

**How far do criminals understand the criminal law?
Evidence from French mandatory sentencing¹**

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Abstract: This paper documents how quickly and how precisely would-be offenders understand and adapt to criminal law. It relies on a mandatory sentencing act against repeat offenders passed by the French parliament in August 2007. It exploits the gap between the public presentation of the law – an overall increase in the severity of sentences on repeat offenders – and the enforcement – an increase on a specific subgroup of repeat offenders. Using duration model and competitive risk analysis on individual data representing the universe of convictions that occurred in France during this period, this paper studies the evolution of the two instantaneous probabilities of committing a new crime targeted or not targeted by the law. The analysis shows that the law equally deterred targeted and untargeted crimes in the short term while only targeted behaviors remain affected in the medium term. These results provide evidence that even a strongly distorted presentation failed to mislead people for a long time. They are coherent with a learning effect of complex criminal law. This learning effect goes faster for more rational criminals or older offenders.

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Introduction

Over the last several decades, public opinion in industrialized countries has been increasingly concerned with questions of crime and insecurity. In the same time, incarceration rates have risen dramatically in the US and more recently in Europe (Buonanno et al, 2011). "Tough on crime" policies, influenced by people's worries (Wacquant, 2009), are partly responsible for the increase in the incarceration rate (Raphael and Stoll, 2013). According to their promoters, such policies aim to deter would-be offenders.

A growing literature has been focusing on this evolution and specifically on the deterrent effect of the increase in sentencing. Numerous papers focus on the effect of "three strikes laws" - life imprisonment without the possibility of parole upon the third strike (Kessler and Levitt, 1998; Sheperd, 2002; Helland and Tabarrok, 2007). They also explore the effect of setting a threshold at 18 years of age (Lee and McCrary, 2005), increasing time for certain crimes (Abrams, 2012) or amnesties (Drago et al, 2009). Other papers focus on harsher sentences (Levitt, 1996) or on the effect of different social costs of sanction (Lee, 2012). Most papers find a significant deterrent effect resulting from increased sentencing even if they also stress negative side effects (Iyengar, 2008; Chen and Shapiro, 2007; Drago et al, 2011).

Papers documenting the effect of an increase in sentence time generally focus on measures that share certain characteristics. First, laws are usually simple (three strikes laws, add-on gun laws) or are clearly notified to targeted offenders (amnesties). Second, evaluations focus on broadly implemented measures. Lastly, increases in time are usually quite high (up to life sentences under three strikes laws).

However, criminal laws are not necessarily simple to understand nor are they broadly implemented. In the US, most states have enacted three strikes laws but only California has been actively enforcing it (Sheperd, 2002). The French Penal Code was modified 30 times between 2002 and 2007 (Mucchielli, 2008), but some laws were only enforced years later, if ever. The effects of unenforced laws or complex changes to criminal law are hard to predict. On the one hand, if people do not know or understand the rules, they have no reason to change their behavior. Just as if a law is not enforced, there is no change in incentives and no reason to modify one's behavior. On the other hand, public "tough on crime" or "zero tolerance" speeches could scare would-be offenders even if nothing has changed in reality. The effect that (mis)perception or biased presentation in justice has on judges (Berdejó and Chen, 2012) or citizens (Pfeiffer et al, 2005; Philippe and Ouss, 2015) has already been documented and is therefore likely for offenders.

Understanding how would-be offenders understand criminal laws is crucial, as increasing sentences is costly for convicted people and for society as a whole. If tough on crime speeches induce significant deterrent effects, even without active enforcement, they represent a cost-effective policy.

This paper focuses on a complex and oversold increase in sentence time in France. In 2007, shortly after the presidential election, the French Parliament passed a bill presented as "reinforcing the struggle against recidivism"³. It introduced mandatory minimum sentencing for recidivists. The thresholds were set at values higher than previous average sentences. However, the law "only" introduced this mandatory sentencing for people who match the legal definition of recidivism. According to the French Penal Code, and contrary to the common sense, people are considered as recidivists only if they commit the *same or similar* act twice⁴. This means that offenders convicted for drug dealing one year after a theft are not eligible for mandatory sentencing while burglars are.

³ Loi n° 2007-1198 du 10 août 2007 renforçant la lutte contre la récidive des majeurs et des mineurs.

⁴ Art. 132-10 and the following. Assimilation rules are defined in articles 132-16 and following.

This subtle but crucial difference was almost completely ignored when the law was presented. Very few articles, interviews or TV reports mentioned it despite the fact that it strongly reduces the scope of the law. As judges were generally opposed to the law,⁵ the application strictly followed the text and did not overflow to what is commonly viewed as recidivism.

This setting gives the opportunity to test how deeply and how quickly people understand the law. The idea is that one behavior, i.e., committing a new crime different from a previous one, was presented as targeted but was not in reality. Another behavior, i.e., committing a new crime identical to the previous one, was presented as targeted and punished more in reality. If a biased presentation has induced confusion, we should observe a joint diminution of "identical" and "different" re-offense. On the contrary, if the law has been perfectly understood, we should observe a diminution of "identical re-offense" comparing to "different re-offense".

I test these hypotheses using criminal records registered by the French Ministry of Justice. For all people convicted between January 2006 and December 2007, I obtain the re-offend behavior (similar, different, no new crime) in the year after the trial.

The identification strategy relies on the comparison between the instantaneous probability of committing a new crime different from the previous one and the instantaneous probability of committing a new crime identical to the previous one (two hazard rates). If the law has been perfectly understood, the latter should decrease in comparison to the former after August 2007. If not, the difference between the two probabilities should evolve later or never. This strategy is implemented using competitive duration models and timing of events. I measure the differences of the two hazard rates at different times: before the law was passed, just after August 2007 and in the medium term. Hazard rates are modeled separately and compared by bootstrap.

The identification mainly relies on differences between the hazard rates for two different behaviors. The values of the point estimates are more difficult to interpret per se. They measure shifts in probabilities. This could be due to the law but also to many other factors such as economic conditions or police force efficiency. For example, a decrease in the two instantaneous probabilities of committing a new crime could come from the law's deterrent effect or an improvement in economic conditions. To have a more straightforward interpretation of the point estimates I control for a large range of covariates. The most important among them is the number of first offenders arrested within a month and the county. Adding this control variable allowed for the measurement of the shifts in hazard rates after August 2007 given the number of first offenders arrested at each time in each county. As the law does not target first offenders, this control captures, at least partly, the effects of both economic conditions and police force activities.

The results show that the law has had a significant deterrent effect of the same magnitude on both behaviors during the months following enforcement. In the short term, there is no difference between identical and different re-offense behaviors. In the medium term, the latter is no longer deterred, with only the former remaining affected. These results provide evidence of a learning process. The law should not have been fully understood at the very beginning. After some time, however, criminals might have adjusted their behavior to the precise enforcement of the law. If un-enforced increase in sentence time does not have reverse side effects as in Bushway and Owens (Bushway and Owens, 2013) it does not mislead people for a long time.

This interpretation is consistent with results upon sub-groups. First, the difference between the two hazard rates is bigger for older criminals. Older offenders are more experienced, and they probably understand the design of the law more quickly. Second, the difference increases faster for robbery than for violence. Theft is generally viewed as one of the most rational crimes, which is consistent with the

⁵ The two most important trade unions ("Syndicat de la Magistrature", "Union Syndicale des Magistrats") publicly expressed their opposition to the law.

fact that offenders adapt their behavior precisely to enforcement. Lastly, counties with crime rates that are higher than average present greater differences between identical and different new offenses. This is consistent with the fact that knowledge on sentence evolution and design of criminal policies are more available when crime rates are higher.

The French context is described in the first section. Section 2 exposes the empirical strategy, and Section 3 covers the data used. Section 4 presents the main results and robustness checks. Section 5 disentangles this effect among sub-groups. Section 6 concludes.

1. French context

1.1. Criminal system before the law

The French criminal system is divided into three parts. We distinguish "contraventions", minor offenses for which prison sentences could not be given (small violence, driving beyond speed limits...), "délits" (misdemeanors), for which the maximal prison terms are shorter than ten years (burglary, drug related offences...), and "crimes" (felonies), the gravest offenses (murder, rape...) for which maximum prison terms are from ten years to life. Criminal procedures differ as follows: "contraventions" are judged by one professional magistrate, "délits" are judged by three professional magistrates, criminal courts (for felonies) are made up of three professional magistrates and nine jury members (six since 2011).

Maximum sentence duration (and minimum) defined in the Penal Code brings together probation ("sursis avec mise à l'épreuve": some obligations are defined; in the case of transgression the convicted person goes to jail), suspended prison ("prison avec sursis" without any obligation or prison but an enhanced prison sentence in case of a new conviction) and actual prison ("prison ferme"). For the purpose of clarity, we will use the English notion of probation, suspended prison and prison in this paper.

Specific dispositions are applied to recidivists. Most maximum prison terms are doubled in the case of recidivism. Recidivism ("récidive légale") is a restrictive notion in the French Penal Code. It applies to all people who are convicted for identical or "related" infractions in a five-year period after a trial⁶. "Related" infractions include all robbery, all road infractions, all violence and all drug infractions⁷. Recidivism used to have no impact on trial or sentences before mandatory sentencing (maximum prison terms are almost never bounded). As a consequence, prosecutors did not always pay attention to recidivism unless instructions from the Ministry of Justice encouraged them to do so.

1.2. Law on mandatory sentencing

Mandatory sentencing bill were brought to the senate for public debate the 5th of July 5th 2007, shortly after the election of Nicolas Sarkozy at the head of the State. The emergency procedure imposed by the government led to a very quick parliamentary adoption and the law has been enforced since August 12th 2007⁸.

⁶ For the most severe offenses ("crimes") there is no limited time period.

⁷ Art. 132-8 and the following qualification rules evolved slightly over time.

⁸ The Senate voted the law on July 5th 2007 and the National Assembly on July 18th. The law was definitively adopted on July 26th 2007, promulgated on August 10th and implemented on August 12th.

The most important part of this law imposes minimum sentencing for re-offenders depending on the new crime's maximum term. The minimums introduced are: one year for a new crime punished by a term of 3 years, two years if the term is 5 years, three years if the term is 7 years, four years if the term is 10 years. The law allows judges to rule out minimum sentencing for extraordinary reasons but they have to motivate their choice in detail⁹.

Minimum sentencing targets only recidivists - defined earlier - and could be filled with probation, suspended prison or prison. The jurisdiction's president is supposed to inform offenders of the risk they take if they commit a new crime in the following five years. This is non mandatory information and the warning should only be given "if it is justified by circumstances or by the offender's personality".

The second part of the law is a technical modification of one sentence (about mandatory care for mentally ill) which was already possible before the law and does not concern an important number of sentences.

Because of the non-retroactivity of the penal law, minimum sentencing could only be applied on offenders who committed a crime after August 2007. People convicted in September for crime committed in July are thus not eligible.

1.3. Media coverage of the law

The law was the third bill promulgated after the election of a new National Assembly in 2007. Since the two previous ones were technical texts (modification of the budget and some international ratifications), the law was the very first political bill passed under Nicolas Sarkozy. Because of its earliness, its political significance and its content, the law received considerable coverage by the French media.

The law has become well known in a very short time. Figure 1 presents the standardized occurrence of Google searches of the words "peine plancher". The level goes from 0 before August 2007 to a peak in September followed by a plateau. Even if the law was mentioned during the electoral campaign, it seems largely unknown before August and unanticipated.

However, political and media presentations were quite distorted. Whereas the "violation" of judges' independence and the level of the thresholds received significant commentary, the precise scope of the law was mainly ignored. The difference between recidivists in the common sense (offenders who commit two crimes) and recidivists in the legal sense (offenders who commit the same crime twice) was rarely explained even though it was far from negligible. In 2006, 39.7% of offenders convicted by criminal courts were recidivists in the common sense but only 7.6% were recidivists in the legal sense.

Table 1 illustrates the gap between the number of articles about the law and precise information about its scope. It represents how many articles in newspapers or TV-reports talked about the law between June and September 2007 and how many of them presented targeted infractions. I focus on the two 8PM news bulletins of TF1 and France 2, which have average respective audiences of 8 million (TF1) and 5 million (France 2) viewers per day (for 60 million inhabitants in France). *Le monde* and *Le Parisien/Aujourd'hui en France* used to record 534,000 and 359,000 readers, respectively. *Le monde* is viewed as a reference newspaper in France. Articles are long (twice as long as articles published in *Le Parisien/ Aujourd'hui en France* in the sample used here) and give detailed analysis. *Le*

⁹ Minimum sentences are also defined for most severe offenses but they still fall under the same sanction rules as before the law and, as those cases are judged slowly, exclusion rule is easy to use by the court.

Parisien/Aujourd'hui en France is viewed as a popular newspaper. Long analyses are less frequent than in *Le Monde*, and the law was usually mentioned in articles related to criminal facts or trial.

Only a small proportion (between 4% and 17%) of the information on mandatory sentencing contains the difference between the common and legal meaning of recidivism. The Minister of Justice, Rachida Dati, was interviewed in the four media mentioned above but never explained such a difference. Even the expression "récidive légale" used in legal publications is not mentioned. The grounds for the law did not explain the difference either. Lastly, the two Wikipedia articles about the law¹⁰ did not explain the legal definition of recidivism.

From the government's point of view, this low precision could be a strategic omission. To maximize the deterrent effect, it is reasonable to let people believe that the law applies to all re-offenders.

1.4. Effect of the law on sentences

Figure 2 presents the evolution of average sentences for crimes committed between 2006 and 2008 and judged in less than two years (the data on criminal records used here are presented below¹¹). Quanta are defined as the sum of prison, probation and suspended prison sentences, the three types of sanction that could fill the mandatory sentence. Three groups are disentangled: first offenders, non-recidivist repeat offenders, and recidivists. Even if legal recidivism is recorded in the database, I will only use a reconstructed measure. Indeed the administrative variable suffers from both legislative evolution of the definition and possible heterogeneous political pressure on prosecutors.

There is a clear and important increase in sentences for recidivists. The two other groups are largely unaffected. Only crimes committed after the implementation of the law are affected; otherwise, the increase would have started before August 2007. Non-retroactivity of the penal law did not prevent judges from applying similar quanta to offenders judged after the law even if their crime was committed before the law was passed. The law defined minima that were already feasible and judges could have homogenized sentencing after August 2007. Figure 2 shows that they did not do so and only increased sentences for targeted criminals.

Figures 3, 4 and 5 present the evolutions of average prison, probation and suspended prison for the same group over the same period. The increase observed in figure 2 is due to an increase in prison (one-third) and probation (two-thirds) but not in suspended prison sentences.

These results are confirmed and quantified by simple difference-in-difference regressions. The outcome variable is sentence duration, and the explanatory variable is a dummy equal to one after August 2007 (*law*), two group dummies for re-offenders (*reoffender*) and recidivists (*recidivist*), and interaction terms.

$$Sentence_i = \beta_0 + \beta_1 * law_i + \beta_2 * law_i * Reoffender_i + \beta_3 * law_i * Recidivist_i + \beta_4 * Reoffender_i + \beta_5 * Recidivist_i + \beta_6 * X_i + \varepsilon_i$$

The results are presented in table 2. They are consistent with the previous graph. There is an overall increase in sentences of 42.5 days for recidivists. This evolution is based on prison and probation

¹⁰ Articles on "peine plancher" and "Loi du 10 août 2007 renforçant la lutte contre la récidive".

¹¹ I keep the delay under two years in order to homogenize the sample. A short lag between facts and judgment allow a longer observation period to be kept. The results are sharper if the database only contains facts judged in less than 15 days (the French accelerate procedure).

(+15.2 and +25.4 days, respectively). Suspended prison remains broadly constant for all categories over the period. If the increase is extremely clear, the amplitude is modest compared to other studies (Drago et al 2009 used an increase of several months while papers on the three strikes law focused on life imprisonment).

The impact differs among crimes. Robbery and drug-related crimes are more affected than violence.

2. Identification strategy

2.1. Principle of identification

The identification strategy relies on the comparison between the instantaneous probability of committing a new crime identical to the previous one and the instantaneous probability of committing a new crime different from the previous one. It implies a focus on people who have already been convicted at least once; otherwise, there is no "new" conviction. If people have been arrested but not yet convicted, they could neither be reoffenders nor recidivists. It is then reasonable to take trials (and not facts) as "reference points" and to follow the probabilities of committing new crimes after this first legal event (and not after an arrest). The first trial will be called the "reference trial" in the following section. Other trials could occur in case of re-offense, but the "reference trial" is the one after which probabilities of committing new crimes will be computed. This "reference trial" is not necessarily the first one in a criminal's life.

I homogenize the population by restricting the sample to "reference trial," punishing crimes committed *before the law was passed*. This first event could not have been affected by the law. Moreover, I only keep "reference trial" with sentences other than jail (probation, fines, suspended prison...). The reason is that instantaneous probability of committing a new crime is roughly zero when people are incarcerated¹².

Criminals are followed for one year after their "reference trial". This period could spread before, after or partly before and partly after the enforcement of the law. In addition to this before-and-after differentiation, I differentiate between short-term and medium-term periods after the law. This design is represented in graph 6. The basic idea is to compare the two instantaneous probabilities, before the law (before the red bar), in the short-term period after the law (between the red and the green bar) and in the medium term (after the green bar). If the law is fully understood, the difference between identical and different re-offenses should be higher after the law than before (before the red bar). If the law is not really understood, there should be no difference over the three periods. If there is any learning effect, the difference should increase over time.

2.2. Measuring re-offense probabilities

Instantaneous probabilities of committing new crimes are measured using duration models and timing of events (see Abbring Van den Berg 2003). Duration models give the opportunity to use the individual dimension of the dataset. An important set of control variables could be added. This class of model also permits defining, for each observation, the precise part of the spell affected by the law.

I measure instantaneous probabilities using the standard mixed proportional hazard of the form:

¹² The database does not contain precise information about the enforcement of decisions, making it impossible to compute instantaneous probabilities after prison since the date of exit is not precisely known.

$$h_i(t) = h_0 * \exp^{\gamma * X_i} \quad (2)$$

where $h_i(t)$ is this instantaneous probability that person i commits a crime at time t , h_0 is the baseline hazard rate, X_i are the variables of interest and γ the vector of the effect of those variables on the hazard rate. This type of model is estimated using maximum likelihood. Individual i 's contribution to the likelihood is of the form:

$$L_i = h_i(t) * S_i(t) = h_i(t) * \exp(-\int_0^t h_u du) \quad (3)$$

where $S_i(t)$ is the survival rate and t is the time of failure. If individual i does not commit a new crime in the following year, the observation is censored and the contribution to the likelihood is reduced to the exponential (without $h_i(t)$) and t is the end of the observation period, one year in this work.

In the paper, I want to compare the effect of the law on different probabilities. This event, fixed in calendar time, occurs at a different time in people's lives depending on their "reference trial". Those convicted in November 2006 will face the risk of higher sentences after nine or ten months, while those convicted in March 2007 will face the same risk after only five or six months. Duration models enable this aspect to be addressed using time-dependent variables. The effect of the law will be captured by a dummy variable taking the value one after the enforcement of the reform in August 2007. The timing of shift from zero to one will depend on people's "reference trial".

People judged during the same month faced both specific characteristics - people convicted in January could be different from those convicted in August, and specific times before and after the law. The first aspect is captured by cohort-fixed effects. The model is still identified as cohort-fixed effects will affect all the hazard rates, while the time dependent variable will only affect part of it.

The models will then be of the form:

$$h_i(t, X_i) = h_0 * \exp(\alpha * law + \sum_{k=2}^{19} \gamma_k * Cohort_k + \beta * X_i) \quad (4)$$

where law is a time-dependent dummy equal to 1 if t is after the law, $Cohort_k$ are dummies equal to one if observation i belongs to cohort k , and X_i are states (socio-demographic characteristics, geographical dummies, etc).

As mentioned in section 2.1., I focused on two shifts in probabilities: the one occurring shortly after the implementation of the law and the other occurring in the medium term. The short term will usually be defined as the first four months after the reform. I take this into account by using two different time-dependent variables in the model:

$$h_i(t, X_i) = h_0 * \exp(\alpha_1 * short.law + \alpha_2 * medium.law + \sum_{k=2}^{19} \gamma_k * Cohort_k + \beta * X_i) \quad (5)$$

$short.law$ takes the value 0 before the law and in the medium term and the value 1 during the first months after the law. $medium.law$ takes the value 1 only in the medium term.

In the rest of the paper, the standard model uses a piecewise-constant baseline hazard rate. This functional form is more flexible than the fully parametric form (like Weibull) and enables controlling for unobserved heterogeneity (contrary to Cox). To avoid convergence problems, I limit the number of constants to four, one per 3-month period of the spell. This means that the baseline hazard rate will be constant for day 1 to 90 days after the trial, 91 to 180 days, 181 to 270 days and 271 to 360 days.

Duration models also give the opportunity to control for unobserved heterogeneity which otherwise, could bias estimates. This comes from the fact that those who share some characteristics will leave

faster (or slower) than the rest. Some unobserved variables will become more frequent as the time from trial increases. The time dependence will present a decreasing hazard rate where there is only dynamic selection¹³.

Different methods enable unobserved heterogeneity to be addressed. The standard one used here consists in adding a multiplicative parametric function following a Gamma distribution to the hazard rate.

2.3. Comparing re-offense probabilities for similar and different new offenses

The identification strategy relies on comparing two different re-offense probabilities. This is performed using competing risk analysis (Van den Berg 2005), which enables different types of "failure" in duration model to be distinguished.

Two types of new offenses are possible: identical reiteration, which could induce the mandatory sentencing, and different reiteration, which could not. The two risks are defined by the Penal Code and they are different for each individual. For a drug dealer, identical risk is the probability of committing a drug related infraction (consumption or deal) while different risk is the probability of committing a robbery, road related offense, etc. For a robber, a drug-related infraction is not an identical reiteration but a different one.

What leads to mandatory sentencing is clearly defined in the Penal Code, which makes it possible to define, depending on the offense punished during the "reference trial", whether each reiteration is an identical offense or a different one.

The simplest way to compute the effect of the law on the two risks is to consider one risk as censoring the other. Models are defined separately. They both have the form described in equation 5, i.e.,

$$h_{id}(t, X_i) = h_0 * \exp(\alpha_1^{id} * short.law + \alpha_2^{id} * medium.law + \beta_{id} * X_i) \quad (6)$$

$$h_{diff}(t, X_i) = h_0 * \exp(\alpha_1^{diff} * short.law + \alpha_2^{diff} * medium.law + \beta_{diff} * X_i) \quad (7)$$

The two models are computed using maximum likelihood estimation.

For example, person i was convicted for drug dealing during the "reference trial" and committed violence 150 days later. This offense is then a different offense and the criminal is a non-recidivist repeat offenders. When evaluating the probability of committing a crime different from the previous one, person i 's contribution to the likelihood is similar to equation 3: hazard rate times survival rate. However, the contribution to the likelihood for identical crime estimation is different. Person i never commits a new crime identical to the previous one but cannot be observed after 150 days. It is then equivalent to a censorship after 150 days. The contribution to the likelihood is equal to the survival rate for 0 to 150.

To measure a strategic deterrent effect, it is not sufficient to measure the effect of the law on both identical and different new offenses. The goal is to compare it. The two estimations are performed separately and I compare the effects by bootstrap. Separate estimations are conducted and the differences $\alpha_1^{id} - \alpha_1^{diff}$ and $\alpha_2^{id} - \alpha_2^{diff}$ are computed. I then test the following hypothesis:

$$\alpha_1^{id} = \alpha_1^{diff}$$

and

¹³ For an overview of the duration model see Van den Berg (2001).

$$\alpha_2^{id} = \alpha_2^{diff}$$

Separate estimations are conducted on 100 resamples of the dataset. The low number of iterations is explained by calculation time. For models with many control variables, the procedure could take up to a week for 100 iterations.

2.4. Interpreting raw estimates

Even if the goal of this paper is to document the understanding of the penal law and that the identification relies on the comparison between two different risks, being able to interpret the coefficient of the duration models per se is useful.

Before having a differential effect on similar and different re-offense behaviors, the law aims to deter crime. In equations 6 and 7, coefficients α measure the shifts in instantaneous probabilities of committing certain types of crimes in the short and medium term after the law. However, those parameters only capture a before/after transition. Point estimates measure the effect of the law but also the evolution of exogenous conditions - police force efficiency, economic conditions, etc. Those exogenous conditions affect both behaviors in a similar way, and the differences between them inform on the effect of the law. However, contrary to the differences, the point estimates themselves could not be directly interpreted as the deterrent effects of the law.

Moreover, the law could affect the environment in addition to changing a criminal's incentives. Two groups could particularly be affected: policemen and prosecutors. Policemen and prosecutors efforts might change if they see their work as more effective when offenders get higher sentences¹⁴.

The direct targeting of repeat offenders by the police is unlikely as it is difficult to know an individual's criminal status during an arrest. Targeting is possible for long and complex cases where the background is checked before. This is generally not the case for road-related offences, drug consumption or violent crime. Moreover, police databases are not equivalent to the justice dataset used here and do not contain judicial outcomes¹⁵. However, it is possible that the police increases its general effort or targets specific kinds of infractions where re-offenders are more numerous.

I use the number of first offender arrests per month and place it as a control for exogenous evolutions¹⁶. The offence probability for somebody without a criminal history should be orthogonal to the law, as sentences are not affected. If the police increases its effort after the law or if there are more legal opportunities, first offenders will be impacted.

This control introduces a bias if there is some anticipation effect so that first offenders are deterred by the law¹⁷ (see Shepherd 2002). This means that using first offenders as a control group gives a lower bound of the deterrence effect of the law. The results will be presented with and without this control.

¹⁴ It is also possible that political power put higher pressure on their activity not necessarily because of the law but simultaneously as a global tough-on-crime policy. We did not find such evidence in memoranda or decrees. This probably comes from the fact that political pressure on those agents had already been high since 2002.

¹⁵ Police databases record every "contact with the police". They include, for example, people who were only witnesses in a case. Conversely, for an arrest, the police database does not contain judicial outcomes. Anecdotal evidence stresses that an important part of "first offenders" are "first" for justice but were already known by the police force – and recorded in police databases. Thus, it is probably difficult for policemen to know whether a previous arrest has led to any judgment.

¹⁶ First offenders could not be used as a control group because their hazard rates could not be computed: they are not in the database before their recording. They are then aggregated by month and place and used as a control variable.

¹⁷ The number of first offenders per month in 2006 and 2007 decreased in 2007 after the vote of the law. This is in line with an increase in police activity or a deterrent effect due to anticipation.

A prosecutor's activity could also evolve and specifically target recidivists. To control for this potential procedural evolution, controls for the percentage of treated cases (treated cases over treated cases plus closed file) and the percentage of proceedings (proceedings over proceedings plus alternatives) per trimester and location (most disaggregate data available) are added.

The extensive models have the following form:

$$h_{id}(t, X_i) = h_0 * \exp(\alpha_1^{id} * short.law + \alpha_2^{id} * medium.law + \beta_{id} * X_i + \delta_{id} * Z_i(t)) \quad (8)$$

$$h_{diff}(t, X_i) = h_0 * \exp(\alpha_1^{diff} * short.law + \alpha_2^{diff} * medium.law + \beta_{diff} * X_i + \delta_{diff} * Z_i(t)) \quad (9)$$

where $Z_i(t)$ are time varying covariates (number of first offender arrests in the department, prosecutors' activity).

3. Data description

3.1. Origins of the dataset

I use criminal record compilations from the statistics service of the French Ministry of Justice (Sous Direction de la Statistique et des Etudes). These contain the results and details of almost all criminal cases judged each year. Only the smallest infractions that are not included in individual criminal records are not registered. They contain all "délits" and "crimes" (defined in part 1.1.).

Each individual is identified by a single ID, which is constant through the panel. This enables the penal history of an individual to be reconstructed. Precise facts and sentence descriptions are available. The dates of both infraction and trial are included but, unfortunately, there is no information on sanction enforcement. Prison terms in particular could be delayed or fully converted into probation. Socio-demographic variables are included such as age, sex, and nationality.

The years from the mid-1990s to 2010 are available. The unit of observation is the penal case, thus the dataset contains trials of the year and not infractions (an infraction committed during year x is recorded in dataset $x+1$ if it is judged into a period of a year).

3.2. Data used

The dataset used in the following analysis contains trials from January 2006 to December 2007. Minor offenses ("contraventions") and the most severe ones ("crimes") are excluded because they are not targeted by the law (or are poorly enforced in the case of the most severe offenses).

As previously mentioned (and illustrated in Figure 6), infractions judged during the "reference trial" should be performed *before* the implementation of the law in order to be unaffected by the reform. It would have been possible to keep only trials before August 2007 to fulfill this condition. However, it is also necessary to have "reference trials" in late 2007 in order to properly estimate the medium-term effect¹⁸. These two conditions are filled by using all trials for offenses committed at least five months

¹⁸ The medium-term effect is generally defined as the effect of the law after December 10, 2007. If all reference trials occurred before August 2007, the medium-term effect would be measured on those who did not commit a new crime during the first four months. Because of dynamic selection, this sample could be biased and, moreover, even with unobserved heterogeneity, the effect would be biased downward as the first months after

before. The database is then composed of trials occurring between January 2006 and December 2007 for crimes occurring before August 2007.

The empirical strategy is based on the re-offense dynamic. Committing crimes is largely impossible when incarcerated. As the dataset does not contain a precise date of entrance and release from prison, it is not possible to build the "at risk" period for people sentenced to prison during their "reference trial". This group is excluded from the dataset.

For each observation, individual criminal records in the five years preceding the "reference trial" are checked and variables measuring previous criminal behavior are constructed.

New cases occurring in the next period are computed. All offenses committed within the following year and judged within the following three years are recorded. I calculated the number of days between the "reference trial" and the date of the new offense. As the vast majority of offenses are judged in less than two years, this methodology avoids an important and artificial drop in re-offense speed when we go further from the trial.

Descriptive statistics are presented in table 3. Columns (1), (2), (3), respectively, present descriptive statistics for 2006, 2007 and the whole sample. Column (4) presents the standard errors.

There are 497,086 trials in the main database used in the paper. They are divided into five important infractions: road-related offenses, the most numerous (38% of trials); robberies (17%); violence and sexual assaults (12%); drug-related infractions (6%) and verbal assaults (mainly against police, 4%). Overall, 88% of offenders are male. The average age is 31 years (median 28), and 89% are French. Numerous offenders have at least one previous conviction: 31% are re-offenders (defined as those who were already sentenced in the five years before their offense). Overall, 18% are re-arrested in the year following the "reference trial", 7.7% for identical new offenses and 10.2% for different new offenses. Those proportions decrease a little in 2007. The identical re-offense proportion is the proportion that decreases the most.

4. Short-term general deterrence and medium-term strategic deterrence

4.1. Main results

The main results for the bootstrap of the duration models developed in equations 8 and 9 are presented in table 4. They presents both the short-term effect and medium-term effect of the law for identical new offenses (columns (1), (3), (5) and (7)) and different new offenses (columns (2), (4), (6) and (8)). As discussed in section 2.2., "short term" is defined as the first four months after the law. Models are estimated using a pairwise constant baseline hazard rate. Unobserved heterogeneity is added.

Estimations are presented in pairs with different sets of control variables: no control (columns (1) and (2)), individual controls (columns (3), (4)), individual controls plus cohort and geographical fixed effects (columns (5), (6)) and the full set of controls including first offenders number variable and prosecutor variable (columns (7), (8)).

trials are the most crucial for recidivism. The second reason in favor of a medium-term effect defined as the time after four months is the correction of the data. When an infraction's precise date is ignored, the date is set as January 1. Even if an exceptionally high number of infractions are committed on this day (because of "traditional" opposition between youth and police forces) part of the crimes set on this date are committed later in the year. If January 1, 2008 is included in the short-term period, it will bias the estimates because it will include crimes that are committed later.

Short-term effects are always negative and significant. The hazard rate decreases as control variables are added. It goes from - 8.6% to -5.7% for "identical risk" and from -8.3% to - 2.7% for "different risks". Those point estimates are coherent with a deterrent effect of the law. The instantaneous probability of committing a new crime of any type is smaller just after the law – i.e., in the short term. The effect decreases with the number of control variables and especially in the last two columns. This is coherent with the fact that models control for the number of "first offender" within month and county. As mentioned in section 2.4., this control could bias estimates downward if first offenders anticipate the effect of the law. Point estimates in columns (5) and (6) are then the lower bounds of the deterrent effect.

The differences between the two effects are presented in the line "difference short term". They are not significant even if the effect on identical reiteration is always bigger. Thus, the law has an indistinguishable (deterrent) effect on the probability of committing a new crime identical or different from the previous one even if the second one is not targeted.

The medium-term effect differs largely between identical and different risks. It is always negative and is significant in three over four models for identical new offenses going from -5.1% to -4%. The effect for the risk of committing a different crime is always positive and significant in three out of four models. This could be interpreted as a persistence of the deterrent effect of the law on identical new offenses but an extinction of the effect on different new offenses or even a displacement effect.

The differences between the two effects are presented in the line "difference medium term". They are large and significant moving from 6.7% to 10.1%. In the medium term, the law results in a difference between the risk of identical new offense and the risk of different new offense. The first one decreases in comparison to the second one.

The combination of an indistinguishable deterrent effect in the short term and a deterrence specifically observed on targeted behaviors in the medium term could be interpreted as evidence of a learning effect. Due to the presentation, biased in a sense of more universality (see section 1.3.), every new offense has been deterred shortly after August 2007: would-be offenders were not able to anticipate the scope of the reform. In the medium term, when information spreads, people started to discriminate between identical and different new offenses.

4.2. Robustness checks

I test the robustness of the results in several ways. The first check concerns the robustness of the specification. Table 5 presents the analysis of table 4 with Cox models (columns (1) to (4)) and pairwise constant models without unobserved heterogeneity (columns (5) to (8)). The results are unchanged.

The second check is a placebo. Columns (1) and (2) of table 6 present the results with a placebo law in August 2006. Point estimates are not significant and differences between identical and different re-offense behavior are null. There is no evidence of a deterrence effect and no shift in the differences between the two hazard rates.

The third check concerns the definition of the medium-term period. Columns (3) and (4) of table 6 present the results with a medium-term effect defined as the effect more than three months after the law. The results are similar to columns (3) and (4) of table 4 (the one with similar control variables). The short-term effects are even more similar and the difference between the two hazard rates in the medium term is smaller. Those characteristics are coherent with a learning effect inducing a higher difference when we go further from the law. This pattern is confirmed with medium term defined as the effect after one or two months (not shown).

The fourth check addresses the correction made on January 1. In columns (5) and (6) of table 6, new offenses committed on January 1 are considered as truncated data. The point estimates for the medium-term effect is becoming more negative, which is coherent with the fact that part of the new crimes are removed. However, the differences remain similar: not significant in the short term and significant in the medium term.

The last check addresses the definitions of "similar" and then "different" re-offenses. Table 7 presents the results with two different definitions. Columns (1) and (2) present the results using a larger definition of similar re-offense. Offenses are considered identical if they both belong to one of the following groups: theft, road related offenses, economic crimes, violence, drug, crimes linked to migrant status, falsification and verbal assault. Columns (3) and (4) present the results using a narrower definition of similar re-offense¹⁹. The definition is restricted to identical offenses (and not to related ones even if the relation is specified in the Penal Code). The results are similar to the results presented in table 4.

5. Differences among subgroups

5.1. *Thieves understand better than violent criminals*

The results presented in section 4 indicate that the reform introduced in August 2007 has had a short-term deterrent effect that is not specific to targeted reoffenders and a medium-term strategic deterrent effect, which has mainly affected identical re-offenses. This pattern is consistent with a learning effect: criminals would gradually understand the way the law is enforced. If this story is true, it should be possible to observe differences among subgroups depending on their capacity to understand the criminal law.

I first studied the differences among crimes. As mentioned in section 3, there are three important crimes in the database (each represents more than 10% of all crimes): road-related offenses, thefts and violence. Some of those crimes are generally considered more rational than others. Whereas theft could be a rational behavior - an alternative to a legal job - violence is generally considered more impulsive. Road-related offenses are in-between. If theft is more rational, thieves should be more reactive to legal incentives. On the contrary, changes in criminal law should have a limited impact on impulsive behavior.

I run the model described in equations 8 and 9 on the subgroup of road related offenders, thieves and violent criminals. The results are presented in table 7. Columns (1) and (2) present the results for road-related offenses, (3) and (4) for thefts and (5) and (6) for violent crimes. The results are similar to the general cases: point estimates are negative and significant for both identical and different re-offenses in the short term, negative for identical re-offenses and positive for different re-offenses in the medium term. However, the differences between the two behaviors are clearly heterogeneous. While identical re-offenses are always more deterred than different ones for thieves, the difference is never significant for violent criminals and only different in the medium term for road-related offenders.

Those results are in line with the hypothesis of a heterogeneous understanding of the law. The "rational criminals" - thieves - have a better understanding of the law and identical re-offenses are deterred more in the short term. On the contrary, more impulsive criminals - violent offenders - react

¹⁹ Models do not include any control variables because of convergence problems in the bootstrap procedure. The small number of identical re-offenses with the narrow definition is responsible for those convergence problems.

in a less strategic way. The gap between identical and different re-offenses increases with time but remains insignificant. Road-related offenders are in-between with a difference that becomes significant in the medium term.

5.2. Older criminals understand better than the younger ones

The second heterogeneity studied in this section is the effect of age. Older criminals have longer criminal careers than the younger ones. They should have more experience with criminal justice and probably have more connections in the criminal population than younger offenders. We can then expect that older criminals understand the law more quickly and better than younger ones.

This hypothesis is tested by running the models described in equations 8 and 9 on the group of "young" criminals (age lower than the average) and the group of "old" criminals (age higher than the average). The results are presented in table 8. Columns (1) and (2) present the results for young criminals, and columns (3) and (4) present the results for old criminals. They have similar form. Short-term effects on the behavior of each sub-group are negative, significant and indistinguishable. Medium-term effects are negative and significant for similar re-offenses and not significant for different re-offenses. The two differences are significant (see last row). However, as expected, old criminals seem to have a better understanding of the law in the medium term. The difference between the two deterrent effects is more than 50% greater for old criminals than for young criminals. The strategic behavior, significant for the two groups, is clearer for old criminals who have more experience.

5.3. Knowledge spreads faster when there are more criminals

If the impact of the law on the difference between identical and different re-offenses depends on people's understanding, it should be affected by the information available. If the law is applied frequently in someone's environment, information on the way it is enforced will be available quickly. On the contrary, isolated criminals have little information on the law and they will probably need more time to adapt their behavior.

The database does not contain information on people's criminal network. However, the crime rate could be used as a proxy for the probability of having information on law enforcement. In counties where the crime rate is high, would-be-offenders will probably have information on people who were sentenced to mandatory sentences on the one hand and those who avoided them on the other hand. They should adapt their behavior to the law more quickly than would-be-offenders living in counties where the crime rate is low.

I tested this hypothesis by running the models on two sub-samples composed of counties where the crime rate is above or below the national average. The results are presented in table 9. Columns (1) and (2) present the results for counties where the crime rate is above the average, and columns (3) and (4) present the results for counties in which the crime rate is below the average. The two structures of the point estimates are similar to those observed before: negative and significant coefficients in the short term, negative coefficients only for identical re-offenses in the medium term. However, the two subgroups diverge for the estimations of the differences between the two behaviors. If all differences are insignificant, those for counties where the crime rate is high are more negative than the differences within the counties where the crime rate is low. This is in line with the idea of a clearer understanding of the law in geographical areas where there are more criminals.

6. Conclusion

In this paper, I document the effect of a complex and largely oversold criminal reform. Using duration models and competitive risk analysis, I provide evidence that distorted presentation of an increase in sentence time could have a deterrent effect on non-targeted offenders. This result only holds in the short term. After some months, would-be offenders adjust their behavior and all "extra" deterrence vanishes.

This result is important for two major reasons. On the one hand, it documents how information spread among would-be-offenders. As criminal law could be very complex, understanding how far people understand it would help to design good incentives. On the other hand, it provides evidence that "tough on crime" speeches only affect offenders in the short term. As a consequence, such speeches cannot constitute an effective long-term policy. However, it could still be an efficient political strategy if voters react more to electoral promises than to implemented policies (Elinder et al, 2015).

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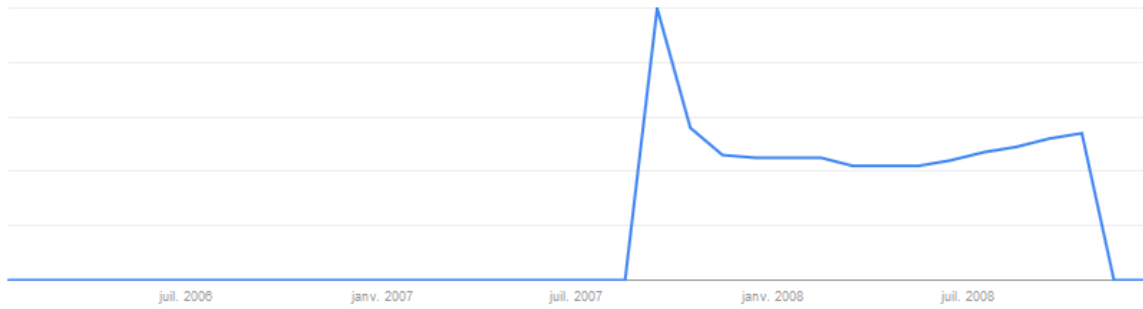


Figure 1: Evolution of the research of the words "peines plancher" in Google.

Measure obtained with Google Trend. The highest number of researches happening in September 2007.

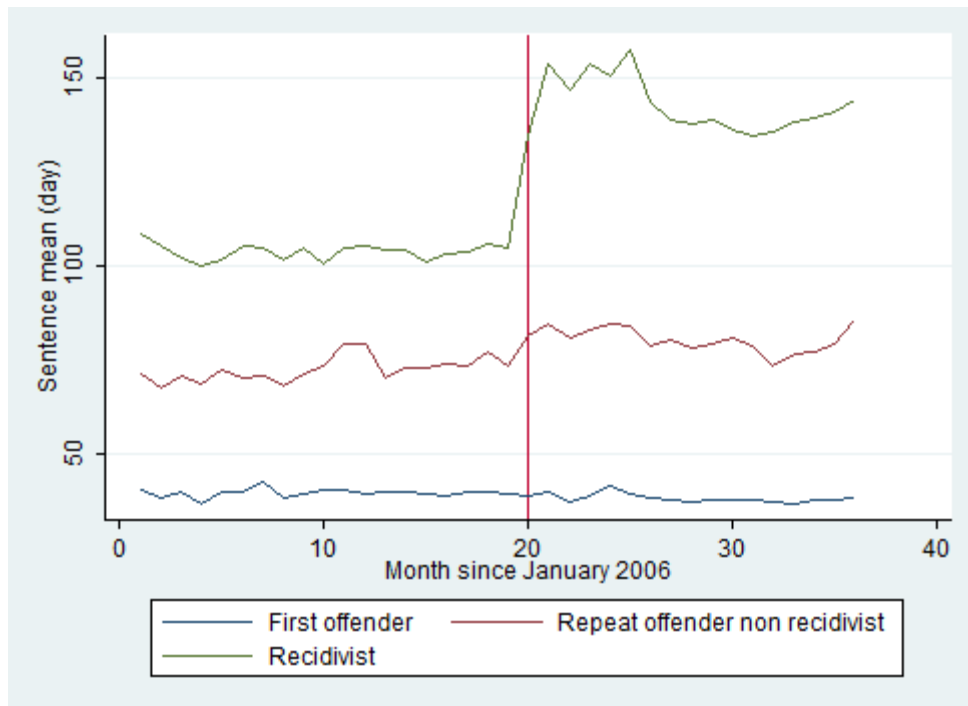


Figure 2: Sentence evolution between 2006 and 2008.

Sentences are the sum of prison, suspended prison and probation. Red line represent the vote of the law.

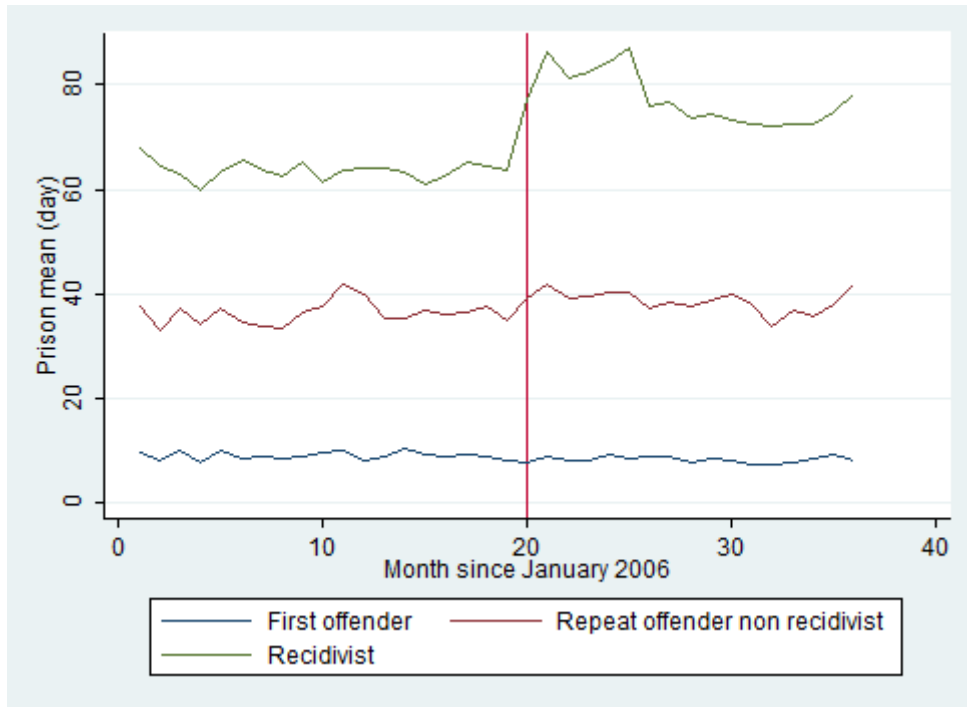


Figure 3: Prison evolution between 2006 and 2008.

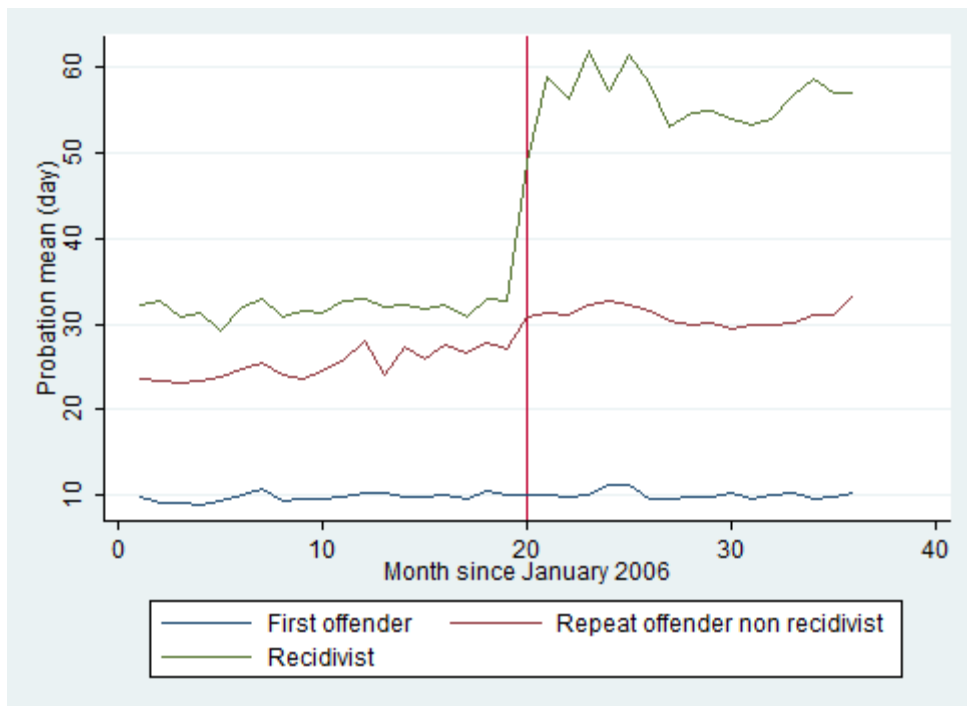


Figure 4: Probation evolution between 2006 and 2008.

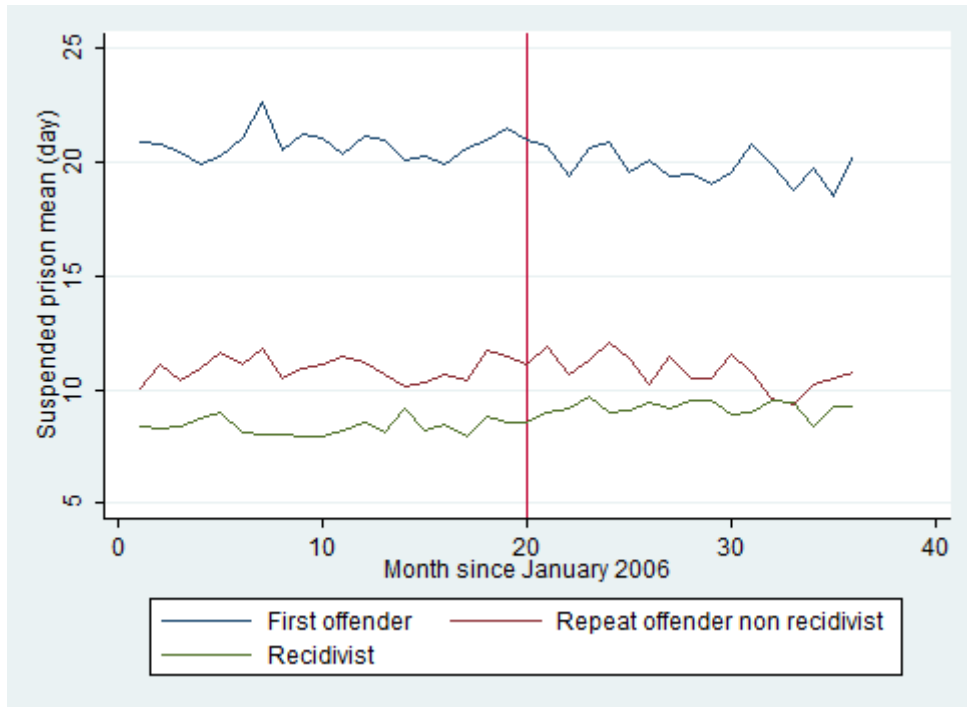


Figure 5: Suspended prison evolution between 2006 and 2008.

Media	Format	Audience	Report mentioning the law	Report explaining the legal definition of recidivism
TF1	TV	7 840 000	7	1
France 2	TV	4 140 000	6	1
Le parisien- Aujourd'hui en France	Newspaper	534 000	45	2
Le monde	Newspaper	359 000	37	2

Table 1: Media coverage of the law

	(1) Sentence	(2) Prison	(3) Probation	(4) Suspended prison
law	-2.276*** (0.335)	-1.326*** (0.251)	0.188 (0.165)	-1.138*** (0.100)
Re-offender non recidivist	32.94*** (0.471)	27.85*** (0.352)	16.04*** (0.232)	-10.95*** (0.141)
Recidivist	63.39*** (0.416)	54.73*** (0.311)	22.42*** (0.205)	-13.75*** (0.124)
Law*re-offender non recidivist	9.499*** (0.687)	2.934*** (0.514)	5.574*** (0.338)	0.991*** (0.205)
Law*recidivist	42.51*** (0.605)	15.15*** (0.453)	25.35*** (0.298)	2.004*** (0.181)
Socio-demographic controls	Yes	Yes	Yes	Yes
Geographical fix effects	Yes	Yes	Yes	Yes
Constant	85.50*** (1.561)	41.42*** (1.168)	13.18*** (0.769)	30.90*** (0.466)
N	1,581,503	1,581,503	1,581,503	1,581,503

Table 2: Effect of the law on sentences for crimes committed between 2006 and 2008 and judged within less than 2 years.

"Law" is a dummy equal to 1 if the second term offense is posterior to the enforcement of "peines planchers".

	2006 (1)	2007 (2)	Mean (3)	sd (4)
N	248 388	248 698	497 086	
Time between fact and trial	481.36	494.82	488.09	349.71
Sex	0.12	0.12	0.12	0.33
Age	31.63	31.79	31.71	13.09
French	0.89	0.89	0.89	0.32
Criminal record				
Reiteration	0.30	0.32	0.31	0.46
Recidivism (reconstructed)	0.14	0.15	0.15	0.35
Recidivism (legal)	0.03	0.04	0.04	0.18
Type of offense				
Theft	0.17	0.17	0.17	0.38
Road	0.38	0.38	0.38	0.49
Violence	0.10	0.10	0.10	0.30
Sexual	0.02	0.02	0.02	0.13
Drug	0.06	0.06	0.06	0.23
Verbal	0.04	0.04	0.04	0.19
Maximum sentence				
One year	0.30	0.29	0.30	0.46
Three years	0.42	0.43	0.42	0.49
Five years	0.17	0.17	0.17	0.38
Seven years	0.07	0.06	0.06	0.25
Ten years	0.04	0.04	0.04	0.20
Re-offence				
No re-offence	0.819	0.822	0.821	0.383
Identical reoffense	0.078	0.075	0.077	0.266
Different reoffense	0.103	0.102	0.102	0.303

Table 3: Descriptive statistics.

Hazard	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Identical	Different	Identical	Different	Identical	Different	Identical	Different
Short term	-0.085*** (0.018)	-0.083*** (0.013)	-0.086*** (0.017)	-0.078*** (0.013)	-0.084*** (0.021)	-0.057*** (0.017)	-0.057*** (0.01971)	-0.027* (0.01597)
Medium term	-0.045*** (0.015)	0.027** (0.013)	-0.051*** (0.014)	0.016 (0.016)	-0.044* (0.026)	0.058** (0.023)	-0.040 (0.02661)	0.062** (0.02401)
First offenders control							Yes	Yes
Prosecutor controls							Yes	Yes
Geographic fix effects					Yes	Yes	Yes	Yes
Cohort fix effects					Yes	Yes	Yes	Yes
Socio-demographic controls			Yes	Yes	Yes	Yes	Yes	Yes
Crime fix effects			Yes	Yes	Yes	Yes	Yes	Yes
Sentence controls			Yes	Yes	Yes	Yes	Yes	Yes
Criminal case controls			Yes	Yes	Yes	Yes	Yes	Yes
N	497 082	497 082	497 082	497 082	497 082	497 082	497 082	497 082
Difference short term	-0.002 (0.023)		-0.008 (0.022)		-0.027 (0.028)		-0.030 (0.025)	
Difference medium term	-0.072*** (0.022)		-0.067*** (0.020)		-0.102*** (0.037)		-0.101*** (0.035)	

Table 4: Strategic deterrence: pairwise constant duration model with unobserved heterogeneity (gamma).

The effects of the law on identical offenses and on different offenses are calculated separately. Differences are computed by bootstrap.

	Cox model				Parametric without unobserved heterogeneity			
	(1) Identical	(2) Different	(3) Identical	(4) Different	(5) Identical	(6) Different	(7) Identical	(8) Different
Hasard								
Short term	-0.075*** -0.015	-0.080*** -0.013	-0.081*** -0.015	-0.076*** -0.013	-0.078*** (0.017)	-0.084*** (0.014)	-0.083*** (0.017)	-0.078*** (0.014)
Medium term	-0.025* -0.014	0.026** 0.012	-0.036** -0.014	0.020* -0.012	-0.040*** (0.015)	0.015 (0.013)	-0.050*** (0.015)	0.012 (0.013)
Socio-demographic controls			Yes	Yes			Yes	Yes
Crime fix effects			Yes	Yes			Yes	Yes
Sentence controls			Yes	Yes			Yes	Yes
Criminal case controls			Yes	Yes			Yes	Yes
N	497 082	497 082	497 082	497 082	497 082	497 082	497 082	497 082
Difference short term	0.005 (0.018)		-0.005 (0.018)		0.006 (0.024)		-0.005 (0.024)	
Difference medium term	-0.051*** (0.018)		-0.056*** (0.018)		-0.055*** (0.020)		-0.062*** (0.020)	

Table 5: Robustness check: cox model and pairwise constant model without unobserved heterogeneity.

The effects of the law on identical offenses and on different offenses are calculated separately. Differences are computed by bootstrap.

Hazard	Placebo		Medium term after 3 months		Without january 1st	
	(1) Identical	(2) Different	(3) Identical	(4) Different	(5) Identical	(6) Different
Short term	0.006 0.015	0.001 0.013	-0.074*** (0.017)	-0.074*** (0.014)	-0.073*** (0.017)	-0.052*** (0.013)
Medium term	-0.002 0.014	0.002 0.011	-0.062*** (0.014)	-0.004 (0.012)	-0.079*** (0.014)	-0.039*** (0.012)
Socio-demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Crime fix effects	Yes	Yes	Yes	Yes	Yes	Yes
Sentence controls	Yes	Yes	Yes	Yes	Yes	Yes
Criminal case controls	Yes	Yes	Yes	Yes	Yes	Yes
N	472600	472600	497 082	497 082	497 082	497 082
Difference short term	-0.005 (0.019)		-0.000231 (0.021)		-0.021 (0.022)	
Difference medium term	-0.004 (0.018)		-0.058*** (0.018)		-0.04*** (0.020)	

Table 6: Robustness check: effect of a placebo law in August 2006 (column 1-2), effect of the law when medium term effect is define as the effect after 3 months (column 3-4), effect of the law if offenses committed on January 1st are considered as truncated observation (column 5-6).

The effects of the law on identical offenses and on different offenses are calculated separately. Differences are computed by bootstrap.

	Larger definition of identical reiteration		Narrower definition of identical reiteration	
	(1) Identical	(2) Different	(3) Identical	(4) Different
Hazard				
Short term	-0.085*** (0.016)	-0.079*** (0.014)	-0.099*** (0.022)	-0.082*** (0.012)
Medium term	-0.055*** (0.014)	0.019 (0.012)	-0.057*** (0.021)	0.012 0.013
Socio-demographic controls	Yes	Yes		
Crime fix effects	Yes	Yes		
Sentence controls	Yes	Yes		
Criminal case controls	Yes	Yes		
N	497 082	497 082	497 082	497 082
Difference short term	-0.006 (0.021)		-0.018 (0.025)	
Difference medium term	-0.074*** (0.017)		-0.068*** (0.025)	

Table 7: Robustness check: effect of the law with a larger (column 1-2) or narrower (column 3-4) definition of "identical reiteration".

The effects of the law on identical offenses and on different offenses are calculated separately. Differences are computed by bootstrap.

Hazard	Road		Theft		Violence	
	(1) Identical	(2) Different	(3) Identical	(4) Different	(5) Identical	(6) Different
Short term	-0,066***	-0,130***	-0,103***	-0,035	-0,095*	-0,082**
	-0,023	-0,029	-0,023	-0,022	-0,056	-0,029
Medium term	-0,057***	0,069**	-0,085***	0,047**	-0,016	0,028
	-0,019	-0,03	-0,029	-0,023	-0,05	-0,033
Socio-demographic controls						
Crime fix effects						
Sentence controls						
Criminal case controls						
N	188 960	188 960	120 307	120 307	96 331	96 331
Difference short term	0.064* (0.037)		-0.068** (0.02)		-0.013 (0.066)	
Difference medium term	-0.126*** (0.035)		-0.133*** (0.036)		-0.044 (0.061)	

Table 8: Heterogeneity: effect of the law by type of offense.

The effects of the law on identical offenses and on different offenses are calculated separately. Differences are computed by bootstrap.

Hazard	Age < 31.7		Age > 31.7	
	(1) Identical	(2) Different	(3) Identical	(4) Different
Short term	-0,101*** -0,019	-0,083*** -0,013	-0,054** -0,029	-0,059** -0,03
Medium term	-0,039** -0,017	0,018 -0,013	-0,069*** -0,026	-0,027 -0,023
Socio-demographic controls	Yes	Yes	Yes	Yes
Crime fix effects	Yes	Yes	Yes	Yes
Sentence controls	Yes	Yes	Yes	Yes
Criminal case controls	Yes	Yes	Yes	Yes
N	274 532	274 532	222 551	222 551
Difference short term	-0.018 (0.027)		0.005 (0.044)	
Difference medium term	-0.057** (0.0122)		-0.096*** (0.035)	

Table 9: Heterogeneity: effect of the law by age.

The effects of the law on identical offenses and on different offenses are calculated separately. Differences are computed by bootstrap.

Hazard	County where crime rate is higher than average		County where crime rate is lower than average	
	(1) Identical	(2) Different	(3) Identical	(4) Different
Short term	-0.105*** (0.031)	-0.054** (0.025)	-0.069** (0.030)	-0.082** (0.027)
Medium term	-0.067** (0.032)	-0.010 (0.027)	-0.043 (0.030)	0.003 (0.025)
Socio-demographic controls	Yes	Yes	Yes	Yes
Crime fix effects	Yes	Yes	Yes	Yes
Sentence controls	Yes	Yes	Yes	Yes
Criminal case controls	Yes	Yes	Yes	Yes
N	245 213	245 213	251 869	251 869
Difference short term	-0.052 (0.039)		0.013 (0.041)	
Difference medium term	-0.058 (0.041)		-0.046 (0.039)	

Table 10: Heterogeneity: effect of the law by crime rate in the county.

The effects of the law on identical offenses and on different offenses are calculated separately. Differences are computed by bootstrap.