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ABSTRACT

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

We provide evidence about voters' response to crime control policies. We exploit a natural experiment arising from the Italian 2006 collective pardon releasing about one third of the prison population. The pardon created idiosyncratic incentives to recidivate across released individuals and municipalities. We show that municipalities where resident pardoned individuals have a higher incentive to recidivate experienced higher recidivism. Moreover, in these municipalities: i) newspapers were more likely to report crime news involving pardoned individuals; ii) voters held worse beliefs on the incumbent governments ability to control crime and iii) with respect to the previous elections, the incumbent national government experienced a worse electoral performance in the April 2008 national elections relative to the opposition coalition. Overall, our findings indicate that voters keep incumbent politicians accountable by conditioning their vote on the observed effects of their policies.

JEL Classification: D72, K42

Keywords: accountability, voting, natural experiment, crime, recidivism

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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 first five (out of 15) most important perceived problems in several European countries (Mastrorocco and Minale, 2014).¹ Accordingly, there is a widespread belief that crime 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; Berdejo and Yuchtman, 2013; 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 as well as—more generally—we know little about how voters respond to the observed effects of public policies, a question that is as relevant as challenging due to the endogeneity of politicians behavior.³

This paper exploits an original case study, based on a natural experiment, to study how voters respond to crime control policies. To the best of our knowledge, this is the first exercise providing evidence concerning voters' response to crime policies in a quasi-experimental setting. By doing so, our study is also informative on how voters assess policymakers in the presence of incomplete information, a broader and fundamental question in political economy.

In July 2006, the Italian center-left (CL) national government implemented an unanticipated collective pardon involving the release of the 37 percent of the total prison population. All the inmates with a residual sentence of less than 3 years were released in August 2006. The design of the policy was such that released prisoners recidivating within a five-year period would be charged an additional sentence equal to their residual sentence at the time of their release. This

¹Concerns on crime are shared by citizens on both sides of the Atlantic. According to Krisberg and Marchionna (2006), the 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.

²According to journalistic 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 seen as being largely due to his “soft on crime” record as a Massachusetts Governor (*The New York Times*, July 5, 1988).

³Incumbent politicians tend to strategically manipulate their policies across space (targeting specific groups of voters or constituencies) and across time (timing policies with respect to electoral years), see for example Rogoff and Sibert (1988); Rogoff (1990); Brender and Drazen (2008).

provision of the bill manipulated pardoned individuals' incentives to recommit crime after release from prison (Drago *et al.*, 2009). Since we have information on the municipality of residence of the former inmates released, we can rank municipalities according to their inmates' average residual sentence. Crucial for our analysis, we observe that there is enough meaningful variation in this variable at the municipality level (that translates into variation in recidivism rates) across the more than 2,000 municipalities where former inmates live.⁴ While voters were not aware of the incentive to recidivate of pardoned individuals because the average residual sentence was not public information, they experienced different recidivism levels depending on their municipality being associated with a high or low average residual sentence of former inmates.

The quasi-experimental nature of the variation is based on the fact that conditional on the original sentence, the residual sentence only depends on the date of entry into prison which is plausibly random. Pardoned inmates entered prison the day of apprehension (provided that a court decided to keep them in prison) or the day in which they were sentenced. The existing variation at the municipality level aggregates this individual heterogeneity. Hence, the conditional independence assumption is that the average incentive to recidivate in a municipality is exogenous once we control for the average original sentence of pardoned inmates. If this assumption holds, potential spatially correlated boom and busts in prison entry should not be correlated with the cross-sectional municipal variation or pre-trends in voters' behavior. We show that the average residual sentence 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. Overall, the design allows us to exploit a margin of variation in the effects of the collective pardon which voters might directly map into the government's policy. This setting approaches the ideal experiment in which the researchers would observe the government randomly manipulating the content of a policy and then mapping it into different outcomes.

Our main finding is that, conditional on the number of released prisoners resident in a municipality and their crime profile including the average original sentence, a higher average incentive to recidivate (corresponding to a lower residual sentence) in a municipality translates into a harsher electoral "punishment" of the incumbent national government. A one standard deviation increase in the incentive to recidivate at the municipal level is associated with a 3%

⁴The 70 percent of cities have less than 3 pardoned individuals.

increase in the margin of victory of the opposition (center-right, CR) coalition in the post-pardon national elections in 2008 relative to the pre-pardon ones in 2006.

We also investigate the mechanism linking policy choices and electoral outcomes. In particular, we assess the impact of the collective pardon on: *i*) policy outcomes; *ii*) voters' information and *iii*) voters' beliefs. For what concerns policy outcomes, we measure recidivism of pardoned individuals at the municipal level. A higher incentive to recidivate effectively increases recidivism in line with the results found at the individual level in Drago *et al.* (2009). With respect to voters' information, by exploiting the *Factiva* database, we assemble an original dataset of news on crime events involving pardoned individuals. We then match news with municipalities to create a measure of municipal level exposure of voters to the effects of the pardon. A higher incentive to recidivate in a municipality increases the probability of newspapers reporting news on crime events involving pardoned individuals (voters are more likely to receive a negative signal on the effects of the policy). Finally, to analyze voters' beliefs we gather individual level data from two independent surveys. The results show that the voters are more likely to report a negative valuation on the incumbent national government's ability to control crime and on its overall competence (voters hold worse posterior beliefs on the incumbent government's type) in municipalities where the incentive to recidivate is higher.

As we explain in greater details in the paper, these effects are not negligible. In terms of political cost, our estimates suggest that—in an average municipality—one more crime by pardoned individuals leads to a drop of 272 votes (1.77% of eligible voters) for the incumbent national government relative to the opposition coalition. Most importantly, the effects can be all accounted within a retrospective voting model (e.g., Persson and Tabellini 2002; Ashworth 2012). Indeed, our findings provide evidence that voters receive private signals and hold beliefs on incumbent politicians that are consistent with the effects of their public policies. Ultimately, the results point out that voters keep incumbent politicians accountable by conditioning their vote on the observed effects of their policies. To this extent, besides identifying how voters respond to crime control policies, this paper contributes to the recent literature on electoral accountability over various dimensions. One part of the literature analyzes whether and how voters respond to events that are orthogonal to government's policies (Achen and Bartels 2004; Wolfers 2002; Healy and Malhotra 2009; Healy *et al.* 2010; Ferraz and Monteiro 2014; Bagues

and Esteve-Volart 2016; Achen and Bartels 2016). That is events (e.g., shark attacks in Achen and Bartels 2004; performance of local sport teams in Healy *et al.* 2010; oil rents in Ferraz and Monteiro 2014; lotteries in Bagues and Esteve-Volart 2016) whose effects might be erroneously *interpreted* by voters as the result of a public policy (enacted by incumbent politicians). In turn, these papers implicitly test whether the accountability mechanism linking policy outcomes and voters' behaviour may be jeopardized by the presence of potential attribution errors on the side of voters.⁵ Another part of the literature evaluates how voters respond to variations in the scope of public policies (Casaburi and Troiano 2016) or to variations in information on incumbent politicians' behavior (Ferraz and Finan 2008).

We improve with respect to this literature as follows. First, while most of the existing papers look at how different incumbents are affected by shocks that are, arguably, exogenous to their policy choice, we are among the few (e.g., Bagues and Esteve-Volart 2016) exploiting a natural experiment where all voters face the same incumbent government. This has important consequence on the mapping between the observed behavior of voters and electoral accountability. As discussed by Ashworth *et al.* (2016), when the design exploits the presence of different incumbents, even random shocks such as natural disasters might be useful in providing relevant information for voters when making inferences on a politician's type. In turn, this may constitute a potential threat to identification due to part of the heterogeneity in the observed effects of an event being driven by the unobserved heterogeneity in incumbent politicians' types.⁶ Since in our setting there is a *single incumbent* and a *single policy* (with heterogeneous effects that are orthogonal to the incumbent's type), the identification strategy allows us to overcome such identification issues present in earlier contributions.

Second, unlike the literature exploiting shocks unrelated to any public policy, we focus on a natural experiment where voters may clearly map the effect of such experiment to a national level governmental policy. That is, our empirical design provides a direct test on how voters respond

⁵Fowler and Hall (2016) show that, contrary to the claim of Achen and Bartels (2004, 2016), there is little compelling evidence that shark attacks influence presidential elections.

⁶While the occurrence of such events themselves might be orthogonal to an incumbent's type, their effects are likely to be correlated to the incumbent politician's type (for example, quality of disaster preparedness, efficient use of oil revenues). Hence, they might provide relevant information on the incumbent's type that rational voters will use when updating their beliefs on such type. This implies that a random shock may affect the probability of an incumbent being reelected while not providing any compelling evidence on electoral accountability (or absence thereof).

to the observed effects of public policies and, ultimately, on politicians' accountability. Finally, our empirical results are also informative for the debate on voters' sophistication (Wolfers, 2002). In line with recent empirical evidence (Kendall *et al.*, 2014), these 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 consistent with retrospective voting models of electoral accountability (e.g., Fearon 1999; Persson and Tabellini 2002; Besley 2006; Ashworth 2012).⁷

The paper is structured as follows. Section 2 provides background information regarding the 2006 Italian collective pardon bill and its political *salience* in the 2008 Elections. Section 3 presents the data. Section 4 discusses the empirical strategy. Section 5 reports the main results on voters' electoral response to the effects of the collective pardon bill. Moreover, Section 5 presents empirical evidence shedding light on the mechanism behind the main results. Section 6 discusses interpretations of the empirical results. Section 7 concludes. Appendix A presents a retrospective voting model providing a theoretical framework for the empirical analysis. Appendix B describes in details the database on crime-related news. Appendix C contains additional tables and figures which are also discussed in the main text.

2 The 2006 Italian Collective Pardon Bill

Our empirical analysis exploits variations in the incentives to commit a crime that follow from the provisions of the collective pardon law approved by the Italian Parliament in July 2006 (Law 241/2006).⁸ The policy was designed, proposed and implemented by the incumbent center-left (CL, henceforth) government coalition elected in the April 2006 elections. 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, also a part

⁷See also Ansolabehere *et al.* (2014) for evidence that state unemployment in the US robustly correlates with evaluations of national economic conditions, and presidential support. Our results are also consistent with the political science literature providing evidence from survey data showing that voters judge politicians on performance rather than on their policy stance (Lenz, 2013).

⁸Drago *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.

of the center-right (CR, henceforth) coalition voted for the pardon bill, a circumstance that we exploit in the empirical section to give empirical support for the accountability mechanism. The main reason that induced the CL coalition government to design such a law and propose it to the Italian parliament as one of its first policy measures was a prison overcrowding emergency, a problem faced by many other countries (including California or France) that recently had to enact some specific policy interventions.⁹ In the 1990s the Italian incarceration rate was constantly increasing while prisons and jails capacity remained substantially stable. Before the collective pardon the average overcrowding index was 131 inmates to 100 places in prison. For many years since the end of 90s, the Catholic Church, leftist parties and civic associations advocated laws alleviating the inhuman and degrading treatment in overcrowded jails.

The bill was approved on July 31, 2006 with immediate effects the day after. The main provisions of the collective pardon bill are the following. It granted a three years reduction in the length of detention for those who committed a crime before May 2, 2006. The exclusion of crimes committed after May 2 was announced at the beginning of the parliamentary debate of the pardon bill and rules out strategic behaviors of potential criminals during the months leading up to the approval of the law. The sentence reduction holds for a large number of offenses, including property, violent crimes, drug trafficking related offenses and white-collar crimes.¹⁰ Thus, as a first consequence of the pardon, an inmate convicted for a crime committed before May 2, 2006 was eligible for immediate release from prison as long as his residual sentence is 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. Indeed, 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 sanction equal to the residual sentence pardoned by the bill. Thus, as far as the residual pardoned sentence is as good as random, this conditional sentence suspension provided a random incentive to commit crime to former inmates. The

⁹See Lofstrom and Raphael (2013) for the case of California and Maurin and Ouss (2009) for the case of France.

¹⁰Mafia related crimes, children abuse and terrorism were excluded from the pardon.

following example helps clarifying 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 entered prison one year before the second and thus has a pardoned sentence of one year, while the second inmate has 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 decide to commit a burglary that has a legal sentence of 3 years, the first individual would be sentenced to four years in prison (3 years for the burglary plus 1 year residual sentence pardoned by the collective pardon bill), while the second individual would be sentenced to 5 years (3 years plus 2 years of residual sentence).

2.1 Political Salience of the 2006 Collective Pardon Bill and the 2008 Electoral Campaign

The July 2006 collective pardon bill put forward by the incumbent CL government represented a very salient issue for Italian voters up to the next (early) national elections in April 2008. Figure 1 summarizes the timing of elections and of the collective pardon bill.¹¹

The high salience of this issue was the combined result of three main facts. First, the sharp drop in the incarceration rate created by this policy (Figure 2), was followed by an increase in the overall number of crimes, as shown by Figure 3 (a 12.4% increase in crimes between June and December 2006 compared with the 0.35% increase in the previous semester and with the 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 the pardon 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,

¹¹Notice 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).

the space devoted to crime by national televisions substantially increased following the increase in crime resulting from the CL government’s decision to implement the collective pardon.¹² 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 salience of the collective pardon bill and of its perceived effects on crime is likely to have been detrimental for the incumbent CL government coalition for two main reasons. The most obvious one is that the government was the one who proposed, designed and then implemented the bill. Hence, in terms of political accountability, the CL coalition was the main political actor who was responsible for the effects of such a policy. At the same time, the “crime issue” is typically *owned* by rightist parties, since they are the ones perceived by voters as the most competent in managing it (Petrocik, 1996; Puglisi, 2011). Accordingly, the CR coalition was the one most likely to gain from an increase in the salience of crime (Belanger and Meguid, 2008; Aragonés *et al.*, 2015).

2.2 Conceptual Framework and Empirical Hypotheses

[Appendix A](#) presents a retrospective voting model formalizing the theoretical framework behind the voters’ response to the observed effects of the collective pardon. In particular, the effects of this policy may be seen as a combination of the quality/effectiveness of the collective pardon in deterring recidivism by pardoned inmates (which in turn is positively correlated with the overall quality of the CL government) and of a random shock at the municipal level (the random incentive to recidivate of pardoned inmates resident in their municipality). Hence, as long as voters cannot observe separately (and thus disentangle) these two effects, they should respond to the collective pardon by voting relatively less in favor of the CL coalition in municipalities where the random shock was more negative (i.e., in municipalities where the incentive to recidivate of

¹²The 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. That is, the higher *news pressure* due to the 2008 political events and electoral campaign is likely to have crowd 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 center-right government took office, i.e., between April and June 2008 (Demos-Unipolis, 2009).

pardoned inmates was higher).¹³ That is, the 2006-2008 increase in the electoral win margin of the CR coalition relative to the CL coalition should be higher in municipalities where the incentive to recidivate of pardoned individuals was higher (where their residual sentence was lower).¹⁴

At the same time, in terms of mechanism, voters are implicitly assumed to have information on the effects of the policy at the local (municipal) level and to form posterior beliefs—on the quality of the CL government—accordingly. Hence, a higher incentive of pardoned individuals to recidivate in a given municipality should also be associated with: *i*) worse observable effects of the policy (i.e. a higher recidivism rate of pardoned individuals resident in that municipality); *ii*) a higher probability of voters receiving a negative signal on the effects of the policy (i.e., a higher probability of newspapers reporting crime news involving pardoned individuals in that municipality); *iii*) worse beliefs of voters regarding the incumbent government (i.e., voters resident in that municipality more likely to report a worse evaluation on the center-left government crime policies and, overall, on the CL coalition).

3 Data

The empirical analysis builds upon several different dataset. The first dataset is on the characteristics of the prisoners released thank to the 2006 collective pardon bill. The data contains information on the municipality where each prisoner has his residency, the length of his residual sentence at the time of release, the length of his original sentence and the type of crime committed. Overall, the data contains information on the entire population of individuals pardoned by the pardon bill. However, while the information on the municipality of residence of each Italian released prisoners is informative of his official residence, the information on the residency of foreign prisoners is not a reliable proxy of their place of residence. Hence, we exclude from the

¹³Notice that the only crucial assumption is that voters cannot observe the average incentive to recidivate of pardoned inmates at the municipal level (nor, of course, the overall quality of the collective pardon bill or of the incumbent government). This is consistent with the issue under analysis. The average residual sentence of pardoned individuals at the municipal level was, to no extent, a publicly available information. Hence, in the 2008 elections, voters could not have inferred whether the observed effects of the policy in a given municipality was the result of a specific realized shock at the municipal level or of the overall effectiveness of the collective pardon in deterring recidivism.

¹⁴Section 6 discusses possible interpretations of the empirical results encompassing retrospective voting as well as alternative explanations.

sample all foreign pardoned individuals. Accordingly, to reduce measurement error, we exclude municipalities with only foreigner released prisoners. As a result, this final dataset is composed by 12,355 Italian pardoned individuals resident in 2,256 municipalities. The summary statistics of this data is 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 incentive to recidivate of pardoned individuals at the municipal level. This figure shows a substantial level of variation in the incentive to recidivate that is not correlated with any regional pattern (for example higher in the south or in the north or in any particular region). We will show that conditional on the average original sentence the variation in the incentive to recidivate is orthogonal to observable city characteristics. If we were to analyze cities with a very large number of pardoned inmates, we would not have had enough variation in our key variable. However, the 70 percent of cities have less than 3 pardoned individuals and the 90 percent have less than 9.

We then complement this dataset with a second one. Namely, the 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 prisoner). This data covers 7,159 municipalities and report information on the votes to political parties in the 2006 and 2008 elections. Both elections were subject to the same proportional electoral law.¹⁵ As we can see in Table 2B the CR and CL coalition lost some votes between the two elections, with the CR losing less than the CL. The difference in the win of margin of victory between 2008 and 2006 is on average 0.08 percentage points. One third of the municipalities in Italy had at least one pardoned individual. Table 2B summarizes the geographic, socio-economic and demographic characteristics at the municipal level that we use 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 crime

¹⁵The electoral law applying both to the 2006 and 2008 parliamentary elections was characterized by a proportional system, 26 electoral districts, and a majority premium granted to the electoral coalition obtaining the higher share of votes at the national level (for the lower chamber). The analysis focuses on the lower chamber as 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 (see section 5).

events involving pardoned individuals for the period August 1, 2006 (i.e., post-pardon) up to March 30, 2008 (i.e., up to 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. [Appendix B](#) provides detailed information on the construction of this dataset. In addition, we use survey-level data from *i*) the Italian National Elections Study Survey (ITANES) to gather information on voters' issue priority and on voters' evaluation the incumbent center-left government's crime policies; *ii*) the IPSOS *Polimetro* to obtain additional information on voters' issue priority (both in Italy and in the municipality where they live) and on voters' overall evaluation of the main CL and CR parties.¹⁶ In particular, the data from ITANES constitutes a post-election survey composed by around 2,800 individuals interviewed in the month after the 2008 elections. The data from the IPSOS *Polimetro* is composed by several waves of weekly and monthly interviews (for a total of around 28,000 interviews) starting after the 2008 elections up to December 2008. The summary statistics of these data are reported in [Table 2B](#).

4 Empirical Strategy: The Pardon Bill as a Natural Experiment

The empirical strategy exploits the 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 sentence commit much more crime than individuals with higher residual sentence. Crucially for our study is the source of the variation in the residual sentence. For the same original sentence, inmates have different residual sentences depending on the date of entry into prison. If the timing of entry is not systematically correlated to unobservables influencing the probability of committing a crime, the residual sentence is as good as random. Drago *et al.* (2009) provides evidence that observables are balanced with respect to the individual residual sentence, conditional on the original sentence. Here, in the next section, we show evidence consistent with absence of pre-trends in electoral outcomes and no systematic

¹⁶ITANES 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 company in Italy (<http://www.ipsos.it/>).

correlation between the average residual sentence and the observable characteristics at municipal level.

Our regression model is the following:

$$\Delta y_i = \alpha + \beta_1 \textit{incentive to recidivate}_i + \beta_2 \textit{original sentence}_i + \beta_3 \mathcal{I}_i + \beta_4 X_i + \beta_5 Z_i + \epsilon_i \quad (1)$$

where *incentive to recidivate* is a standardized measure of the average incentive to recidivate of pardoned individuals in municipality i .¹⁷ Δy is the difference in the political outcome of interest (i.e., the margin of victory of the CR coalition with respect to the CL coalition) between the national elections in 2008 and 2006. The variable *original sentence* indicates the average original sentence of former inmates resident in municipality i and \mathcal{I}_i represents a dummy indicating whether there is at least one pardoned individual resident in the same municipality.¹⁸ X_i is a vector of controls at the municipal level including the municipal crime rate in 2005, 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 the municipality population (per 1,000 inhabitants) and all other observable demographics 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, for violent crimes). For all the municipalities with $\mathcal{I}_i = 0$, i.e. municipalities with no pardoned individuals, all variables in Z_i as well as the original and the incentive to recidivate are set equal to zero.

Hence, in specification (1) the estimated coefficient β_1 measures the impact of one standard deviation increase in the average incentive to recidivate (i.e., around 8.2 less months of residual sentence) of former inmates from municipality i . The estimation of β_1 is obtained exploiting the variation in the average residual sentence for all municipalities with at least one pardoned

¹⁷We define the individual incentive to recidivate as 36 (the maximum pardoned residual sentence according to the design of the bill) minus the individual residual sentence (i.e., an individual with one month of residual sentence has an incentive to recidivate equal to 35, whereas an individual with a 35 months residual sentence has a incentive to recidivate equal to one).

¹⁸In 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 with less than 500 inhabitants. All results are robust to including these municipalities in the analysis and they are available upon request to the authors.

individual. We keep all municipalities (also those with $\mathcal{I}_i = 0$) because in the estimation these contribute to estimating the residual variance of the set of variables X_i that is used to estimate our coefficient of interest.

Finally, while, later on, we show that our key variable (the average incentive to recidivate) predicts recidivism at the municipal level (Table 7), we do not use it as an instrument for the crime rate at the local level because the exclusion restriction would easily 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) and in turn voters' welfare and electoral outcomes (Drago and Galbiati, 2012). This failure of the exclusion restriction may be exacerbated if the effect of the incentive to recidivate on the overall crime rate is mediated by the news media and if this has an impact on the electoral outcome. Hence, we see 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.¹⁹

4.1 Balancing tests and pre-trends

The specification (1) is a reduced form model estimating the effects of the costs imposed by the collective pardon to voters on the electoral outcomes. Our key identifying assumption is that conditional on the average original sentence of pardoned inmates and the municipality indicator \mathcal{I}_i , the incentive to recidivate is orthogonal to unobservable characteristics. Tables 3A, 3B and 3C present results consistent with the idea that the incentive to recidivate is exogenous. Specifically, in these tables we regress our main variable on each of the variables X_i while, consistently with specification 1, controlling for the characteristics of pardoned individuals at the municipal level.²⁰

As expected, the dummy \mathcal{I}_i and the original sentence predict the incentive to recidivate: the first is positive because we set the incentive to recidivate equal to zero when $\mathcal{I}_i = 0$, while

¹⁹It 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 incentive to recidivate 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. Indeed, when excluding municipalities more likely to be at risk of recidivism by individuals not resident in that municipality (e.g., provincial capital cities) the estimates are typically larger than the baseline ones.

²⁰In fact, controlling or not for the characteristics of pardoned individuals does not change the results.

the second is negatively correlated with the incentive to recidivate since we obviously have that a larger original sentence is associated with a lower incentive to recidivate (larger residual sentence). Indeed, the residual sentence is bounded from above and it is always lower than the original sentence. Most importantly, none of the geographical, socio-economic or demographic variables X_i are significantly correlated with the incentive to recidivate. In particular, it is worth remarking that the crime rate in 2005 and the number of pardoned individuals per 1,000 residents are both orthogonal to the incentive to recidivate.

As for the presence of the pre-trends, in Table 4 we run a “placebo” specification where we use the main dependent variable (the differences between the win margin 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. The dependent variables are pre-determined with respect to the effect of the average residual sentence. If the incentive to recidivate were to pick-up some existing trends in voters’ behavior, Table 4 should have shown a significant impact on the incentive to recidivate on pre-2008 voting patterns. Instead, the results are consistent with the notion that the average incentive to recidivate of pardoned individuals released in August 2006 is orthogonal to any pre-trend in the votes to political coalitions in the previous elections. In fact, the point estimates not only are imprecisely estimated but more importantly they are very low in magnitude (compared to the estimates from our main regression, see below Table 5).

5 Results

5.1 Voters’ Electoral Response

Table 5 illustrates the main results. We estimate variations of equation (1) with ordinary least squares by including as dependent variable 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. In all the specifications we cluster standard errors at the provincial level. In this table we show results excluding and including municipalities with no pardoned prisoners. As it is clear from Table 5 the incentive to recidivate, relative to the

national election in 2006, has a positive effect on the margin of victory of the CR coalition.²¹ The effects are precisely estimated and imply (in our preferred specification in column (4) where we consider the sample of all municipalities and include municipal level controls) that a one standard deviation increase in the incentive to recidivate (around 8.2 months less in the average residual sentence) leads to a 0.25 percentage points increase in the margin of victory of the center right coalition, corresponding to a 3 percent increase in its margin of victory. This overall effect seems to be driven by the combined positive effect of the incentive to recidivate on the increase in the votes (per eligible voters) of the CR coalition and negative effect on the ones of the CL coalition. As reported in Tables C.2 and C.3 in Appendix C, relative to the election in 2006, in 2008 the votes for the center-right coalition increase where the incentive to recidivate is higher and votes for the CL coalition decrease where the incentive is higher.

There is another important piece of evidence that is consistent with our conceptual framework. As discussed in Section 2, the CL coalition representing the incumbent government was clearly the one responsible for proposing, designing and implementing the bill. However a part of the CR coalition ended up voting in favor of it.²² We exploit the circumstance that some center-right MPs might have also been held accountable by voters for the realized effects of the policy. According to our framework the response to the effect of the policy should favor less the CR coalition in electoral districts where the percentage of CR candidate voting for the pardon was higher. In order to test this hypothesis, we gathered data from the Italian Minister of Internal Affairs regarding the identity of all CR candidates in each electoral district in 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.²³ Finally, we computed for each electoral district (typically sub-regional entities), the percentage of candidates of the main center-right party (i.e., PDL) in the 2008 elections who voted in favor

²¹This effect is essentially the same when we control for the number of pardoned individuals non-parametrically (i.e. by including 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).

²²According to some policy reports (Eurispes, 2007), part of the CR voted in favor of the bill due to the fact that the pardon was extended to white-collar criminals (e.g., convicted for financial or tax-evasion crimes) who accounted for a very limited fraction of released prisoners.

²³MP voting records are available at: http://www.camera.it/_dati/leg15/lavori/stenografici/sed033/v002.pdf.

of the collective pardon bill in July 2006.²⁴ In Table 6 we present the results from our main specification interacting our main explanatory variable on the incentive to recidivate with the percentage of candidates of the CR coalition who voted in favor of the collective pardon. In these specifications we control for any selection of particular CR candidates into districts with electoral district fixed effects. In fact, selection in this case may be relevant especially for CR candidates who voted for the pardon bill.²⁵ Table 6 shows a negative and significant coefficient of this interaction term on our main outcome of interest, i.e., the variation in 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, as also shown in column (2) and (3), in districts where more candidates of the main opposition parties ended up voting 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 in the incentive to recidivate at the municipal level implies an increase in the CR win margin of 14.2 percent in districts where none of the CR candidates voted in favor of the pardon. When we consider for the cities with a least one pardoned individual the median level of the percentage level of CR MP (0.1538) who voted for the bill, we get the same result we have in Table 5.

Hence, consistent with the hypothesis put forward in Section 2 and with the conceptual framework in Appendix A, we observe that the CR coalition - the opposition coalition at the time when the pardon was approved - experienced an increase in its electoral support relative to the CL, in municipalities where the incentive to recidivate is higher. Moreover, this effect decreases with the presence of CR MP candidates who voted for the pardon. In the next section we explore the underlying mechanism generating these results.

²⁴The electoral law allowed only to express a preference for a party but not for a specific candidate. Hence, voters willing to hold accountable a candidate for her/his voting record on the pardon could have done so only through their voting choice pro/against the party she/he belonged to. The percentage of center-right candidates (of the main party) in a district who voted for the pardon goes from zero up to around 26% (on average 17.5%, with a standard deviation of 6%). Results are robust to excluding the districts in the “tail” of the distribution of candidates who voted for the pardon (i.e., the ones with a percentage equal to zero or above 25).

²⁵Controlling for electoral district fixed effects improves the precision of the estimates of the interaction term. Without electoral fixed effects the interaction terms remains negative but with larger standard errors.

5.2 The Mechanism

We now turn to the mechanism linking the idiosyncratic component in effects of the policy (the incentive to recidivate of pardoned individuals) with the voters' observed behavior in the 2008 elections. In particular, the following results show that the idiosyncratic incentives of pardoned individuals to recidivate generated variation in the observed recidivism rate at the municipality level. A higher average incentive to recidivate translated also into *i*) more crime-related news involving pardoned individuals at the municipal level and *ii*) a worse evaluation of the incumbent government.

Effects of the public policy. The first and immediate effect of the policy is 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.²⁶ Table 7 shows how the incentive to recidivate of pardoned individuals does indeed affect the observed recidivism at the municipal 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 created by the design of the pardon translates into different recidivism rates at the municipal level. Hence, in municipalities where the average incentive to recidivate of pardoned individuals is higher, the collective pardon bill translates into worse policy effects (higher recidivism rate). The effect is not trivial; a one standard deviation increase in the incentive to recidivate implies a 15.9 percent increase in the recidivism in a municipality with at least one pardoned individual (which is consistent with Drago *et al.* 2009).

Voters' Information. We now turn to look at the effects of the incentive to recidivate on the information available to voters' about local recidivism. This exercise is helpful since voters' evaluation of the consequences of the collective pardon crucially depends on the information they receive about recidivism. In Table 8 we report the results on how the pardoned individuals' incentive to recidivate maps directly into the amount of news in a municipality related to crime involving pardoned individuals. As explained in Appendix B, these are news containing any word related to crimes (theft, robbery, extortion, scam, murder, drug, burglary, beatings, do-

²⁶As 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 the recidivism measured two years later. In fact, the estimates from Mastrobuoni and Rivers (2016) - that we use in Section 5.3 - show that the effect of the residual sentence is persistent at least up to 17 months after the pardon.

mestic violence, rape, etc.) and containing at the same time words immediately identifiable with the collective pardon. Keeping constant the number of pardoned individuals per capita present in a municipality and all the other characteristics of former inmates, the higher the incentive to recidivate of pardoned individuals resident in that municipality, the more likely that newspapers reported at least one crime-news involving pardoned individuals in the post-pardon period up to the 2008 elections.²⁷ Therefore, the different policy effects of the collective pardon bill across municipalities due to the idiosyncratic incentives of pardoned individuals to recidivate, translate into voters receiving different information on the effects of such a policy in different municipalities. In particular the coefficients imply that one standard deviation increase in our key variable implies a 1.1% higher probability of having newspapers reporting at least one crime-news involving pardoned individuals. While this effect does not seem large, we argue that news media are not necessarily the only channel of information on the effects of the public policy for voters. For example, voters may also receive a private signal via a direct experience (e.g., being a victim of a crime committed by a pardoned individual) or an indirect one (e.g., 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 incentive to recidivate affects recidivism and media coverage of their crimes at the municipal level, we now look at how the average incentive to recidivate affects voters' perceptions about the incumbent CL government. Using the responses to the ITANES post-electoral survey, in Table 9 we look at the effects of the incentive to recidivate on voters' perceptions about the CR and CL coalitions' competences to deal with crime. Table 9 points out that individuals living in municipalities where pardoned individuals have a higher average incentive to recidivate are more likely to report a worse evaluation of the CL incumbent government crime control policies and, in general, of the ability of the center-left to deal with crime. The coefficient reported in column 2 implies a 1.66 percentage points (4.14%) higher probability of reporting an overall negative evaluation of the CL coalitions crime control interventions following a one standard

²⁷Appendix B provides some examples of this type of news.

deviation increase in the incentive to recidivate.²⁸ Using the IPSOS polimetro survey we are also able to investigate whether the negative perception of the CL incumbent in dealing with crime is also associated with a general more negative evaluation of the CL. Table 10 shows that when the incentive to recidivate is higher, voters are also more likely to have an overall negative evaluation of the main CL party (i.e., Partito Democratico). Finally, Tables 9 and 10 do not provide evidence on the presence of any significant effect of the incentive to recidivate on the probability of individuals perceiving crime as the most important political issue either in Italy or in the municipality where the respondent lives.

5.3 Crime & Votes

In this section we provide a back-of-the-envelope calculation of the implied effect of one additional crime by a pardoned individual (i.e., one more recidivist) on the votes gained by the center-right coalition relative to the center-left one. While such calculation should be taken with caution, it may provide a useful assessment of the implied magnitude of our effects.²⁹

In order to obtain this implied effect, first we need to compute the average number of pardoned individuals committing a crime (i.e., recidivating) in a municipality. Then, we assess how the incentive to recidivate (i.e., the random component of the policy) affects such number. Finally, we relate the variation in the random component of the policy needed to induce one more recidivist with its corresponding impact in terms of votes.

The average number of pardoned individuals resident in a municipality is 5.48. Mastrobuoni and Rivers (2016) show that there is a 22% average recidivism rate of pardoned inmates up to 17 months after the pardon.³⁰ This implies that, in an average municipality, there are 1.2 individuals who re-committed a crime after the pardon (5.48×0.22). Table 7 shows that a one standard deviation increase in the incentive to recidivate increases the average recidivism rate by

²⁸Notice that the questions regarding the performance of the previous center-left government in dealing with crime and whether the CL or the CR are best suited to deal with crime, are only asked to the subsample of individuals who state that crime is the most important issue that the government should face in Italy.

²⁹We compute such magnitude for the subset of cities with at least one pardoned individual. Indeed, 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.

³⁰Our data on recidivism cover only a period up to seven months after the pardon, i.e., the information on the 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 have a more meaningful figure of the average number of pardoned individuals who recidivate over the period of interest.

15.9%. As a consequence, a one standard deviation increase in the incentive to recidivate would lead to 0.19 more pardoned individuals recidivating in the average municipality (0.159×1.2). Or, put it differently, a 5.3 standard deviations increase in the incentive to recidivate would lead to one more crime by pardoned individuals in such municipality. Given that the average gap of votes in favor of the center-right coalition in 2008 was equal to 1,702 and that a one SD increase in the incentive to recidivate leads to a 3% increase in the win margin of the center-right coalition (52 votes), this translates in one more crime by a pardoned inmate generating a gain of 272 votes for the center-right coalition relative to the center left one. Since the average number of eligible voters in 2008 was equal to 15,355, this corresponds to an additional crime leading to a relative gain for the center-right of 1.77% in terms of the overall pool of eligible voters (or, viceversa, a relative loss of 1.77% for the center-left).

6 Explanations

The evidence reported above shows that, in a given municipality, an increase in the incentive to recidivate for resident pardoned inmates is associated to a worse electoral performance of the incumbent in the parliamentary elections immediately following the pardon. Hereafter we discuss two alternative explanations for these findings. The first is based on multi-dimensional voting and salience; the second and our preferred interpretation is based on forward-looking retrospective voting theory.

The multi-dimensional voting and issue salience interpretation implies that an increased salience of crime might have favored CR parties, who are typically perceived as the most competent on this issue (Petrocik, 1996; Puglisi, 2011). As we discussed in Section 2, the collective pardon bill was a very salient political issue up to the 2008 elections, if this translated into a general increase in crimes salience our results could be then explained by a salience driven increase in the support for right-wing parties. This is a mechanism suggested by models of multi-dimensional voting (Belanger and Meguid 2008; Aragonés *et al.* 2015). However, while this mechanism is consistent with the overall results, it does not seem to square with three other pieces of evidence. First, as shown by Table 6, the gain that the center-right coalition obtained in cities where the (negative) effects of the pardon were more salient was lower in districts where

more center-right candidate voted in favor of the pardon. Hence, the salience of the crime issue induced by the collective pardon did not translated in an overall higher support for the center-right but this higher support was conditional on the past stance of center-right candidates with respect to such a policy. Second, the perception on the salience of the crime issue reported in the two survey data analyzed in Section 5.2 do not seem to suggest any impact of the incentive to recidivate on the probability of voters perceiving crime to be the most important issue either in Italy (Table 9 and Table 10) or in the municipality where they live (Table 10). Finally, if the increase in the salience of crime favored right-wing parties in general, we should find some effect of our main variable of interest not only in national parliamentary elections but also in other elections. Table C.4 in Appendix C tests this implication. Results show that the incentive to recidivate did not have any impact on voter behavior in European Elections (2009 vs. 2004). Hence, it does not seem that CR parties experienced an overall (relative) political gain where the realized effects of the policy were likely to be worse.

As for the forward-looking retrospective voting explanation, the key mechanism underlying modern theories of electoral accountability (e.g., Fearon 1999; Persson and Tabellini 2002; Besley 2006; Besley and Prat 2006; Ashworth 2012; Ashworth *et al.* 2016) relies on two main elements.³¹ First, a politician’s past action should provide information about her future behavior (i.e., 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). As suggested by Fearon (1999), rational voters are concerned with selecting high quality politicians because such politicians are expected to provide good future outcomes. The results presented in the paper seem to be consistent with this mechanism. The design of the collective pardon bill created idiosyncratic incentives to recidivate across pardoned individuals. These individual incentives created different policy effects across municipalities. Municipalities where the average incentives to recidivate of resident pardoned individuals were higher experienced a higher recidivism rate (Table 7). The higher incentive to recidivate also translated in newspaper being more likely to report crime news involving pardoned individuals (Table 8). Hence, this suggests that voters living in different municipalities had different probabilities of receiving a negative *private signals* on the policy effects of the

³¹See Barro (1973) and Ferejohn (1986) for earlier retrospective voting models.

pardon. Most importantly, these probabilities were correlated with the idiosyncratic incentives to recidivate created by the design of the policy. The evidence concerning the voters' evaluation of the incumbent government's crime policies (Table 9) and regarding the overall perceived quality of the main CL party (Table 10), is consistent with a mechanism where voters updated their beliefs on the incumbent government's quality (*type*) according to the observed effects of the policy. Hence, the main results shown in Section 5 are consistent with a retrospective voting model where voters receive private signals, form posterior beliefs and then keep the incumbent government accountable, according to the observed effects of the public policy implemented by the government. Appendix A presents a simple retrospective voting model formally illustrating such mechanism in the context of our empirical setting.

Overall, we find the data to be most consistent with the second interpretation, that is voters use the information on the observed effects of the incumbent government policy choices to update their beliefs on the incumbent's type and then condition their voting behavior on such updated beliefs.

7 Conclusions

While politicians and elected officials exert a lot of effort to show their commitment to be effective in crime control (for instance by being tougher on crime when elections approach, Levitt 1997), we know very little 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 effects of their actions on crime rates?

In this paper we provide causal evidence about voters' reaction to the consequences of a national governmental criminal justice policy intervention. Our exercise shows how voters responded to the local consequences of the 2006 collective pardon bill in Italy. The Italian case-study has a series of desirable features since it allows 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 about the government's attitudes at the national level, 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 incentives to recidivate of resident pardoned individuals were higher, experienced a higher recidivism rate. At the same time, a higher incentive to recidivate at the municipal level lead to: *i*) newspapers being more likely to report crime news involving pardoned individuals; *ii*) voters holding worse beliefs on the incumbent national government. Exploiting these features of the collective pardon bill, our main results provide causal evidence of voters keeping the incumbent governments 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 incumbent national government.

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 evidence about voters holding politicians accountable for the consequences of their policies. Our analysis suggests that voters receive private signals and hold beliefs on incumbent politicians that are consistent with the effects of public policies. Ultimately, voters keep incumbent politicians accountable by conditioning their vote on the observed effects of their 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	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

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)	7159	.01	.01	0	.37
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

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

	(1)	(2)	(3)	(4)	(5)
	Incentive to recidivate	Incentive to recidivate	Incentive to recidivate	Incentive to recidivate	Incentive to recidivate
Municipal area (squared km)	-0.0000 (0.0001)				
Latitude		-0.0025 (0.0025)			
Longitude			0.0018 (0.0027)		
Landlocked municipality				0.0216 (0.0282)	
Montaneous municipality					-0.0022 (0.0065)
Pardoned individuals per 1,000 residents	0.0082 (0.0665)	0.0075 (0.0664)	0.0100 (0.0660)	0.0076 (0.0664)	0.0107 (0.0663)
Average original sentence	-0.0154*** (0.0013)	-0.0154*** (0.0013)	-0.0154*** (0.0013)	-0.0154*** (0.0013)	-0.0154*** (0.0013)
City with at least one pardoned individual	3.2852*** (0.1325)	3.2811*** (0.1321)	3.2813*** (0.1324)	3.2892*** (0.1316)	3.2825*** (0.1331)
Pardoned individuals controls	YES	YES	YES	YES	YES
Observations	7,159	7,159	7,159	7,159	7,159
R-squared	0.8517	0.8517	0.8517	0.8517	0.8517

Notes. Entries are coefficients from the equation model estimated with OLS. Pardoned individuals control include: average age of pardoned individuals, 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. 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)	(2)	(3)	(4)
	Incentive	Incentive	Incentive	Incentive
	to recidivate	to recidivate	to recidivate	to recidivate
Crimes per capita in 2005	0.2771 (0.4152)			
Taxable income per capita (2008)		0.0000 (0.0000)		
Private sector employees per capita, in 2001			0.0037 (0.0379)	
Municipal unemployment rate, in 2001				0.0288 (0.0797)
Pardoned individuals per 1,000 residents	0.0097 (0.0666)	0.0112 (0.0676)	0.0099 (0.0670)	0.0082 (0.0672)
Average original sentence	-0.0154*** (0.0013)	-0.0154*** (0.0013)	-0.0154*** (0.0013)	-0.0154*** (0.0013)
City with at least one pardoned individual	3.2835*** (0.1326)	3.2845*** (0.1328)	3.2839*** (0.1328)	3.2820*** (0.1319)
Pardoned individuals controls	YES	YES	YES	YES
Observations	7,159	7,159	7,159	7,159
R-squared	0.8517	0.8517	0.8517	0.8517

Notes. Entries are coefficients from the equation model estimated with OLS. Pardoned individuals control include: average age of pardoned individuals, 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. 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)	(2)	(3)	(4)
	Incentive to recidivate	Incentive to recidivate	Incentive to recidivate	Incentive to recidivate
Municipal population, in 2001	0.0000 (0.0000)			
Share of population older than 65, in 2001		-0.0555 (0.1069)		
Share of population between 20-34, in 2001			0.0638 (0.2963)	
Share of population with diploma laurea, in 2001				0.1482 (0.1041)
Pardoned individuals per 1,000 residents	0.0107 (0.0665)	0.0110 (0.0658)	0.0101 (0.0657)	0.0192 (0.0679)
Average original sentence	-0.0154*** (0.0013)	-0.0154*** (0.0013)	-0.0154*** (0.0013)	-0.0154*** (0.0013)
City with at least one pardoned individual	3.2800*** (0.1325)	3.2805*** (0.1311)	3.2823*** (0.1316)	3.2814*** (0.1330)
Pardoned individuals controls	YES	YES	YES	YES
Observations	7,159	7,159	7,159	7,159
R-squared	0.8517	0.8517	0.8517	0.8517

Notes. Entries are coefficients from the equation model estimated with OLS. Pardoned individuals control include: average age of pardoned individuals, 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. 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	-0.0013 (0.0017)	-0.0005 (0.0015)	-0.0013 (0.0017)	0.0001 (0.0015)
Pardoned individuals controls	YES	YES	YES	YES
Municipal level controls	NO	YES	NO	YES
Only municipalities with at least one pardoned	YES	YES	NO	NO
Observations	2,252	2,252	7,139	7,139
R-squared	0.0461	0.1506	0.0131	0.1550

Notes. Entries are coefficients from the equation model estimated with OLS. 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 (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: Voters' Response to the Effects of the Collective Pardon

	Δ Win Margin 2008-2006 Center-right vs. Center-left			
	(1)	(2)	(3)	(4)
Incentive to recidivate	0.0030** (0.0013)	0.0029** (0.0011)	0.0030** (0.0013)	0.0025** (0.0012)
Pardoned individuals controls	YES	YES	YES	YES
Municipal level controls	NO	YES	NO	YES
Only municipalities with at least one pardoned	YES	YES	NO	NO
Observations	2,256	2,256	7,159	7,159
R-squared	0.0785	0.2305	0.0278	0.1217

Notes. Entries are coefficients from the equation model estimated with OLS. 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 (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' Response to the Effects of the Collective Pardon

	Δ Win Margin 2008-2006 Center-right vs. Center-left			
	(1)	(2)	(3)	(4)
Incentive to recidivate	0.0124*** (0.0031)	0.0112*** (0.0031)	0.0129*** (0.0032)	0.0116*** (0.0031)
Incentive to recidivate \times % CR cand. who voted for pardon	-0.0589*** (0.0154)	-0.0504*** (0.0162)	-0.0603*** (0.0160)	-0.0535*** (0.0159)
Municipality with at least one pardoned \times % CR cand. who voted for pardon			0.1757*** (0.0539)	0.1683*** (0.0545)
Pardoned individuals controls	YES	YES	YES	YES
Municipal level controls	NO	YES	NO	YES
Only municipalities with at least one pardoned	YES	YES	NO	NO
Observations	2,256	2,256	7,159	7,159
R-squared	0.3790	0.4458	0.3512	0.3753

Notes. Entries are coefficients from the equation model estimated with OLS. 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 (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: Incentive to Recidivate and Recidivism

	Share of Recidivists			
	(1)	(2)	(3)	(4)
Incentive to recidivate	0.0142** (0.0057)	0.0140** (0.0057)	0.0142** (0.0057)	0.0141** (0.0057)
Average original sentence - pardoned	0.0003+ (0.0002)	0.0002+ (0.0002)	0.0003+ (0.0002)	0.0002+ (0.0002)
Pardoned individuals controls	YES	YES	YES	YES
Municipal level controls	NO	YES	NO	YES
Only municipalities with at least one pardoned	YES	YES	NO	NO
Observations	2,256	2,256	7,159	7,159
R-squared	0.0369	0.0419	0.1324	0.1347

Notes. Entries are coefficients from the equation model estimated with OLS. 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 (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			
	(1)	(2)	(3)	(4)
Incentive to recidivate	0.0018** (0.0007)	0.0014** (0.0007)	0.0018** (0.0007)	0.0015** (0.0007)
Pardoned individuals controls	YES	YES	YES	YES
Municipal level controls	NO	YES	NO	YES
Only municipalities with at least one pardoned	YES	YES	NO	NO
Observations	2,256	2,256	7,159	7,159
R-squared	0.0122	0.1871	0.0523	0.1409

Notes. Entries are coefficients from the equation model estimated with OLS. 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 (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 (0.0016)	0.0166** (0.0071)	-0.0008*** (0.0006)	0.0020 (0.0080)
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	0.0696	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 (2005). Observation are weighted according to the sample political weights provided by ITANES. 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 (0.0010)	0.0005 (0.0012)	0.0019** (0.0009)	0.0005 (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	0.0741	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 (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

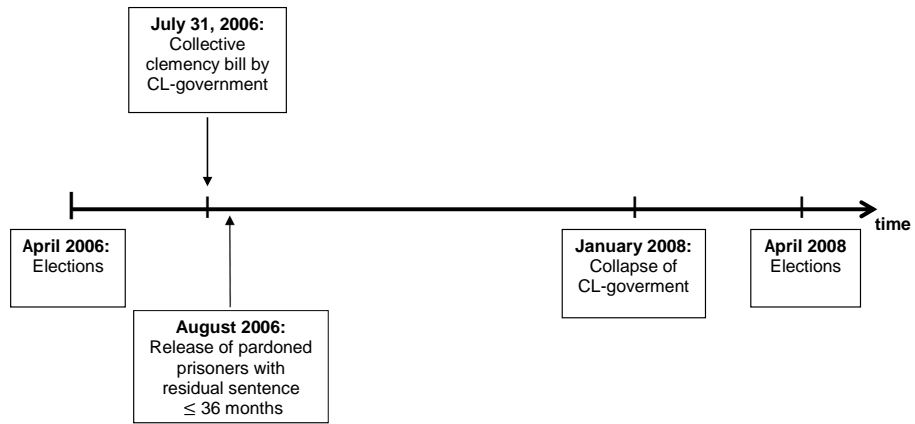
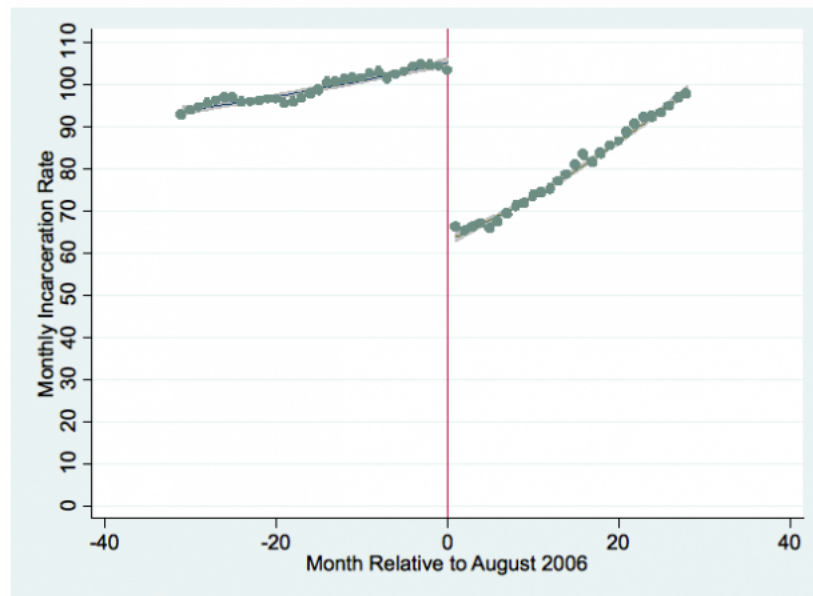
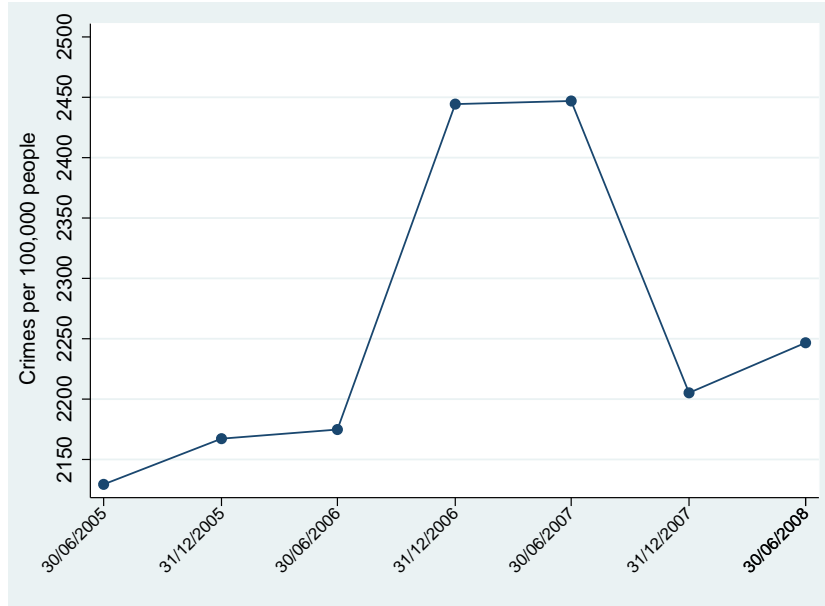


Figure 2: Incarceration rate



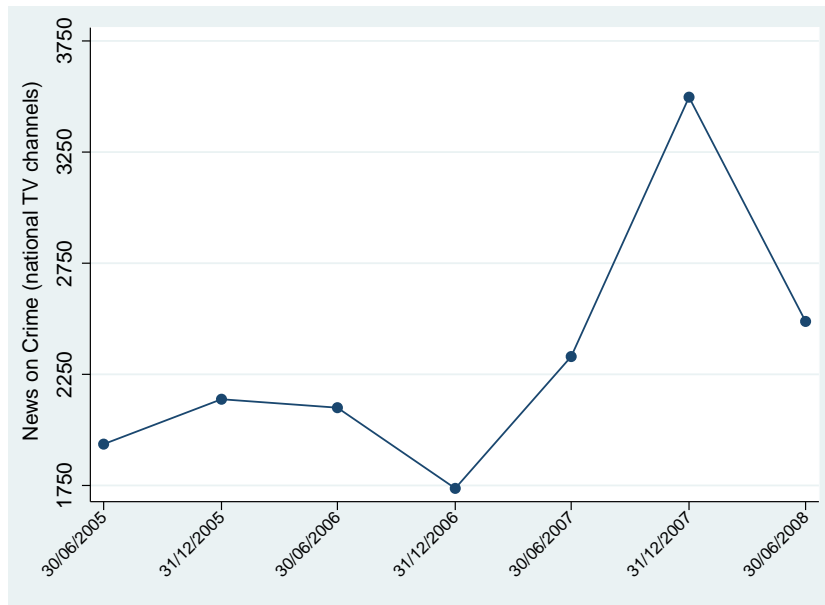
Notes: The figure illustrates the variation in the incarceration rate (i.e., per 100,000 people) in Italy before and after the collective pardon bill.

Figure 3: Crimes per 100,000 people



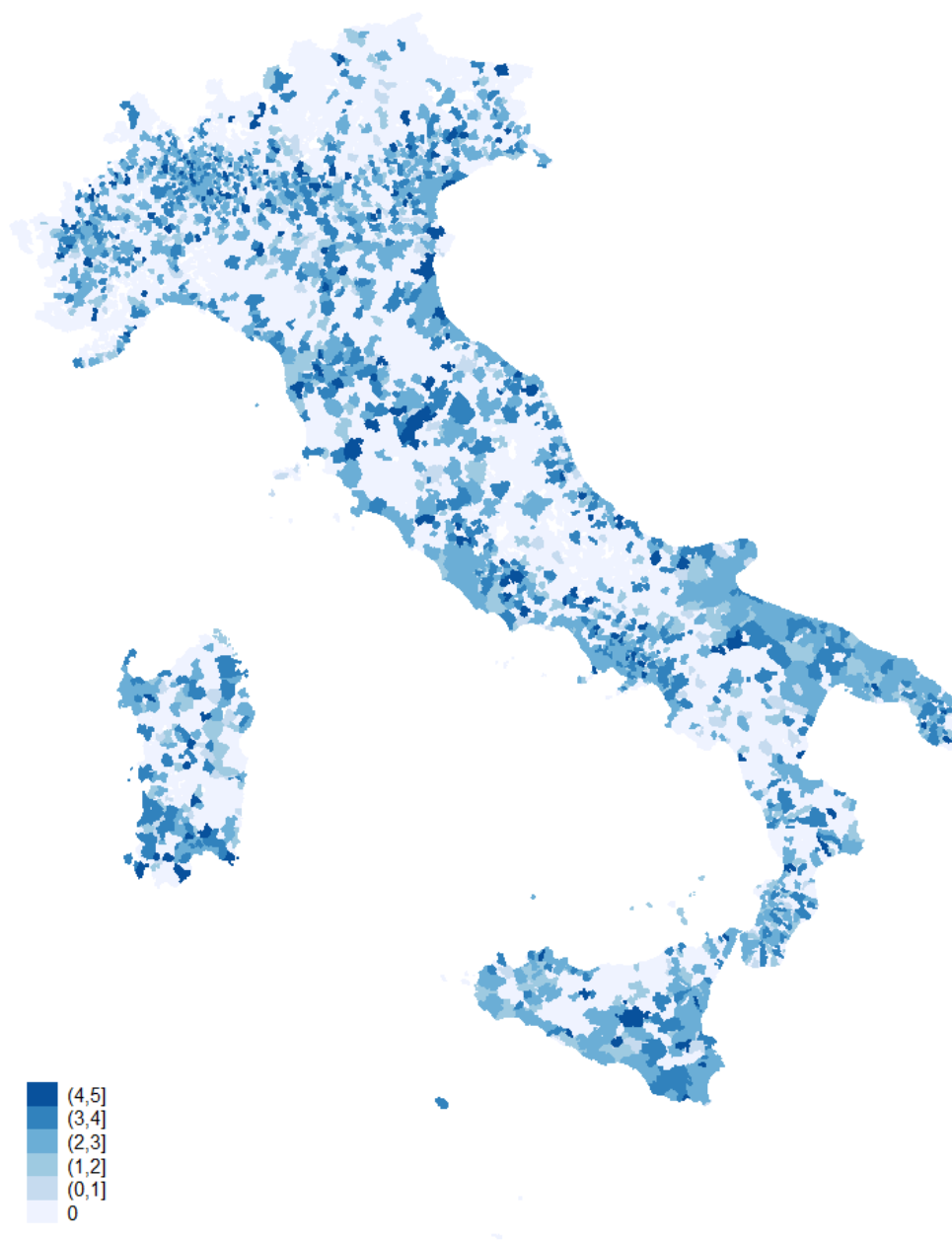
Notes: The figure illustrates the variation in the total number of crimes per 100,000 people in Italy between the first semester of 2005 and the first semester of 2008.

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 one standard deviation increase in the incentive to recidivate (i.e., around 8.2 months less of residual sentence).

Appendix A Theoretical Framework.

In order to better frame the theoretical mechanism behind our empirical results, this section presents a retrospective probabilistic voting model linking voters and politicians. There is a continuum of voters of measure one. Voters' payoffs are additive over two periods and there is no discounting. In period one, an incumbent of type (ability) θ_I is in office. In period two, voters may decide to vote for the incumbent or elect a challenger. In each period t where a politician is in office, she implements a policy g_t . The value of such policy is heterogeneous across municipalities and depends on the politician's ability $\theta \in [0, 1]$ and on a random shock at the municipality level $\kappa > 0$. Specifically, the policy g_t implemented by a politician with ability θ may take two values for each municipality i , $g_{t,i} \in \{0; 1\}$ where:

$$\mathbb{P}(g_{t,i} = 1 | \theta, \kappa_i) = \theta^{\kappa_i}$$

The incumbent's ($\theta_I^{\kappa_i}$) and the challenger's ($\theta_C^{\kappa_i}$) probability of implementing a high-value policy are *i.i.d.* across politicians and across municipalities, and uniformly distributed in $[0, 1]$. Hence, the probability of observing a positive impact of the policy in period t (i.e., $g_{t,i} = 1$) is positively correlated with the ability of the politician in office.³²

The expected utility of voter v at time t in municipality i simply depends on the effects of the policy at time t , $U_{v,i}^t = E(g_{t,i})$. Voters do not know either θ or κ_i . They just observe whether $g_{t,i} = 1$ or $g_{t,i} = 0$. For example, $g_{t,i} = 1$ may indicate that voters in municipality i observed a low recidivism level of pardoned individual. Hence, voters see the impact of the policy at the local level, which is positively correlated with the true state of the world (i.e., with the politician's type θ) and may use this information to update their beliefs on the politician's type. Notice that while voters are aware that the observed effect of the policy is influenced by an (unobserved) shock at the municipality level, κ_i (e.g., the random incentive to recidivate of pardoned inmates resident in municipality i), at the same time they take into account that the realized value of the policy depends also on the incumbent's type (e.g., on the quality/effectiveness of the design of the policy bill in deterring recidivism by pardoned inmates). More formally, since in period one the incumbent is in office, for voters in municipality i , $g_{1,i}^I$ represents an informative signal

³²Notice that framework immediately generalizes to the introduction of an additional random noise (at the national level) in the policy realization.

on the incumbent's type (the voters' Bayesian updating is based on the standard Beta-binomial model). Then, in period two, they may decide to vote for the incumbent or elect a challenger, based on their posterior beliefs on the incumbent's type upon having observed $g_{1,i}^I$. In particular, the expected utility, at time $t = 2$, of a voter v in municipality i from electing the incumbent is:

$$U_{v,i}^I(g_{2,i}^I, g_{1,i}^I) = E(g_{2,i}^I | g_{1,i}^I) + \beta_v$$

where β_v is an idiosyncratic preference shock about the incumbent that affects the utility of voter v when the incumbent is in office. β_v is *i.i.d.* across voters and uniformly distributed in $[-1; 1]$. On the other hand, the expected utility of a voter v in municipality i from electing the challenger is simply:

$$U_{v,i}^C(g_{2,i}^C) = E(g_{2,i}^C) = \frac{1}{2}$$

Hence, at $t = 2$, a voter v in municipality i would prefer to vote for the incumbent rather than for the challenger if and only if:

$$\beta_v \geq \frac{1}{2} - E(g_{2,i}^I | g_{1,i}^I)$$

Hence, it is possible to derive some simple empirical predictions by interpreting the random shock at the municipality level affecting the observed quality of the incumbent's government policy at $t = 1$ (i.e., κ_i) as being determined by the random incentive to recidivate of pardoned individuals in municipality i . Specifically, in municipalities where pardoned individuals have a high incentive to recidivate (i.e., where $\kappa_i > 1$) with respect to municipalities where pardoned individuals have a low incentive to recidivate (i.e., where $\kappa_i < 1$):³³

1. Since $\mathbb{P}(g_{t,i} = 1 | \theta, \kappa_i) = \theta^{\kappa_i}$, for any given θ voters are less likely to observe a positive policy outcome ($g_{1,i}^I = 1$) rather than a negative one ($g_{1,i}^I = 0$). Hence, the recivism rate of pardoned individuals should be higher and news media should be more likely to report news on crime events related to pardoned individuals.
2. In turn, since $E(\theta^{\kappa_i} | g_{1,i}^I = 1) > E(\theta^{\kappa_i} | g_{1,i}^I = 0)$, voters are more likely to hold worse posterior beliefs on the incumbent's type. Hence, the perceived competence of the incumbent

³³Notice that the only crucial assumption is that voters cannot observe κ_i (nor, of course, θ). This is consistent with the issue under analysis. The average residual sentence of pardoned individuals at the municipal level was, to no extent, a publicly available information. Hence, in the 2008 elections, voters could not have inferred whether the observed effects of the policy were due to θ or κ_i .

government coalition should be lower.

3. Finally, since $E(g_{2,i}^I | g_{1,i}^I = 1) > E(g_{2,i}^C) > E(g_{2,i}^I | g_{1,i}^I = 0)$, voters are less likely to re-elect the incumbent.³⁴ Hence, the win margin of the challenger (center-right coalition) should be higher in the 2008 elections.

³⁴In particular, $E(g_{2,i}^I | g_{1,i}^I = 1) = \frac{2}{3}$, $E(g_{2,i}^C) = \frac{1}{2}$, and $E(g_{2,i}^I | g_{1,i}^I = 0) = \frac{1}{3}$. Hence, if we denote a voter v resident in municipality i as v_i , then:

$$\mathbb{P}(v_i \text{ votes } I | g_{1,i}^I = 1) = \mathbb{P}\left(\beta_v \geq \frac{1}{2} - E(g_{2,i}^I | g_{1,i}^I = 1)\right) = \frac{7}{12}.$$

Instead,

$$\mathbb{P}(v_i \text{ votes } I | g_{1,i}^I = 0) = \mathbb{P}\left(\beta_v \geq \frac{1}{2} - E(g_{2,i}^I | g_{1,i}^I = 0)\right) = \frac{5}{12}.$$

Appendix B Dataset on crime-related news.

The data on crime-related news is extrapolated from the *Factiva* database. We extrapolated news on crime-related events from all the Italian news sources present in the database with regular frequency over the period of interest (2006-2008). These sources include some of the main Italian national and regional newspapers (*Corriere della Sera; La Repubblica; La Stampa; Il Giornale; Il Giorno; Il Resto del Carlino; La Nazione*) and the main Italian news agencies and their local branches (*ANSA; ANSA Regional; AGI*).³⁵ Specifically, we run a script looking for news in the period post-2006 pardon and pre-2008 elections (i.e., August 1, 2006-March 30, 2008) containing any word related to crimes (theft, robbery, extortion, scam, murder, drug, burglary, beatings, domestic violence, rape, etc.) and containing at the same time words immediately identifiable with the collective pardon (pardoned individual, collective pardon) and semantic variations of them. To reduce measurement error, we excluded news containing words identifying stories not related to real crimes (e.g., movie, fiction, TV), referring to crimes different from the ones involving pardoned prisoners (e.g., mafia, terrorism) or crimes committed by foreign citizens (e.g., foreigner, immigrant).

The following are examples of stories extrapolated from the *Factiva* query:

- “Trieste. Free thanks to the pardon yesterday afternoon at 4 p.m. a Trieste-resident was caught just 12 hours later by the Police of the Core Mobile Radio Trieste while attempting to steal a car” (*Trieste. Scarcerato grazie all’indulto alle 16 di ieri pomeriggio un triestino è stato beccato appena 12 ore dopo dai Carabinieri del Nucleo Radiomobile di Trieste mentre tentava di rubare un autovettura*).
- “He resisted for two months then Tarquinio Colantoni, 46 years old, from Giffoni Sei Casali did not resist and has again committed a sexual assault. On August first, he was released from prison thanks to the pardon” (*Ha resistito per due mesi poi Tarquinio Colantoni di 46 anni di Giffoni Sei Casali non ha resistito e ha commesso nuovamente una violenza sessuale. Il primo agosto era uscito dal carcere per l’indulto*).

³⁵We include national newspapers since in Italy national newspapers have several local editions (Drago *et al.*, 2014). The presence of news agencies allow us to increase the external validity of our sample since local newspapers not included in the *Factiva* database are often using these agencies as their main sources of information and, at the same time, news agencies report news gathered from local newspapers as well.

- “They were free for a few weeks thanks to the pardon. Bergamo. Four youngsters from Bergamo were arrested with the charges of raping and robbing a prostitute on Saturday night” (*Erano in liberta' da poche settimane grazie all'indulto. Bergamo. Quattro giovani bergamaschi sono stati arrestati con l'accusa di aver violentato e rapinato una prostituta sabato notte*).

Appendix C Additional Tables and Figures.

Table C.1: Δ Perceived Competence Perception Center-Left vs. Center-Right Coalitions

	Center-Left Voters	Center-Right Voters	Independent/ abstainers	All Voters
Fighting Crime	+27%	-72.1%	-12.2%	-22.3%
Enhance Economic Growth	+49.9%	-68.9%	-8.3%	-14.3%
Managing Immigration	+45.3%	-71.1%	-4.3%	-13.9%
Reduce Political Corruption	+41.0%	-48.5%	+0.4%	+5.4%

Notes. The data are drawn from a survey (N=1307) representative of the Italian population aged 16 and above. The data reports the difference in terms of percentage of people that answer “The incumbent center-left government” rather than “A center-right government” to the question “Who is better able to handle this problem?”. Source: *Osservatorio sul Capitale Sociale. Demos & Pi*, June 2007.

Table C.2: Voters' Response to the Effects of the Collective Pardon (2)

	Δ Votes per eligible voter 2008-2006 Center-right			
	(1)	(2)	(3)	(4)
Incentive to recidivate	0.0012 (0.0009)	0.0013* (0.0008)	0.0012 (0.0009)	0.0013* (0.0008)
Pardoned individuals controls	YES	YES	YES	YES
Municipal level controls	NO	YES	NO	YES
Only municipalities with at least one pardoned	YES	YES	NO	NO
Observations	2,256	2,256	7,159	7,159
R-squared	0.0482	0.2133	0.0202	0.1107

Notes. Entries are coefficients from the equation model estimated with OLS. 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 (2005). Standard errors clustered at the provincial level are in parentheses. Significance at the 15% level is represented by +, 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table C.3: Voters' Response to the Effects of the Collective Pardon (3)

	Δ Votes per eligible voter 2008-2006 Center-left			
	(1)	(2)	(3)	(4)
Incentive to recidivate	-0.0018** (0.0008)	-0.0016* (0.0008)	-0.0018** (0.0008)	-0.0012+ (0.0008)
Pardoned individuals controls	YES	YES	YES	YES
Municipal level controls	NO	YES	NO	YES
Only municipalities with at least one pardoned	YES	YES	NO	NO
Observations	2,256	2,256	7,159	7,159
R-squared	0.0673	0.1724	0.0200	0.1227

Notes. Entries are coefficients from the equation model estimated with OLS. 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 (2005). Standard errors clustered at the provincial level are in parentheses. Significance at the 15% level is represented by +, 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table C.4: European Elections

	Δ Win Margin 2009-2004			
	Center-right vs. Center-left			
	(1)	(2)	(3)	(4)
Incentive to recidivate	0.0001 (0.0002)	0.0001 (0.0002)	0.0001 (0.0002)	0.0001 (0.0002)
Pardoned individuals controls	YES	YES	YES	YES
Municipal level controls	NO	YES	NO	YES
Only municipalities with at least one pardoned	YES	YES	NO	NO
Observations	2,252	2,252	7,136	7,136
R-squared	0.0132	0.1149	0.0059	0.1006

Notes. Entries are coefficients from the equation model estimated with OLS. 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 (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 ***.