

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? At least since the 70's economists have been trying to shed light on this question by studying whether suicide rates are related to contemporaneous economic 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.

Keywords: Suicides, Rationality, Pardons

JEL classification codes: K40, K42, H11

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1 Introduction

For a long time now, applied economists but also policy makers have been trying to establish whether incentives and marginal considerations of the kind typically addressed in economic analysis do play a role, even when the underlying decision-making scenario is extreme, and the choice itself might be irreversible. Undoubtedly, one of the first extreme decision-making scenarios that comes to mind entails an irreversible choice in the absolute sense, framing the issue with respect to the “economics” of 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 option value 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]).

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

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]).

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 quite 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. This might well be at play for instance if on average societies in which more individuals suffer from mental disorders tend to be also less productive. By contrast, the present paper does not attempt to test the existence of a rationalizable component of suicides using measures of current economic conditions. Instead, we look at changes in current expectations about future conditions, brought about by new information that renders future changes more probable without altering one's current wellbeing.

In particular, we test whether good news 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 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

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. In what follows, we will use the legislative proposals for collective pardons to measure changes in the inmates' expectations about the length of their sentences, and 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 that the underlying decision-making process has at a minimum some rational component, which does respond to informational incentives [see DellaVigna, 2009, for an overview of potential departures from perfect rationality where agents would still respond to such incentives].

The balance of the paper is structured as follows. Sections 2-3 describe the policy instrument and the decision under investigation. Section 4 presents the data and the empirical analysis while Section 5 concludes.

2 Collective Pardons

The policy instrument that we will use to measure changes in prisoners' expectations are proposals in the Italian Parliament for collective pardons (*Amnistie e Indulti*). These are non-discriminatory sentence reductions, typically by 2-3 years,

¹Other things being equal, expectations about one's length of sentence come in to 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].

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.²³ 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. To be approved, the proposal has to be voted without further changes in either of the two Chambers of Parliament, the *Camera dei Deputati* and the *Senato della Repubblica*, by an absolute majority of two-thirds (Constitutional Law 6/1992).⁴

Historically, collective pardons in Italy have not been part of any systematic prison reform. They rather represent isolated and ad-hoc attempts to address pressing problems of prison overcrowding. In this sense, pardon proposals represent 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 amnesty laws between 1980 and today. Shortly before the laws get passed, an event shown by both a vertical line, proposals start being presented. Sometimes,

²To give a historical perspective, between the unification of Italy in 1865 and Mussolini's defeat 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 reductions in prison overcrowding.

³Two recent papers use collective pardons to identify deterrence [Drago et al., 2009] and incapacitation [Barbarino and Mastrobuoni, 2011].

⁴Before 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 also 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.

though, proposals do not make it into a law. In June and July 2000, year of the Great Jubilee, Pope John Paul II asked policy makers to pardon inmates. He also visited the Roman prison of Regina Coeli, one of the largest in the country. This led to a surge in proposals. As no law was passed, in November 2002 he went to visit the Italian Parliament, asking again for a pardon. This led to a second surge, but again no pardon was passed.⁵

Since the data on suicides goes back to only 2002, Figure 2 focuses on such period, adding prison population (dotted line: on 1 August 2006, more than 20,000 inmates were released) and suicides to the picture. The number of proposals (diamonds) clearly signals the ongoing parliamentary activity: it increases from six in May 2006, to seven in June, and peaks at 14 in July. Throughout the period, of course, this is a noisy signal as it might well be wrong: in September, October, and November 2002 there were, respectively, 4, 1, and 6 parliamentary proposals that did not lead to any pardon.⁶

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. Individual level data of inmates pardoned on 1 August 2006 shows that the average reduction in the sentence length was 15 months. More impressively perhaps, this means that on average 45 percent of one’s remaining sentence was pardoned. Figure 3 depicts the fraction of the sentence that was pardoned as a function of one’s total remaining sentence. On average, the inmates

⁵Given how easy it was to predict the Pope’s request the surge actually anticipated the visit by a couple of month.

⁶Overall, our sample has 18 months in which at least one proposal was put forward in the Italian Parliament. Needless to say, a comprehensive look at the relation between pardon proposals and amnesty laws should try to estimate the likelihood that parliamentary proposals culminate to an actual pardon conditional on the history of recent proposals, or how different patterns in proposals influence this likelihood. However, this kind of analysis would require a much longer than the available time series.

whose total remaining sentence did not exceed 3 years saw it being reduced by half. The remainder of inmates, which gets proportionally smaller as the sentence length increases, had their sentence reduced by a smaller fraction, albeit on average almost never less than 10 percent. 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 total 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 know already, 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 signing of the law. As a result, for those who happen to have committed a crime too close to the amnesty date, the pardon is likely to bring nothing but the psychological burden of seeing their fellow inmates released while they have to stay behind. Nevertheless, 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 in their date of release.

3 Suicides in Prison

In Italy, the suicide rate amongst prisoners 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. 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. But an inmate may well choose to not attempt suicide on the onset of a serious depression, and rather wait to see what the future brings.⁷ 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

⁷See, 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.

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 is deemed to be likely, one should expect a decline even in these “hyper-rational” suicides.⁸

4 Data and 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 comes 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.

The raw data on the three variables of interest (prison population, suicides in prison, and legislative proposals for collective pardons) does not lend itself readily to recognizing a pattern, though deseasonalizing (subtracting monthly averages) and smoothing the monthly suicide rates (taking 3-month moving averages $MA(y) = 1/3(y_{t-1} + y_t + y_{t+1})$) shows that periods with prolonged parliamentary activity on

⁸The 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.”

pardons tend to be associated with reduced suicide rates.

Another way to take seasonal components into account is to look at the average suicide rate by each month of the year - for instance, the average suicide rate in January across our 10-year sample. In addition, we restrict attention 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.⁹

This is depicted in Figure 4, whose left panel shows that the months which were preceded by legislative proposals exhibit lower suicide rates. There is only one month that defies this rule, August, but there is also a very peculiar August in our sample. In August 2006 a pardon proposal succeeded into actually becoming law (the only time during the period under investigation). And it could well be that the August rates are driven by suicides of inmates who did not receive a sentence reduction on 1 August 2006. This category of prisoners had to cope with the psychological burden of seeing their fellow inmates released or soon to be released, as well as with the historical regularity that following an actual pardon the next one might be a decade or more away. Indeed, when we exclude this August from the data (see the right-hand panel of Figure 4), the August suicide rate following pardon proposals in July becomes three times lower than the suicide rate in August when no pardons have been proposed in July.

⁹Of course, it may be argued that one month is not long enough a period to rule out contemporaneous relations 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.

In fact, the monthly suicide rate when pardons have been proposed during the preceding month is now lower for each and every of the seven months which were preceded by a month in which legislative proposals were made. While this already attests to a negative relation between pardon proposals in the Italian parliament and suicides in Italian prisons, 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 $0.5^7 = 0.008$), in what follows we will investigate this rigorously by means of regression analysis.

For the time being, however, as 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.¹⁰ To this end, Figure 5 depicts the monthly death rate in Italian prisons from natural and unknown causes, again by whether there were legislative proposals during the preceding month. In either panel, the August 2006 observation has been excluded, and the hypothesis that the observed relation between pardon proposals and death rates is due to independent Bernoulli “up-down” trials using a fair coin cannot be rejected (the respective p-values for the left and right panel are 0.19 and 0.31).

4.1 Regression Analysis

Needless to say, claiming descriptive evidence of a relationship between pardon proposals and changes in suicide rates assumes 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

¹⁰One 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.

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.¹¹ 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.¹² 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.

4.1.1 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

¹¹The articles in question are freely available on the journals' respective websites (www.corriere.it and www.lastampa.com).

¹²*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.

2 presents the estimates of different Poisson regressions, the exposure variable being prison population (which in Italy is measured by semi-annual census).¹³ 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 reduction, 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 2 controls for pardon proposals presented up to 6 months earlier ($t-5$), while each of following columns tests the exclusion of the last lag using the Akaike Information Criterion. This suggests the last column as the preferred specification, which follows from the very fact that all but the immediately preceding month correspond to lags with coefficients close to zero and only seldom significantly different from zero.

The first row of Table 2 indicates that each pardon proposal presented in the

¹³The expected number of suicides in month t is given by $\mathbb{E}[y_t|\mathbf{x}_t] = Pe^{\theta^\top \mathbf{x}_t}$, 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: $\theta_i = \mathbb{E}[y_t|\mathbf{x}_t]^{-1} \frac{\partial \mathbb{E}[y_t|\mathbf{x}_t]}{\partial x_{it}}$.

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 across the two models.

4.1.2 Difference in Differences Results

Recall now that we also have information on deaths related to causes other than suicides. Our next identification strategy, therefore, is going to be based on a difference in difference procedure. Instead of the suicide rate being our dependent variable, we will use the difference in the monthly death rates (per 10,000 inmates) from suicides and other causes. Table 3 presents regression results with respect to four such differences: in the monthly death rates from suicides and, respectively, due to natural, unknown, or other (i.e. natural and unknown combined) causes, and, as a placebo test, in the monthly death rates from natural and unknown causes.¹⁴ 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.

As shown by columns (1)-(6) of the table, following more legislative proposals

¹⁴Unfortunately, our data does not permit a finer categorization of the other causes of death. Needless to say, had this been possible (for example, into health problems, infectious diseases, violence, etc.), it would enable a more thorough investigation.

in the preceding month, there are consistently fewer suicides in the current month, as compared to natural deaths, deaths due to unknown causes, or the latter two types of death combined. The corresponding regressions deploy only two lags, for the previous month and the one before, but adding more lags leads to the same conclusion: only proposals introduced in the immediately preceding month have a significant effect in lowering the difference in the respective death rates (for evidence on additional lags, see Table 4).

The coefficients in Table 3 are around 6 percent, meaning that the typical proposal is associated on average and in the short run with 0.06 fewer suicides per month and per 10,000 inmates than deaths by other causes. Given that on average there are 0.72 suicides every month, in relative terms, the negative effect on the suicide rate is close to 8 percent, and thus comparable to the Poisson estimates above. Regarding the placebo test, on the other hand, the last two columns of Table 3 show that legislative proposals produce no significant differences between the deaths rates from natural and unknown causes.

4.1.3 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. In particular, given that for a pardon proposal to become amnesty law an absolute parliamentary majority of two-thirds is required, proposals put forward by parliamentary members of the largest political parties could be more important than proposals presented by members of parliament who belong to minor

parties.

The results reported in Column 1 of Table 5 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, and the 12 proposals put forward by members of FORZA ITALIA, the main center-right party) do carry more weight than the ones presented by the remaining minor parties. In terms of politicians belonging to the majority government or minority it seems that the later carry more weight (Column 2). This results might be driven by the very large absolute majority requirement of $2/3$, a requirement that has been largely out of reach by the majority government.

Yet another determinant of the relation between the origin of a legislative pardon proposal and its power in signalling the likelihood of an amnesty law is whether pardon proposals presented in parliament by politicians who have 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.¹⁵ The inmates' inferences on the proposals likelihood of success will depend on what they know about the credibility of the proposer, much like any receiver's belief update depends on the sender's credibility [see DellaVigna and Gentzkow, 2010, for an overview on persuasion in economics].

The final column of Table 5 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

¹⁵For 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.

track record of fewer than 3 proposals.

4.1.4 Robustness Checks

It remains to examine the robustness of the preceding analysis. To this end, Table 6 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 3. Year fixed effects are added in Columns 3 and 4 of Table 6. 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 6 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 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 7 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 7 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.

5 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 prison. 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 (see, for example, [Becker, 1992]).

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 different ways to treat 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." Pardon proposals are frequent but negative votes on them are extremely rare. Most proposals that are doomed to be unsuccessful are 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. Which is far from enough to perform any meaningful statistical test (although there is a significant increase in suicides immediately after that date).

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, along with 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.

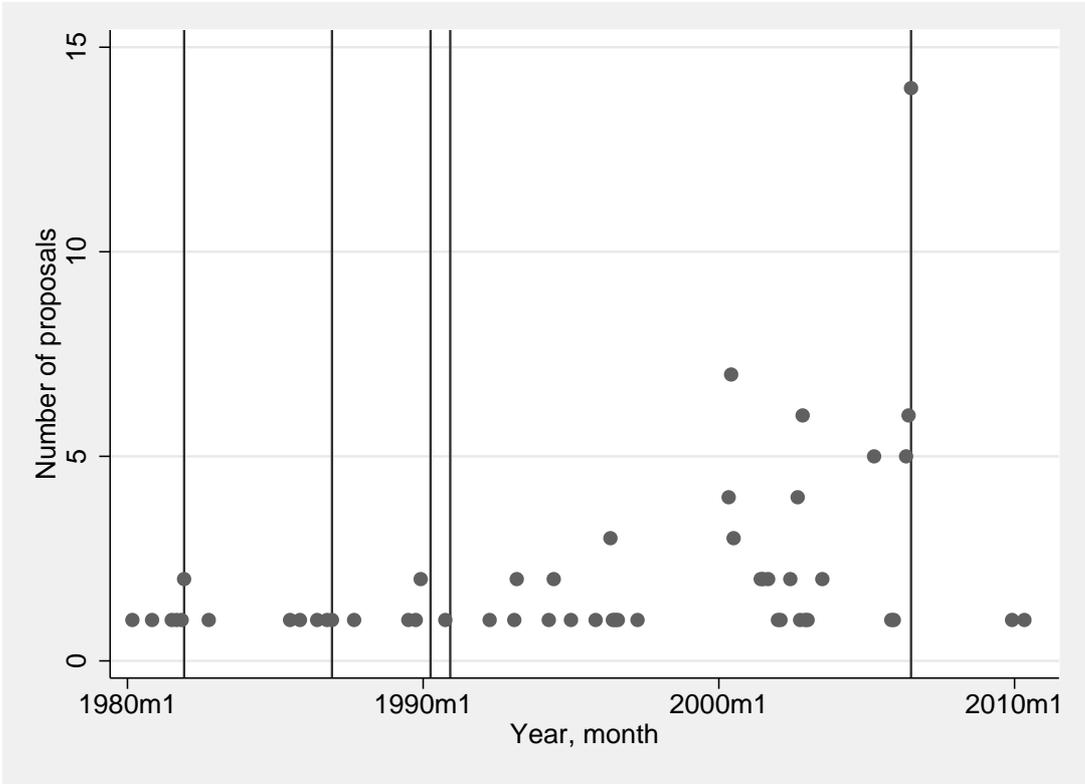
Our scope is limited into establishing that the decision to not commit suicide in prison includes at a minimum some rational component: expectations about future conditions do play a role, and do respond to relevant public information. And in this sense, if anything, ours ought to be a conservative test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

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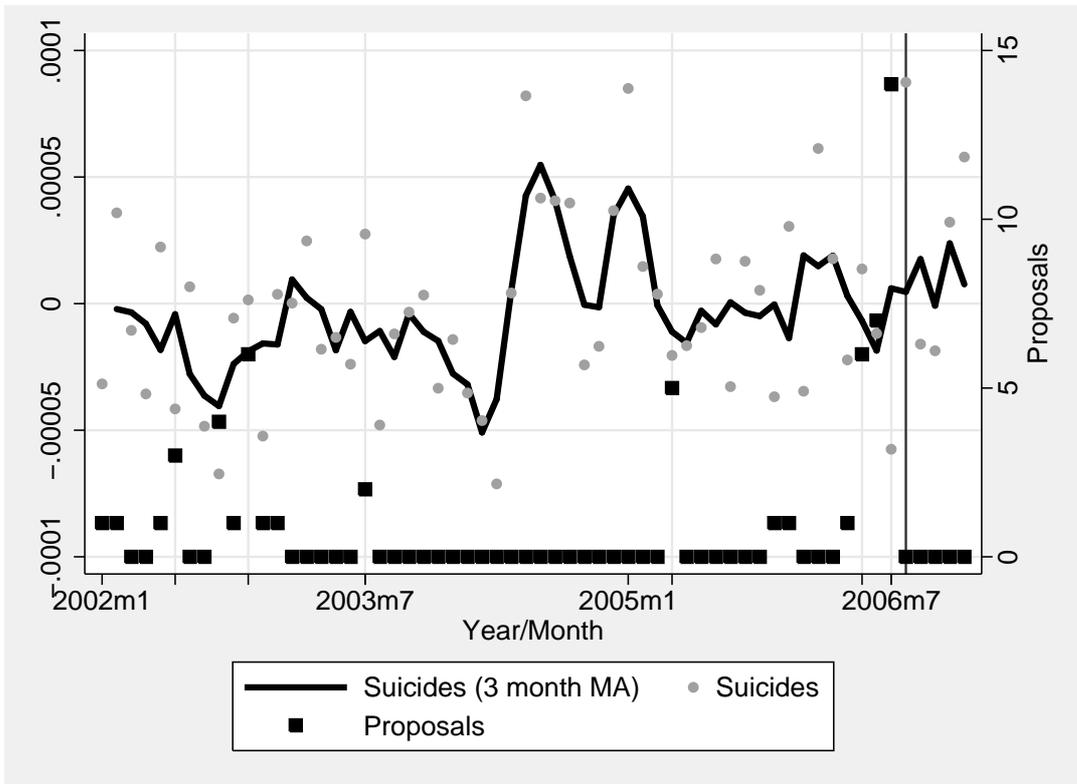


Figure 2: Legislative Proposals and Prison Population.

Notes: The vertical line indicates the event that constitutes our critical test, a successful proposal (the amnesty law of August 2006). Suicide rates are deseasonalized (net of monthly averages). The 3 year moving averages are $MA(y) = 1/3(y_{t-1} + y_t + y_{t+1})$.

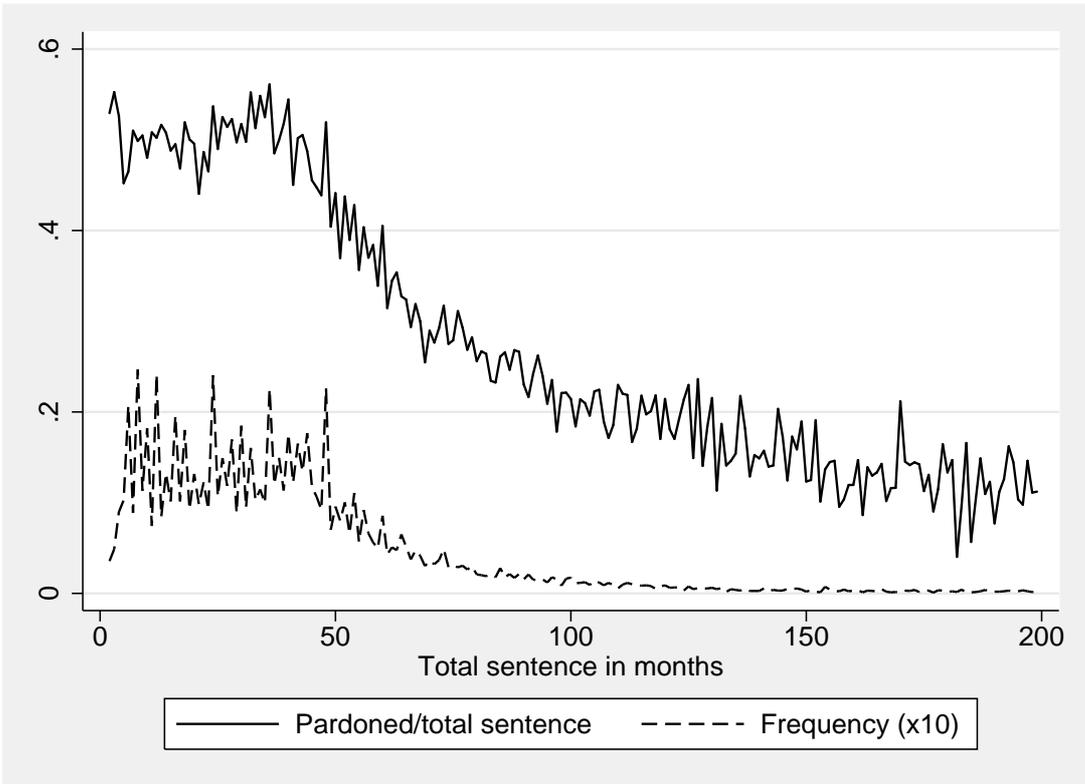


Figure 3: Fraction of Sentence Pardoned in August 2006

Based on DAP [2006]. The frequency of total sentences has been multiplied by 10 to facilitate the exposition.

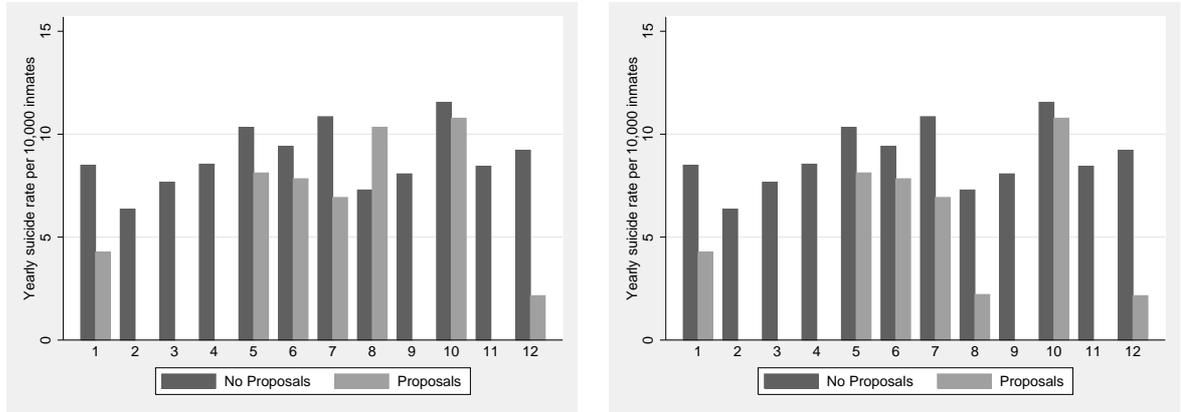


Figure 4: Monthly suicide rates with (left) and without (right) the August 2006 observation. In either panel, the light-shaded bars refer to the case in which there was at least one pardon proposal in the preceding month.

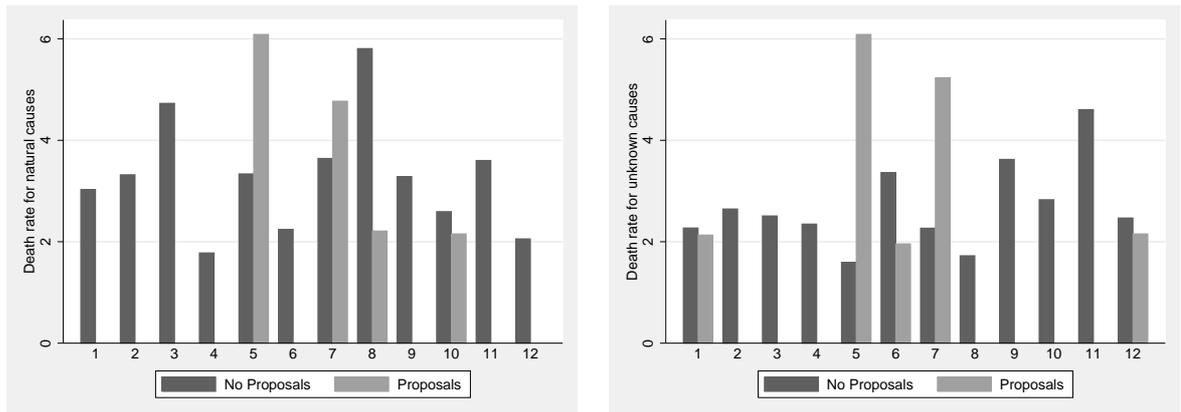


Figure 5: Monthly rates of death from natural (left) and unknown (right) causes. In either panel, the light-shaded bars refer to the case in which there was at least one pardon proposal in the preceding month.

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: The Effect of Pardon Proposals on Suicide Rates

	(1)	(2)	(3)	(4)	(5)
	Suicide rate				
Number of proposals (t-1)	-0.0744*** (0.0282)	-0.0761*** (0.0286)	-0.0753** (0.0295)	-0.0764** (0.0310)	-0.0802** (0.0321)
Number of proposals (t-2)	-0.0176 (0.0200)	-0.0214 (0.0194)	-0.0192 (0.0202)	-0.0276 (0.0180)	
Number of proposals (t-3)	-0.0414** (0.0203)	-0.0347* (0.0199)	-0.0269 (0.0190)		
Number of proposals (t-4)	0.0120 (0.0163)	0.0240 (0.0155)			
Number of proposals (t-5)	0.0300* (0.0171)				
August 2006 Pardon (0/1)	2.394*** (0.460)	2.400*** (0.467)	2.336*** (0.470)	2.249*** (0.473)	2.118*** (0.484)
Pseudo R2	0.0415	0.0403	0.0393	0.0383	0.0343
AIC	488.23	491.08	493.31	494.97	498.47
p-value overdispersion	0.5	1	0.5	0.5	0.5
Observations	109	110	111	112	113

Each column represents a different Poisson regression. The prison population measures the exposure: $\mathbb{E}[y_t|\mathbf{x}_t] = Pe^{\theta^\top \mathbf{x}_t}$, 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 3: Difference in Difference Estimates of The Effect of Pardon Proposals

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	<i>Suicides-Natural</i> per 10,000 inmates		<i>Suicides-Unknown</i> per 10,000 inmates		<i>Suicides-Other</i> per 10,000 inmates		<i>Natural-Unknown</i> per 10,000 inmates	
Number of proposals (t-1)	-0.0490** (0.0217)	-0.0479** (0.0213)	-0.0676** (0.0274)	-0.0692** (0.0283)	-0.0606* (0.0348)	-0.0579* (0.0335)	-0.0186 (0.0130)	-0.0213 (0.0147)
Number of proposals (t-2)	0.00694 (0.0138)		-0.00944 (0.0164)		0.0135 (0.0179)		-0.0164 (0.0123)	
2006 Pardon	1.802*** (0.324)	1.833*** (0.309)	1.845*** (0.399)	1.805*** (0.403)	1.787*** (0.499)	1.840*** (0.471)	0.0428 (0.213)	-0.0274 (0.229)
Observations	112	113	112	113	112	113	112	113
R-squared	0.166	0.171	0.150	0.145	0.141	0.145	0.141	0.129

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 4: Difference in Difference Estimates of The Effect of Pardon Proposals

	(1)	(2)	(3)	(4)	(5)
	Suicides-Unknown causes per 10,000 inmates				
Number of proposals (t-1)	-0.0626** (0.0268)	-0.0652** (0.0269)	-0.0666** (0.0278)	-0.0676** (0.0274)	-0.0692** (0.0283)
Number of proposals (t-2)	-0.00928 (0.0185)	-0.0140 (0.0179)	-0.0101 (0.0176)	-0.00944 (0.0164)	
Number of proposals (t-3)	-0.0133 (0.0167)	-0.00593 (0.0156)	0.00286 (0.0136)		
Number of proposals (t-4)	0.0105 (0.0168)	0.0233 (0.0143)			
Number of proposals (t-5)	0.0324 (0.0221)				
2006 Pardon (0/1)	1.849*** (0.390)	1.869*** (0.390)	1.818*** (0.401)	1.845*** (0.399)	1.805*** (0.403)
Observations	109	110	111	112	113
R-squared	0.161	0.155	0.148	0.150	0.145

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 5: 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 (1.430)	2.640* (1.468)	0.326 (3.359)
Observations	113	113	113

Each column represents a different Poisson regression. The prison population measures the exposure: $\mathbb{E}[y_t|\mathbf{x}_t] = Pe^{\theta^T \mathbf{x}_t}$, 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 6: Robustness checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Benchmark		With year fixed effects		No exposure	Dynamic regression	
	Poisson	DinD	Poisson	DinD	Poisson	Poisson	DinD
Number of proposals (t-1)	-0.0802** (0.0321)	-0.0579* (0.0335)	-0.0703* (0.0362)	-0.0858** (0.0393)	-0.0924** (0.0377)	-0.0768* (0.0463)	-0.0572* (0.0331)
August 2006 Pardon	2.118*** (0.484)	1.840*** (0.471)	1.893*** (0.497)	1.941*** (0.529)	1.885*** (0.558)	2.131*** (0.779)	1.925*** (0.481)
Lagged dependent var.						0.0140 (0.0225)	0.109 (0.109)
Observations	113	113	113	113	113	113	113
R-squared		0.145		0.323			0.155

All but one Poisson regression 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 7: 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: $\mathbb{E}[y_t|\mathbf{x}_t] = Pe^{\theta^T \mathbf{x}_t}$, 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 (***).