Rationalizable Suicides: Evidence from Changes in Inmates' Expected Length of Sentence*

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Abstract

Is there a rational component in the decision to commit suicide? Economists have been trying to shed light on this question by studying whether suicide rates are related to contemporaneous conditions. This paper goes one step further: we test whether suicides are linked to forward-looking behavior. In Italy, collective sentence reductions (pardons) often lead to massive releases of prisoners. More importantly, they are usually preceded by prolonged parliamentary activity (legislative proposals, discussion, voting, etc.) that inmates seem to follow closely. We use the legislative proposals for collective pardons to measure changes in the inmates' expectations about the length of their sentences, and find that suicide rates tend to be significantly lower when pardons are proposed in congress. This suggests that, amongst inmates in Italian prisons, the average decision to commit suicide responds to changes in current expectations about future conditions. At least partially, therefore, the decision seems rationalizable.

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JEL classification codes: I1, D1, K4

1 Introduction

According to the US Centers for Disease Control and Prevention in 2011 suicides were the 10th leading cause of death across all age groups, and the second cause of death between the age of 15 and 34; with an estimated medical and work loss that is close to $ 35 billion a year. Based on the last Substance Abuse and Mental Health Services Administration survey in the United States alone about 8 million adults had suicidal thoughts in 2008, and about 1 million attempted suicide (one in 25 attempts is successful).

For a long time now, applied economists but also policy makers have been trying to establish whether incentives and changes at the margin of the kind typically addressed in economic analysis do matter even when the underlying decision-making scenario is as extreme and the choice itself as irreversible as for suicides.

More precisely, when economic theory considers the rationality of the decision to commit suicide it predicts that individuals decide to do so when their discounted expected lifetime utility falls below a certain threshold. The evolution of the underlying theoretical framework has ranged from simple comparisons of net present values (see Hamermesh and Soss, 1974, for a seminal work) to elaborate models in which the irreversibility of the act generates an option value to waiting (i.e., postponing suicide) when the uncertainty about the future is high (see Dixit and Pindyck, 1994).

And there have been of course several attempts to test these theories empirically, or at least whether suicides respond to economic incentives (see Goldsmith et al. (2002) for a wide and comprehensive overview of the literature on suicides, and Chen et al. (2009) for a thorough survey of the empirical literature). One approach (see, for example, Koo and Cox, 2008) has been to argue that human capital depre-

ciation, as a consequence of unemployment and lack of job training, undoubtedly worsens an individual's prospects for the future. Suicide might become then an acceptable option if one's myopic considerations limit the decision-making foresight to within the horizon of current hardship. Another approach, adopted by the grand majority of the literature in question, has been to look for correlations between suicides and current socioeconomic factors - such as income, income growth, education, unemployment, gender, race, monetary incentives (insurance), health, religion, social capital, and other cultural parameters. Typically, the data are aggregated over time and space, and a positive correlation between suicides and adverse economic conditions emerges (see for instance Ruhm (2000), Cutler et al. (2001), Brainerd (2001), or Andres (2005)).

Instead, a set of recent papers use individual level data on suicides. Daly and Wilson (2009) finds that the suicides and subjective measures of wellbeing share the same socioeconomic determinants. Similar to our approach, Daly et al. (2013) model suicides as choice variable that depends on the expected present and future utility. The authors find that the likelihood of suicide decreases with own income and increases with the reference-group income, highlighting the importance of relative statuses (see also the related paper Daly et al., 2011).

In either approach, however, the focus is on contemporaneous correlations, which makes the identification of a causal relationship between the socioeconomic variables and the suicide rate more difficult. Even though the respective results might be capturing, at least to some degree, the forward-looking behavior of the underlying theoretical framework, they cannot rule out reverse causality. The latter issue could well be at play for instance if on average individuals who suffer from (unobserved) mental disorders tend to be also less productive. By contrast, the present paper

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does not attempt to test the existence of a rationalizable component of suicides using measures of current economic conditions. Instead, our identification strategy rests on changes in current expectations about future conditions, brought about by new information that renders certain future changes more probable, that do not alter the one's current wellbeing.

In particular, we test whether new information about the future can influence the decision to commit suicide in a rather peculiar environment: Italian prisons. Given that individual expectations are both hard to measure and highly diverse, prisons and suicides in prisons represent almost ideal experimental grounds and events, respectively. As the environment in which the experiment takes place, prisons are closed and isolated - which reduces the influence of external factors that might be latent or hard to measure (family, friends, career prospects, current socioeconomic conditions, etc.). As events, suicides in prisons are fully recorded while changes in the typical underlying individual expected utility depend almost exclusively upon changes in one's expected length of sentence.[1]

Needless to say, new information about future conditions may reflect "good" or "bad" news. Previous research has shown that bad news might indeed lead to extreme self-harming behavior. In order to prevent suicides, criminologists and psychologists have tried to build the typical profile of suicides in prison. Despite lack of data and small sample issues (Liebling, 1995, 1999) this research has concluded, among other things, that a period of risk for pretrial inmates is around the time of a court appearance, especially when a guilty verdict and harsh sentencing may be anticipated. For sentenced prisoners, on the other hand, triggers for suicide are to

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[1]Other things being equal, expectations about one's length of sentence come into play because, as in the typical prison system, Italian prisons hold also inmates awaiting trial. For the already sentenced criminals, on the other hand, it is the length of sentence that really matters since "an inmate's dream is to fall asleep one day and wake up half a year later" (Kaminski, 2004).

be found in negative legal dispositions, such as loss of an appeal or the denial of parole (Wicks, 1972, Hayes, 1995, WHO, 2000).

Our focus in the present paper will be instead on "good" news. In Italy, collective sentence reductions (pardons) often lead to massive releases of prisoners. More importantly, they are usually preceded by prolonged parliamentary activity (legislative proposals, discussion, voting in subcommittees, etc.) that inmates seem to follow closely.[2]

Our contribution is to use legislative proposals for collective pardons to measure changes in the inmates' expectations about the length of their sentences, and to show that suicide rates tend to be significantly lower when pardons are proposed in congress. Our findings indicate that, amongst inmates in Italian prisons, the average decision to commit suicide does depend upon current expectations about future conditions. This necessitates of course that the underlying decision-making process has at a minimum some component which does respond to informational incentives and which can be thus rationalized.[3]

We also find that not all proposals carry the same signal. In particular, proposals presented by congressman with a history of unsuccessful proposals (more than two) do not induce any changes in suicides. Similarly, proposals presented in the future, that thus are not yet known by the inmates, do not induce any behavioral change (this finding also alleviates concerns about reverse causality).

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[2]Unfortunately in this context we cannot study "bad news," as proposals that are expected to be unsuccessful are not even put to vote but rather kept on hold indefinitely.

[3]It should be noted here that our findings do not by any means render the decision to commit suicide rational per se. They do point out, however, that some of its constituting components may be rational. As an assertion, this has been made before in the literature but in decision-making contexts that differ sharply from the one under consideration here. See DellaVigna (2009) for an overview of decision-making situations that reflect potential departures from perfect rationality yet in which the agents do respond to informational incentives. See also Drago et al. (2009) as well as Barbarino and Mastrobuoni (2014) for another use of collective pardons as a policy instrument: to identify deterrence and incapacitation, respectively.

The balance of the paper is structured as follows. Sections 2-3 describe the policy instrument and the decision-making situation under investigation. Section 4 presents the data and the empirical analysis while Section 6 concludes.

2 Collective Pardons

The policy instrument we will use to measure changes in the current prisoners' expectations about their future conditions are legislative proposals in the Italian Parliament for collective pardons (*Amnistie e Indulti*). These are non-discriminatory sentence reductions, typically by 2-3 years, for crimes committed before the law gets passed, and lead to the immediate release of all inmates whose remaining sentence is less than the reduction itself. For a collective pardon to become Italian law, it has to be proposed first by a member of the Parliament and then assigned to a Parliamentary Commission (*Commissione Parlamentare*). From there, once the proposal has been discussed and appropriately prepared, it is sent to the parliamentary chamber that currently sets the agenda. And to be approved, the proposal has to be voted without further changes in both Chambers of Parliament, the *Camera dei Deputati* and the *Senato della Repubblica*, by an absolute majority of two-thirds (Constitutional Law 6/1992).[4]

Of course, collective pardons in Italy have not been part of any systematic prison reform. In the recent past especially, they rather represent isolated and ad-hoc attempts to address pressing problems of prison overcrowding.[5] In this sense, legisla-

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[4]Before 1992, a simple legislative majority sufficed for collective amnesties and pardons to become Italian law. It should be pointed out also that collective pardon proposals can be made in addition by the government, the regional councils (*Consigli Regionali*), and the citizens themselves (as long as at least 50,000 signatures are collected so as to constitute an *Iniziativa legislativa Popolare*). More often than not, however, and in line with most legislative proposals in Italy, they are initiated in the Parliament.

[5]To give a historical perspective, between the unification of Italy in 1865 and Mussolini's defeat

tive proposals for collective pardons reflect by themselves good news for people that are behind bars. For they are not only necessary for an amnesty law to be passed, but their very number is correlated with the likelihood that this happens.

This is depicted vividly in Figure 1 with respect to the period between 1980 and 2011: shortly before the amnesty law gets passed (an event shown by a vertical line in the figure), the number of proposals for such a law increases dramatically.[6] More specifically, between January 1980 and June 2011, the introduction of at least one pardon proposal in the Italian Parliament in a given month increases the likelihood that in the following month an amnesty law is passed from less than 1 percent to 3.7 percent (at the 10 percent level of significance). In fact, each proposal by itself increases the likelihood in question by 1.3 percentage points (at the 1 percent significance level).[7]

Equally importantly perhaps for the purposes of our study, pardon proposals are noisy signals of parliamentary activity on collective pardons as they do not make it into a law all the time. In June and July 2000, the year of the Great Jubilee, Pope John Paul II asked policy makers to pardon inmates while visiting the Roman prison of Regina Coeli, one of the largest in the country. This led to a surge in pardon proposals. Nevertheless, no actual law was passed and, in November 2002, the Pope went to visit the Italian Parliament, asking again for a pardon. This led to a second surge in proposals - in fact, given how easy it was to predict the Pope's request,

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in 1943, there have been approximately 200 pardons or amnesties. Some of these were fiscal pardons or amnesties for very specific crimes. Others were aimed at easing social tensions or magnifying the royal family. Between 1945 and the present, on the other hand, approximately a dozen of collective clemency acts have been passed. Initially, they were aimed at reconciling a politically-divided nation. More recently, however, the main goal has been to alleviate severe overcrowding in prisons.

[6]For example, the number of pardon proposals in the sample peaks at 7 and 14 in June and July 2006, respectively, anticipating the amnesty law of August 2006.

[7]To avoid our exposition being unnecessarily lengthy, the full regressions results have been omitted from the present version of the paper. They are of course available upon request.

the surge actually anticipated his visit exhibiting 4, 1, and 6 pardon proposals in September, October, and November 2002, respectively. However, again no amnesty law was passed.[8]

Since pardon proposals will be our measure of "good" news, it is important to understand how favorable and for whom amongst the Italian prisoners the corresponding event is likely to be. The period under consideration (between January 2002 and June 2011) saw only one amnesty law, on 1 August 2006, and individual-level data of inmates pardoned on that date shows the average reduction in the length of their sentence to be 15 months. More impressively perhaps, this means that on average 45 percent of one's remaining sentence was pardoned. Figure 2 depicts the relative frequency of the length of the remaining sentence amongst the Italian prisoners on 1 August 2006, and the fraction of this length that was pardoned on that date - either variable as a function of one's total remaining sentence. On average, an inmate whose total remaining sentence did not exceed 3 years saw it being reduced by half. Inmates with longer remaining sentences had their sentence reduced by a smaller fraction; yet, on average this fraction almost never falls below 10 percent while the measure of these cases in the total population decreases sharply as the remaining sentence length increases. Clearly, based on the August 2006 amnesty law, we may safely assert that pardons induce sizable reductions in one's prison time.

Regarding which inmates in particular are likely to receive a sentence reduction, Table 1 shows the distribution of criminals by crime type, before and after the August 2006 amnesty law, together with the relative reduction in the corresponding

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[8]Our data on suicides in Italian prisons goes back to only January 2002 and, thus, our study of the effect of pardon proposals on suicides refers to the period between January 2002 and June 2011.

prison population. Evidently, the last pardon generated large releases amongst all types of criminals, with the exception of those imprisoned for mafia-related crimes (even though other types of crime that comprise a mobster's sentence may get pardoned, mafia-related ones never do). In fact, 37 percent of the total population in Italian prisons were released immediately while, as we have pointed out above, almost all of the remainder received a substantial sentence reduction, rendering them eligible to be released quite ahead of their initial schedule.

Of course, there is some uncertainty about receiving a pardon: as a rule, to prevent abnormal increases in crime just prior to the introduction of an amnesty law, collective pardons in Italy apply only to crimes committed up to a specific date, usually three to six months before the date on which the law is signed. As a result, for those who happen to have committed a crime too close to the latter date, the pardon is likely to bring nothing but the psychological burden of seeing their fellow inmates released while they stay behind. This notwithstanding, based again on the August 2006 amnesty law, many inmates ought to expect to be released immediately following an amnesty law whereas the vast majority of them ought to expect a significant anticipation of their release date.

3   Suicides in Prison

The yearly suicide rate in Italian prisons is close to 1/1000. This is about 10 times (20 times) higher than that amongst the general (male) population, slightly above the average across the prisons of other European countries, and twice the current average suicide rate across the U.S. jails. Since the number of inmates fluctuates around 60,000, there are about 60 suicides per year (i.e., 5 per month) generating a relatively noisy evolution of suicides.

At first glance, the relatively high suicide rates in Italian prisons may seem paradoxical with respect to the claim that the decision to commit suicide might exhibit a rational component. After all, if being imprisoned is very likely to lead to the ultimate penalty, a rational agent should have foreseen and avoided this contingency at all costs, including not becoming a criminal in the first place.

Theoretically, the apparent paradox may be explained by the hypothesis that criminal behavior reflects the choice to accept a gamble of high returns from crime against committing suicide if apprehended and convicted (see Becker and Posner (2004) for a detailed discussion). In more practical terms, it could be driven by error in the sense that some criminals might not be able to foresee how harsh prison conditions can be. Of course, this does not mean that medical conditions do not matter, or that suicides in prison cannot be prevented.[9] It does render though medical conditions as but one of the drivers of suicides in prisons. For an inmate may well choose to not attempt suicide on the onset of a serious depression, but rather wait to see what the future brings. More importantly, one should keep in mind that the goal of our study is to identify a rational component in the decision to *not* commit suicide.

In prison, attempted suicides tend to be 10 times more than actual ones. Apart from committing a successful suicide being not the easiest of tasks, this may reflect also attempts by inmates to induce their transfer to the hospital, to a different cell, or even their eventual release (see Kaminski, 2004). Since penitentiary authorities are aware of this, the attempts themselves need to be credible, which intensifies the risk of them turning into actual suicides. This notwithstanding, whenever an amnesty

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[9]See, for example, Ludwig et al. (2009) for a recent economic study on anti-depressants and suicides. For a review of the large literature on the relation between the health status of prisoners and suicides in prison, which is outside the scope of the present paper, see Fazel and Baillargeon (2011) and Hayes (1995) among others.

is deemed to be likely, one should expect a decline even in these "hyper-rational" suicides.[10]

4   Data and Preliminary Evidence

Our identification strategy uses detailed information on the timing of (i) actual suicides in Italian prisons, and (ii) proposals for collective pardons in the Italian Senate or House of Representatives, over a 10-year period (2002-2011). We obtained the data on the prison population from the Italian Penitentiary Administration (DAP) while that on the legislative proposals for collective pardons can be found on the Italian Senate's website (www.senato.it). Regarding the exact dates of individual deaths, our data come from the research group *Ristretti Orizzonti* (www.ristretti.it) - unfortunately, it does not include any information about the actual or expected length of sentence of the respective inmates.[11]

The summary statistics are shown in Table 2. On average there are more than 4 suicides and about 1.5 natural deaths a month. There is on average a proposal every two months, and minor parties, minority parties, and congressmen with a history of pardon proposals tend to present more proposals, which is something we are going to exploit later.

The parliamentary activity on pardon proposals typically coming in concentrated waves in our sample, Figure 3 sets the event times to be the months in our sample in which uninterrupted strings of proposal activity (more precisely, sequences of consecutive months for which in each and every month at least one pardon proposal

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[10]The term is borrowed from Kaminski (2004), where it is argued that the stakes can be so high in prison to render even what appears to be inhuman and bizarre behavior "hyper-rational."

[11]The same data have been used by Drago et al. (2011) to look at the relationship between prison conditions (proxied by deaths in prison) and recidivism (see, also, Katz et al., 2003).

appears in the Italian parliament) come to an end. For a period of four months before and four months after the event time,[12] the average number of pardon proposals per month (depicted by squares in the figure) is compared to the average number of suicides per month (depicted by vertical bars in the figure). A local polynomial regression (depicted by the dashed line in the figure) smooths the monthly averages to ease the analysis. Suicides in Italian prisons appear to respond to the ongoing parliamentary activity. More precisely, suicides tend to decrease while pardon proposal activity is being built up in the parliament and increase back again after a few months when this comes to an end. The number of suicides exhibit a local minimum around the time a wave of proposals ends.

Pardon proposals are frequent but negative votes on them are extremely rare. Most proposals that are doomed to be unsuccessful are past the first parliamentary discussions kept on hold, or (one might say) forgotten in some drawer. In fact, there was only one date (12 December 2006) in our nine-year sample when a pardon proposal was turned down by vote.[13]

While "good news" might translate into immediate changes in expectations (the negative event times), in the absence of "bad news" a smooth increase in suicides following the end of a wave of proposals is the evidence one would expect as hopes of freedom are being shattered.

As a preliminary placebo tests, Figure 4 shows that for the same period of four months before and four months after the event time, no relationship emerges between proposals and natural deaths in prison.[14]

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[12]Such spacing is determined by the typical spacing between waves of proposals.

[13]One observation is far from enough to perform any meaningful statistical test (although there is a significant increase in suicides immediately after that date).

[14]Again, to smooth the monthly averages (the vertical bars) a local polynomial regression (depicted by the dashed line in the figure) shows now no evidence of any relation between deaths from natural causes and the ongoing parliamentary activity.

Based on Figure 3, another way to present the evidence is to relate suicides to proposals presented in previous months. In order to take potential seasonal components into account, Figure 5 presents the average suicide rate by each month of the year - for instance, the average suicide rate following January across our 10-year sample. In doing so, we restrict attention moreover at whether there were any legislative proposals during the month that precedes the one in which the suicide takes place so as to avoid any reverse causality, in the sense that members of congress may be more likely to propose collective pardons when suicide rates are particularly high.[15]

The results of this approach are depicted in Figure 5. Namely, for a given month of the year, the suicide rate for that month is lower if the month was preceded by legislative proposals than when no pardon proposals have been made in the preceding month. As an observation, this refers to any month of the year but February, March, April, September, and November - these five months are not included in the figure because no pardon proposals were ever made in our sample in any of these months.

As a rule, this evidence applies across the years in our sample with only one exemption, August 2006. If we were to include this month in our analysis, the August suicide rate when pardon proposals occurred in July would be higher across the sample than the suicide rate in August when no pardons have been proposed in July. Of course, the August in question is a very peculiar August. On 1 August 2006, a pardon proposal succeeded into actually becoming law - the only time this occurs

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[15]Of course, it could be argued that one month is not long enough a period to rule out contemporaneous relations that may be driving the observed correlation between pardon proposals and changes in suicide rates. Indeed, one could surmise that current poor living conditions in prisons (for instance, due to over-crowding or budgetary problems) force inmates to end their lives, and subsequently legislators to propose collective sentence reductions. Under this hypothesis, however, the implied correlation between pardon proposals and changes in suicide rates ought to be positive, not negative as in the data.

during the period under investigation. And it could well be that the discrepancy this particular month is causing on the August suicide rates in our sample are driven by the suicides of inmates who did not receive a sentence reduction on 1 August 2006. In fact, this category of prisoners had to cope not only with the psychological burden of seeing their fellow inmates released or soon to be released (which would be in line with evidence on relative comparisons, as in Daly et al., 2013), but also with the historical regularity that following an actual pardon the next one might be a decade or more away.

Evidently from Figure 5, for each and every of the seven months shown in the figure and across the years in our sample, the monthly suicide rate when pardons have been proposed during the preceding month is lower than when no such legislative proposals were made. While this already attests to an apparently significant , that appears to be significant (the likelihood of getting seven "downs" out of seven independent Bernoulli "up-down" trials using a fair coin is only $0.5^7 = 0.008$) negative relation between pardon proposals in the Italian parliament and suicides in Italian prisons, in what follows we will investigate this rigorously by means of regression analysis.

Before proceeding, however, and as a preliminary placebo test, we can check that there is no apparent relation between pardon proposals and the death rate amongst inmates from causes other than suicides.[16] To this end, Figure 6 depicts the monthly death rate in Italian prisons from natural causes, categorizing again the data by whether there were legislative proposals during the preceding month, and excluding again the August 2006 observation.

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[16]One might consider comparisons of the pardon proposals with the national suicide rate in Italy, or the suicide rate amongst Italian men, as another placebo test. Prisons being isolated environments, however, the power of such tests is bound to be inadequately small.

Our comparisons here refer to any given month of the year across our sample but for the months January, June, and December - as there were no deaths from naturals or unknown causes in our sample in any of these months when at least one pardon proposal was made during the respective preceding month. As opposed to our observations for the suicide rates, there seems to be no clearly obvious pattern between pardon proposals and deaths from causes other than suicides - in fact, the monthly death rates for May and July are now significantly higher when pardons have been proposed during the preceding month than when no legislative proposals were made. To put it more formally, the hypothesis that the observed relation between pardon proposals and death rates from natural or other causes is due to independent Bernoulli "up-down" trials using a fair coin cannot be rejected.

5   Regression Analysis

Needless to say, claiming descriptive evidence of a relationship between pardon proposals and changes in suicide rates assumes at a minimum that inmates do follow the parliamentary activity on pardons from within the prison confines. Of course, in the absence of any evidence that suicides respond to pardon proposals, one would not be able to rule out that inmates do not or cannot follow parliamentary activity. By contrast, the fact that such evidence seems to exist implies that inmates do follow the parliamentary activity on pardons, at least on average. Nonetheless, this is an assumption and, as such, it needs to be checked.

To this end, we looked at the distribution of articles that appeared on two of the main Italian newspapers, *Il Corriere della Sera* and *La Stampa*, and found that in 75 percent of the weeks in which a pardon proposal was introduced in parliament one or more articles dedicated to pardons appeared in press, a likelihood that drops to

less than 20 percent for the weeks without proposals.[17] Furthermore, conversations with Francesco Morelli and Riccardo Arena, the respective directors of the monthly prison magazine *Ristretti Orizzonti* and the weekly prison radio program *Radio Carcere*, revealed that inmates are well aware about the legislative proposals while news inside prisons tend to travel very fast by word of mouth.[18] Unfortunately, we could not find any hard evidence on these claims, other than the fact that *Ristretti Orizzonti*'s website has about 4,000 unique visits per day.

5.1 Benchmark Results

Given that we have only anecdotal evidence about how information flows into prisons, we proceed with the deployment of regression analysis to test whether pardon proposals lead to significant differences in suicide rates. Since legislative proposals are bound to be orthogonal to any individual characteristic that might affect an inmate's suicidal behavior, parsimonious regression specifications are sufficient. Table 3 presents the estimates of different Poisson regressions, the exposure variable being prison population (which in Italy is measured by semi-annual census).[19] We aggregate the data by month to reduce the variability in suicides and regress the total number of suicides on the number of proposals presented in previous months. Given the presence of seasonality in suicidal behavior, each regression includes monthly fixed effects. To account, moreover, for the fact that the actual pardon in August 2006 might be "bad news" for those inmates who did not receive a sentence re-

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[17]The articles in question are freely available on the journals' respective websites (www.corriere.it and www.lastampa.com).

[18]*Ristretti Orizzonti* (Limited Horizons) was established in 1998 while *Radio Carcere* (Radio Prison) has been broadcasted since 2002 by *Radio Radicale* (Radical Radio), a well-known Italian radio station.

[19]The expected number of suicides in month t is given by $\mathbb{E}[y_t|\mathbf{x}_t] = Pe^{\theta^\intercal \mathbf{x}_t}$, where P represents the prison population and $\mathbf{x}_t$ the array of pardon proposals in the preceding months. The coefficients can be interpreted as simple semi-elasticities: $\theta_i = \mathbb{E}[y_t|\mathbf{x}_t]^{-1} \frac{\partial \mathbb{E}[y_t|\mathbf{x}_t]}{\partial x_{it}}$.

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duction, each regression includes also a dummy equal to one for August 2006. Its coefficient is always significant and approximately equal to two. That is, amongst those inmates who had to remain behind bars while nearly 20,000 of their fellow inmates were released following the amnesty law of August 2006, suicide rates were 200 percent higher than expected, despite a reduced overcrowding!

We do restrict attention to the pardon proposals presented in the preceding months. The use of such lags reduces the concern of reverse causality which would bias the estimated coefficient upwards. It accounts also for the possibility that it might take on average several days for the information about legislative proposals to reach all inmates. Column 1 of Table 3 controls for pardon proposals presented up to 3 months earlier $(t-3)$, while each of following columns tests the exclusion of the last lag using the Akaike Information Criterium. The latter suggests the last column as the preferred specification - indeed, for all but the immediately-preceding month the lags correspond to coefficients that are very close to zero in terms of value and not different from zero in terms of statistical significance.

The first row of Table 3 indicates also that each pardon proposal in the previous month reduces the number of suicides by between 7.5 and 8 percent. Each column of the table presents also the p-value of an overdispersion likelihood-ratio test that compares the Poisson model to the Negative Binomial one. The latter regression approach might seem as an even more natural benchmark, being at least more flexible as it relaxes the assumption that the conditional variance equals the conditional mean. Yet, none of the specifications allows us to reject the Poisson model in favor of the Negative Binomial one. Indeed, under either regression approach, the estimated dispersion parameter is always very close to zero, while the estimated coefficients are almost identical between the two models.

Conditional on proposals presented in the previous month the ones presented before do not seem to matter.[20] This evidence is fully in line with our previous event study (see Figure 3). What seems to matter for inmates are signals that the legislative process is progressing, meaning that in the previous period proposals have been presented.

Following the same informational logic, conditional on the proposals presented future proposals should not influence the suicides rate (unless proposals are presented to reduce the number of suicides). Table 4 tests this prediction using proposals presented up to 3 months after the suicides. There is no evidence that future proposals influence current suicide rates.[21]

5.2 Difference in Differences Results

The evidence of a relationship between pardon proposals and suicide rates appears not only descriptive, nevertheless it could be that the observed correlation is driven by some other underlying factor to which both pardon proposals and suicides in prison respond. Of course, the first such factor that comes to mind has to do with living conditions in prisons - in particular, overcrowding. And this is precisely the story behind the central claim of the present paper. For if pardon proposals are the product of severe problems with living conditions in prisons, the resulting reduction in suicides rates cannot but be due to the inmates' improved expectations about their future living conditions.

Another factor, however, could be that, for some reason, pardon proposals in the

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[20]They matter more if one did not control for the subsequent ones. Results are available upon request.

[21]Adding the potentially endogenous present number of proposals to the analysis does not alter the results, and the coefficient on the present number of proposals is negative and close to being significant. Given that the proposals presented in the same month that suicides are recorded are potentially endogenous we prefer to exclude them from the regressions.

Italian parliament are decreasing the levels of violence inside Italian prisons, so that suicides in prisons might not be the product of forward-looking considerations, but of the resulting reduced levels of violence. With respect to this hypothesis, recall that we also have information on deaths related to causes other than suicides. Our next identification strategy, therefore, is going to exploit information on the number of deaths that are supposedly due to natural causes (using a difference in difference procedure). Instead of the suicide rate being our dependent variable, we will use now the difference in the monthly death rates (per 10,000 inmates) from suicides and natural causes. Table 5 presents difference-in-differences regression results.[22] Since the differences in question take both positive and negative values, we deploy estimation by ordinary least squares, controlling again for the August 2006 amnesty law dummy and for monthly fixed effects.[23]

As shown by columns (1)-(3) of the table, following more legislative proposals in the preceding month, there are consistently fewer suicides in the current month, as compared to natural deaths. The corresponding regressions deploy three, two, and one lag: only proposals introduced in the immediately preceding month have a significant effect in lowering the difference in the respective death rates.

The coefficients in Table 5 are around 5 percent, meaning that the typical proposal is associated on average and in the short run with 0.05 fewer suicides per month per 10,000 inmates than deaths by natural causes.

Given that on average there are 0.72 per 10,000 inmates suicides every month, in relative terms, the negative effect on the suicide rate is close to 7 percent, and thus

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[22]Unfortunately, our data does not permit a finer categorization of the other causes of death. Needless to say, had this been possible (for example, into violence, health problems, infectious diseases, etc.), it would allow for a more thorough investigation.

[23]Estimating a Poisson regression controlling for the number of deaths from natural causes leads to the same results.

comparable to the Poisson estimates above. Given that there are about 60 suicides a year, on yearly basis this translates in four saved lives. Since we are dealing with changes in expectations that are notoriously hard to measure, and since this is the first study to directly test such forward-looking behavior in suicides, it is difficult to judge the size of the estimate.

5.3   Political Heterogeneity of the Effects

Overall, our analysis is quite conclusive that the typical pardon proposal tends to be associated with a reduction in the suicide rate, but not in the rates of death due to other causes. Yet, it may well be that not all legislative proposals are equally powerful signals in inducing inmates to adjust their expectations about the likelihood of an amnesty law.

It should be noted, in particular, that for a pardon proposal to become amnesty law an absolute parliamentary majority of two-thirds is required. As a consequence, the pardon proposals that are put forward by parliamentary members of the largest political parties could be more important signals than those proposals which are presented by members of parliament who belong to minor parties. The results reported in Column 1 of Table 6 suggest that this is indeed the case, despite the lack of statistical significance. Proposals presented by politicians who belong to the major parties (namely, the 9 proposals presented by members of ULIVO, the main center-left party at the time, and the 12 proposals put forward by members of FORZA ITALIA, the main center-right party at the time) do carry more weight than the ones presented by the remaining minor parties.

Given that an absolute parliamentary majority is required for a pardon proposal to become amnesty law, restricting attention also to whether or not a given pardon

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proposal is made by a politician belonging to the governing parliamentary majority refers to another partition of the Italian parliament that may as well matter for signalling. In this sense, Column 2 of Table 6 indicates that proposals put forward by members of the parliamentary opposition carry more weight, a result in line with the very fact that, throughout the period under consideration here, the required absolute majority of two thirds had been impossible for the governing parliamentary majority to achieve on its own.

Yet another determinant of the relation between a given legislative pardon proposal and its power in signalling the likelihood of an amnesty law has to do with the "credibility" of the politician who puts it forward. Indeed, as in any signalling model the receiver's belief-updating rule is bound to depend on the sender's credibility (see DellaVigna and Gentzkow, 2010, for an overview on persuasion in economics), the inmates' inferences on the likelihood of success of a given pardon proposal will depend on what they know about the proposer.

To frame the issue more precisely, the question here is whether proposals presented in parliament by politicians with a long record of such proposals (measured by the total number of pardon proposals they have put forward) induce fewer changes in suicides than proposals by members of parliament without a reputation for such initiatives.[24] To this end, the final column of Table 6 shows that pardon proposals originating from members of parliament with more than 3 such proposals under their belt (which corresponds to 46 percent of the total number of pardon proposals) tend to reduce suicides in prisons by less than the proposals that are initiated by members with track record of fewer than 3 proposals.

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[24]For example, between 2000 and 2006, Marco Boato of the Partito Radicale introduced 11 pardon proposals. The corresponding frequency, on average almost two proposals per annum, coupled with the subsequent failures to reach the required parliamentary approval, might reduce significantly the value of a given proposal as a signal that an amnesty law is more likely.

5.4 Robustness Checks

It remains to examine the robustness of the preceding analysis. To this end, Table 7 presents several checks. Columns 1 and 2 show our two benchmark regressions: the Poisson one and that based on the most conservative difference-in-differences specification, respectively. The latter refers to the difference between the rate of suicides and the death rate from other causes, as been depicted by Columns 5-6 of Table 5. Year fixed effects are added in Columns 3 and 4 of Table 7. In these specifications, the identification is based only on the differences in pardon proposals across months within each year. Compared to the first two columns of the table, the results now change a little. The Poisson coefficient goes from -8 to -7 percent, and its significance falls from the 5 to the 10 percent level. By contrast, the difference-in-differences coefficient increases from -5.8 to -8.6 percent, while its significance rises from the 10 to the 5 percent level.

Equally small changes are obtained also if one does not control for exposure in the Poisson model. This is shown by Column 5 of Table 7 while its last two columns depict another important robustness check. This consists of including past suicide rates (Column 6) or past differences between suicides and death rates from other causes (Column 7), so as to control for reverse causality. More precisely, if past suicide rates were correlated with past proposals, the coefficient on past proposals might just capture autocorrelation between present and past suicide rates. However, this does not seem to be the case. The autoregressive coefficient is precisely estimated as being close to zero while the coefficient on past proposals is only slightly lower than before.

Finally, the estimated coefficients of interest become only slightly larger when we restrict attention to those years in which most of the proposals were presented

(2002-2006). The pertinent results are shown in Table 8 whose first two rows refer to the corresponding contemporaneous relations. These are based upon the assumption that the effects of pardon proposals on suicide rates materialize within the month that follows a proposal.

Nonetheless, to account also for the possibility that it takes longer for the news about legislative proposals to reach the prison confines and induce inmates to adjust their expectations, the subsequent rows of Table 8 introduce expectation proxies that are based on the average number of proposals in the preceding 2, 3, or 4 months. The largest effect (10.9 percent reduction in the suicide rate per proposal) obtains when we use a lag of 3 months to measure expectations, but the differences are small overall. And as before, the Akaike Information Criterion favors the specification that restricts attention to the immediately-preceding month.

6   Concluding Remarks

Clearly, by falling on average when the legislature proposes new pardons, suicide rates in Italian prisons do respond to changes in expectations about future conditions. Pardon proposals constitute "good news" for the inmates, who might expect reductions in their length of sentence, but also improvements in their living conditions via an alleviation of over-crowding in prisons. Overall, our analysis provides evidence of forward-looking behavior in the decision to not commit suicide, and in this sense our findings are consistent with two different strands of the literature: the one on the rationality of suicides, and that on the rationality of criminals (Becker, 1992, see, for example,).

To arrive at this conclusion, we exploited variations in inmates' expectations as measured by legislative proposals for collective pardons. Methodologically, our

main underlying assumptions have been that (i) actual amnesty laws are preceded by several pardon proposals, (ii) inmates follow the respective parliamentary activity, and (iii) all this modifies their expectations about the likelihood of an actual amnesty law. With respect to assumptions (i)-(ii), we provided evidence in support of the first and showed that the second can be defended on the basis that national newspapers, a prison magazine, and a weekly radio program devoted to life in prison cover parliamentary activity on pardon proposals extensively.

Regarding assumption (iii), the precision of our measure of changes in expectations is hardly verifiable, and prone to error. For example, inmates might not get immediately informed about a given proposal, or have additional information about its likelihood of turning into amnesty law. Given that we do not have data on the underlying heterogeneity in expectations, we undertook several robustness checks and tried different ways in our treatment of pardon proposals with respect to their origin (in time, but also in parliament). Overall, our analysis points towards a significant and rather quick adjustment in inmates' expectations about their future in prison following legislative pardon proposals.

Of course, the adjusted expectations in question might not have to do solely with a new date of release. Perhaps the hope of early release makes family re-engage or engage more with prisoners (and, thus, affect them psychologically for the better), or independently improve the psychology and moral in prison, turning it temporarily into a less unpleasant place. In either case, however, pardon proposals ought to be "good news" for those behind bars, and the presence of any rationality in their decision to commit or not suicide should force them to respond accordingly.

Unless a pardon proposal turns at some point from "good news" into "bad news," crushing inmates' hopes. This ought to become the case as time passes without the

proposal being turned into an actual pardon. What matters then would be the net effect of pardon proposals on inmates' expectations. For example, under certain types of preferences, such as habit-formation or loss-aversion, more pardon proposals could lead to an increase in suicides as agents prefer no news to a variety of good and bad ones.

Needless to say, ultimately, whether the net effect of pardon proposals on inmates' expectations is positive or negative cannot be but an empirical question. And, unfortunately, there is no readily available measure of "bad news."

The above considerations notwithstanding, given the noise that pardon proposals entail as signals of future amnesty laws, our analysis agrees broadly with the option value theory of rational suicides. The latter predicts that the value of postponing a suicide should increase when the uncertainty about future conditions increases. In this sense, our results indicate that suicides in Italian prisons would probably be higher if there were no amnesty laws, even if these were to be replaced by a mean-preserving reduction in the variability of the length of sentence.

Yet, this is far from saying that pardons should be used as policy instruments to reduce suicides in prisons. Even though by all means a central policy question, if amnesties are to be treated more seriously than simple ad hoc measures to reduce overcrowding in prisons, the relevant cost-benefit analysis is beyond the reach of the present study. We do not have the required data to estimate parameters such as the number of lives saved per year of sentence pardoned, the potential crimes that criminals released ahead of schedule might commit, or the corresponding social benefits and costs.

It is equally hard to draw conclusions regarding the effect of fewer or more pardon proposals. Obviously, if the proposals were to be increased without altering

the number of actual pardons, the perceived impact of the typical proposal on one's expectation about the likelihood of an amnesty law should diminish. In the absence, therefore, of the necessary data to estimate in any meaningful sense the size of the effect proposals exert on the likelihood of an actual pardon (and, in turn, on the suicide rate in prison), the present paper has little to say in terms of policy implications or recommendations.

Our scope is limited into establishing that the decision to not commit suicide in prison includes at a minimum some rational component: expectations about future conditions do play a role, and do respond to relevant public information. And in this sense, ours ought to be a conservative test of rationality in suicides. For given that criminals are often viewed as boundedly-rational or even irrational individuals, if anything, our estimates ought to be biased toward zero.

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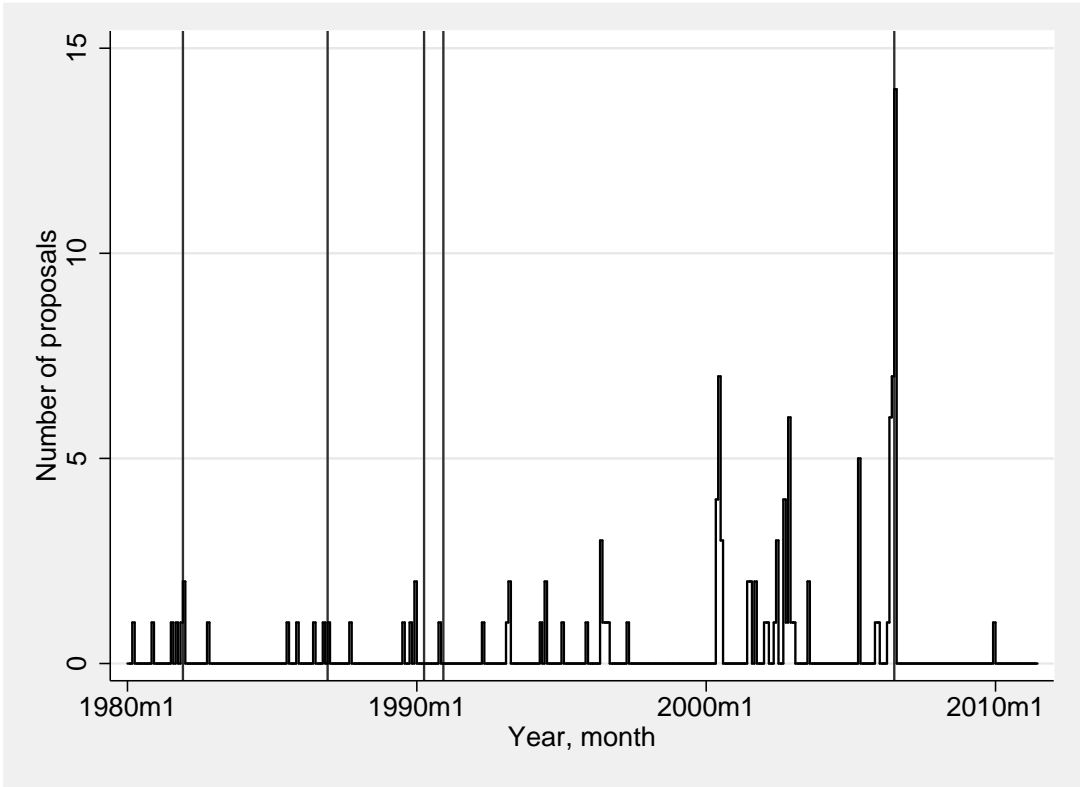
Figure 1: Legislative Proposals and Laws

Note The vertical lines indicate the passing of the laws, squares indicate the number of pardon proposals.
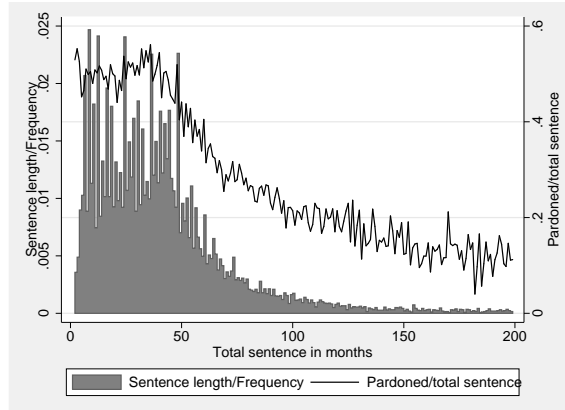
Figure 2: Fraction of Sentence Pardoned on 1 August 2006, and the Relative Frequency

Based on DAP (2006).
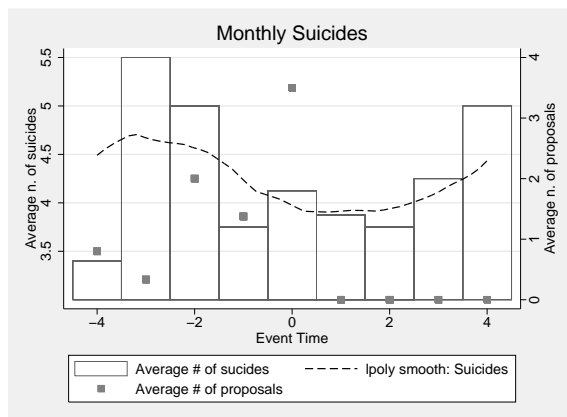
Figure 3: Cumulative-Peak Proposals and Suicides
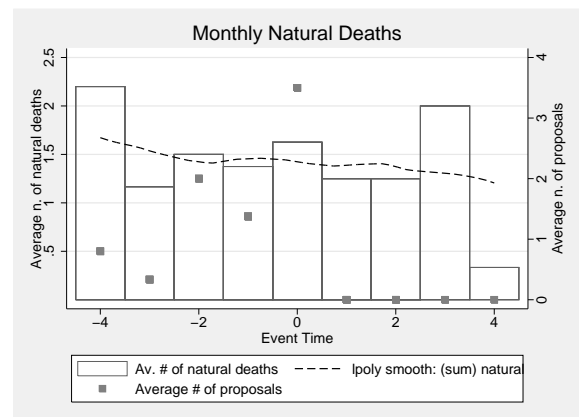
Figure 4: Cumulative-Peak Proposals and Natural Deaths

Note: In either figure, the event time (in months) is centered around the month where an uninterrupted monthly series of pardon proposals ends. The dashed line represents a local polynomial regression with a bandwidth of one month. The vertical bars (squares) indicate the average number of suicides (pardon proposals) per month.

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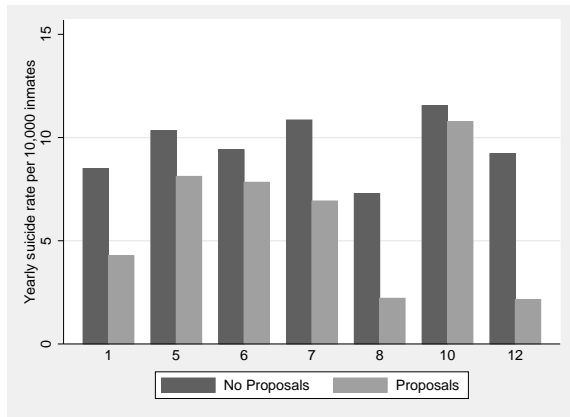
Figure 5: Monthly suicide rates by month excluding August 2006 (the month a pardon was approved). The light-shaded bars refer to the case in which there was at least one pardon proposal in the preceding month.
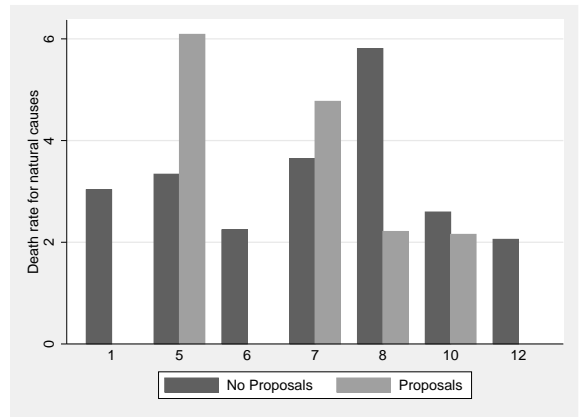
Figure 6: Monthly rates of death from natural causes. The light-shaded bars refer to the case in which there was at least one pardon proposal in the preceding month.

Table 1: The Distribution of Prison Population by Type of Crime Before/After the August 2006 Pardon.

	July 2006	rank	September 2006	rank	% Reduction
Crimes against wealth	0.309	1	0.277	1	-0.43
Crimes against persons	0.149	2	0.167	2	-0.29
Drug related crimes	0.146	3	0.166	3	-0.28
Illegal possession of weapons	0.141	4	0.144	4	-0.36
Public trust	0.048	5	0.041	5	-0.46
Crimes against the public administration	0.038	6	0.032	7	-0.47
Crimes against the justice department	0.034	7	0.027	8	-0.50
Third book of administrative sanctions	0.025	8	0.025	9	-0.37
Mafia related crimes	0.025	9	0.033	6	-0.17
Other crimes	0.085	.	0.088	.	-0.35
Total	1	.	1		-0.37
Total number of prisoners	60,710		38,326		

Based on DAP (2006). The last column depicts the percentage change in the number of prisoners by main crime typology.

Table 2: Summary statistics

Variable	Mean	Std. Dev.
Natural deaths	1.544	1.476
Suicides	4.158	2.18
Proposals		
Total	0.5	1.766
by major parties	0.105	0.570
by minor parties	0.342	1.977
by congressmen/women who presented fewer than 3 proposals	0.211	0.867
by congressmen/women who presented more than 3 proposals	0.237	1.897
by majority	0.175	0.823
by minority	0.272	1.441
N		114

The political variables that define the proposals are not fully balanced with the respect to the total number of proposals.

Table 3: The Effect of Past Pardon Proposals on Suicide Rates

	(1)	(2)	(3)
		Suicide rate	
Number of proposals $(t-1)$	-0.0753**	-0.0764**	-0.0802**
	(0.0295)	(0.0310)	(0.0321)
Number of proposals $(t-2)$	-0.0192	-0.0276	
	(0.0202)	(0.0180)	
Number of proposals $(t-3)$	-0.0269		
	(0.0190)		
2006 Pardon (0/1)	2.336***	2.249***	2.118***
	(0.470)	(0.473)	(0.484)
Pseudo R2	0.0393	0.0383	0.0343
AIC	493.31	494.97	498.47
p-vale overdispersion	0.5	0.5	0.5
Observations	111	112	113

Each column represents a different Poisson regression. The prison population measures the exposure: $\mathbb{E}\left[y_t|\mathbf{x}_t\right] = Pe^{\theta^\intercal \mathbf{x}_t}$, where P is the prison population, $\mathbf{x}_t$ depicts proposals in each of the preceding months, while θ corresponds to semi-elasticities. All regressions control for prison population and monthly fixed effects. The p-value refers to an overdispersion likelihood-ratio test that compares the Poisson model to the Negative Binomial model. Robust standard errors are shown in parentheses. Levels of significance are shown at 10 percent (*), 5 percent (**), and 1 percent (***).

Table 4: The Effect of Past and Future Pardon Proposals on Suicide Rates

| | (1) | (2) | (3) |
		Suicide rate	
Number of proposals $(t+3)$	0.0104		
	(0.0337)		
Number of proposals $(t+2)$	-0.0146	-0.0113	
	(0.0287)	(0.0240)	
Number of proposals $(t+1)$	0.00744	0.00925	0.00431
	(0.0221)	(0.0218)	(0.0203)
Number of proposals $(t-1)$	-0.0911**	-0.0924***	-0.0886**
	(0.0354)	(0.0355)	(0.0358)
Pseudo R2	0.0393	0.0388	0.0371
Observations	110	111	112

Each column represents a different Poisson regression. The prison population measures the exposure: $\mathbb{E}\left[y_t | \mathbf{x}_t\right] = P e^{\theta^{\intercal} \mathbf{x}_t}$, where P is the prison population, $\mathbf{x}_t$ depicts proposals in each of the preceding months, while θ corresponds to semi-elasticities. All regressions control for prison population and monthly fixed effects. Robust standard errors are shown in parentheses. Levels of significance are shown at 10 percent (*), 5 percent (**), and 1 percent (***).

Table 5: Difference in Difference Estimates of The Effect of Pardon Proposals

	(1)	(2)	(3)
	Suicides-Natural cause per 10,000 inmates		
Number of proposals $(t-1)$	-0.0509**	-0.0490**	-0.0479**
	(0.0217)	(0.0217)	(0.0213)
Number of proposals $(t-2)$	0.00854	0.00694	
	(0.0165)	(0.0138)	
Number of proposals $(t-3)$	-0.00654		
	(0.0140)		
August 2006 Pardon (0/1)	1.856***	1.802***	1.833***
	(0.325)	(0.324)	(0.309)
Observations	111	112	113
R-squared	0.171	0.166	0.171

Each column represents a different linear regression estimated by OLS. All regressions control for monthly fixed effects. Robust standard errors are shown in parentheses. Levels of significance are shown at 10 percent (*), 5 percent (**), and 1 percent (***).

Table 6: Political Heterogeneity of the Effect of Pardon Proposals on Suicide Rates

	(1)	(2)	(3)
		Suicide rate	
Number of proposals:			
by minor parties	-0.0429		
	(0.0438)		
by major parties	-0.145		
	(0.0881)		
by minority		-0.137*	
		(0.0709)	
by majority		-0.0216	
		(0.0601)	
by congressmen/women who presented fewer than 3 proposals			-0.109**
			(0.0499)
by congressmen/women who presented 3 or more proposals			0.00774
			(0.128)
August 2006 Pardon (0/1)	1.506	2.640*	0.326
	(1.430)	(1.468)	(3.359)
Observations	113	113	113

Each column represents a different Poisson regression. The prison population measures the exposure: $\mathbb{E}\left[y_t|\mathbf{x}_t\right] = Pe^{\theta^{\intercal}\mathbf{x}_t}$, where P is the prison population, $\mathbf{x}_t$ depicts proposals in each of the preceding months, while θ corresponds to semi-elasticities. All regressions control for prison population and monthly fixed effects. Standard errors are shown in parentheses. Levels of significance are shown at 10 percent (*), 5 percent (**), and 1 percent (***).

Table 7: Robustness checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Benchmark		With year fixed effects		No exposure	Dynamic regresion	
	Poisson	DinD	Poisson	DinD	Poisson	Poisson	DinD
Number of proposals $(t-1)$	-0.0802**	-0.0479**	-0.0703*	-0.0590**	-0.0924**	-0.0768*	-0.0494**
	(0.0321)	(0.0213)	(0.0362)	(0.0281)	(0.0377)	(0.0463)	(0.0212)
August 2006 Pardon	2.118***	1.833***	1.893***	1.802***	1.885***	2.131***	1.824***
	(0.484)	(0.309)	(0.497)	(0.378)	(0.558)	(0.779)	(0.313)
Lagged dependent var.						0.0140	-0.0497
						(0.0225)	(0.106)
Observations	113	113	113	113	113	113	113
R-squared		0.171		0.249			0.173

All but in Column (5) we control for prison population. All regressions control for monthly fixed effects. Robust standard errors are shown in parentheses. Levels of significance are shown at 10 percent (*), 5 percent (**), and 1 percent (***).

Table 8: The Effect of Pardon Proposals on Suicide Rates

	Measure of proposals	Sample	β	SE	AIC	Obs.
(1)	Last month	Full	-0.0802**	(0.0321)	498.47	113
(2)	Last month	2002-2006	-0.110***	(0.0373)	254.32	59
(3)	Last two months	Full	-0.0979***	(0.0351)	493.49	112
(4)	Last two months	2002-2006	-0.126***	(0.0458)	250.88	58
(5)	Last three months	Full	-0.109***	(0.0399)	490.16	111
(6)	Last three months	2002-2006	-0.138***	(0.0525)	247.95	57
(7)	Last four months	Full	-0.0762*	(0.0397)	488.24	110
(8)	Last four months	2002-2006	-0.0882*	(0.0519)	248.23	56

Each row represents a different Poisson regression. The prison population measures the exposure: $\mathbb{E}\left[y_t | \mathbf{x}_t\right] = P e^{\theta^\intercal \mathbf{x}_t}$, where P is the prison population, $\mathbf{x}_t$ depicts proposals in each of the preceding months, while θ corresponds to semi-elasticities. All regressions control for prison population and monthly fixed effects. Standard errors are shown in parentheses. Levels of significance are shown at 10 percent (*), 5 percent (**), and 1 percent (***).