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Nadia Campaniello
Theodoros Diasakos
Giovanni Mastrobuoni

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

Nadia Campaniello[†], Theodoros Diasakos[‡] and Giovanni Mastrobuoni[§]

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Abstract

Are suicides rational? 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 their date of release, 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 has a rational component.

Keywords: Suicides, Rationality, Pardons, Amnesty, Prison Conditions
JEL classification codes: K40, K42, H11

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[†]CERIS-Consiglio Nazionale delle Ricerche, Via Real Collegio 30, Moncalieri, Italy, n.campaniello@ceris.cnr.it.

[‡]Collegio Carlo Alberto, Via Real Collegio 30, Moncalieri, Italy, theodoros.diasakos@carloalberto.org.

[§]Collegio Carlo Alberto, CeRP, IZA, and Netspar, Via Real Collegio 30, Moncalieri, Italy, giovanni.mastrobuoni@carloalberto.org.

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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 [11] 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 [8]).

Empirically, there have been several attempts to test these theories, or at least whether suicides respond to economic incentives (see Goldsmith et al. [10] for a wide and comprehensive overview on suicides, and Chen et al. [5] for a thorough survey of the empirical literature). One approach in the literature (see Koo and Cox [14] 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 or space, and a positive correlation between suicides and economic conditions emerges (see, for instance, Ruhm [17], Cutler et al. [6], Brainerd [4], Marcotte [16], or Andres [1]).

In either approach, however, the focus is on contemporaneous correlations. As such, even though they might be capturing, at least to some degree, the forward-looking behavior of the theoretical framework, the respective results make identifying a causal relationship between the socioeconomic variables and the suicide rate quite difficult. For example, reverse causality might well be at play 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 use measures of current economic conditions to test the rationality of suicides. Instead, we look at changes in current expectations about the future, which come about due to the announcement of events

that will occur in the future but do not alter at all 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: prisons. Given that individual expectations are both hard to measure and highly diverse, prisons represent an almost ideal experimental ground. As environment in which the experiment takes place, they are closed and isolated, which reduces the influence of external factors that might be latent or hard to measure (family, friends, career prospects, etc.). As events, suicides are recorded while the underlying expected utilities depend almost exclusively on one's expected sentence length.¹ As has been put, "(A)n inmate's dream is to fall asleep one day and wake up half a year later" (Kaminski [13]).

The policy instrument that we will use to measure changes in prisoners' expectations are Italian collective pardons (*Amnistie e Indulti*), non-discriminatory sentence reductions. Typically, these reduce sentences for crimes committed before the law gets passed by 2-3 years and lead to the immediate release of all inmates whose remaining sentence is less than this amount of time.²

Whereas before a simple legislative majority sufficed for collective amnesties and pardons to become Italian law, since 1992 an absolute majority of two-thirds is required (Constitutional Law 6/1992). And before such a law may be up for vote, it has to be proposed first by a member of the Parliament and then assigned to a Parliamentary Commission (*Commissione Parlamentare*).³ 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*.

Undoubtedly, pardon proposals represent good news for people that are behind bars. For

¹Apart from sentenced criminals, the Italian prison system holds also inmates awaiting trial.

²To give a historical perspective, between the unification of Italy in 1865 and the defeat of Mussolini in 1943, there have been approximately 200 pardons or amnesties. Some of these were just 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, another dozen of collective clemency acts have been passed. At the beginning, they were aimed at reconciling a politically-divided nation but, more recently, the goal has been to reduce prison overcrowding.

³Proposals 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*). Yet, as it is the case with most Italian laws, pardons are usually proposed by the Parliament.

they are not only necessary for an amnesty law to be passed, but their number itself is correlated with the likelihood that this happens. This is depicted vividly in Figure 1 with respect to the amnesty law of August 2006. Before the law got passed, an event shown by both a vertical line and the drop in prison population (dotted line - on 08/01/2006, more than 20,000 inmates were released), the number of proposals shows presented by the parliament (diamonds) is a clear signal of the ongoing parliamentary activity: it increases from six in May 2006, to seven in June, and peaks at 14 in July. Of course, this signal is also noisy as it might well be wrong: in September, October, and November 2002 there were, respectively, 4, 1, and 6 proposals which did not lead to any pardon. Overall, our sample has 18 months in which at least one proposal was put forward.

Since pardon proposals represent our measure of “good news,” it is important to understand which inmates 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 percentage reduction in the prison population. Clearly, the last pardon generated large releases amongst all types of criminals, with the exception of those imprisoned for mafia-related crimes.⁴ This means that many inmates ought to expect to be released immediately due to an amnesty law. In fact, following the August 2006 pardon, 37% of the prison population were released immediately while almost all of the remainder received a sentence reduction of 3 years and, thus, were to be released ahead of their initial schedule.

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

⁴While other types of crime that might comprise a mobster’s sentence may get pardoned, mafia-related ones never do.

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 [3] for a detailed discussion). In more practical terms, it could be driven by error, the inability of some criminals to foresee how harsh prison conditions can be. And in either case, one should keep in mind that the goal of our study is to identify a rational component in the decision to *not* commit suicide. This does not mean that medical conditions do not matter, or that suicides in jail cannot be prevented. But an inmate may not attempt suicide on the onset of a serious depression, and rather wait to see what the future brings (see, for example, Ludwig et al. [15] for a recent economic study on anti-depressants and suicides). A review of the large literature on the health status of prisoners and on prison suicides is outside the scope of this paper (see, among others, Fazel and Baillargeon [9] and Hayes [12]).

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 (Kaminski [13]). Since prison authorities are aware of this, the attempts themselves need to be credible, which intensifies the risk of them turning into actual suicides. Yet, even regarding these “hyper-rational” suicides, we should expect a decline whenever a pardon is in sight.⁵

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). Our data on the Italian prison population comes from the Italian Penitentiary Administration (DAP) while that on the exact dates of individual deaths in Italian prisons (between 2002 and 2011) from the research group *Ristretti Orizzonti* (www.ristretti.it). The research group does not provide any information about the actual or expected sentences of these inmates. The data on the legislative proposals for collective pardon can be found on the Senate’s website (www.senato.it).

⁵The term is borrowed by Kaminski [13]. This author argues that in prison the stakes are so high that even what appears to be inhuman and bizarre behavior may be instead “hyper-rational.”

The raw data on the three variables of interest (prison population, suicides, and proposals) does not lend itself readily to recognizing a pattern, but evidence of a correlation between proposals and suicides does emerge when we look at the average suicide rate by each month of the year (for instance, what is average January suicide rate across our 10-year sample). Of course, we summarized the data by month so as to capture any seasonality in suicides. We looked also at whether there were any proposals during the preceding month so as to avoid any reverse causality, in the sense that members of congress may be more likely to propose pardons when suicide rates are particularly high.⁶

The data in question is depicted in Figure 2, whose left panel shows that the months which were preceded by 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 then. Indeed, when we exclude August 2006 from our data, the August suicide rate following a pardon proposal (in July) becomes three times lower than the suicide rate in August when no pardons have been proposed (see the right panel of Figure 2).

In fact, the right panel of Figure 2 shows lower suicide rates when pardons have been proposed during the preceding month, for all seven months in which proposals were made during the preceding month. While this already attests to a negative relation between proposal and suicides that is likely to be significant (the likelihood of getting seven negative differences out of seven when the differences are seen as independent Bernoulli trials using a fair coin is only $0.5^7 = 0.008$), later we are going to use regression analysis to better test this significance.

⁶Of course, it may be argued that one month is not long enough a period to rule out that the observed correlation between pardon proposals and changes in suicide rates is due to contemporaneous relations. And even though, as a robustness check in our regression analysis below, we do extend the length by which proposals precede suicides as much as 4 months (see Table 2 and the respective analysis below), there might well be reasons for contemporaneous reverse causality. 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 such a scenario, however, the implied correlation between pardon proposals and changes in suicide rates ought to be positive, not the negative one we observe in the data.

For the time being, 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. Figure 3 shows the death rate from natural (left) and unknown (right) causes, again by month and by whether there were legislative proposals during the preceding month. In either panel, the August 2006 observation has been excluded. In either panel, the hypothesis that the differences between the death rates are due to independent Bernoulli trials using a fair coin cannot be rejected (the respective p-values for the left and right panel are 0.19 and 0.31).

Of course, this 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. For 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 remains an assumption that 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% of the weeks in which a proposal was introduced in parliament one or more articles dedicated to pardons appeared in press, a likelihood that drops to less than 20% for those weeks that saw no proposals.⁷ 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 proposals while news inside jails tend to travel very fast by word of mouth.⁸ Unfortunately, we could not find any hard evidence on these claims, other than *Ristretti Orizzonti*'s website displaying about 4,000 unique visits per day.

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

⁷The articles in question are freely available on the journals' respective websites, www.corriere.it and www.lastampa.com.

⁸*Ristretti Orizzonti* (limited horizons) was established in 1998 while *Radio Carcere* (Radio Jail) has been broadcasted since 2002 by *Radio Radicale* (Radical Radio), a well-known Italian radio station.

differences in suicide rates. In Table 2, each row represents a different Poisson regression, where the exposure variable is measured by semestral prison population.⁹ Since proposals are orthogonal to any other individual characteristic that might be related to suicidal behavior we can specify parsimonious regressions with the monthly number of pardon proposals. Given our evidence about the effects on suicidal behavior of seasonality and of the actual pardon in August 2006, in each regression we also introduce month fixed effects and a dummy equal to one for August 2006. Even though not shown in the table, the coefficient on the latter dummy is always significant and approximately equal to two. This means that, amongst those inmates who had to stay in jail while thousands of fellow inmates were about to be released following the amnesty law of August 2006, suicide rates were 200% higher than expected. 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 the previous month. Using the lag reduces the concern of reverse causality which would bias the estimated coefficient upwards. Moreover, using the lagged number of proposals we are implying that it might sometimes take longer for the information about proposals to reach all inmates.

The first row of Table 2 indicates that each pardon proposal reduces the number of suicides by about 8%. The results are slightly larger when we restrict attention to those years in which most of the proposals were presented (2002-2006). Yet, these are contemporaneous effects based on the assumption the effects of pardon proposals on suicide rates materialize within the month that follows a proposal. Nonetheless, the following rows of the table proxy expectations based on the average number of proposals in the preceding 2, 3, or 4 months. The largest effect (10.9% per proposal) is estimated when we use 3 months to measure expectations, but the differences are small overall, indicating that even the first month to follow a pardon proposal is a good proxy.

Table 3 presents several robustness checks. Its second row shows that using a negative binomial regression (thus, relaxing the assumption that the conditional variance equals the

⁹In a Poisson model, the expected number of suicides is given as $\mathbb{E}[y|\mathbf{x}] = Pe^{\theta^T \mathbf{x}}$, where P represents the prison population. The coefficients here can be interpreted as simple semi-elasticities: $\theta_i = \frac{\partial \mathbb{E}[y|\mathbf{x}]}{\partial x_i} \frac{1}{\mathbb{E}[y|\mathbf{x}]}$.

conditional mean) leaves the results unchanged. The same outcome obtains if one does not control for exposure. This is shown by row 3, which includes also the past suicide rate so as to control for reverse causality. 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, given our information on deaths not due to suicides, natural deaths and deaths from unknown causes (including homicides), and as there is no reason to expect these deaths to depend on pardon proposals, we can formally perform also our placebo test. Table 4 replicates the previous regressions using natural deaths and deaths from unknown causes as the dependent variable. None of the coefficients is significantly different from zero: some are positive, some are negative, and most are also quite close to zero. Overall, therefore, our results are quite conclusive that pardon proposals tend to be associated with a reduction in suicide rates, but not with other causes of death.

Clearly, by falling on average when the legislature proposes new pardons, suicide rates in Italian prisons do respond to changes in expectations. Pardon proposals constitute “good news,” for the inmates, who might expect reductions in their sentence length, but also an improvement in prison conditions via an alleviation of over-crowding. Hence, our analysis provides evidence of forward-looking behavior, 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 [2]).

To arrive at this conclusion, we exploited variations in inmates’ expectations as measured by our main explanatory variable, 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 jail life cover parliamentary activity on pardon proposals extensively.

With respect to 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 becoming law. Given that this heterogeneity in expectations is unknown, we weighted all pardon proposals equally. If anything, this ought to make our measure prone to error and our estimates biased toward zero. More importantly perhaps, ours is likely to be a conservative test of rationality from the outset since criminals are often viewed as boundedly rational or even irrational individuals.

Given the noise that pardon proposals entail as signals of future amnesty laws, assumption (iii) brings our analysis to agree with the option value theory of rational suicides, which predicts that the value of postponing a suicide should increase when the uncertainty increases. In this sense, our results indicate that suicides in Italian prisons would probably be higher if there were no pardons, even if these were to be replaced by a mean-preserving reduction in the variability of sentences. Unless, of course, somehow “good news” develop into “bad news,” and crush inmates’ hopes. In this case, the net effect of pardon proposals on inmates’ expectations would matter: 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.

Ultimately, of course, whether the net effect of pardon proposals on inmates’ expectations is positive or negative is 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 (12/12/2006) in our nine-year sample when a pardon proposal was turned down by a negative vote. Needless to say, this is far from enough to perform a meaningful statistical test (although there is a significant increase in suicides immediately after that date). More research is needed to identify the effects of “bad

news” on expectations.

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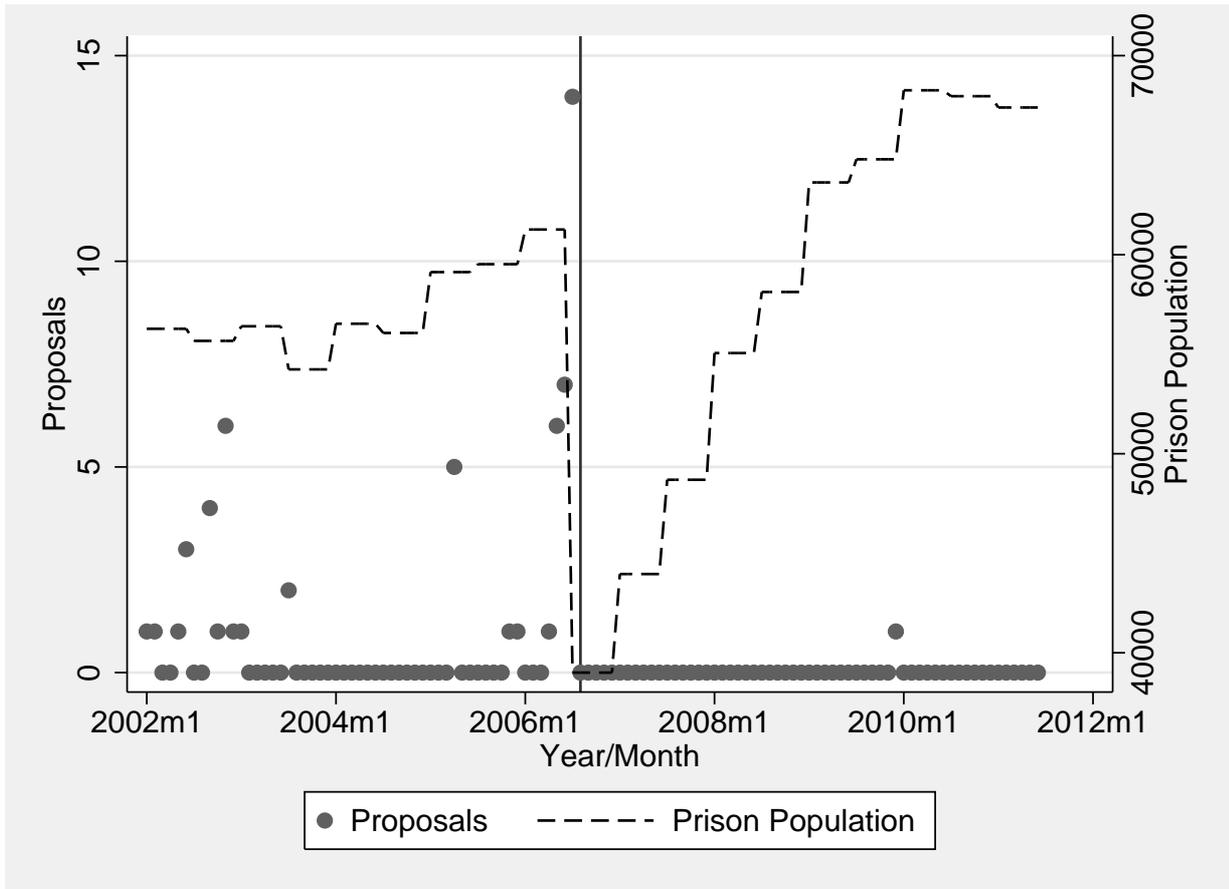


Figure 1: Legislative Proposals, and Prison Population. The vertical line indicates the event that constitutes our critical test, a successful proposal (the amnesty law of August 2006)

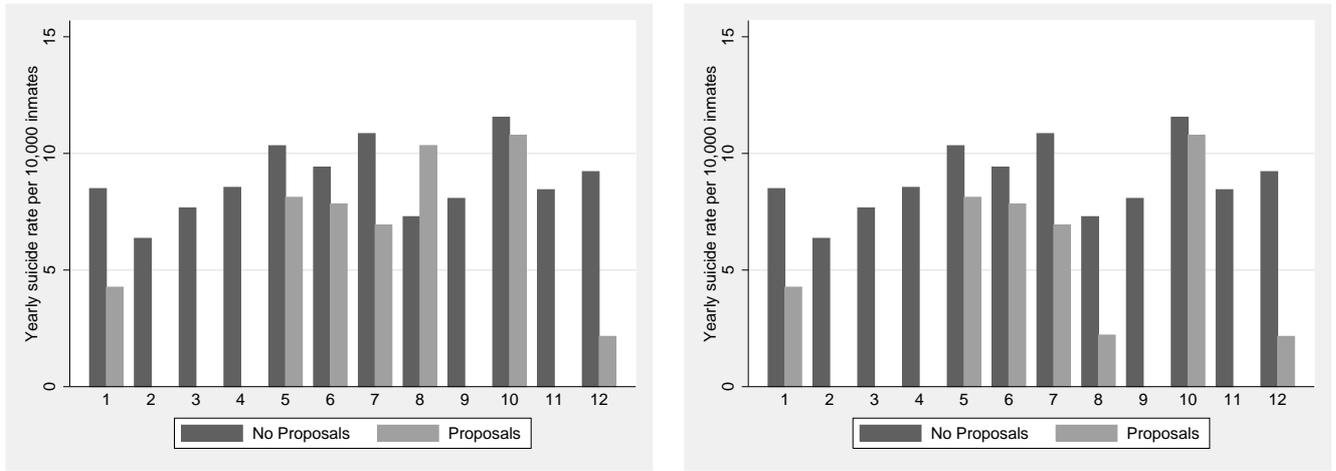


Figure 2: Monthly suicide rates with (left) and without (right) the August 2006 observation. In either panel, the light-shaded bars refer to rates when there was at least one pardon proposal in the preceding month.

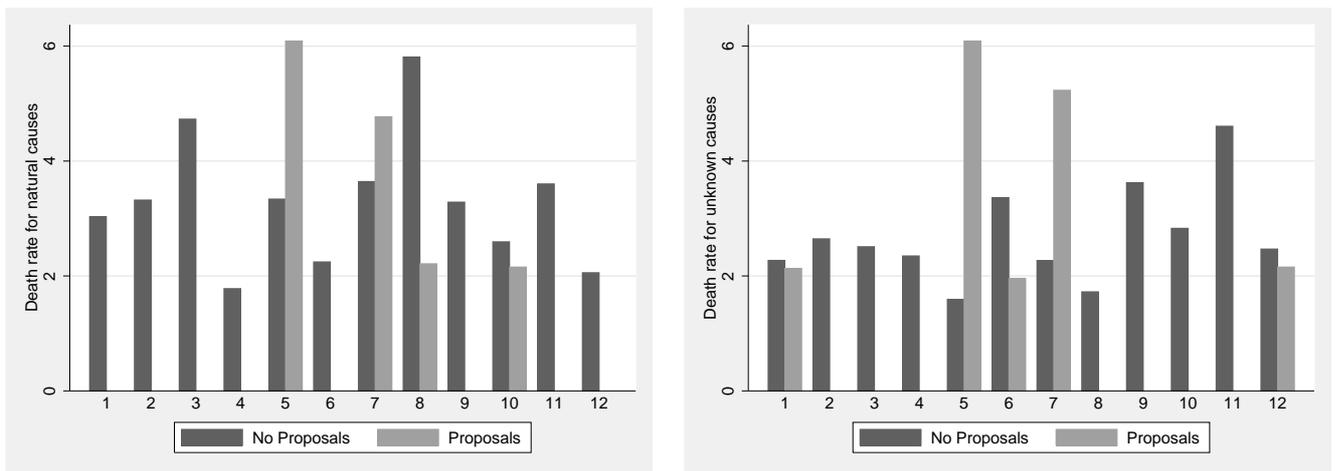


Figure 3: Monthly rates of death from natural (left) and unknown (right) causes. In either panel, the light-shaded bars refer to rates when there was at least one pardon proposal in the preceding month.

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		

Notes: Based on DAP [7]. 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

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

Notes: Each row represents a different Poisson regression. The prison population measures the exposure: $\mathbb{E}[suicides_t | proposals_t] = Pe^{\theta^\top proposals_t}$. All regressions control for prison population. Standard errors in parentheses. Levels of significance at 10 percent (*), 5 percent (**), and 1 percent (***).

Table 3: Robustness regressions

Specification	β	SE
(1) Benchmark (Poisson, last month proposals)	-0.0802**	(0.0321)
(2) Negative binomial regression	-0.0802**	(0.0321)
(3) Without controlling for exposure (prison pop.)	-0.0924**	(0.0377)
(4) Controlling for lagged suicide rate ($\rho = 0.014(0.023)$)	-0.0768*	(0.0463)

Notes: Each row represents a different regression. All but one regressions control for prison population (exposure). Standard errors in parentheses. Levels of significance at 10 percent (*), 5 percent (**), and 1 percent (***)

Table 4: The Effect of Pardon Proposals on Rates of Death from Natural and Unknown Causes

	Cause of death	sample	β	SE	Obs.
(1)	Natural	full sample	-0.0515	(0.0697)	113
(2)	Natural	2002-2006	-0.0222	(0.0719)	59
(3)	Unknown	full sample	0.0277	(0.0637)	113
(4)	Unknown	2002-2006	0.0729	(0.0612)	58

Notes: Each row represents a different Poisson regression. The prison population measures the exposure:

$\mathbb{E}[suicides_t | proposals_t] = Pe^{\theta^T proposals_t}$. All regressions control for prison population. Standard errors in parentheses. Levels of significance at 10 percent (*), 5 percent (**), and 1 percent (***)