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

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Abstract

We provide evidence about voters' response to crime policies. Our research design exploits a natural experiment arising from the Italian 2006 collective pardon bill suddenly releasing more than one third of the prison population. The design of the bill created idiosyncratic incentives to recidivate across pardoned individuals and municipalities. We show that municipalities where the incentive to recidivate of resident pardoned individuals was higher, experienced higher recidivism. At the same time, in these municipalities: i) newspapers were more likely to report crime news involving pardoned individuals; ii) voters held worse beliefs on the incumbent government's ability to control crime. Moreover, with respect to the previous elections, the incumbent national government experienced a significantly worse electoral performance in the April 2008 elections relative to the opposition coalition. In terms of political cost, our estimates suggest that a one standard deviation increase in the incentive to recidivate (i.e., the random component in the effects of the policy) reduced the margin of victory of the incumbent national government by 3.3%. Overall, our findings indicates that voters keep incumbent politicians accountable by conditioning their vote on the observed effects of their policies.

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

1 Introduction

Crime is a costly phenomenon and it is perceived as a crucial social issue. In the Euro-barometer survey, for instance, crime ranks among the first five (out of 15) most important perceived problems in a number of European countries (Mastrorocco and Minale, 2014).[1] The public support for tough policies against criminals is also an indirect indicator of these concerns.[2] The perceived public concern about crime translates in a widespread belief that crime policies have a significant impact on voting behavior. According to journalistic accounts, if in 1994 Bill Clinton "hadn't embraced a "tough on crime agenda" [he] might never have become—or remained—president".[3] 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.[4] Elected officials seem to share the belief that being soft on crime does not pay off. US judges respond to political pressure by sentencing serious crimes more severely (Berdejo and Yuchtman, 2013) and (non-partisan) elected judges tend to be tougher on crime when they fall under closer media scrutiny (Lim *et al.*, 2015). At the same time, politicians in the US tend to disproportionally allocate police resources around electoral years (Levitt, 1997) and they seem to be more likely to enact stricter mandatory minimum sentence legislation prior to an election (Murakawa, 2014).

Despite the importance of this issue for potential voters and the findings about elected officials' behavior, existing studies on the link between crime control policies and voters' behavior are mostly correlational and provide mixed evidence (e.g., Hall 2001; Krieger 2011).[5] Thus, we know very little about whether voters respond to crime policies and why. In particular, if elected officials tend to be tougher on crime before the elections, do they do

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[1] Concerns on crime are shared by citizens on both side of the Atlantic. According to a 2006 report by the National Council on Crime and Delinquency, 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.

[2] Cullen *et al.* (2000) show that in the US there is a strong public support for stiff crime control policies such as "three strikes" laws and increased use of incarceration.

[3] *The Atlantic*, "Hillary Clinton and the Tragic Politics of Crime" May, 2015.

[4] *The New York Times* "Prison Furloughs in Massachusetts Threaten Dukakis Record on Crime", July 5, 1988.

[5] The relevance of this type of policies from a social welfare perspective is exemplified by the yearly social costs of crime which are estimated to be around $500 billion just in the US, i.e., 4 percent of the U.S. gross domestic product (Cooter and Ulen, 2016).

it because they simply want to match some ideological expectation of their constituencies or because their constituencies hold them accountable for the consequences of their choice (e.g., their effectiveness in controlling crime)? In this paper we study how voters respond to crime control policies resorting to an original case study based on a natural experiment. To the best of our knowledge, this is the first exercise providing evidence concerning voters' response to crime policies in a quasi-experimental design.

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 (Drago *et al.*, 2009). This provision of the bill manipulated pardoned individuals' incentives to recommit crime after release from prison. 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. Conditional on the original sentence, the residual sentence only depends on the date of entry into prison which is plausibly random. In turn the design of the collective pardon created an exogenous variation in the incentive to recidivate of former inmates across municipalities. 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 findings show 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 (i.e., 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 3.3%

increase in the margin of victory of the opposition (center-right, CR) coalition in the post-pardon national elections relative to the pre-pardon ones (i.e., 2008 vs. 2006). Consistently with the absence of pre-trends in the average incentive to recidivate, no effect is found in elections prior to 2008.

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 at the city level 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 (i.e., 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 (i.e., voters hold worse posterior beliefs on the incumbent government's type) in cities where the incentive to recidivate is higher.

As we explain in greater details in the paper, these effects are not negligible and can be all accounted within a retrospective voting model (e.g., Persson and Tabellini 2002; Ashworth 2012). Indeed, our findings provide evidence showing 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.[6] To this extent, this paper

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[6]Identifying whether voters hold politicians accountable for the effectiveness of their choices is as relevant as difficult for properly modeling voters' and politicians' interactions. Indeed, politicians endogenously choose their policies to enhance their reelection probability. Incumbent politicians may choose policies merely to please their constituencies' ideological preferences. For instance a voter's response to a redistributive policy may simply reflect her liberal stance rather than her valuation of the costs and benefits of such a policy. Similarly, more lenient crime-control policies might simply be positively judged by liberal voters and

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.[7] 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), even random shocks (e.g., natural disasters) might be useful in providing relevant information for voters when making inferences on a politician's type.[8] 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.[9]

negatively by conservatives. More generally, incumbent politicians may strategically manipulate their policies across space (i.e., targeting specific groups of voters or constituencies) and across time (i.e., timing policies with respect to electoral years), e.g., Rogoff and Sibert (1988); Rogoff (1990); Brender and Drazen (2008).

[7]Fowler 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.

[8]Indeed, while 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 (e.g., 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. In turn, 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).

[9]It is also worth noting that in earlier contributions the exogenous events creating a shock in voters' welfare

Second, differently from 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 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), our findings suggest that voters respond to the observed effects of a public policy (both in terms of beliefs and behavior) in a way that is consistent with retrospective voting models of electoral accountability (e.g., Fearon 1999; Persson and Tabellini 2002; Besley 2006; Ashworth 2012).[10]

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

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are, in principle, publicly observable by voters (e.g., the presence and consequences of a natural disaster, the actual change in oil prices, the lotteries results). In turn, existing papers may not easily assess whether the observed response of voters is due to an attribution error or if voters are correctly updating their beliefs given their available noisy information. Instead, in our natural experiment, voters cannot observe the extent of the random policy shock since the incentive of recidivate of pardoned individuals was not public information.

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

(Law 241/2006).[11] The policy was designed, proposed and implemented by the incumbent center-left government coalition (CL, henceforth) 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 of the center-right coalition (CR, henceforth) voted for the pardon bill, a circumstance that we exploit in the empirical section to investigate the accountability mechanism that is consistent with the results. 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.[12] 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 on August 1. The main provisions of the collective pardon bill are the following. It granted a reduction in the length of detention for those who committed a crime before May 2, 2006. The backdating of the collective pardon, announced immediately after the Parliament began to debate the bill, aimed at impeding any possible effect of the collective pardon on crime rates during the months leading up to the approval of the law. Pursuant to the content of the law, each inmate benefited of an immediate three years sentence reduction for a large number of offenses, including property, violent crimes, drug trafficking related offenses and white-collar crimes.[13] Thus an inmate convicted of 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

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

[12]See Lofstrom and Raphael (2013) for the case of California.

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

a result, the prison population dropped from a total of 60,710 individuals on July 31, 2006 to 38,847 on August 1, 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 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 August 1, 2006. As a consequence of the new law they are both released from prison on August 1, 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 (i.e., in April 2008).[14] Figure 1 summarizes the timing of elections and of the collective pardon bill.[15]

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[14]For a salience theory of choice under risk see Bordalo et al. (2012).

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

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 (i.e., 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, 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.[16] 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 government (CL) 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, i.e., 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; Aragones *et al.*, 2015).

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[16]The observed decrease in the number of news on crime between the end of 2007 and June 2008 could be explained by two factors. First, the collapse of the incumbent government in January 2008 and the consequent early April 2008 Elections increased the space devoted to political news by news programs. 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).

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 (i.e., 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 pardoned inmates was higher).[17] 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 (i.e., where their residual sentence was lower).[18]

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).

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[17]Notice 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.

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

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 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 12,356 Italian pardoned individuals resident in 2,257 municipalities. The summary statistics of this data is reported in the first panel of Table 2 where we average-out the data on the characteristics of pardoned individuals at the municipality level.[19] 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 (e.g. higher in 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 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 data 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 7,164 municipalities

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[19]In order to obtain a more homogeneous sample, since all municipalities with at least one pardoned individual resident in the municipality have 500 inhabitants or more, we exclude all municipalities below 500 inhabitants. All results are robust to including these municipalities in the analysis and they are available upon request to the authors.

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 that divided Italy into 26 electoral districts. As we can see in Table 2 the CR and CL coalition lost some votes between the two elections, with the CR loosing less than the CL. The difference in the win of margin of victory between 2008 and 2006 is on average 0.07 percentage points. One third of the municipalities in Italy had at least one pardoned individual. In Table 2 we report the socio-economic characteristics at the municipal and provincial 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 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.[20] 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 last panel of Table 2.

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

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 for individuals with lower and higher residual sentence, conditional on their original sentence.

Our regression model is the following:

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

where *incentive to recidivate* is a standardized measure of the average incentive to recidivate of pardoned individuals in municipality i.[21] 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. X_i is a vector of controls at the municipal level including the municipal crime rate before the pardon (2005) and municipal characteristics in 2001 (i.e. the most recent Census year relative to 2008) - see Table 2 for the list of variables. X_i also includes the log of the provincial unemployment rate in levels (i.e., in 2006) and in differences (i.e., between 2008

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[21]We define the individual incentive to recidivate as 36 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).

and 2006). 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 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.[22]

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[22]It is important to note that in the presence of inmates from municipality i at risk of recidivism in municipality j, our coefficient should be interpreted as a lower bound of the causal effect of the 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

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 observable characteristics. Table 3 presents results consistent with the idea that the incentive to recidivate is exogenous. Specifically, in column 1 we regress our key variable on the set of variables X_i. As expected, the dummy $\mathcal{I}_i$ and the original sentence predict (mechanically) the incentive to recidivate: the first is positive because we set the incentive to recidivate equal to zero when $\mathcal{I}_i = 0$, while 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 (i.e., larger residual sentence). Indeed, the residual sentence is bounded from above and it is always lower than the original sentence. With a few exceptions, all the other variables do not significantly predict the incentive to recidivate. Most notably, the crime rate in 2005 is not correlated with the incentive to recidivate. When we look at the results in column 2 and 3, the coefficients on the variables that appear statistically significant are low in magnitude.

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 were orthogonal to any pre-trend in the votes to political coalitions in the previous elections.

estimates are typically larger than the baseline ones. Results are available upon request to the authors.

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 variables 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 specification 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.[23] 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 and provincial level controls) that a one standard deviation increase in the incentive to recidivate (i.e., around 8.2 months less in the average residual sentence) leads to a 0.27 percentage points increase in the margin of victory of the center right coalition, corresponding to a 3.3 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.[24] We exploit the circumstance that

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[23]This 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).

[24]According 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.

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 candidates voting for the pardon was higher.

We gathered data on the identity of CR candidates in each electoral district in the 2008 elections. We classified each CR MP according to whether she/he voted in favor of the collective pardon bill in 2006 and we computed for each electoral district (typically subregional entities), the percentage of candidates of the main center-right party (i.e., PDL) in the 2008 elections who voted in favor of the collective pardon bill in July 2006.[25] 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.[26] 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 there 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, this translated on one standard deviation in the incentive to recidivate increasing the variation in the CR win margin of 15.42% in districts were none of the CR candidates voted in favor of the pardon.

Hence, consistent with the hypothesis put forward in Section 2 (and with the conceptual framework in Appendix A), we observe that the CR coalition (i.e., the opposition coalition at the time when the pardon was approved) experienced an increase in its electoral support

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[25]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).

[26]Controlling 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.

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.

5.2 The Mechanism

We now turn to the mechanism linking the idiosyncratic component in effects of the policy (i.e., 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 created an exogenous variation in the observed recidivism rate at the municipality level. In turn, 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.[27] 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 in 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 (i.e., higher recidivism rate). The effect is not trivial. A one standard deviation increase in the incentive to recidivate implies a 15.5 percent increase in the recidivism in a municipality with at least one pardoned individual (which is consistent with Drago *et al.* 2009).

Voters' Information. Table 8 provides evidence on how the pardoned individuals'

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[27]As in Drago *et al.* (2009), the recidivism rate is measured seven months after the release. This is less of a concern as long as we expect the residual sentence having an effect on the recidivism measured two years later. In fact, Mastrobuoni and Pinotti (2015) show that the residual sentence impacted the recidivism of foreign inmates 17 months after release. There is no obvious reason to believe that the effect does not hold for Italians.

incentive to recidivate maps directly into the amount of news related to crime involving pardoned individuals. 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 mentioning the municipality (in the period post-pardon up to the 2008 elections).[28] 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% higher probability of having newspapers reporting at least one crime-news involving pardoned individuals. While this effect seems not 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. 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 center-left incumbent government's crime policies and, in general, of the ability of the center-left to deal with crime.[29] At the same time, Table 10 shows that they are also more likely to have an overall negative evaluation of the main CL party (i.e., PD). The coefficient implies a 11.2% higher probability of reporting an overall negative evaluation of the main CL party following a one standard deviation increase in the incentive to recidivate. Viceversa, Tables 9 and 10 do not provide evidence on the presence of any significative effect of the incentive to recidivate on the probability of individuals perceiv-

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[28]Appendix B provides some examples of this type of news.

[29]Notice 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.

ing crime as the most important political issue either in Italy or in the municipality where the respondent lives.

6   Explanations

Our estimates imply that a one standard deviation increase in the average incentive to recidivate (i.e., around 8.2 months less in the average residual sentence) is associated with a 3.3% increase in the win margin of the opposition coalition with respect to the one representing the incumbent government. We discuss two possible explanations for this finding: one based on multi-dimensional voting and issue salience and one based on (forward-looking) retrospective voting.

Multi-dimensional voting and issue salience. As we discussed in Section 2, the collective pardon bill was a very salient political issue up to the 2008 elections. Hence, one possibility is that also crime became a very salient issue between 2006 and 2008 and, in turn, the increased salience of crime ended up benefiting CR parties, who are typically perceived as the most competent on this issue (Petrocik, 1996; Puglisi, 2011). This is a mechanism suggested by models of multi-dimensional voting (Belanger and Meguid 2008; Aragones *et al.* 2015). However, while this mechanism is consistent with the 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, Table C.4 in Appendix Appendix C shows 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.

Retrospective voting. 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.[30] 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 pardoned individuals resident in that municipality 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 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 seem quite 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, while we cannot pin-down a single explanation, we find the data to be most

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[30]See Barro (1973) and Ferejohn (1986) for earlier retrospective voting models.

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 pardoned individuals resident in that municipality 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 (i.e., a lower residual sentence) in a municipality translates into a harsher electoral "punishment" of the

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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 2: Summary statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Incentive to recidivate	2257	20.60	8.22	1	35
Average original sentence - pardoned	2257	40.56	29.57	2	254
Mean age - pardoned	2257	40.26	8.48	20	78
% employed - pardoned	2257	0.27	0.37	0	1
% married - pardoned	2257	0.27	0.36	0	1
% primary education - pardoned	2257	0.71	0.38	0	1
% secondary education - pardoned	2257	0.07	0.20	0	1
% college education - pardoned	2257	0.01	0.08	0	1
% convicted for drug crimes	2257	0.30	0.37	0	1
% convicted for property crimes	2257	0.47	0.41	0	1
% convicted for violent crimes	2257	0.13	0.28	0	1
% convicted for other crimes	2257	0.02	0.10	0	1
Pardoned individuals per 1,000 residents	7164	0.33	0.32	0.02	4.39
Municipality with at least one pardoned invididual	7164	.32	.46	0	1
ΔWin Margin 2006-2008 C.Right vs. C.Left	7164	.07	.07	-.3	.6
Δ Votes per eligible voters 2006-2008 C. Right	7164	-.04	.04	-.31	.27
Δ Votes per eligible voters 2006-2008 C. Left	7164	-.11	.05	-.54	.14
Crimes per capita in 2005	7164	.01	.01	0	.37
Elderly index 2001	7164	.2	.06	.06	.55
Education index 2001	7164	.26	.06	.07	.57
Log population 2001	7164	8.07	1.12	6.21	14.75
Unemployment rate 2006	7164	1.72	.56	.83	2.92
Diff Unempl. rate 2006-2008	7164	.04	.17	-.46	.5
At least one news on crime & collective pardon	7164	.06	.24	0	1
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 3: Balancing tests

	(1) Incentive to recidivate	(2) Incentive to recidivate	(3) Incentive to recidivate
Municipality with at least one pardoned individual	3.1007***	3.2795***	3.2464***
	(0.0470)	(0.1325)	(0.1307)
Average original sentence - pardoned	-0.0151***	-0.0154***	-0.0155***
	(0.0013)	(0.0013)	(0.0013)
Mean age - pardoned		-0.0039	-0.0038
		(0.0024)	(0.0024)
% employed - pardoned		-0.0944	-0.0898
		(0.0763)	(0.0764)
% married - pardoned		-0.0174	-0.0212
		(0.0717)	(0.0712)
% primary education - pardoned		0.1087*	0.1094*
		(0.0632)	(0.0631)
% secondary education - pardoned		-0.2096*	-0.2091*
		(0.1066)	(0.1062)
% college education - pardoned		0.2131	0.2195
		(0.3682)	(0.3679)
% convicted for drug crimes		-0.1447*	-0.1478*
		(0.0821)	(0.0825)
% convicted for property crimes		-0.0397	-0.0428
		(0.0842)	(0.0844)
% convicted for violent crimes		0.2304**	0.2299**
		(0.1130)	(0.1131)
% convicted for other crimes		-0.0467	-0.0580
		(0.1859)	(0.1853)
Pardoned individuals per 1,000 residents		0.0168	0.0443
		(0.0639)	(0.0599)
Crimes per capita in 2005	0.0258		0.1217
	(0.4015)		(0.3978)
Elderly index 2001	-0.0099		0.0073
	(0.1125)		(0.1096)
Education index 2001	0.0912		0.1212
	(0.1127)		(0.1160)
Log population 2001	0.0086		0.0137*
	(0.0080)		(0.0070)
Unemployment rate 2006	0.0207*		0.0146
	(0.0124)		(0.0120)
Δ Unempl. rate 2006-2008	0.0440		0.0361
	(0.0401)		(0.0400)
Observations	7,164	7,164	7,164
R-squared	0.8490	0.8518	0.8519

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

Table 4: Placebo Regressions

	(1)	(2)	(3)
	Δ Win Margin	Δ Votes per eligible voter	
	2006-2001	2006-2001	2006-2001
	C.Right vs. C.Left	C.Right	C.Left
Incentive to recidivate	-0.0002	0.0004	0.0006
	(0.0014)	(0.0010)	(0.0010)
Pardoned individuals controls	YES	YES	YES
Municipal & provincial level controls	YES	YES	YES
Observations	7,144	7,144	7,144
R-squared	0.1550	0.0525	0.2781

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: education index, log population and elderly index in census year 2001 and crime rate pre-pardon (2005). Provincial level control include: unemployment rate in 2006 and the 2006-2008 difference in unemployment rate. 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**	0.0029**	0.0030**	0.0027**
	(0.0013)	(0.0012)	(0.0013)	(0.0011)
Pardoned individuals controls	YES	YES	YES	YES
Municipal & provincial level controls	NO	YES	NO	YES
Only municipalities with at least one pardoned	YES	YES	NO	NO
Observations	2,257	2,257	7,164	7,164
R-squared	0.0783	0.1825	0.0278	0.1053

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: education index, log population and elderly index in census year 2001 and crime rate pre-pardon (2005). Provincial level control include: unemployment rate in 2006 and the 2006-2008 difference in unemployment rate. 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***	0.0121***	0.0129***	0.0126***
	(0.0031)	(0.0030)	(0.0033)	(0.0032)
Incentive to recidivate	-0.0590***	-0.0554***	-0.0603***	-0.0058***
% of CR cand. who voted for the pardon	(0.0154)	(0.0155)	(0.0160)	(0.0159)
Pardoned individuals controls	YES	YES	YES	YES
Municipal & provincial level controls	NO	YES	NO	YES
Only municipalities with at least one pardoned	YES	YES	NO	NO
Observations	2,257	2,257	7,164	7,164
R-squared	0.3790	0.4277	0.3516	0.3697

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: education index, log population and elderly index in census year 2001 and crime rate pre-pardon (2005). Provincial level control include: unemployment rate in 2006 and the 2006-2008 difference in unemployment rate. 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

	(1) Share of recidivists	(2) Share of recidivists
Incentive to recidivate	0.0142**	0.0138**
	(0.0056)	(0.0056)
Average original sentence	0.0003	0.0002
	(0.0002)	(0.0002)
Pardoned individuals controls	YES	YES
Municipal & provincial level controls	NO	YES
Observations	7,164	7,164
R-squared	0.1323	0.1350

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: education index, log population and elderly index in census year 2001 and crime rate pre-pardon (2005). Provincial level control include: unemployment rate in 2006 and the 2006-2008 difference in unemployment rate. 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

	(1) At least one news on crime & collective pardon in the municipality	(2) At least one news on crime & collective pardon in the municipality
Incentive to recidivate	0.0018**	0.0013*
	(0.0008)	(0.0007)
Pardoned individuals controls	YES	YES
Municipal & provincial level controls	NO	YES
Observations	7,164	7,164
R-squared	0.0523	0.1329

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: education index, log population and elderly index in census year 2001 and crime rate pre-pardon (2005). Provincial level control include: unemployment rate in 2006 and the 2006-2008 difference in unemployment rate. 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	-0.0070	0.0684**	-0.1341***	0.0084
	(0.0153)	(0.0344)	(0.0412)	(0.0335)
Pardoned individuals controls	YES	YES	YES	YES
Municipal & provincial level controls	YES	YES	YES	YES
Individual level controls	YES	YES	YES	YES
Observations	2,826	347	347	347
Pseudo R-squared	0.0638	0.196	0.444	0.173

Notes. Entries are coefficients from a logit model. 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: education index, log population and elderly index in census year 2001 and crime rate pre-pardon (2005). Provincial level control include: unemployment rate in 2006 and the 2006-2008 difference in unemployment rate. 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 ***.

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	0.0056	0.0086	0.0158**	0.0070
	(0.0256)	(0.0161)	(0.0075)	(0.0082)
Pardoned individuals controls	YES	YES	YES	YES
Municipal & provincial level controls	YES	YES	YES	YES
Individual level controls	YES	YES	YES	YES
Observations	3,719	3,719	27,854	28,005
Pseudo R2	0.114	0.119	0.0759	0.173

Notes. Entries are coefficients from a logit model. 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: education index, log population and elderly index in census year 2001 and crime rate pre-pardon (2005). Provincial level control include: unemployment rate in 2006 and the 2006-2008 difference in unemployment rate. 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 ***.

Figure 1: Timing of Elections and Collective Pardon Bill

```
                    ┌─────────────────┐
                    │ July 31, 2006:  │
                    │   Collective    │
                    │ clemency bill by│
                    │  CL-government  │
                    └─────────────────┘
                            │
                            ↓
  ├─────────────────────────┼──────────────────────────┼──────────────┼──────→ time
                            ↑
┌──────────────┐                        ┌──────────────┐  ┌──────────────┐
│ April 2006:  │                        │January 2008: │  │ April 2008   │
│  Elections   │                        │ Collapse of  │  │  Elections   │
└──────────────┘                        │ CL-goverment │  └──────────────┘
          ┌──────────────────┐          └──────────────┘
          │  August 2006:    │
          │Release of pardoned│
          │   prisoners with  │
          │ residual sentence │
          │     ≤ 36 months   │
          └──────────────────┘
```
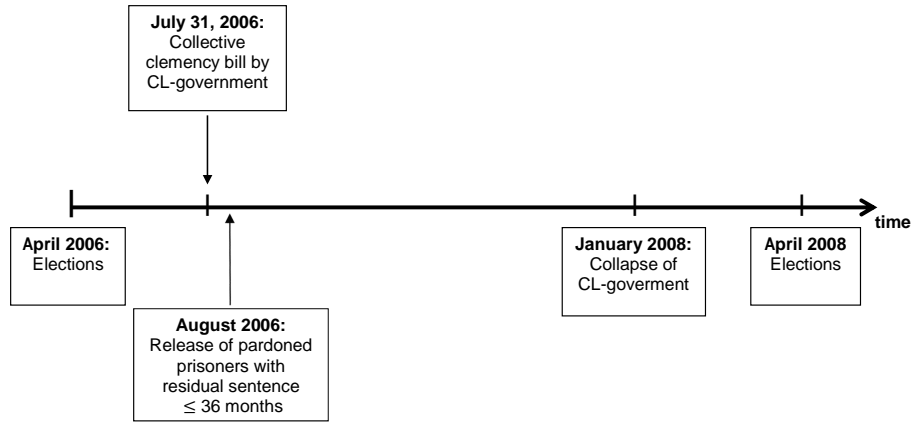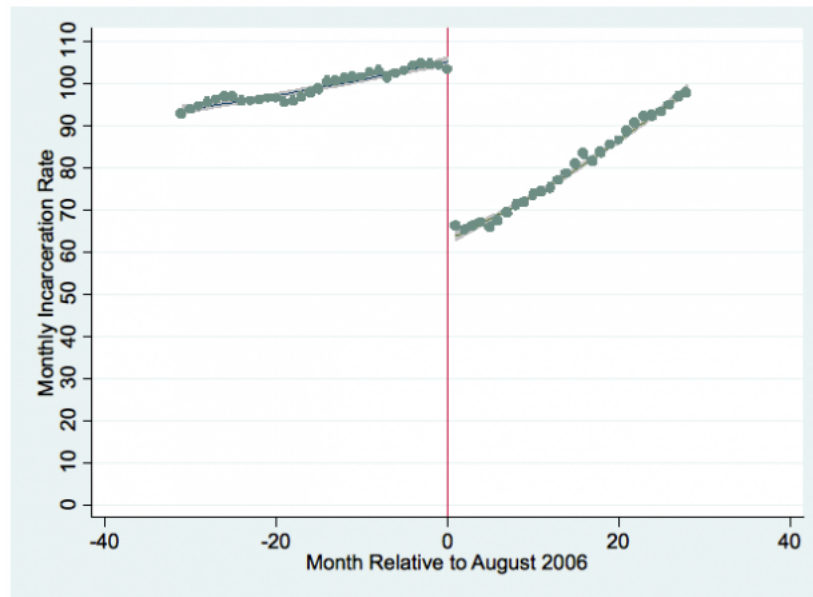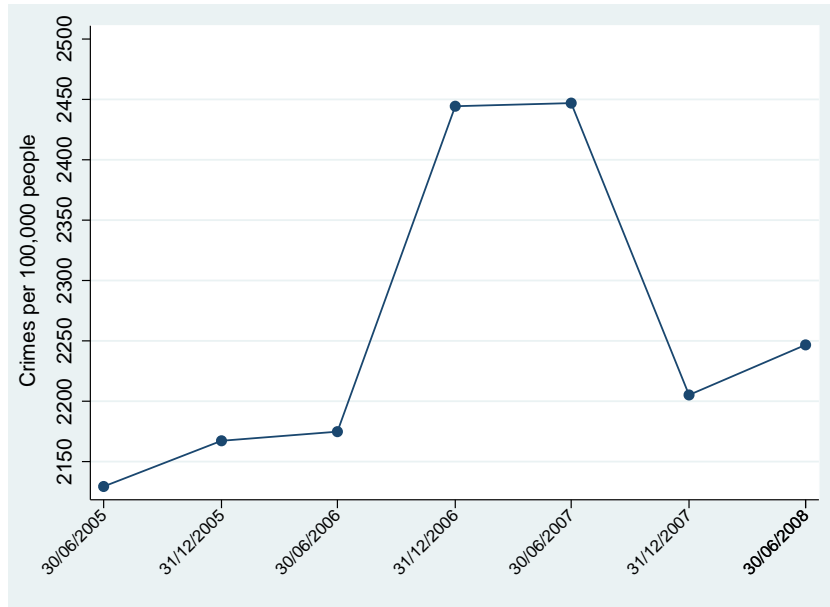
Figure 2: Incarceration rate

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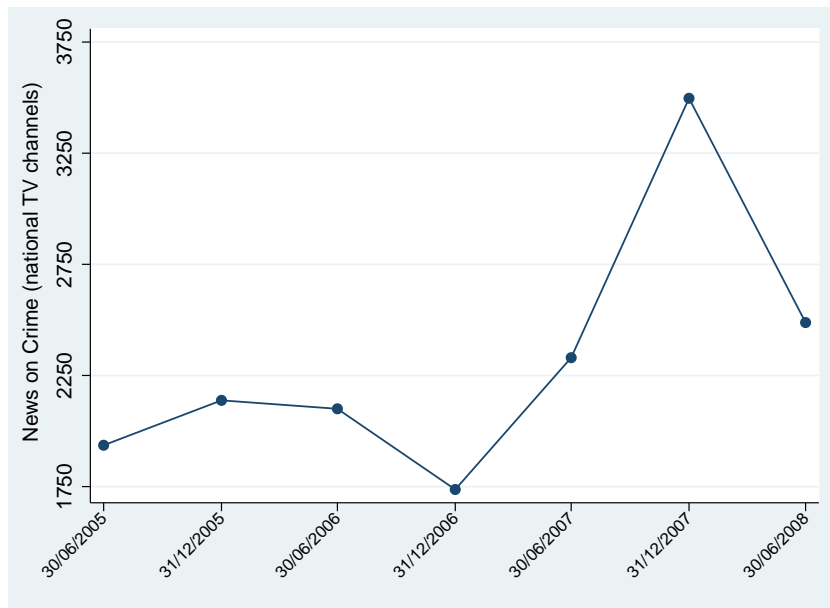
Notes: The figures illustrates the variation in the number of news on crime (on the main Italian national TV channels) between the first semester of 2005 and the first semester of 2008. (Source: "Indagine sulla Sicurezza in Italia, 2009, UNIPOLIS").

Figure 5: Geographical distribution of the average incentive to recidivate of pardoned individuals (standardized)
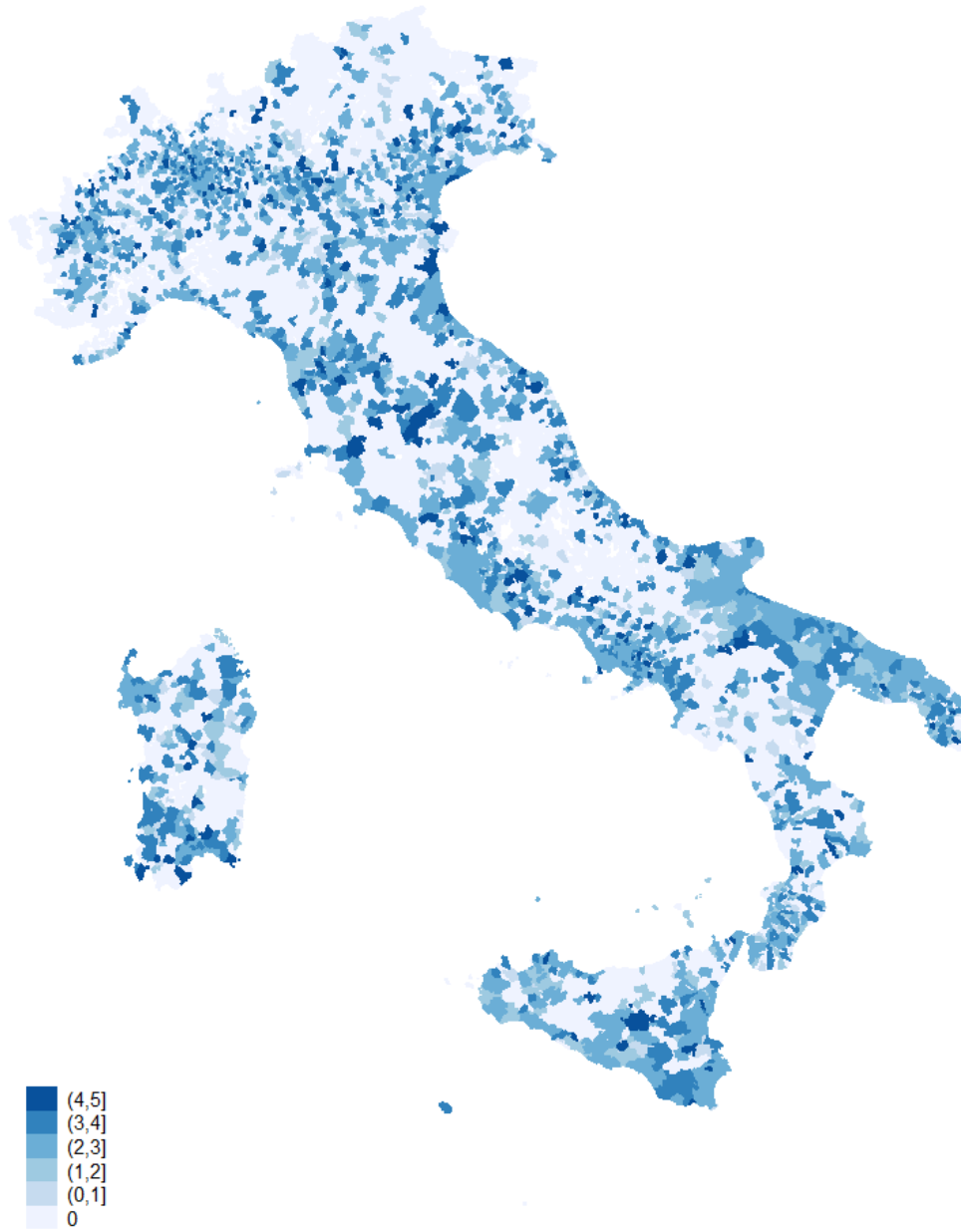
Notes. The figure illustrates the geographical distribution of the (standardized) average incentive to recidivate of pardoned individuals at the municipal level. A one unit increase corresponds to one standard deviation increase in the incentive to recidivate (i.e., around 8.2 months less of residual 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.[31]

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

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

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 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 $\kappa_i < 1$):[32]

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

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[32]Notice 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.

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 goverment 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.[33] Hence, the win margin of the challenger (center-right coalition) should be higher in the 2008 elections.

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[33]In 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:

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

Instead,

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

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*).[34] 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).*

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[34]We 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	0.0013*	0.0012	0.0013*
	(0.0009)	(0.0007)	(0.0009)	(0.0007)
Pardoned individuals controls	YES	YES	YES	YES
Municipal & provincial level controls	NO	YES	NO	YES
Only municipalities with at least one pardoned	YES	YES	NO	NO
Observations	2,257	2,257	7,164	7,164
R-squared	0.0484	0.1747	0.0203	0.0965

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: education index, log population and elderly index in census year 2001 and crime rate pre-pardon (2005). Provincial level control include: unemployment rate in 2006 and the 2006-2008 difference in unemployment rate. 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**	-0.0016*	-0.0018**	-0.0014+
	(0.0008)	(0.0008)	(0.0008)	(0.0008)
Pardoned individuals controls	YES	YES	YES	YES
Municipal & provincial level controls	NO	YES	NO	YES
Only municipalities with at least one pardoned	YES	YES	NO	NO
Observations	2,257	2,257	7,164	7,164
R-squared	0.0668	0.1411	0.0198	0.1088

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: education index, log population and elderly index in census year 2001 and crime rate pre-pardon (2005). Provincial level control include: unemployment rate in 2006 and the 2006-2008 difference in unemployment rate. 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

	(1)
	Δ Win Margin
	2009 vs. 2004
	C.Right vs. C.Left
Incentive to recidivate	0.0001
	(0.0002)
Observations	7,141
R-squared	0.0828
Pardoned individuals controls	YES
Municipal & provincial level controls	YES

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: education index, log population and elderly index in census year 2001 and crime rate pre-pardon (2005). Provincial level control include: unemployment rate in 2006 and the 2006-2008 difference in unemployment rate. 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 ***.