects of the policy. If different inmates coming from the same municipality have similar characteristics and entered prison in the same period, it is possible that observable and unobservable inmates' characteristics will be correlated to the ITR. In fact, we do expect this type of pattern if there are complementarities in crime of similar individuals from the same city (Drago and Galbiati, 2012). In this case, we might observe some correlation between inmates' observable characteristics and their residual sentences when we estimate municipal level regressions. This represents a threat to identification if these potential micro-level waves of criminals entering prison in the same period are correlated with voting behavior. In what follows, we address this concern by showing that a large set of municipal level characteristics are orthogonal to the ITR. More importantly, we show that different measures of crime and crime waves before the pardon are uncorrelated with the ITR. Finally, we show the absence of pre-trends, namely the absence of any impact of the ITR on elections held before 2006.

Tables 3A, 3B and 3C present results from the balancing tests. In these tables we regress the ITR on each of the variables X_i, conditional on the average original sentence and the indicator $\mathcal{I}_i$. As expected, the dummy variable $\mathcal{I}_i$ and the original sentence are mechanically correlated with the ITR. The first is positively correlated with the ITR because we set the ITR equal to zero when $\mathcal{I}_i = 0$. The second is negatively correlated with the ITR since, obviously, a longer original sentence is associated with a lower ITR (longer residual sentence). Indeed, the residual sentence is bounded from above and it is always lower than the original sentence. The correlation between some former inmates' characteristics (Table A2 in the online Appendix) demonstrates

the possibility that in some municipalities inmates with similar characteristics and with similar original sentences enter into prison in the same period.[19] However, none of the geographical, socio-economic or demographic variables in X_i are significantly correlated with the ITR (Tables 3A, 3A and 3C). In particular, it is worth remarking that the average crime rate in 2004 and 2005 and the number of pardoned individuals per 1,000 residents are both orthogonal to the ITR. In Table A3 in the online Appendix we show the results our balancing exercise for the crime rate in 2004 and in 2005 and for the rate of growth of crime between 2004 and 2005 and note that potential crime waves at the municipal level (reflected in the growth of crime or its levels) are not correlated with the ITR. Taken together, these results are consistent with our identifying assumption.[20]

As for the presence of pre-trends, in Table 4 we run a "placebo" specification where we use the main dependent variable (the sdifferences between the winning margins of the CR coalition) and the votes per eligible voters of the CR and CL coalitions in the 2006 elections with respect to the 2001 elections, at the municipal level. If the ITR were to pick-up some existing trends in voters' behavior, Table 4 should show a significant impact of this variable on pre-2008 voting patterns. Instead, the results are consistent with the average ITR of pardoned individuals released in August 2006 being orthogonal to any pre-trend in the votes to political coalitions in previous elections. The point estimates are not only imprecisely estimated but, more importantly, are very low in magnitude (by comparison to the estimates from our main regression, see Table 6).

Another potential threat to identification derives from the observation that besides potential criminals, local crime is also dependent on the number of police officers present on the streets. One possible concern is that the supply of police officers responds to potential criminals released thank to the clemency bill. In addition, if this response varied across municipalities, our coefficient

[19]However, the effects are modest and the coefficient on the ITR when estimating (1) remains unchanged when including these variables.

[20]The potential link between ITR and local crime waves is based on the presumption that the date of a crime coincides with the date of admission to prison. However, we note that almost all the individuals (99%) in our sample have been tried and convicted at least at the first instance with an enforceable sentence. For these individuals, the disposition time (the trial length) crucially affects the date of entry into prison. In this regard, it is important to point out that the Italian criminal justice system has a high variability in disposition time both across and within districts. Such heterogeneity is due to factors present at the district level (regarding the efficiency of the court) and the judge level. Indeed, even within districts we observe significant differences in the length of a trial depending on the flexibility that each judge has in the organization of her/his working time, as illustrated by Coviello *et al.* (2015). This suggests that in the Italian criminal justice system elements such as the efficiency of the district and the judge in charge of a case, have an important impact on the length of a trial. This reduces the relevance of the date in which a crime was committed with respect to the incentive to recidivate (i.e., individuals committing a similar crime on similar dates might enter into prison at quite different times and then end up with rather different residual sentences at the time of the pardon).

of interest in Model (1) may pick up the voters' reaction to the variation in the supply of police officers on the streets. We note, however, that the supply of police officers is governed at the national level by the Ministry of the Interior and the Ministry of Defense (for the *Polizia di Stato* and the*Carabinieri*, respectively). There are no significant variations over time in the supply of these national police forces across municipalities. In fact, the percentage of police officers across municipalities is historically determined. It is possible to observe some short-run variations, for example for football matches or political events involving threats to public order. On the other hand, the local police (*Polizia Municipale*), which makes a lesser contribution to maintaining public order, is under the control of the municipality mayor. Yet, variation in the local police forces requires a long implementation time and it can be possibly related to the local political cycle. Since we measure the voters' response to the Collective Clemency Bill enacted by the Parliament by looking at the national election, we see these concerns as not relevant in our setting. In Section 5, we show the results by controlling for macro-area and region fixed effects. This exercise reassures that the results are substantially robust when using variation within regions where potential changes in police forces may occur.

5 Results

5.1 Policy Outcomes

The first and most immediate effect of the collective pardon was a spike in crime - as documented in Figure 3. A fraction of this crime that is correlated to the public policy under analysis is due to the recidivism of pardoned individuals.[21] Table 5 shows the results from estimating variations of equation (1) in which the dependent variable is not the electoral outcome but the recidivism rate at the municipal level.[22] In all of the specifications we cluster standard errors at the provincial level. When looking at the number of pardoned individuals recommitting a crime after being released from prison, it is possible to observe that the idiosyncratic individual incentives to recidivate

[21] As in Drago *et al.* (2009), the recidivism rate is measured seven months after the release. This is less of a concern as long as we expect the residual sentence having an effect on recidivism measured two years later. In fact, the estimates from Mastrobuoni and Rivers (2016) - that we use in Section 5.4 - show that the effect of the residual sentence is persistent at least up to 17 months after the pardon.

[22] As both the ITR and the share of recidivists is null in municipalities with no pardoned individuals, we report results only for the sample of municipalities with at least one pardoned individual

translates into different recidivism rates at the municipal level. Hence, in municipalities where the average ITR of pardoned individuals is higher, the collective pardon bill translates into worse policy effects (higher recidivism rate). The effect is not trivial: taking into account column (3), a one standard deviation increase in the ITR implies an increase of 1.41 percentage points of pardoned individuals being re-arrested, which corresponds to a 15.9 percent increase in recidivism in a municipality with at least one pardoned individual. In Table A4 in the online Appendix, we also document that the average residual sentence weakly increases the overall crime rate at the city level. Although the recidivism of pardoned individual arguably represents a small part of the overall crime, we are able to detect a correlation between our key driver of recidivism and crime.[23]

5.2 Voters' Electoral Response

Table 6 illustrates the first set of results. We estimate variations of Equation (1) using ordinary least squares. The dependent variable is the difference in electoral win margin (in terms of total votes per eligible voters) of the CR coalition relative to the CL coalition between the 2008 and the 2006 national elections. As is clear from Table 6 the ITR has a positive effect on the margin of victory for the CR coalition. This effect is unchanged whether or not we control for inmates' characteristics (for example, in column (1) and (2)). The coefficients are precisely estimated. In our preferred specification in column (4), where we consider the sample of all municipalities and include municipal level controls, a one standard deviation increase in the ITR (around an 8.2 months shorter average residual sentence) leads to a 0.25 percentage points increase in the CR coalition's margin of victory, corresponding to a 3.06 percent increase in its margin of victory (we discuss more in detail these magnitudes in the next section). Table A5 in the online Appendix reports the coefficients of all the variables included in the analysis.[24]

The main results are essentially unchanged (larger standard errors but also larger point estimates) when we weight each observation with the number of eligible voters in 2008 (Table A6 in

[23]Note that the spike in crime documented in Figure 3 takes place in the last semester of 2006 and the first semester of 2007. As we have yearly data at the municipal level, in Table A4 we report the results regressing crime rates in 2007.

[24]This effect is essentially the same when we control for the number of pardoned individuals non-parametrically by including the number of pardoned individual fixed effects. Identical results are also obtained when we control non-parametrically for the number of pardoned individuals per capita (by creating discrete intervals for this continuous variable).

the online Appendix).[25] Overall, the effect appears to be driven by the combined positive effect of the ITR on the increase in the votes (per eligible voters) of the CR coalition and the negative effect on the CL coalition (see Tables A8 and A9 in the online Appendix). The coefficients are positive and statistically significant when we exploit the within-region variation by including macro-area (the North, Center, South and Islands) fixed effects (Table A10) and region fixed effects (Table A11). When we exploit within-province variation (Table A12), the coefficient is not statistically significant for the larger sample (Column 4). However, despite the inclusion of indicators for macro-areas, regions and provinces that absorb useful variation, we still observe an economically relevant effect of our driver of recidivism. We also provide the IV estimates of the effect of recidivism on voters' electoral response in Table A13. In this table we take as the reduced form the results provided in Table 6 and as the first stage those reported in Table 5. To conclude this section, we remark that it might be important to investigate whether or not the effect of the ITR is increases with the number of pardoned individuals per capita. In Section 5 of the online Appendix we present and discuss this analysis that is complicated by the fact that the ITR is a variable that is weighted by the number of pardoned individuals resident in a municipality. As we explain in the online Appendix, we find weak and not compelling evidence that the ITR is increasing with the number of pardoned individuals resident in a municipality.

5.3 Heterogeneity in Voters' Response

In this section, we present another important piece of evidence that is consistent with an accountability mechanism. As discussed in Section 2, the CL coalition was perceived as being responsible for proposing and designing the pardon bill. However, some MPs of the CR coalition also voted in favor of the bills.[26] We exploit this circumstance, as such CR MPs might have also been held accountable by voters for the realized effects of the policy. If an accountability mechanism is at work, the voters' response to the effects of the policy should favor less the CR coalition in electoral districts where the percentage of CR candidate who voted for the pardon was higher.[27] In order to

[25] In Table A7 we control for higher moments of the ITR in a municipality and find that these do not seem to play any role while the effect of the mean of the ITR remains unaltered.

[26] According to some policy reports (Eurispes, 2007), some of the MPs of the CR coalition voted in favor of the bill due to the fact that the pardon was extended to white-collar criminals (e.g., those convicted for financial or tax-evasion crimes) who accounted for a very limited fraction of released prisoners.

[27] The electoral law allowed voters to express a preference for a party but not for a specific candidate. Hence, voters willing to hold a candidate accountable for her/his voting record on the pardon could have done so only by

test this hypothesis, we gathered data from the Italian Ministry of Internal Affairs regarding the identity of all CR candidates in each electoral district for the 2008 elections. We then analyzed the voting records of each Italian MP regarding the 2006 Collective Pardon Bill and classified each CR MP according to whether she/he voted in favor or against the bill.[28] Finally, we computed for each electoral district (typically sub-regional entities including hundreds of municipalities), the percentage of candidates of the main center-right party (i.e., PdL) in the 2008 elections who voted in favor of the Collective Pardon Bill in July 2006.[29] Formally, the model that we estimate is a variation of model 1:

$$\begin{aligned}\Delta y_{ij} = \alpha + \alpha_1 ITR_{ij} + \alpha_2 \textit{original sentence}_{ij} + \alpha_3 \mathcal{I}_i + \alpha_4 X_{ij} + \alpha_5 Z_{ij} \\ + \alpha_6 ITR_{ij} \times \textit{\% CR candidates who voted for the pardon}_j + \gamma_j + \epsilon_{ij}\end{aligned} \tag{2}$$

where Δy_{ij} is again the difference in the margin of victory of the CR coalition with respect to the CL coalition between the national elections in 2008 and 2006. Unlike in Model 1, here we include the dimension of the electoral district j and electoral district effects (γ_j) that control for any selection of particular CR candidates into districts.[30] In fact, in this case, selection may be relevant especially for CR candidates who voted for the pardon bill.[31]

Table 7 presents the results of Model 2 where the coefficient of interest is α_6.[32] Table 7 shows

voting against the party she/he belonged to. In particular, it is important to remark that the pardon was approved by almost all MPs belonging to one of the main CR parties (*Forza Italia*, FI) and was opposed by the almost all the ones belonging to the other main CR partier (*Alleanza Nazionale*, AN). In the 2008 elections these two parties merged in a single one (*Popolo delle Libertà*, PdL). Hence, voters may have adopted a "simple rule of thumb" by looking at how many candidates within the 2008 PdL list for their district belonged to either of these two former parties (FI vs. AN). This was quite straightforward since the former affiliation of candidates to either of these two parties was reasonably well known to Italian voters.

[28]MP voting records are available at: `http://www.camera.it/_dati/leg15/lavori/stenografici/sed033/v002.pdf`.

[29]The percentage of CR candidates (of the main party, PdL) in a district who voted for the pardon ranges from 0 up to around 26% (for an average 17.5%, with a standard deviation of 6%). Results are robust to excluding the districts in the two "tails" of this distribution (i.e., the districts with a percentage equal to zero or above 25).

[30]We omit from the model the non-interacted variable *% CR candidates who voted for the pardon* which varies at the district level and would be absorbed by the electoral district fixed effects.

[31]Controlling for electoral district fixed effects improves the precision of the estimates for the interaction term. Without electoral fixed effects the interaction term remains negative but with larger standard errors. In addition, when we estimate the model by including municipalities with no pardoned individuals, we include an interaction between the indicator $\mathcal{I}_i$ and the percentage of CR candidates who voted for the pardon.

[32]Our analysis focuses on the lower chamber of the parliament since it is characterized by a larger number of MPs and of electoral districts. This allows us to exploit a higher degree of heterogeneity across districts when looking at the differential impact of the percentage of CR candidates who voted in favor of the pardon on voters' behavior as in Table 7. We also report the main results for the upper chamber in the online Appendix: Table B2 presents the results of the same specification as in Table 7, while Table B1 reports the results of the specifications used in Table 6.

a negative and significant coefficient for this interaction term α_6 on the CR win margin between the 2006 and 2008 elections. This suggests that the higher the percentage of CR candidates in a district who voted for the pardon, the lower the variation in margin of victory of the CR coalition. In other words, in districts where more candidates of the CR coalition voted in favor of the pardon, the CR gained relatively less votes and the CL lost relatively less votes. In terms of magnitudes, a one standard deviation increase in the ITR at the municipal level implies an increase in the CR winning margin of 14.2 percent in districts were none of the CR candidates voted in favor of the pardon. When we consider municipalities with a least one pardoned individual, where the median percentage of CR MPs who voted for the bill is 0.154, we get the same baseline result as the one of Table 6.

To summarize, we observe that the CR coalition experienced an increase in its electoral support relative to the CL, in municipalities where the ITR was higher. Moreover, this effect decreases with the presence of CR candidates who voted for the pardon.

5.4 Crime & Votes

In this section we provide a back-of-the-envelope calculation of the implied effect of a one standard deviation increase of the ITR on the votes gained by the CR coalition relative to the CL one. While such calculation should be taken with caution, it may provide a useful assessment of the implied magnitude of our effects.[33] Column 4 of Table 6, shows that a one standard deviation increase in the ITR leads to a 3.06 percent increase in the winning margin of the CR coalition (0.0025 percentage points with respect to an average winning margin of the CR coalition of 0.08 in 2008 elections relative to 2006). Considering an average difference in absolutes votes of 1,702 between the CR and CL elections in 2008, this corresponds to a decrease of 52 votes for each standard deviation increase in the ITR.

To understand how big a one standard deviation increase in the ITR is in terms former inmates recidivating, note that the average number of pardoned individual (per 1000 inhabitants) resident in a municipality is 0.33. Mastrobuoni and Rivers (2016) report an average recidivism rate of pardoned inmates up to 17 months after the pardon of 0.22.[34] This implies that the average number

[33] We compute this magnitude for the subset of cities with at least one pardoned individual. As we explained in Section 4, the estimation of our main coefficients of interests is obtained by exploiting the variation in the average residual sentence for all municipalities with at least one pardoned individual.

[34] Our data on recidivism only cover a period of seven months following the pardon (i.e., the information on the

of recidivists per 1000 people is 0.07 (0.33×0.22). Table 5 shows that a one standard deviation increase in the ITR increases the average recidivism rate by 15.9 percent. As a consequence, a one standard deviation increase in the ITR would lead to an increase in the number of recidivist equal to 0.0115 (0.159×0.07). To put it differently, the number of standard deviations increase in the ITR necessary to induce a 10 percent increase in the number of recidivists is 0.6. Given the numbers above, this translates into a gain of 33.8 votes for the CR coalition relative to the CL one (i.e., 0.2% in terms of the overall pool of eligible voters).

6 Additional Evidence: Information and Beliefs

We now provide additional evidence to unmask the link between the idiosyncratic component in the effects of the policy (the ITR of pardoned individuals) with voters' observed behavior in the 2008 elections. In particular, we show that a higher average ITR translated into both a higher probability of bein exposed to crime news involving pardoned individuals at the municipal level and a more negative evaluation of the incumbent CL government.

Voters' information. In order to assess the effects of the incentive to recidivate on the information available to voters' about the effects of the pardon, we focus on news about pardoned former inmates re-committing a criminal act. As explained in the online Appendix, these news stories—extrapolated from the *Factiva* database—contain words related to crime categories included in the pardon (i.e., theft, robbery, extortion, scamming, murder, drug dealing, burglary, beatings, domestic violence, rape, etc.) as well as words immediately identifiable with the collective pardon.[35] This exercise is helpful because voters' evaluation of the consequences of the collective pardon crucially depends on the information they receive about the recidivism of pardoned inmates. Table 8 shows how the pardoned individuals' ITR relates to news on crimes involving pardoned individuals at the municipal level. In Columns (1)-(4) of Table 8 we discretize the number of news stories (at least one news) and use a Probit specification. In Columns (5)-(8) we use the number of news stories and estimate a Poisson model (Table A14 in the online Appendix, reports OLS estimates for both dependent variables - at least one news and the number of news). Keeping the number of pardoned

recidivism of pardoned inmates captures only a fraction of the relevant electoral period). Hence, we rely on the information provided by Mastrobuoni and Rivers (2016) to produce a more accurate figure of the average number of pardoned individuals who recidivate over the period of interest.

[35]The online Appendix presents some examples of this type of news.

individuals per capita present in a municipality and the other characteristics of former inmates constant, the higher the ITR of pardoned individuals resident in a municipality, the more likely it is that newspapers report crime-news involving pardoned individuals in the post-pardon period up to the 2008 elections. Therefore, the different policy effects of the Collective Pardon Bill across municipalities, due to the idiosyncratic incentives of pardoned individuals to recidivate, translate into different information regarding the effects of this policy available for voters living in different municipalities. For example, in Column (2) the coefficient implies that a one standard deviation increase in our key variable implies a 1.1% higher probability of newspapers reporting at least one crime-related news story involving pardoned individuals in a given municipality. Although this effect may not seem large, it is worth remarking that *Factiva* does not contain the universe of all local news sources available to voters. At the same time, the news media is not necessarily the only channel of information on the effects of the public policy. For example, voters may also receive a private signal via a direct experience (for example, being a victim of a crime committed by a pardoned individual) or an indirect one (knowing someone who had such a direct experience). While we cannot clearly test these potential additional channels, we expect the direction of the effects to resemble the one observed for crime-related news reported by media outlets.

Voters' posterior beliefs. After having documented that the pardoned individuals' average ITR affects recidivism and media coverage of their crimes at the municipal level, we now look at how the average ITR affects voters' perceptions on the CL coalition. Using the responses to the ITANES post-electoral survey, in Table 9 we look at the effects of the ITR on voters' perceptions of both the CR and CL coalitions' competences in tackling crime. In Column (1) we investigate whether the incentive to recommit a crime at the municipality level led voters to consider crime the most important issue that the government should face. From the full sample of about 2,800 individuals, there is no compelling evidence that our margin of variation translated into a general increase in the relevance of crime (a finding that we use later to interpret the mechanism).

The survey asks specific questions on the ability of the CL and CR to deal with crime to the sub-sample of individuals (about 350) that responded yes to the first question (that crime is the main issue government should face). The results in Column (2)-(4) of Table 9 show that individuals living in municipalities where pardoned individuals have a higher average ITR are more likely to report a worse evaluation of the CL incumbent government's crime control policies

and the ability of the CL to deal with crime in general. The coefficient reported in Column (2), for example, implies a 1.66 percentage points (4.14%) higher probability of individuals reporting an overall negative evaluation of the CL coalition's crime control interventions following a one standard deviation increase in the ITR.

We find evidence in line with Table 9 from another dataset (IPSOS surveys). We first assess whether the incentive to recommit a crime is associated with an increase of the relevance of crime from the voters' perspective, both in Italy and in the local area where they live. From columns (1) and (2) of Table 10 that reports the results, we find again that the incentive to recommit a crime does not increase the relevance that voters attach to crime. Next, we are able to investigate whether the negative perception of the CL incumbent in dealing with crime (Table 9) is also associated with a more negative evaluation of the CL in general.[36] Table 10 shows that when the ITR is higher, voters are also more likely to negatively evaluate the main CL party (the *Partito Democratico*).[37]

7 Interpretations of the Results

In the previous sections we documented voters' response to criminal justice policies and the mechanisms underlying this response. In this section, we discuss how this evidence might be related to a general theoretical framework of voters' response to the effects of public policies. The key mechanism underlying modern theories of electoral accountability which is based on forward-looking retrospective voting (Fearon, 1999; Persson and Tabellini, 2002; Besley, 2006; Besley and Prat, 2006; Ashworth, 2012; Ashworth *et al.*, 2016) relies on two main elements. First, a politician's past action should provide information about her future behavior such that voters should be able to infer information on politician's quality from policy outcomes. Second, voters should condition their electoral behavior on such information (i.e., voters should respond to the observed effects of public policies). The results presented in the paper are consistent with this mechanism. In the on-

[36]The IPSOS database contains a series of repeated cross-sections spanning from April 2008 to December 2008. We consider all individuals interviewed within this period and control for date of the interview fixed effects. The frequency with which the question on the relevance of the crime issue was asked is lower than the one regarding the valuation of the main CL party. This explains the different number of observations in Columns (1) and (2) with respect to Columns (3) and (4).

[37]Table 9 and 10 report the marginal effects from a Probit model. In Tables A15 and A16 in the online Appendix we report the marginal effects from ordinary least squares.

line Appendix we present a retrospective probabilistic voting model linking voters and politicians consistent with our results in the context of our empirical design. Municipalities where the average ITR of resident pardoned individuals was higher experienced a higher recidivism rate (Table 5). In these municipalities, the CL coalition experienced a worse electoral performance relative to the CR one (Table 6). A higher ITR also translated in newspaper being more likely to report crime-related news involving pardoned individuals (Table 8). Hence, this suggests that voters living in different municipalities had different probabilities of receiving a negative *private signal* on the policy effects of the pardon. Most importantly, these probabilities were correlated with the idiosyncratic incentives to recidivate created by the design of the policy. The evidence concerning voters' evaluation of the CL incumbent government's crime policies (Table 9) and the overall perceived quality of the main CL party (Table 10), is consistent with a mechanism where voters update their beliefs on the incumbent government's quality (*type*) according to the observed effects of the policy.

Alternatively, our basic results (Table 6) might be explained by multi-dimensional voting. The multi-dimensional voting interpretation implies that an increased relevance of crime might have favored CR parties, given that they are typically perceived as the most competent on this issue (Petrocik, 1996; Puglisi, 2011). As discussed in Section 2, the Collective Pardon Bill was a very relevant political issue up to the 2008 elections. If the collective pardon and its effects translated into a general increase in the relevance of crime, our results could be then explained by a salience-driven increase in the support for CR parties (this is a mechanism suggested by models of multi-dimensional voting Belanger and Meguid, 2008; Aragones *et al.*, 2015). However, while this mechanism is consistent with the baseline results, it does not seem to square with three other pieces of evidence. First, as shown by Table 7, the gain that the CR coalition obtained in municipalities where the (negative) effects of the pardon were more relevant was lower in districts where more CR candidates voted in favor of the pardon. Hence, the relevance of the crime issue induced by the collective pardon did not translated in an overall higher support for the CR. Rather, this higher support was conditional on the past stance of CR candidates with respect to such a policy. In addition, the evidence from the two survey datasets analyzed in Section 6 does not seem to imply any impact of the ITR on the probability of voters perceiving crime to be the most important issue either in Italy as a whole (Table 9 and Table 10) or in their municipality of residence (Table 10). Finally, if the increase in the relevance of crime favored right-wing parties in general, the ITR should influence not only national parliamentary elections but also other elections. Table A17 in

the online Appendix tests this implication. The results show that the ITR did not have any impact on voter behavior in European elections (for 2009 compared to 2004). Hence, it does not seem that CR parties experienced an overall political gain in municipalities where the realized effects of the policy were likely to be worse.

8 Conclusions

While politicians and elected officials exert a lot of effort to demonstrate their commitment to crime control, very little is known about how voters respond to crime policies. Do voters reward "tough on crime" politicians independently from the actual effects of their policies or do they respond to the observable effects of their actions on crime rates?

This paper provided causal evidence about voters' reaction to the consequences of a criminal justice policy intervention. Our study has analyzed how voters responded to the local consequences of the 2006 Collective Pardon Bill in Italy. The Italian case-study has a series of attractive features since it allowed us to exploit a unique national level natural experiment. Indeed, the collective pardon implemented by this bill implies random variation in the consequences of the policy at the municipality level. While the approval of the collective pardon itself may have given a uniform signal at the national level about the attitudes of the ruling CL coalition, the empirical evidence shows that idiosyncratic incentives to recidivate across pardoned individuals (created by the design of the bill) lead to heterogeneous policy effects across municipalities. Municipalities where the average incentive to recidivate of resident pardoned individuals was higher, experienced a higher recidivism rate. At the same time, a higher incentive to recidivate at the municipal level led to newspapers being more likely to report crime news involving pardoned individuals and voters hardening their views on the CL coalition. Exploiting these features of the collective pardon bill, our main results provide causal evidence of voters holding politicians accountable for their policy choices. Specifically, our main results show that, conditional on the number of released prisoners resident in a municipality and their crime profile including the average original sentence, a higher incentive to recidivate in a municipality translates into a harsher electoral "punishment" of the CL coalition.

Besides providing evidence about the electoral payoffs of effective crime policies, to the best of our knowledge, our empirical analysis is among the few existing studies providing direct evi-

dence about voters holding politicians accountable for the consequences of their policies (Ferraz and Finan, 2008; Casaburi and Troiano, 2016). Our analysis suggests that voters receive private signals and hold beliefs on incumbent politicians that are consistent with the effects of their public policies. Ultimately, voters keep incumbent politicians accountable by conditioning their vote on the observed effects of public policies.

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Tables and Figures

Table 1: Perceived overall effects of the collective pardon on crime

	Center-Left Voters	Center-Right Voters	Independent/ abstainers	All Voters
Large increase in crime	29.7%	66.7%	52.1%	51.3%
Limited increase in crime	38.2%	22.0%	25.0%	27.4%
No increase in crime	26.7%	8.2%	11.4%	14.2%
Does not know/Does not answer	5.3%	3.1%	11.4%	7.1%

Notes. The data are drawn from a survey (N=1307) representative of the Italian population aged 16 and above. The data reports the percentage response by type of answer and by voter's political ideology to the question "In your opinion, has the collective pardon lead to an increase in crime in Italy". Source: *Osservatorio sul Capitale Sociale. Demos & Pi*, June 2007.

Table 2A: Summary statistics: Pardoned individuals (municipal level)

Variable	Obs	Mean	Std. Dev.	Min	Max
Incentive to recidivate (ITR)	2256	2.51	1	.12	4.26
Average original sentence	2256	40.56	29.57	2	254
Mean age	2256	40.26	8.48	20	78
% employed	2256	.26	.37	0	1
% married	2256	.27	.36	0	1
% primary education	2256	.71	.38	0	1
% secondary education	2256	.07	.21	0	1
% college education	2256	.01	.08	0	1
% convicted for drug crimes	2256	.3	.37	0	1
% convicted for property crimes	2256	.47	.41	0	1
% convicted for violent crimes	2256	.13	.28	0	1
% convicted for other crimes	2256	.02	.1	0	1
Pardoned individuals per 1,000 residents	2256	.33	.32	.02	4.39

Notes. The table reports summary statistics on the individuals released in each city with at least one individuals. Number reported are at the city level.

Table 2B: Summary statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
City with at least one pardoned individual	7159	.32	.46	0	1
Municipal area (squared km)	7159	39.59	52.02	.2	1307.7
Latitude	7159	43.27	2.62	35.5	47.04
Longitude	7159	11.74	2.75	6.7	18.49
Landlocked municipality	7159	.91	.29	0	1
Montaneous municipality	7159	1.88	.95	1	3
Crimes per capita pre-pardon (2005&2004)	7159	.01	.01	0	.33
Mean taxable income per capita (2008)	7159	10309.79	3254.3	3030.83	30545.7
Private sector employees per capita (2001)	7159	.21	.17	.01	3.06
Municipal unemployment rate (2001)	7159	.11	.09	0	.51
Municipal population (2001)	7159	7876.69	41749.45	500	2546804
Share of population older than 65 (2001)	7159	.2	.06	.06	.55
Share of population between 20-34 (2001)	7159	.21	.02	.1	.29
Share of population with diploma laurea (2001)	7159	.28	.06	.07	.62
At least one news on crime & collective pardon	7159	.06	.24	0	1
Δ Win Margin 2006-2008 C.Right vs. C.Left	7159	.07	.07	-.3	.6
Δ Votes per eligible voters 2006-2008 C. Right	7159	-.04	.04	-.31	.27
Δ Votes per eligible voters 2006-2008 C. Left	7159	-.11	.05	-.54	.14
Crime main political issue gov. should face	2853	.12	.33	0	1
Incumbent gov. poorly managed crime	350	.4	.49	0	1
Center-left best suited to deal with crime	350	.07	.26	0	1
Center-right best suited to deal with crime	350	.49	.5	0	1
Negative valuation main C-Left party	27965	.14	.35	0	1
Positive valuation main C-Right party	28116	.11	.31	0	1
Crime main issue in the municipality	3734	.07	.26	0	1
Crime main issue in Italy	3734	.12	.32	0	1

Notes. The table reports summary statistics on all the cities in our sample. Number reported are at the city level.

Table 3A: Balancing tests: geographical characteristics of the municipality

	(1) Incentive to recidivate	(2) Incentive to recidivate	(3) Incentive to recidivate	(4) Incentive to recidivate	(5) Incentive to recidivate
Municipal area (squared km)	-0.0000				
	(0.0001)				
Latitude		-0.0036			
		(0.0026)			
Longitude			0.0018		
			(0.0028)		
Landlocked municipality				0.0251	
				(0.0306)	
Montaneous municipality					-0.0013
					(0.0067)
Average original sentence - pardoned	-0.0150***	-0.0151***	-0.0150***	-0.0150***	-0.0150***
	(0.0012)	(0.0013)	(0.0013)	(0.0013)	(0.0013)
City with at least one pardoned invididual	3.1181***	3.1163***	3.1169***	3.1197***	3.1168***
	(0.0485)	(0.0479)	(0.0482)	(0.0473)	(0.0478)
Observations	7,159	7,159	7,159	7,159	7,159
R-squared	0.8488	0.8489	0.8488	0.8488	0.8488

Notes. Entries are coefficients from regressions in which the dependent variable is the incentive to recidivate regressed on each of the variables X_i of model (1), conditional on the average original sentence and the indicator $\mathcal{I}_i$ (city with at least one pardoned individual). Models estimated are with OLS. Standard errors clustered at the provincial level are in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 3B: Balancing tests: socio-economic characteristics of the municipality

	(1) Incentive to recidivate	(2) Incentive to recidivate	(3) Incentive to recidivate	(4) Incentive to recidivate	(5) Incentive to recidivate
Crimes per capita in 2005 & 2004	0.2342				
	(0.4869)				
Pardoned individuals per 1,000 residents		0.0317			
		(0.0599)			
Taxable income per capita (2008)			-0.0000		
			(0.0000)		
Private sector employees per capita, in 2001				-0.0064	
				(0.0388)	
Municipal unemployment rate, in 2001					0.0658
					(0.0801)
Average original sentence - pardoned	-0.0150***	-0.0151***	-0.0150***	-0.0150***	-0.0151***
	(0.0012)	(0.0012)	(0.0013)	(0.0013)	(0.0013)
City with at least one pardoned invididual	3.1172***	3.1078***	3.1175***	3.1176***	3.1159***
	(0.0482)	(0.0546)	(0.0485)	(0.0484)	(0.0477)
Observations	7,159	7,159	7,159	7,159	7,159
R-squared	0.8488	0.8488	0.8488	0.8488	0.8488

Notes. Entries are coefficients from regressions in which the dependent variable is the incentive to recidivate regressed on each of the variables X_i of model (1), conditional on the average original sentence and the indicator $\mathcal{I}_i$ (city with at least one pardoned individual). Models estimated are with OLS. Standard errors clustered at the provincial level are in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 3C: Balancing tests: demographic characteristics of the municipality

	(1) Incentive to recidivate	(2) Incentive to recidivate	(3) Incentive to recidivate	(4) Incentive to recidivate
Municipal population, in 2001	0.0000			
	(0.0000)			
Share of population older than 65, in 2001		-0.0490		
		(0.1176)		
Share of population between 20-34, in 2001			0.0643	
			(0.3181)	
Share of population with diploma laurea, in 2001				0.0979
				(0.1019)
Average original sentence - pardoned	-0.0150***	-0.0150***	-0.0150***	-0.0150***
	(0.0013)	(0.0013)	(0.0013)	(0.0013)
City with at least one pardoned individual	3.1152***	3.1158***	3.1166***	3.1151***
	(0.0485)	(0.0472)	(0.0474)	(0.0486)
Observations	7,159	7,159	7,159	7,159
R-squared	0.8488	0.8488	0.8488	0.8488

Notes. Entries are coefficients from regressions in which the dependent variable is the incentive to recidivate regressed on each of the variables X_i of model (1), conditional on the average original sentence and the indicator $\mathcal{I}_i$ (city with at least one pardoned individual). Models estimated are with OLS. Standard errors clustered at the provincial level are in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 4: Placebo

	Δ Win Margin 2006-2001 Center-right vs. Center-left			
	(1)	(2)	(3)	(4)
Incentive to recidivate	-0.0017	-0.0013	-0.0005	0.0001
	(0.0017)	(0.0017)	(0.0015)	(0.0015)
Pardoned individuals controls	NO	YES	YES	YES
Municipal level controls	NO	NO	YES	YES
Only municipalities with at least one pardoned	YES	YES	YES	NO
Observations	2,252	2,252	2,252	7,139
R-squared	0.0098	0.0461	0.1508	0.1549

Notes. Entries are coefficients from the equation model (1) estimated with OLS where the dependent variable is the difference in the margin of victory of the CR coalition with respect to the CL coalition between the national elections in 2006 and 2001. Pardoned individuals control include: number of pardoned individuals per 1,000 residents in the municipality, average length of original sentence, average age, percentage of pardoned individuals that are employed, percentage of pardoned individuals with primary, secondary and college education, percentage of pardoned individuals convicted for drug, property, violent or other types of crime; Municipal level controls include: municipal area, latitude, longitude, dummy for landlocked municipality, indicator of montaneous or partially montaneous municipality, resident population, share of population with diploma laurea, share of population over 65, share of population 20-34, private sector employees per capita, municipal unemployment rate (in census year 2001), mean taxable income per capita (2008) and crime rate pre-pardon (average between 2004-2005). Standard errors clustered at the provincial level are in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 5: Incentive to Recidivate and Recidivism

	Share of Recidivists		
	(1)	(2)	(3)
Incentive to recidivate	0.0159***	0.0142**	0.0141**
	(0.0057)	(0.0057)	(0.0057)
Pardoned individuals controls	NO	YES	YES
Municipal level controls	NO	NO	YES
Only municipalities with at least one pardoned	YES	YES	YES
Observations	2,256	2,256	2,256
R-squared	0.0063	0.0369	0.0419

Notes. Entries are coefficients from the equation model (1) estimated with OLS where the dependent variable is the share of released inmates recommitting a crime (recidivism). Pardoned individuals control include: number of pardoned individuals per 1,000 residents in the municipality, average length of original sentence, average age, percentage of pardoned individuals that are employed, percentage of pardoned individuals with primary, secondary and college education, percentage of pardoned individuals convicted for drug, property, violent or other types of crime; Municipal level controls include: municipal area, latitude, longitude, dummy for landlocked municipality, indicator of montaneous or partially montaneous municipality, resident population, share of population with diploma laurea, share of population over 65, share of population 20-34, private sector employees per capita, municipal unemployment rate (in census year 2001), mean taxable income per capita (2008) and crime rate pre-pardon (average between 2004-2005). Standard errors clustered at the provincial level are in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 6: Voters' Electoral Response

	Δ Win Margin 2008-2006 Center-right vs. Center-left			
	(1)	(2)	(3)	(4)
Incentive to recidivate	0.0030**	0.0030**	0.0029**	0.0025**
	(0.0013)	(0.0013)	(0.0011)	(0.0011)
Pardoned individuals controls	NO	YES	YES	YES
Municipal level controls	NO	NO	YES	YES
Only municipalities with at least one pardoned	YES	YES	YES	NO
Observations	2,256	2,256	2,256	7,159
R-squared	0.0200	0.0785	0.2306	0.1217

Notes. Entries are coefficients from the equation model (1) estimated with OLS where the dependent variable is the difference in the margin of victory of the CR coalition with respect to the CL coalition between the national elections in 2008 and 2006. Pardoned individuals control include: number of pardoned individuals per 1,000 residents in the municipality, average length of original sentence, average age, percentage of pardoned individuals that are employed, percentage of pardoned individuals with primary, secondary and college education, percentage of pardoned individuals convicted for drug, property, violent or other types of crime; Municipal level controls include: municipal area, latitude, longitude, dummy for landlocked municipality, indicator of montaneous or partially montaneous municipality, resident population, share of population with diploma laurea, share of population over 65, share of population 20-34, private sector employees per capita, municipal unemployment rate (in census year 2001), mean taxable income per capita (2008) and crime rate pre-pardon (average between 2004-2005). Standard errors clustered at the provincial level are in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 7: Voters' Electoral Response - Heterogeneity

	Δ Win Margin 2008-2006 Center-right vs. Center-left			
	(1)	(2)	(3)	(4)
Incentive to recidivate	0.0122***	0.0124***	0.0112***	0.0116***
	(0.0031)	(0.0031)	(0.0031)	(0.0031)
Incentive to recidivate × (% CR cand. who voted for pardon)	-0.0590***	-0.0589***	-0.0504***	-0.0535***
	(0.0158)	(0.0154)	(0.0162)	(0.0159)
Municipality with at least one pardoned individual				0.1685***
× (% CR cand. who voted for pardon)				(0.0545)
Pardoned individuals controls	NO	YES	YES	YES
Municipal level controls	NO	NO	YES	YES
Only municipalities with at least one pardoned	YES	YES	YES	NO
Observations	2,256	2,256	2,256	7,159
R-squared	0.3668	0.3790	0.4458	0.3754

Notes. Entries are coefficients from the equation model (2) estimated with OLS where the dependent variable is the difference in the margin of victory of the CR coalition with respect to the CL coalition between the national elections in 2008 and 2006. Pardoned individuals control include the interaction between the number of pardoned individuals per 1,000 residents in the municipality, average length of original sentence, average age, percentage of pardoned individuals that are employed, percentage of pardoned individuals with primary, secondary and college education, percentage of pardoned individuals convicted for drug, property, violent or other types of crime; Municipal level controls include: municipal area, latitude, longitude, dummy for landlocked municipality, indicator of montaneous or partially montaneous municipality, resident population, share of population with diploma laurea, share of population over 65, share of population 20-34, private sector employees per capita, municipal unemployment rate (in census year 2001), mean taxable income per capita (2008) and crime rate pre-pardon (average between 2004-2005). Standard errors clustered at the provincial level are in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 8: Incentive to recidivate & news on crime

	At least one news on crime & collective pardon in the municipality				Number of news on crime & collective pardon in the municipality			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Incentive to recidivate	0.0018**	0.0018**	0.0016*	0.0007*	0.0119*	0.0135**	0.0193**	0.0178**
	(0.0008)	(0.0008)	(0.0009)	(0.0004)	(0.0064)	(0.0064)	(0.0098)	(0.0088)
Pardoned individuals controls	YES	YES	YES	YES	YES	YES	YES	YES
Municipal level controls	NO	YES	YES	NO	YES	YES	YES	NO
Only municipalities with at least one pardoned	YES	YES	YES	NO	YES	YES	YES	NO
Observations	2,256	2,256	2,256	7,159	2,255	2,255	2,255	7,156

Notes. Marginal effects from a Probit model evaluated at the sample mean of all other variables are reported in columns (1)-(4), where the dependent variable is a dummy equal to zero if there is at least one local news on crime and collective pardon in a municipality and zero otherwise. Coefficients from a Poisson model are reported in columns (5)-(8), where the dependent variable is the number of local news on crime and collective pardon in a municipality. Pardoned individuals control include: number of pardoned individuals per 1,000 residents in the municipality, average length of original sentence, average age, percentage of pardoned individuals that are employed, percentage of pardoned individuals with primary, secondary and college education, percentage of pardoned individuals convicted for drug, property, violent or other types of crime; Municipal level controls include: municipal area, latitude, longitude, dummy for landlocked municipality, indicator of montaneous or partially montaneous municipality, resident population, share of population with diploma laurea, share of population over 65, share of population 20-34, private sector employees per capita, municipal unemployment rate (in census year 2001), mean taxable income per capita (2008) and crime rate pre-pardon (average between 2004-2005). Standard errors clustered at the provincial level are in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 9: Issue Priority & Perceived Competence of Political Coalitions (*ITANES*)

	(1) Crime most important issue gov. should face	(2) C-Left gov. dealt very bad with crime	(3) C-Left best suited to deal with crime	(4) C-Right best suited to deal with crime
Incentive to recidivate	-0.0006	0.0171**	-0.0008	0.0021
	(0.0016)	(0.0071)	(0.0006)	(0.0081)
Pardoned individuals controls	YES	YES	YES	YES
Municipal level controls	YES	YES	YES	YES
Individual level controls	YES	YES	YES	YES
Observations	2,826	347	347	347
Pseudo R-squared	0.069	0.229	0.477	0.203

Notes. Marginal effects from a Probit model evaluated at the sample mean of all other variables are reported. Individual level controls include: age, gender, religiosity level, marital status, employment status, self declared left-right political position, frequency of newspaper readership and whether the most viewed TV news channel belongs to the Mediaset media group (owned by the leader of the center-right coalition, Silvio Berlusconi). Pardoned individuals control include: number of pardoned individuals per 1,000 residents in the municipality, average length of original sentence, average age, percentage of pardoned individuals that are employed, percentage of pardoned individuals with primary, secondary and college education, percentage of pardoned individuals convicted for drug, property, violent or other types of crime; Municipal level controls include: municipal area, latitude, longitude, dummy for landlocked municipality, indicator of montaneous or partially montaneous municipality, resident population, share of population with diploma laurea, share of population over 65, share of population 20-34, private sector employees per capita, municipal unemployment rate (in census year 2001), mean taxable income per capita (2008) and crime rate pre-pardon (average between 2004-2005). Observation are weighted according to the sample political weights provided by ITANES (2008). Standard errors clustered at the provincial level are in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***, which report the results of the test of the underlying coefficient from the Probit model being 0.

Table 10: Valuation of Political Parties and Issue Priority (*IPSOS*)

	(1) Crime main issue in municipality	(2) Crime main issue in Italy	(3) Negative valuation main CL party	(4) Positive valuation main CR party
Incentive to recidivate	-0.0000	0.0005	0.0019**	0.0005
	(0.0010)	(0.0012)	(0.0009)	(0.0007)
Pardoned individuals controls	YES	YES	YES	YES
Municipal level controls	YES	YES	YES	YES
Individual level controls	YES	YES	YES	YES
Observations	3,719	3,719	27,853	28,004
Pseudo R2	0.122	0.122	0.0740	0.172

Notes. Marginal effects from a Probit model evaluated at the sample mean of all other variables are reported. Individual level controls include: age, gender, religiosity level, employment status, self declared left-right political position, graduate degree. Pardoned individuals control include: number of pardoned individuals per 1,000 residents in the municipality, average length of original sentence, average age, percentage of pardoned individuals that are employed, percentage of pardoned individuals with primary, secondary and college education, percentage of pardoned individuals convicted for drug, property, violent or other types of crime; Municipal level controls include: municipal area, latitude, longitude, dummy for landlocked municipality, indicator of montaneous or partially montaneous municipality, resident population, share of population with diploma laurea, share of population over 65, share of population 20-34, private sector employees per capita, municipal unemployment rate (in census year 2001), mean taxable income per capita (2008) and crime rate pre-pardon (average between 2004-2005). Observation are weighted according to the sample political weights provided by IPSOS. The econometric specification includes fixed effects for the date of the interview. Standard errors clustered at the provincial level are in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***, which report the results of the test of the underlying coefficient from the Probit model being 0.

Figure 1: Timing of Elections and Collective Pardon Bill

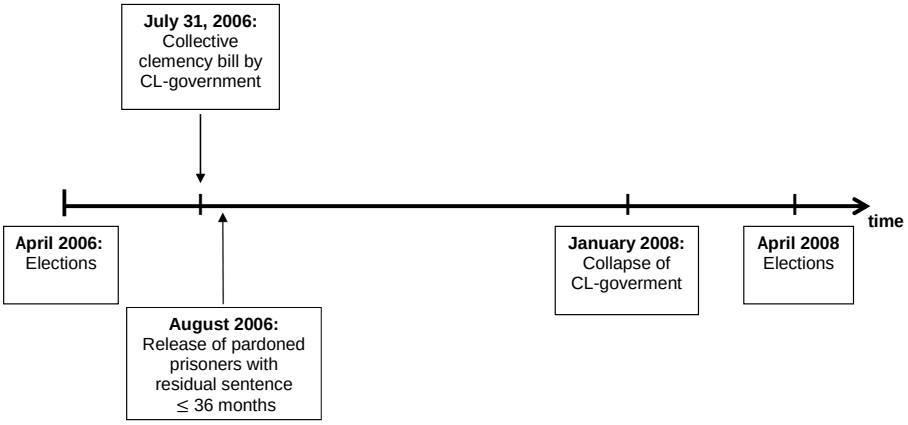
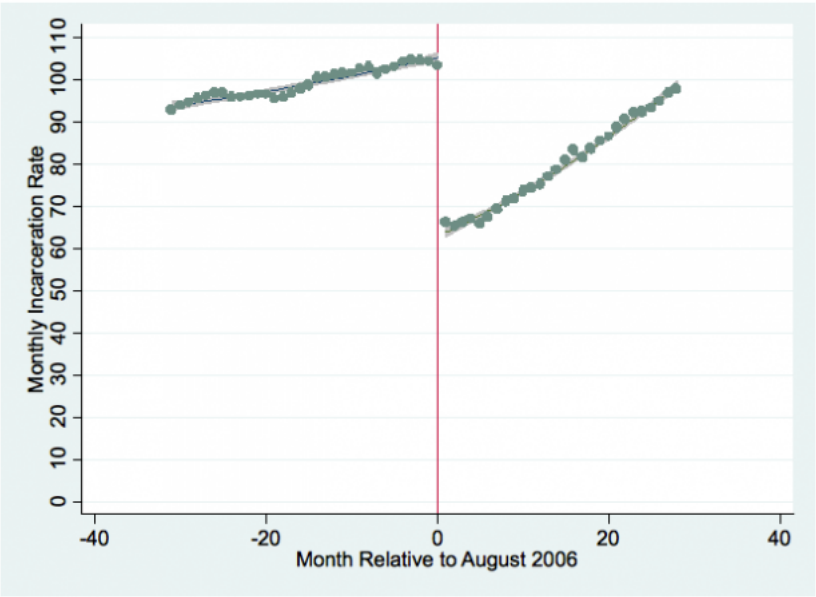
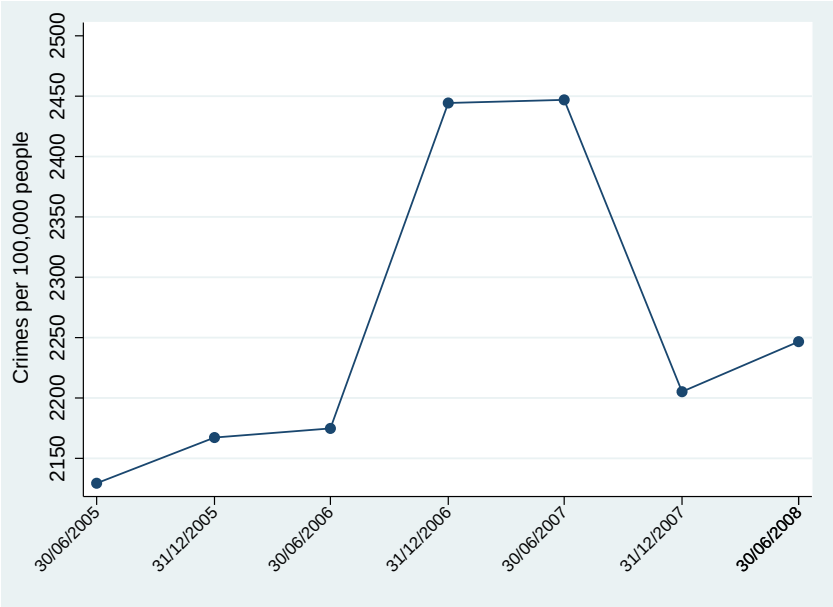


Figure 2: Incarceration rate



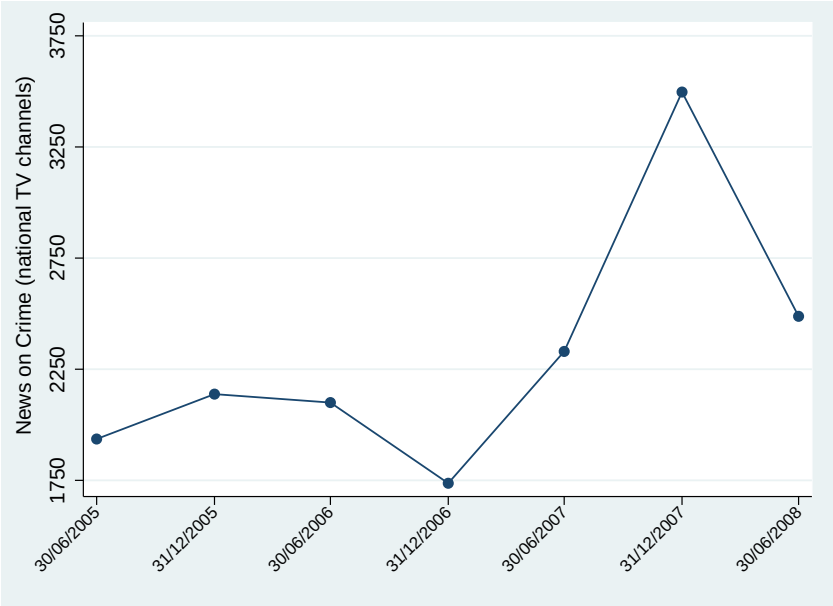
Notes: The figure illustrates the variation in the incarceration rate (i.e., per 100,000 people) in Italy before and after the collective pardon bill.

Figure 3: Crimes per 100,000 people



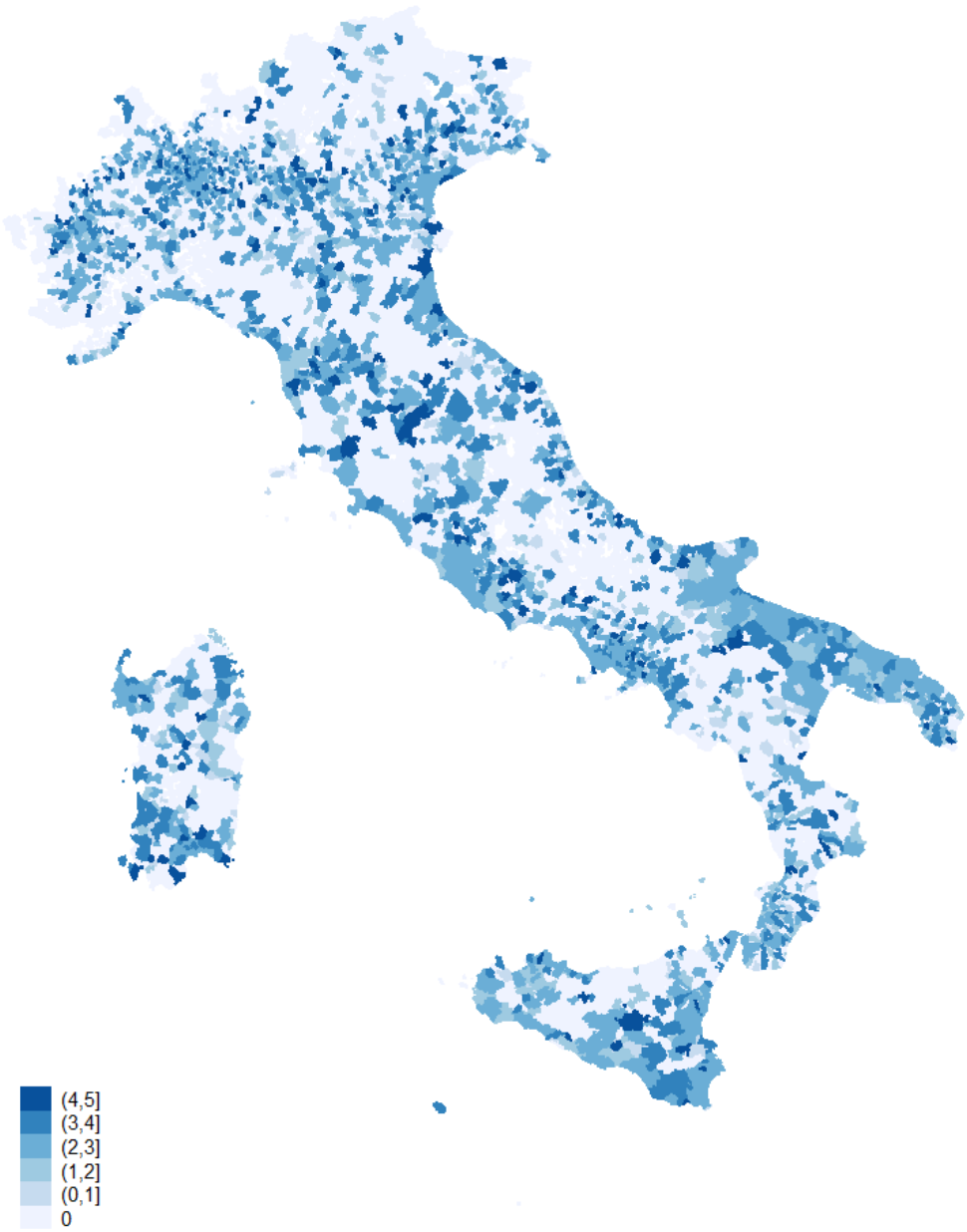
Notes: The figure illustrates the variation in the total number of crimes per 100,000 people in Italy between the first semester of 2005 and the first semester of 2008.

Figure 4: News on Crimes (national TV channels)



Notes: The figures illustrates the variation in the number of news on crime (on the main Italian national TV channels) between the first semester of 2005 and the first semester of 2008. (Source: "Indagine sulla Sicurezza in Italia, 2009, UNIPOLIS").

Figure 5: Geographical distribution of the average incentive to recidivate of pardoned individuals (standardized)



Notes. The figure illustrates the geographical distribution of the (standardized) average incentive to recidivate of pardoned individuals at the municipal level. A one unit increase corresponds to a one standard deviation increase in the incentive to recidivate (i.e., around 8.2 months less of residual sentence).