Time To Get Out ?
How Sentence Reductions Affect Recidivism After Prison

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Abstract

Does shorter incarceration promote successful reentry or lead to recidivism ? In this paper, I propose a new theory to reconcile these two competing views, which both have solid empirical and theoretical grounding. My reference-dependent model of criminal decision-making incorporates the concrete design of sentence reductions, in terms of timing, anticipation and adaptation by prisoners. The model has several testable implications, which are generally supported by the data. Exploiting a collective pardon in France in July 1996, I show that sentence reductions have very differential effects on prisoners' propensity to reoffend, depending on their ability to anticipate the pardon and their capacity to adapt to the news before release, both psychologically and materially. Overall, these results suggest that sentence reductions may neither be good or bad *per se* in terms of recidivism, and that prisoners' expectations and adaptation are key.

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Introduction

On November 1st 2015, more than 6,000 drug offenders were released early from U.S. federal prisons thanks to a large, retroactive reduction of sentence guidelines by the U.S. Sentencing Commission[1]. Overall, more than 20,000 prisoners have already benefited from this revision, obtaining an average sentence reduction of 21 months, or 17% of their original sentence (USSC, 2015). This historic early release plan illustrates a turn toward new strategies to fight crime and recidivism in the United States and in other countries, after several decades of ”tough on crime” policies[2].

As argued by the U.S. Sentencing Commission and many scholars, sentence reductions have many attractive features: they save public money, reward good conduct inside prison, incentivize rehabilitation efforts, and allow judges and parole boards to target the right time to release inmates[3] (Kuziemko, 2013; Polinsky, 2015). In addition, shorter incarceration reduces prisoners' exposure to the potentially criminogenic effects of prison, such as criminal hardening, labor-market detachment and stigma, and can therefore prevent recidivism (Durlauf and Nagin, 2011).

However, sentence reductions can also backfire by reducing the deterrent effects of incarceration among actual prisoners *and* potential offenders: first, inmates who obtain early release from prison may revise downward their perception of the cost of punishment, as they expect future incarceration to be shorter too. This reduction in so-called specific deterrence would induce more recidivism. Second, the threat of future punishment may lose deterrent power even among non-treated, potential offenders, who are aware that sentence reductions will be available in case of conviction (lower general deterrence). Moreover, sentence reductions have the mechanical effect of releasing prisoners at younger ages, i.e. with intrinsically greater propensities to reoffend[4].

Interestingly, these two competing views about sentence reductions (criminogenic effect *vs* crime-preventing effect) both receive empirical support from high-quality studies which deal

[1]The Sentencing Commission reduced the (very long) recommended prison sentences for a series of drug crimes, making thousands of federal prisoners eligible for early release. This news received a lot of media coverage in the United States as it is one of the largest early release in American history. See for example the New York Times: http://www.nytimes.com/2015/10/07/us/us-to-release-6000-inmates-under-new-sentencing-guidelines.html

[2]The “tough on crime” approach, exemplified by the laws on ”Three Strikes” (mandatory sentence enhancements for repeat offenders) and ”Truth-in-Sentencing” (lower access to early release), led to large increases in the average lenght of time served behind bars, both in the U.S. (2.9 years in 2009, up by +36% since 1990, Pew (2011)) and in many European countries. In France for example, the average prison stay went up from 8.6 months to 11.5 months between 2006 and 2013, yielding a 15% increase in total prison population (+9,000 prisoners) while the number of admissions into prison was actually declining (from 81,000 to 70,000 annual entries)

[3]The “right time” being when the marginal cost and benefit of an additional day in prison equalize

[4]There is widespread evidence that criminal propensity declines with age (after a peak around 18-20 years old), the so called ”age-crime curve”. Ganong (2012) shows how aging partly explains the effect of incarceration length on reoffending.

properly with omitted-variable bias[5]: for example, Berecochea and Jaman (1981), Maurin and Ouss (2009) and Kuziemko (2013) show that the probability of recidivism significantly increases among prisoners who are released early; conversely, Mueller-Smith (2014) and Hunt and Peterson (2014) find that shorter incarceration prevents recidivism, whereas Kling (2006) and Abrams (2011) do not find any significant effect on future crime or labor market outcomes.

A major shortcoming of this research, besides its large focus on U.S. data, is that it pays no attention to the framing of punishment, i.e. the concrete design which leads similar prisoners to serve different amounts of time behind bars[6]. In this paper, I argue that the design of sentence reductions (in terms of timing, anticipation and adaptation by prisoners) is a key mediating factor, and can help explain the apparent inconsistency in prior research: sentence reductions tend to be criminogenic when they are unexpected and lead to the rapid release of prisoners shortly after the news (Berecochea and Jaman, 1981; Maurin and Ouss, 2009; Kuziemko, 2013); conversely, when inmates can easily anticipate their eventual date of release well in advance (i.e. no shock between trial and final release), shorter incarceration can have null (Kling, 2006; Abrams, 2011) or even beneficial effects on recidivism (Mueller-Smith, 2014; Hunt and Peterson, 2014).

My main contribution in this paper is therefore to explicitely consider the concrete design of sentence reductions in their interplay with recidivism. To do so, I first build on a reference-dependent utility model (Kőszegi and Rabin, 2006) to incorporate expectations and adapation to surprise in a typical Beckerian model of criminal decision-making (Becker, 1968). Second, I test the main predictions of the model on a dataset of prisoners released in France in 1996-1997. Most of those inmates benefited from the presidential collective pardon of July 1996, which granted additional sentence reductions to inmates based on their residual sentence on the day of the pardon. Using the pardon as an exogenous source of variation, I show that such sentence reductions had very differential effects on French inmates: prisoners who were released soon after the pardon and were the least likely to anticipate it performed significantly worse during the next 5 years (greater probability of recidivism) than prisoners who were likely to expect the pardon (because of prior experience) and who had a lot of time remaining to adapt to the news (both materially and psychologically) before actual release.

[5]Omitted-variable bias stems from the fact that sentence reductions are granted endogenously to the best prisoners (in terms of current behavior and future reentry prospects). Therefore, a negative correlation between sentence reductions and future crime could arise even in the absence of causal effects, if judges effectively grant sentence reductions to the good prisoners.

[6]A notable exception is Bushway and Owens (2013) who show how discrepencies between expected and actual sentences generate recidivism

1 Theory and current evidence

In order to illustrate the potential effects of sentence reductions on recidivism, let's consider a Beckerian model of crime participation. An ex-prisoner contemplating a new criminal opportunity in time t after release from prison decides to reoffend if the expected utility derived from crime exceeds its opportunity cost, or when:

$$B(r) - p.C(r) > \underline{U}(r) \tag{1}$$

where B is the expected benefit from crime (monetary or psychological), C is the expected cost of future punishment, p is the probability of punishment, and $\underline{U}$ is the expected utility from not reoffending (such as the value of free life, social ties, legitimate work).

Conditional on original sentence length s_i, the amount of sentence reductions one obtained during previous incarceration, noted r for simplicity[7], may affect all three expected utilities. Regarding the cost function $C(.)$, receiving more sentence reductions is likely to reduce one's expectation of the length of future incarceration, and therefore the perceived cost of future punishment. This hypothesis, known as specific deterrence, therefore suggests that $\frac{\partial C}{\partial r} < 0$. However, the signs of $\frac{\partial U}{\partial r}$ and $\frac{\partial B}{\partial r}$ are far less certain: there is no direct evidence on the effect of prison time on future criminal benefits[8], and the current research on incarceration length and future labor market outcomes is not conclusive (Kling, 2006; Landerso, 2015)[9]. We therefore have little theoretical guidance as to the direction of the overall effect of sentence reductions on recidivism after release.

Empirically, a growing number of papers use convincing designs to overcome omitted-variable bias (the fact that sentence reductions are endogenously targeted to the prisoners with the best prospects and lowest risk of recidivism) and estimate this overall causal effect. As early as 1970, California ran a large-scale experiment on 1,138 prisoners who were about to be released under parole after spending three years behind bars on average: a random group of 564 prisoners was granted early release six months in advance of their scheduled date of parole, while the other prisoners (control group) were released on their original date of parole. As reported by Berecochea and Jaman (1981), the one-year rate of return to prison was 14% for the early-

[7]Note that r refers to the amount of sentence reductions obtained during the previous prison spell conditional on original sentence s_i, so a more precise notation could be $r_{t-1}|s_i$. Also note that the amount of sentence reductions is simply the difference between initial sentence s_i and actual length of incarceration s_f: $r = s_i - s_f$

[8]Related literatures investigate peer effects inside prison (Bayer) and the returns to criminal experience (Loughran), both finding evidence of criminal capital accumulation and specialization

[9]In Sweden, a country with famously short prison sentences, Landerso (2015) finds that increasing incarceration length can promote labor-market outcomes after release. However, Kling (2006) do not find evidence of any significant effect of time served on future employment and wages in Florida and California

release group and 10.9% for the control group, which suggests that recidivism was significantly larger among prisoners released early (p-value = 5.6%)[10]. This result is supported by recent quasi-experimental evidence from Georgia, where Kuziemko (2013) exploits discontinuities in parole guidelines. She finds that the probability of 3-year recidivism decreases by 1.3 percentage points for each additional month of incarceration[11]. The common feature of these two papers is that they exploit changes in date of parole, i.e. release decisions made by a parole board *during* incarceration. Those discretionary decisions are difficult to predict and anticipate by prisoners, and typically lead to rapid release from prison (in the next weeks or months): therefore, prisoners obtaining early release in Berecochea and Jaman (1981) and Kuziemko (2013) may have failed to anticipate and prepare their upcoming release, yielding more recidivism.

Interestingly, when researchers exploit designs where prisoners can better anticipate their eventual release date, the estimated effects of incarceration length on recidivism are no longer significant (Kling, 2006; Abrams, 2011) or even criminogenic (Mueller-Smith, 2014; Hunt and Peterson, 2014). Most papers use sentencing disparities between randomly-assigned judges, but a notable exception is Hunt and Peterson (2014) who exploit a retroactive reduction of sentence guidelines on crack-cocaïne offenses decided by the U.S. Sentencing Commission in 2007 (very similar to the recent 2015 revision discussed above). This amendment took effect on March 1, 2008, and led thousands of prisoners to seek and obtain early release under parole. Hunt and Peterson (2014) compare rearrests and returns to prison between two groups of observably similar prisoners: those who obtained sentence reductions thanks to the crack-cocaïne amendment of March 2008 and were released later that year, between July and November 2008 ; and prisoners who would have been eligible to sentence reductions but were released in the year before the amendment took effect (March 2007 - March 2008). As expected, the treated group served much less time in prison than the control group (85 months *versus* 97 months on average). After 5 years, the rate of recidivism was lower among the 836 early-release prisoners (43.3%) than in the control group (47.7%, N=486), with a *p-value* of 5.7%[12]. As opposed to Berecochea and Jaman (1981), this finding of a beneficial effect of early release may be due to the fact that, in their study, Hunt and Peterson (2014) focus on early-release prisoners who had a lot of time to prepare and adapt to the news: their actual release (July-Nov 2008) occured long after the announcement of the new sentence guidelines (2007).

[10]We use a two-proportion z-test, with test statistic equal to 1.58

[11]Also see Ganong (2012) and Zapryanova (2014) who extend the study of Kuziemko (2013) in Georgia to investigate the role of aging and supervision under probation

[12]Using the same two-proportion z-test as for Berecochea and Jaman (1981)

A theory of expectations and adaptation to surprise

Many reasons may explain why sound empirical research on the effects of sentence reductions on future crime produces such divergent results: heterogeneity of effects by criminal profile or prison conditions, use of different measures of recidivism or follow-up periods, etc. However, a main shortcoming of this literature is that it pays no attention to the allocation process of sentence reductions: as illustrated in Equation 1, sentence reductions are assumed to affect recidivism independently of their design and timing. Yet, I argue in this paper that a similar amount of sentence reductions can have dramatically different effects on recidivism depending on whether prisoners were able to anticipate them and, for those who were surprised by the news, whether they had the time and capacity to adapt before actual release.

In practice, there are several types of sentence reductions with very different concrete designs: "earned good time credit", for example, is easy to track over time by prisoners since it is based on their conduct inside prison (plus, they usually receive frequent feedback): thus, prisoners can easily update their priors and predict eventual date of release. Conversely, mass release, pardons and parole are highly discretionary decisions, and are therefore less predictable (in both probability and amount). Plus, such decisions can occur late during incarceration, leaving prisoners with little time to adapt.

Thus, conditional on initial sentence length, the decision to commit a new crime in time t after prison release may depend not only on the actual amount of sentence reductions obtained during incarceration, r, but also on anticipations, $\tilde{r}$, and adaptation to surprise, α:

$$B(r) - p.C(r, \Delta^{\tilde{r}}, \alpha) > U(r, \Delta^{\tilde{r}}, \alpha) \tag{2}$$

where $\Delta^{\tilde{r}} = r - \tilde{r}$ captures the anticipation error (how much of actual sentence reductions were not expected by the prisoner), and the coefficient α measures prisoner's capacity to adapt to the news by the time he is released, both materially (in terms of securing housing, finding a job, etc.) and psychologically (processing the information into updated beliefs).

To formally illustrate the intuition, let's consider a model of reference-dependent preferences. Kőszegi and Rabin (2006) propose a simple approach to plug expectations into a model of utility-maximizing decision-making. Early applications of such models to economics of crime are found in Bushway and Owens (2013), who estimate how the discrepency between expected and actual sentences affects recidivism (finding evidence of criminogenic framing effects)[13] and Card and

[13] Exploiting a policy change in Maryland which altered recommended sentences but not actual sentences, Bushway and Owens (2013) show that, conditional on actual sentence length, a larger gap between expected and actual punishement generates significantly more crime after release.

Dahl (2011) (on how unexpected losses of professional football games generate family violence). In the case of sentence reductions, I can decompose utility functions $C(.)$ and $U(.)$ in two additive components: the pure effects of *actual* sentence reductions, c_1 and u_1 (respectively the specific deterrent effect of sentence reductions, and their effect on future legitimate prospects) ; and the reference-dependent effects of sentence reductions deriving from poor anticipation (c_2 and u_2).

$$C(r, \Delta^{\tilde{r}}, \alpha) = c_1(r) + (1 - \alpha).c_2(\Delta^{\tilde{r}}) \tag{3}$$

$$\underline{U}(r, \Delta^{\tilde{r}}, \alpha) = u_1(r) + (1 - \alpha).u_2(\Delta^{\tilde{r}}) \tag{4}$$

The cost function $c_2(.)$ represents prisoners' way of processing the surprise (in this case, an unanticipated shock in sentence reductions) into new beliefs regarding the cost of future punishment: intuitively, some prisoners may well interpret unexpected sentence reductions as a signal of impunity (further suggesting that the justice system is "all bark and no bite") so that $\frac{\partial c_2}{\partial \Delta^{\tilde{r}}} < 0$, consistent with the framing effects found by Bushway and Owens (2013). I expect this criminogenic feeling of impunity to be particularly strong on the most short-sighted prisoners, i.e. young and impulsive offenders, and to affect all types of offenses similarly.

The function $u_2(.)$ captures prisoners' change in reservation utility (the prospects of a law abiding life) due to unanticipated sentence reductions. Considering the difficult social background of most prisoners, early release can adversely affect their material conditions at release: surprise release can for example reduce their capacity to secure housing, seek social assistance, find a job, and more generally reenter society successfully. In support of this hypothesis, Wolff et al. (2012) provide qualitative evidence that time in prison allows prisoners to better plan/prepare their release. Moreover, Munyo and Rossi (2014) show how short-term recidivism in Uruguay is driven by the liquidity constraints faced by prisoners at release: they estimate that each prison releasee generates 0.25 additional property crime locally on the day of release. Interestingly, they find that this large correlation completely disappeared in 2010 after an increase in the stipend given to each prisoner at release. Therefore, I expect that unanticipated early release reduces material preparedness for release, and thus reduces the utility of a law-abiding life outside prison, i.e. $\frac{\partial u_2}{\partial \Delta^{\tilde{r}}} < 0$. This "unprepared release" effect is likely to be stronger on prisoners with low resources (in the form of wealth, housing, human and social capital) and should mainly trigger income-generating crime (e.g. property crime).

Finally, expectation-dependent utilities c_2 and u_2 are weighted by a parameter of adapation $\alpha \in [0, 1]$. α captures prisoners' capacity to adapt to and cope with unexpected sentence reductions (both materially and psychologically) *before they actually exit prison.* Operationally, I compute it as the amount of time (in absolute terms or relative to initial sentence) that remains to be served by the date of the news (date of the pardon). The intuition is that a shock in sentence reductions should have negligible effects on prisoners who are at the beginning of their sentence ($\alpha \approx 1$), since they have a lot of time to adapt (materially) and “get used to it” (psychologically): thus, by the time they are released, the shock is no longer salient. Conversely, the model predicts that the detrimental effects of unanticipated sentence reductions should be maximal on prisoners who are near the end of their sentence when the news comes ($\alpha \approx 0$).

Overall, this reference-dependent model of recidivism yields the following formula for the propensity to reoffend:

$$Recid^{*} = B(r) - p[c_1(r) + (1-\alpha).c_2(\Delta^{\tilde{r}})] - [u_1(r) + (1-\alpha).u_2(\Delta^{\tilde{r}})] \tag{5}$$

The model unambiguously predicts that unexpected shocks in sentence reductions are criminogenic, through increased feeling of impunity (psychological channel) and lowered preparedness for release (material channel): $\frac{\partial Recid^{*}}{\partial \Delta^{\tilde{r}}} = -(1-\alpha)(p\frac{\partial c_2}{\partial \Delta^{\tilde{r}}} + \frac{\partial u_2}{\partial \Delta^{\tilde{r}}}) > 0$. Therefore, prisoners who correctly anticipate sentence reductions ($\Delta^{\tilde{r}} \approx 0$) should benefit the most (or suffer the least) from sentence reductions. Second, the adverse effect of unanticipated sentence reductions should be maximal among prisoners who had little time or capacity to adapt ($\alpha \approx 0$) between the news and eventual release.

2 Testing the role of anticipations and adaptation to surprise

In order to test the main predictions of the model, I build on prior work by Maurin and Ouss (2009) and exploit the collective pardon of July 1996 in France. From the 1980’s to 2006, French Presidents Mitterrand and Chirac regularly issued collective pardons near the National Holiday (the 14^{th} of July), granting sentence reductions to almost all incarcerated prisoners. This practice, which was at the discretion of the President, suddenly stopped after Sarkozy’s election in 2007.

French collective pardons have several attractive features to test my theory. First, all prisoners who are in prison on the day of the pardon are eligible, except for a short list of specific offence types (terrorism, crime againt children or police, and serious drug trafficking for the July 1996 pardon). Second, the amount of sentence reductions granted follows a universal rule which applies to all eligible inmates, based on their residual sentence (in 1996, it was one week of sentence reduction for each month that remains to be served in prison on the day of the pardon). Third, collective pardons do not have any consequence other than granting sentence reductions: importantly, they do not affect the minimum or maximum penalty in case of recidivism (conversely to the famous Italian clemency bill of 2006 studied by Drago et al. (2009)), or the period of post-release supervision (conversely to parole-based experiments such as Kuziemko (2013)). Therefore, French collective pardons provide individual-level variation in the amount of sentence reductions that is unrelated to prisoners' personal traits or behavior: after constructing a purely exogenous instrumental variable, this design allows me to get rid of omitted-variable bias and identify the true causal effect of sentence reductions on future crime. Finally, a nice feature of French collective pardons is that they affect all prisoners on the same day, with some inmates being at the beginning of their sentence and others at the end of their term: this feature allows me to test the role of adaptation to surprise, using time before release (in absolute or relative terms) as a proxy for capacity to adapt.

However, the fact that collective pardons were a yearly tradition every month of July in France (until 2007) may seem problematic: it suggests that prisoners were able to perfectly anticipate pardons. I argue that it is highly unlikely and that prisoners differed in their capacity to anticipate the pardon: first, note that collective pardons were at the full discretion of the President, who could decide whether to issue a pardon or not, without any justification. Thus, French pardons were not a matter of long parliamentary debate, conversely to Italian pardons for example (Campaniello et al., 2014). Second, the precise design of the pardon (which offense types are eligible or not, how to compute time remaining, how much sentence reductions to grant, etc.) often varied from one year to the next[14]. Therefore, though prisoners could expect the occurence of a pardon in July, it was impossible for them to perfectly anticipate how much sentence reductions they would obtain, and therefore when they would be eventually released. As noted by Evans (2006), during the 1990's, many prisoners were suddenly released on the day of the pardon with no prior notice. The Ministry of Justice became aware of this problem in the early 2000's, and explicitely instructed parole officers to anticipate upcoming pardons to better prepare the exit of those *soon-to-be-released* prisoners. Overall, both anecdotal evidence and the concrete design of French pardons tend to reject the idea that prisoners easily anticipate those sentence reductions. Therefore, the pardon of July 1996 presumably induced at least *some* surprise among French prisoners.

In order to empirically test the adverse effect of such surprise on recidivism, I assume that prisoners who had large prior criminal experience (because they were already incarcerated during the previous pardon of July 1995, or because they were repeat offenders) were more likely to expect the collective pardon of 1996, more able to correctly anticipate its effect on eventual date

[14]For a complete historical and legal analysis of pardons in France, see Evans (2006)

of release, than prisoners with little to no criminal experience.

2.1 Data

The dataset is drawn from a nationaly representative survey by Kensey and Tournier (2005) for the French Prison Administration. It contains individual information on 2408 convicted prisoners who were released from French prisons between May 1st, 1996 and April 30th, 1997. Data include sociodemographic and judicial characteristics recorded at entry (gender, age, educational level, employment status, marital status, homelessness, French citizenship), detailed information regarding the initial conviction (type of offense committed, prison sentence length) and the number of prior convictions. Importantly, the dataset precisely records the dates of incarceration and release, the amount of pre-trial detention, as well as the total amount of sentence reductions by type (earned good time credit, sentence reductions from collective pardons, individual early-release programs, etc.). Recidivism is measured as any new conviction in criminal records by June 2002, allowing a follow-up period of 5 years after prison release[15]. Finally, I retrieve rudimentary prison-level characteristics for each prisoner (region, type of prison, capacity, overcrowding rate as of Jan 1, 1996)[16].

By design, most of the offenders in the sample were in prison when the collective pardon of 1996 was enacted on July 9 ; a fraction of them, serving long sentences, also benefited from previous pardons (July 1995, July 1994, etc.), but none benefited from the next pardon of July 1997. In this paper, I focus on the effect of the 1996 pardon on sentence reductions and future recidivism among prisoners who were in prison on July 9, 1996: I exclude sampled prisoners whose expected release date was before July 9 because they were not eligible to the 1996 pardon[17], and prisoners who entered prison after July 9 because pardon eligibility for this group can not be recovered from the data (eligibility was based on (missing) date of conviction) [18]. These two sample restrictions are necessary to obtain robust identification of the causal

[15]Criminal records cover all offenses that led to any new conviction by June 2002, and include date and type of new offense(s) in a quite detailed format, as well as type and length of new sentence(s). This measure of recidivism is particularly reliable for three reasons : (a) it requires final conviction for a new crime (and not simply rearrest or parole violation) committed after release ; (b) it covers a 5-to-6 years follow-up period, much longer than most empirical studies in the field ; (c) it captures new offenses no matter whether the new sentence is custodial or not.

[16]For the minority of inmates who served time in several different prisons for the current sentence, I can only retrieve information for the last prison facility before release, since previous facilities are not recorded. Among the 183 different prisons represented in the original dataset, it was impossible to clearly identify three facilities except for their type and region (probably because they shut down since 1996) so I replace the few missing values for capacity and overcrowding by their $region * type$ averages.

[17]Note that this group of prisoners released just before July 9, 1996, could serve as a control group in a Regression Discontinuity design with the day of pardon as the cutoff, following Maurin and Ouss (2009). However, this group is rather small in the dataset, and there is evidence that trends in recidivism are not parallel on both sides of the July 9 cutoff, yielding biased RD estimates. Therefore, I choose to avoid this problem and use an alternative empirical strategy on a smaller sample.

[18]Some offenders who entered prison after July 9 could benefit from the 1996 pardon (and obtain a flat sentence

effect of sentence reductions on future crime.

Additionaly, in order to improve the precision of my instrument (avoiding weak-IV problem) and study a more homogenous group of prisoners, I exclude the fraction of prisoners who were convicted to prison sentences longer than 3 years (N=303) and the handful of prisoners who benefited from amnisties (N=36). The study sample reduces to 952 observations.

In this new sample, sentence reductions remain highly right-skewed, as depicted in Figure 1: 95% of prisoners obtain less than one year of sentence reductions, and 5% of prisoners obtain between 360 and 720 days. A similar pattern is observed for the instrumental variable ($TimeRem$, see below). Such large skewness, i.e. extreme values, could well bias standard estimators. Therefore, I winsorize these two variables at the 95th percentile (capping extreme values at the 95th percentile), i.e. at 360 days for sentence reductions and 212 days for time remaining. Plus, I treat the handful of negative values for sentence reductions as zeros, since such cases only occur when prisoners serve too much time in pre-trial detention compared to their eventual prison sentence. As expected, these manipulations have a limited impact on the overall distribution of these two variables, but treat the problem of outliers (see Figure 1 in Appendix).

Descriptive statistics about initial sentence and sentence reductions appear in Table 1. On average, sampled prisoners were initially convicted to 14 months of prison (424 days) but actually served about 10 months, thanks to 130 days of sentence reductions (31% of the initial sentence). "Good behavior" explains more than half of total sentence reductions (72 days obtained on average), but pardons also contribute to a large extent (about one third of sentence reductions, or 40 days). Early-release programs do not play a major role on average, since they only concern about 20% of prisoners.

Table 1: Sentence length and time served

Variables (in days)	Mean	Std. Dev.	Share of initial sentence
Initial sentence length	424.2	281.9	100%
Sentence reductions	129.9	98.5	30.6%
Good behavior	71.7	57.9	16.9%
Pardon	39.5	46.2	9.3%
Early-release programs	23.4	74.8	5.5%
Sample Size		952	

Table 2 provides summary statistics for the outcome variables. My main dependent variable for recidivism is reconviction in the 5 years following prison release: in the sample, 60% of

reduction of 2 months) depending on their date of *conviction* to prison. However, date of conviction is not recorded in the dataset, so this cutoff can not be exploited as a source of identification.

prisoners are reconvicted in this time window. Reconviction is already 38% after one year. About two thirds of reoffenders are reconvicted to a new prison sentence for their first new offense. Finally, recidivism is largest in property crime (24% commit a property crime in the 5 years after release) but violent, traffic and other crimes also exhibit large rates of recidivism, over 10%. Note that these crime-specific frequencies do not add up to 60% since offenders may be reconvicted for several offenses in a single criminal case.

Table 2: Recidivism Variables

Outcome	Share
Any Reconviction after 5 years	59.9%
... 1 year	37.7%
Reconviction to ...	
Prison sentence	40.0%
Alternative sentence	19.9%
Reconviction for ...	
Property crime	23.8%
Violent crime	10.7%
Drug crime	5.4%
Traffic crime	11.0%
Other	19.4%
Sample Size	952

3 Empirical strategy: exploiting the French collective pardon of 1996

In order to exploit the collective pardon of July 1996, I need to construct a credibly exogenous instrumental variable. As explained earlier, all prisoners who were in prison on July 9 were eligible to the pardon, except for those convicted for terrorism, serious drug trafficking, and crimes against children under 15, police or correctional officers. Descriptive statistics from the study sample suggest that about 80% of incarcerated prisoners obtained sentence reductions from the pardon.

More problematic is the computation of sentence reductions: according to the pardon decree, eligible prisoners are granted one week of sentence reduction for each month that remained to be served in prison on July 9, 1996, in the limit of 4 months of sentence reductions. In their evaluation of the pardon, Maurin and Ouss (2009) consider this rule as exogenous: they calculate the *actual* amount of prison time remaining on July 9 and use it to construct their IV. However, this approach presumably introduces bias in their IV estimates, since the rule of allocation of sentence reductions is actually endogenous: conditional on original sentence length, the amount of reductions granted by the pardon depends (negatively) on how much sentence reductions were already obtained by prisoners prior to July 9 (in the form of "earned good time credit" for example)[19]. Therefore, all else equal, the collective pardon tends to reward the worst prisoners -who accumulated little sentence reductions prior to July 9- more than the good ones.

This design of French pardons is likely to bias upward IV estimates where *actual* time remaining serves as an instrument. To obtain a more credible, exogenous instrument for the total amount of sentence reductions obtained by prisoners, I construct the variable *TimeRem* capturing the *theoretical* time remaining by July 9, 1996: this simple measure is computed as the difference between the *ex-ante* date of exit from prison *had the prisoner served the whole of his original sentence* (i.e. date of incarceration t_i + length of original prison sentence s_i), and the exact date of the pardon (July 9, 1996).

$$TimeRem = t_i + s_i - t_{July9} \tag{6}$$

This computation of time remaining is agnostic about what happened inside prison before July 9, i.e. whether prisoners earned good-time credit or not. It only depends on the date of incarceration (which is supposedly exogenous) and the length of the original prison sentence (which can be easily controlled for in regressions). Therefore, I view this variable as a credibly valid instrument for the total amount of sentence reductions, in contrast to the measure of "actual" time remaining used by Maurin and Ouss (2009).

Model specification The main goal of my econometric model is to estimate the effect of sentence reductions on the probability of recidivism among ex-prisoners, treating (potential) omitted-variable bias by exploiting the exogenous variation in sentence reductions induced by the collective pardon of July 1996. Therefore, the baseline econometric model writes as follows:

$$Recid_i^* = \beta_0 + \beta_1 r_i + \boldsymbol{\Gamma' s_i} + \boldsymbol{\Pi' Pretrial_i} + \boldsymbol{\Psi' X_i} + \epsilon_i \tag{7}$$

$$r_i = \alpha_0 + \alpha_1 TimeRem_i + \boldsymbol{\Omega' s_i} + \boldsymbol{\Theta' Pretrial_i} + \boldsymbol{\Lambda' X_i} + e_i \tag{8}$$

[19] In France as in many countries, prisoners can obtain "earned good time credit" periodically throughout incarceration, as long as they do not misbehave in prison. Most prisoners are granted the maximum amount of good time credit (4 months for the first year of incarceration, then 3 months per year), but these sentence reductions are often withdrawn in case of misconduct (such as violence or use of drugs). Conversely, some prisoners can obtain extra "good time credit" if they behave particularly well inside prison, demonstrate exceptional rehabilitation efforts, or help to solve a criminal case for example. Because they are based on merit and rehabilitation efforts, these sentence reductions are likely endogenous, i.e. correlated with intrinsic propensity to reoffend.

where $Recid^*$ is the latent propensity to reoffend after release expressed in Equation 5. The coefficient of interest, β_1, is identified by the instrument $TimeRem$ included in Eq. 8. However, for this variable to be a credible instrument, I need to precisely control for original sentence length s_i and pre-trial detention: to do so, I include a third-degree polynomial for s_i, and two dummy variables measuring whether prisoners served some pre-trial detention before July 9, and after July 9.

Less important for identification, but possibly useful to improve the precision of the estimates, I occasionaly include an additional vector of control variables $\boldsymbol{X}$ measured at the individual- and prison-levels. The individual-specific variables include: type of initial offense (five dummy variables), prior criminal convictions (one dummy for any conviction, and one continuous variable for the total number), gender, age and age squared on July 9, and type of release (three dummies capturing the three main types of early-release programs)[20]. Regarding prison-specific variables, I retrieve data on the type of prison[21] and the overcrowding rate in the facility as of January 1, 1996.

Since the dependent variable is dichotomous, I mostly rely on Maximum Likelihood estimation of IV Probits to estimate my two-equation model (though I also use the more straighforward 2SLS estimator on occasions). However, for benchmark purposes, I also estimate naïve regressions of Equation 7 alone by Probit or OLS, treating sentence reductions r as exogenous.

4 Results

4.1 Benchmark results

Table 3 reports naïve Probit and OLS estimates: they consider the total amount of sentence reductions obtained, r, as exogenous. They only serve as benchmarks before turning to the more credible IV estimates.

Columns 1 to 3 report Average Marginal Effects after Probit estimation. On average, when only the most basic control variables are included (original sentence and pre-trial detention), there is a significant negative relationship between sentence reductions and propability of recidivsm in the 5 years after release: a 10-day increase in sentence reductions is associated with a 0.9 percentage point decrease in probability of recidivism. However, as we include more and more control variables, the estimates diminish in absolute terms and lose statistical significance. The results are essentially unchanged when OLS is used.

[20] *Libération conditionnelle* (parole), *Semi-liberté* (halfway houses) et *Placement à l'extérieur*

[21] There are three main types of prisons in France: most prisons are devoted to pre-trial detainees and short sentences (Maisons d'Arrêt); others are dedicated to host medium sentences (Centres de Détention); and a few prisons only hosts very serious criminals and gang members (Maisons Centrales)

Table 3: Benchmark Estimates on 5-year Recidivism: Full Sample

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Probit	Probit	Probit	OLS	OLS	OLS
Sentence Reductions (/10)	-0.0090**	-0.0039	-0.0038	-0.0092**	-0.0042	-0.0039
	(0.0040)	(0.0045)	(0.0045)	(0.0041)	(0.0045)	(0.0046)
Initial Sentence	-0.0001	-0.0001	-0.0001	-0.0001	-0.0001	-0.0001
	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
—— squared	-0.0338	-0.0149	-0.0120	-0.0361	-0.0107	-0.0096
	(0.0657)	(0.0579)	(0.0585)	(0.0674)	(0.0616)	(0.0624)
—— cubed	0.0913	0.0507	0.0473	0.0956	0.0529	0.0492
	(0.0632)	(0.0559)	(0.0568)	(0.0651)	(0.0586)	(0.0595)
Any Pre-trial Detention	0.0139	0.0229	0.0173	0.0139	0.0194	0.0123
	(0.0368)	(0.0329)	(0.0330)	(0.0365)	(0.0329)	(0.0331)
Any Pre-trial After July 9	-0.1095**	-0.0316	-0.0251	-0.1117**	-0.0329	-0.0237
	(0.0522)	(0.0484)	(0.0493)	(0.0538)	(0.0516)	(0.0529)
Constant				0.6929***	0.4123	0.6234*
				(0.2129)	(0.3219)	(0.3469)
Observations	952	952	932	952	952	932
R-squared				0.0504	0.2437	0.2487
Indiv. Controls	-	X	X	-	X	X
Prison Controls	-	-	X	-	-	X

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

4.2 IV results: overall effects

In Table 4, I now treat sentence reductions as endogenous (using $TimeRem$ as my instrument), paying no particular attention to anticipation and adaptation -following prior research. Both Maximum Likelihood (IV Probit) and 2SLS (Linear Probability Model) results are shown.

Regarding the First Stage, the instrument $TimeRem$ behaves as expected: *theoretical* time remaining on July 9 is a (very) significant predictor of overall amount of sentence reductions. The estimates suggest that, conditional on original sentence and pre-trial detention, each additional day that remains to be served on July 9 generates about 0.2 additional days of sentence reductions. This figure is very consistent with the rules of the July 1996 pardon, as it suggests that a 30-day (one month) increase in time remaining leads to an overall reduction of time served by about 6 days[22]. This is confirmation that the collective pardon of 1996 introduced large, arbitrary differences in incarceration length between silimar prisoners, depending on how much of their original sentence remained to be served by July 9 (i.e. depending on their date of incarceration).

Regarding the Second Stage estimates of the effect of sentence reductions on future crime, all point estimates are slighlty negative but insignificant. Thus, there is no detectable evidence that sentence reductions have any overall causal effect on recidivism after 5 years, similarly to Abrams (2011) for example. Moreover, compared to the Probit and OLS results, all these IV estimates are of smaller magnitude (in absolute terms). Though these differences are thin and non-significant, this pattern is consistent with the presence of omitted variable bias, i.e. a selective allocation of sentence reductions, with good prisoners (low intrinsic propensity to reoffend) receiving more sentence reductions than bad prisoners.

Table 4: IV Estimates on 5-year Recidivism: Full Sample

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	IV Probit	IV Probit	IV Probit	2SLS	2SLS	2SLS
Second Stage						
Sentence Reductions (/10)	-0.0025	-0.0015	-0.0024	-0.0025	-0.0012	-0.0018
	(0.0073)	(0.0079)	(0.0083)	(0.0073)	(0.0079)	(0.0083)
First Stage						
TimeRem (/10)	0.2305***	0.1937***	0.1876***	0.2305***	0.1937***	0.1876***
	(0.0110)	(0.0094)	(0.0095)	(0.0111)	(0.0095)	(0.0096)
Observations	952	952	932	952	952	932
Indiv. Controls	-	X	X	-	X	X
Prison Controls	-	-	X	-	-	X
R-squared				0.0477	0.2434	0.2485
First Stage R-squared				0.8980	0.9301	0.9314
F-stat				433	419	380

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

[22] Following the rules of the pardon, readers may have expected a relationship of 7-day per month remaining, not 6 days or so. However, this result should not come as a surprise since the dependent variable r sums all sentence reductions obtained during incarceration, both before and after the pardon. Prisoners who benefited a lot from the pardon had lower opportunities to benefit from additional sentence reductions after the pardon, compared to prisoners who benefited less from the pardon. This is the reason why the overall effect of the July 1996 pardon on sentence reductions is a bit smaller than 7 days per month. Another reason may also have to do with the 4-month limit for the pardon.

4.3 IV results: mediating effects of anticipation and adaptation

In order to test the main implications of the model on anticipations and adaptation, I successively estimate Equations 7 and 8 on different subgroups. First, to test the mediating effect of adaptation, I use the lenght of original sentence remaining by July 9 as a proxy for prisoners' capacity to adapt to the news before release, both materially and psychologically. The sample is divided in two same-sized groups of time remaining (more or less than 6 months remaining). Since absolute time remaining is also used as the intrumental variable, I later consider interaction terms between sentence reductions and *relative* time remaining (constructed as the share of the original sentence that remains to be served on July 9).

Second, to test the mediating effect of surprise (i.e. poor anticipations), I split the sample in two same-sized groups of prisoners based on their prior exposure to pardons and the criminal justice system: prisoners who were already in prison during the previous pardon of July 1995 and inmates who had at least 4 previous criminal convictions are coded as "large prior experience" and are expected to better anticipate the pardon of July 1996 and eventual date of release. By contrast, I hypothesize that less experienced prisoners, those who were not in prison for the pardon of July 1995 and who had less than 4 prior convictions, are more likely to be surprised by the pardon of 1996.

Table 5 reports average marginal effects from the Second Stage estimates after running IV Probits on each subgroup. First, in Columns 1 and 2, I split the sample by absolute time remaining (more than 6 months versus less than 6 months). The results are in stark contrast with Table 4: they suggest that sentence reductions have a large, significant criminogenic effect among prisoners who had less than 6 months between the news of the pardon (July 9) and original date of release. The average marginal effect is large: a 10-day rise in sentence reductions increases probability of recidivism within five years by 5 percentage points. Conversely, the point estimate is negative and insignificant among prisoners who had more than 6 months remaining to adapt and prepare for release. These results are consistent with the predictions of the model: the effect of sentence reductions on future crime is mediated by prisoners' time to adapt between news and release.

The model also predicts that anticipations matter: the criminogenic effects of sentence reductions should concentrate on prisoners who are unable to anticipate them, whereas inmates who are not surprised should not react. To test this prediction, I further split the sample between prisoners who had ''large prior experience" of pardons or the justice system (presumably high capacity to anticipate) and those with low or null prior experience (low capacity to anticipate). The results appear in Columns 3 to 6. The estimates are again consistent with the implications of the model: specifically, sentence reductions have a large criminogenic effect on inmates who combine low experience and little time left to adapt (recidivism is up by 8 pp for each 10-day increase in sentence reductions), a large crime-preventing effect on prisoners with low experience but a lot of time to adpat to the news (-4 pp), and no detectable reaction from experienced inmates, whatever the amount of time remaining. As expected from my design, these results are robust to the exclusion of individual-level and prison-level control variables (Table 12 in Appendix).

Table 5: IV Estimates on 5-year Recidivism, by Anticipation and Adaptation

	(1)	(2)	(3)	(4)	(5)	(6)
	All		Little Experience		Large Experience	
VARIABLES	$<$ 180 days	$\geq$ 180 days	$<$ 180 days	$\geq$ 180	$<$ 180 days	$\geq$ 180
Second Stage						
Sentence Reductions (/10)	0.0465**	-0.0152	0.0966***	-0.0362*	0.0021	0.0084
	(0.0186)	(0.0168)	(0.0262)	(0.0217)	(0.0208)	(0.0252)
First Stage						
TimeRem (/10)	0.2149***	0.1325***	0.1840***	0.1975***	0.2663***	0.1226***
	(0.0183)	(0.0171)	(0.0197)	(0.0311)	(0.0331)	(0.0253)
Observations	463	469	268	224	195	245
Indiv. Controls	X	X	X	X	X	X
Prison Controls	X	X	X	X	X	X

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

As an additional test of the role of anticipation and adaptation, I use a second approach. Instead of splitting the sample in two by absolute time remaining (which is also my instrument), I construct the share of original sentence remaining by July 9, *ShareRem*, and interact it with sentence reductions. In order to estimate causal effects by IV, I also interact *ShareRem* with the instrument in the first stage. The results after 2SLS estimation appear in Table 6.

The pattern is similar to the previous results: sentence reductions tend to have large criminogenic effects when prisoners only have a small share of their original sentence left to serve on July 9. Conversely, when prisoners have more and more of their original sentence remaining to adapt to the news and prepare eventual release, the criminogenic effect of sentence reductions diminishes significantly. As expected, this pattern of a diminishing effect of sentence reductions as *ShareRem* increases is much clearer on prisoners with low experience of pardons or the justice system (the reaction of experienced prisoners to sentence reductions seems much smaller and flatter with regards to time remaining).

Table 6: IV Estimates on 5-year Recidivism, by Anticipation and Adaptation

	(1)	(2)	(3)
VARIABLES	All	Little Experience	Large Experience
Sentence Reductions (/10)	0.0693	0.0890	0.0333
	(0.0438)	(0.0675)	(0.0398)
r * ShareRem	-0.0381**	-0.0677*	-0.0112
	(0.0188)	(0.0346)	(0.0211)
ShareRem	0.0638	0.3182**	-0.1337
	(0.1248)	(0.1546)	(0.1820)
Observations	932	492	440
R-squared	0.1663	0.1372	0.2736
Indiv. Controls	X	X	X
Prison Controls	X	X	X

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

4.4 Mechanism: psychology or capital ?

These results are consistent with the main implications of the model, i.e. the fact that the impact of sentence reductions on recidivism crucially depends on prisoners' capacity to anticipate them, adapt, and prepare for early release. However, the results do not allow to disentangle between the two potential mechanisms, $c_2(.)$ and $u_2(.)$, discussed in Section 2.

According to the *psychological* channel, unexpected sentence reductions leading to rapid release are particularly salient in prisoners' mind and foster feelings of impunity. I expect this kind of reasoning to lead to recidivism among impulsive prisoners, such as young offenders convicted for expressive crime (e.g. violence).

According to the *capital* channel, unexpected sentence reductions leading to rapid release leave prisoners worse off, with less time to plan their release and make material arrangements (in terms of housing, employment, etc.). This adverse effect of sentence reductions is likely stronger on offenders with low social, human and economic capital at their disposal. Moreover, this criminogenic effect should mostly lead to income-generating crime (e.g. property crime).

In order to disentangle these two mechanisms, I investigate the presence of heterogenous effects based on prisoners' age and initial offense type (two possible proxies for impulsivity), and based on their socio-economic capital. I also estimate crime-specific regressions to study the differential effect of sentence reductions on five types of new crime: property, violent, drug, traffic, and other offenses.

Impunity/Impulsivity In Table 8, I split the sample by age on July 9 (above/below median) to test whether the criminogenic effect of sentence reductions diminishes with age. The estimates do not reveal large differences: younger and older offenders seem to react similarly to sentence reductions (more recidivism when they have little time left to adapt). The results are essentially unchanged when I interact age with sentence reductions, instead of running separate regressions on two age groups: the coefficient for the interaction term is virtually zero for all groups (Table 11 in Appendix).

Table 7: IV Estimates on 5-year Recidivism, by Adaptation and Age

	(1)	(2)	(3)	(4)
	Age < 29		Age ≥ 29	
VARIABLES	$<$ 180 days	$\geq$ 180 days	$<$ 180 days	$\geq$ 180
Sentence Reductions (/10)	0.0430*	-0.0400	0.0520*	-0.0075
	(0.0239)	(0.0260)	(0.0288)	(0.0203)
Observations	243	224	220	245
Indiv. Controls	X	X	X	X
Prison Controls	X	X	X	X

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

To further test the impunity/impulsvity mechanism, I also split the sample between 4 types of original offense: property, violent, drug, and traffic or other offenses[23]. Initial offense can possibly serve as a proxy for impulsivity, with violent offenders more impulsive than property offenders for example. The separate regressions yield similar results for the different types of initial offenses, and notably for property offenders (0.004) and violent offenders (0.003).

Overall, there is little evidence that the effect of sentence reductions is mediated by these two proxy measures of impulsivity.

Score of Capital To test the implication that prisoners with low capital were more adversely affected by the pardon, I build an individual score of socio-economic resources for each prisoner. This score is obtained from a Multiple Correspondance Analysis aggregating 5 socio-economic

[23] There are only 53 prisoners convicted for traffic offenses initially, so I add them to the "other" category

Table 8: IV Estimates on 5-year Recidivism, by Initial Offense Type

VARIABLES	(1) Property	(2) Violent	(3) Drug	(4) Traffic+Others
Sentence Reductions (/10)	0.0043	0.0030	0.0039	-0.0128
	(0.0124)	(0.0152)	(0.0183)	(0.0190)
Observations	284	203	174	271
Indiv. Controls	X	X	X	X
Prison Controls	X	X	X	X

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

characteristics measured at entry: marital status, employment status, education level, homelessness, property offender. These dummy variables are rather crude but they likely proxy the social, economic, human capital available to prisoners to reenter society successfully. The results of the MCA are reported in Figure 2 (Appendix).

After constructing the score of capital at entry, I slip the sample in two same-size groups: prisoners with low capital and prisoners with high capital (MCA score below/above median). The results are reported in Table 9. Sentence reductions have a very large criminogenic effect on prisoners with little time remaining and a low score of capital: it is estimated that a 10-day increase in sentence reductions increases probability of 5-year recidivism by 7 percentage points in this group. Conversely, sentence reductions have no large or significant effect on low capital prisoners with more time to adapt, or on high capital prisoners. This pattern is consistent with a criminogenic ”impreparedness for release” mechanism.

Table 9: IV Estimates on 5-year Recidivism, by Score of Capital at Entry

	(1)	(2)	(3)	(4)	(5)	(6)
	All		Low Capital		High Capital	
VARIABLES	$<$ 180 days	$\geq$ 180	$<$ 180 days	$\geq$ 180	$<$ 180 days	$\geq$ 180 days
Sentence Reductions (/10)	0.0464**	-0.0152	0.0707***	-0.0168	0.0150	-0.0308
	(0.0186)	(0.0168)	(0.0220)	(0.0197)	(0.0342)	(0.0254)
Observations	463	469	245	258	218	211
Indiv. Controls	X	X	X	X	X	X
Prison Controls	X	X	X	X	X	X

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Crime-specific Effects Another approach to test this mechanism is to estimate the effect of sentence reductions on different types of new crimes. According to the *capital* channel, I expect the criminogenic effect to concentrate on new income-generating crime (property crime). Therefore, I successively consider 5 crime-specific dependent variables[24]: property crime, violent crime, drug crime, traffic crime, and other.

Significant estimates are obtained only for property crime. As shown in Table 10, sentence reductions generate property crime recidivism among prisoners with little experience and little time to adapt. Conversely, when these offenders have more time to adapt, sentence reductions tend to be beneficial and reduce property crime. Again, prisoners with a large prior experience of pardons and the justice system do not react. The same output is reported for the other types of crimes in Table 13 (Appendix): estimates are much smaller and never significant.

Table 10: IV Estimates on 5-year Property Crime, by Anticipation and Adapation

	(1)	(2)	(3)	(4)	(5)	(6)
	All		Little Experience		Large Experience	
VARIABLES	< 180 days	≥ 180 days	< 180 days	≥ 180	< 180 days	≥ 180
Sentence Reductions (/10)	0.0147	-0.0241	0.0564**	-0.0206	-0.0068	-0.0198
	(0.0191)	(0.0150)	(0.0275)	(0.0189)	(0.0248)	(0.0251)
Observations	463	469	268	224	195	245
Indiv. Controls	X	X	X	X	X	X
Prison Controls	X	X	X	X	X	X

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Taken together, these findings that the criminogenic effect of sentence reductions concentrates on low-capital prisoners and in property crime provide suggestive evidence that capital matters: sentence reductions tend to generate crime among prisoners with little capacity to adapt materially to early release. Conversely, using age and type of initial offense as proxies, there is no evidence that impulsivity is a major explanation of prisoners' reactions to sentence reductions.

[24] These crime-specific variables are not mutually exclusive, since some reoffenders may commit several types of crimes for which they will be reconvicted in a single criminal case

5 Conclusion

This paper aims to introduce a new, more realistic framework to study how sentence reductions may affect future criminal behavior. Prior research makes the implicit assumption that sentence reductions only affect the "treatment intensity" of incarceration, or that they only change the amount of time served behind bars. In this paper, I argue that the concrete design of these changes, i.e. the framing of sentence reductions, may matter crucially: using a reference-dependent framework, I show how similar levels of sentence reductions can have different effects on prisoners' propensity to reoffend after release, depending on their expectations and their capacity to adapt to these changes. The theoretical section distinguishes between two potential mechanisms, one psychological (based on the saliency of impunity feelings) and one material (based on prisoners' capital available to adapt to early release).

The empirical part of the paper attempts to test the main implications of the model. While hypothesis testing is rather indirect (*real* expectations and adaptation are not observed), I find consistent evidence supporting the key role of framing: sentence reductions have very different effects on recidivism depending on prisoners' capacity to anticipate and adapt before release. As expected, my estimates show that the most criminogenic effects of sentence reductions are observed among prisoners who were unlikely to correctly anticipate the pardon of July 1996 and who had little time to adapt to this change, materially and psychologically. Conversely, most of my estimates for sentence reductions and recidivism are negative and insignificant among prisoners who had more chances of correctly predicting the pardon or more time to adapt to the news. Finally, I find suggestive evidence in favor of a *material* channel, i.e. the idea that unexpected sentence reductions leave prisoners worse off in terms of their preparedness for release. In a word, it seems that, for many prisoners released just after the pardon of July 1996, *it wasn't the right time to get out.*

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Appendix

Figure 1: Distribution of Sentence Reductions Before/After Winsorizing

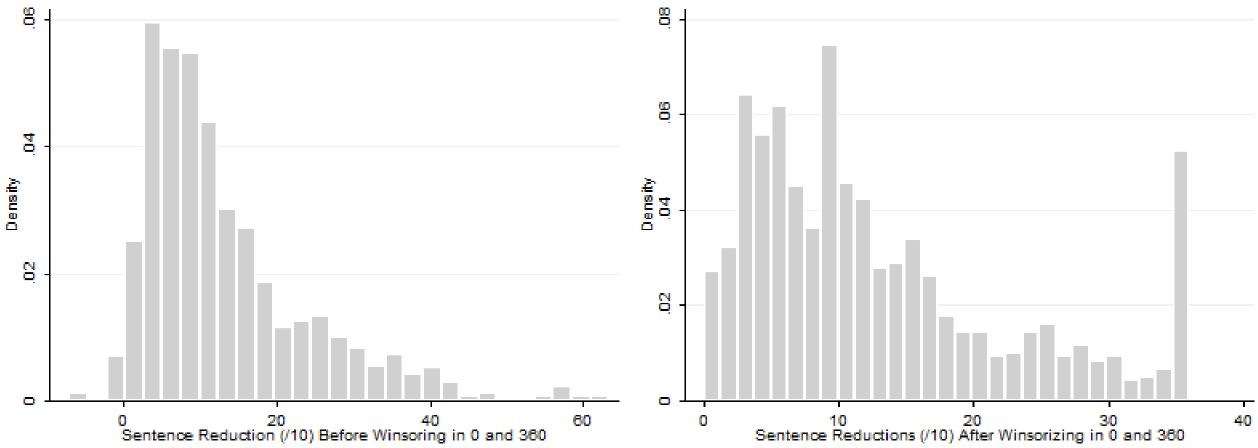

Figure 2: Distribution of MCA score of capital

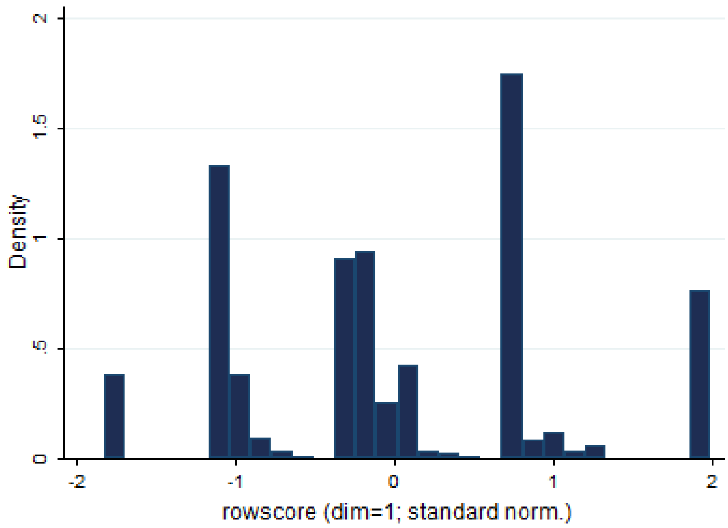

Table 11: IV Estimates on 5-year Recidivism, by Adaptation and Age (Interaction)

	(1)	(2)	(3)
	All	Little Experience	Large Experience
VARIABLES	recid_y5	recid_y5	recid_y5
Sentence Reductions (/10)	0.0032	0.0527*	-0.0170
	(0.0095)	(0.0309)	(0.0182)
sr10_wage	-0.0002	-0.0002	0.0001
	(0.0001)	(0.0008)	(0.0002)
age	-0.0105	0.0465	-0.0482
	(0.0550)	(0.0788)	(0.0800)
Observations	932	463	469
R-squared	0.2491	0.1903	0.3167
Indiv. Controls	X	X	X
Prison Controls	X	X	X

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 12: IV Estimates on 5-year Recidivism, by Anticipation and Adaptation

IV Probit estimates by Absolute Time Remaining

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	$<$ 180 days	$\geq$ 180 days	$<$ 180 days	$\geq$ 180	$<$ 180 days	$\geq$ 180
Sentence Reductions (/10)	0.0498***	-0.0148	0.0771***	-0.0446***	0.0235	0.0194
	(0.0172)	(0.0123)	(0.0225)	(0.0155)	(0.0229)	(0.0160)
Observations	475	477	274	229	201	248
Indiv. Controls	-	-	-	-	-	-
Prison Controls	-	-	-	-	-	-

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 13: IV Estimates on Different Crime Types after 5 years

	(1)	(2)	(3)	(4)	(5)	(6)
	All		Little Experience		Large Experience	
VARIABLES	$<$ 180 days	$\geq$ 180 days	$<$ 180 days	$\geq$ 180	$<$ 180 days	$\geq$ 180
Property Recid.	0.0147	-0.0241	0.0564**	-0.0206	-0.0068	-0.0198
	(0.0191)	(0.0150)	(0.0275)	(0.0189)	(0.0248)	(0.0251)
Violent Recid.	-0.0058	0.0096	-0.0151	0.0001	0.0237	0.0229
	(0.0181)	(0.0135)	(0.0267)	(0.0154)	(0.0299)	(0.0221)
Drug Recid.	0.0092	-0.0123	-0.0029	0.0003	0.0204	-0.0425
	(0.0132)	(0.0132)	(0.0218)	(0.0152)	(0.0219)	(0.0263)
Traffic Recid.	0.0145	0.0047	0.0292	-0.0113	-0.0055	0.0059
	(0.0154)	(0.0135)	(0.0262)	(0.0175)	(0.0194)	(0.0218)
Other Recid.	0.0123	-0.0053	0.0153	0.0058	0.0040	-0.0094
	(0.0189)	(0.0155)	(0.0295)	(0.0212)	(0.0264)	(0.0251)
Observations	463	469	268	224	195	245
Indiv. Controls	X	X	X	X	X	X
Prison Controls	X	X	X	X	X	X

Separate IV probit regressions for each dependent variable and each group.
Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1