Civil Unrest, Emergency Powers, and Opportunism: a Mixed Methods Analysis of the 2005 French Riots

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Abstract

From early to mid-November 2005, many French urban suburbs experienced riots. The government declared a state of emergency in those areas. It remained in place until January. I investigate whether the riots generated criminal spillovers, whether the emergency powers deterred criminal activity, and whether the police used those powers to bust crime unrelated to the riots. I supplement the regression approach with a non-parametric bounded variation framework and interviews. The riots triggered a surge of opportunistic thefts. The state of emergency did not deter crime; rather, the data suggest an emboldening effect. Evidence of opportunism by the police is scant.

JEL Classification: C14; D74; K42; H12

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1. Introduction

Urban riots are still prevalent in several Western countries. While the focus is on understanding the causes of those phenomena, and on finding ways to prevent them, less attention is devoted to their possible indirect effects on other forms of criminality. Further, states of emergency which may arise after the eruption of riots, or the perpetration of terrorist attacks (or threats thereof), involve massive amounts of security personnel and can be coupled with the use of otherwise impermissible police powers. Conceivably, increased police presence and powers may influence ordinary criminal activity; and symmetrically, the police may seize those powers to crack down on crime unrelated to rioting or terrorism.¹ At issue, here, is whether criminals and police forces behave opportunistically when the parameters of the environment in which they operate undergo some unexpected, drastic change.

The 2005 French riots provide an opportunity to explore these questions. In the first days of November 2005, riots broke out in many French urban areas, particularly in densely immigrant-populated city suburbs. The scale of the violence, measured by e.g., the destruction of property and arson, was such that soon after the French government declared a state of emergency in the affected areas. In most cases, specific zones within départements (henceforth counties) were targeted. Some counties were declared emergency areas in their entirety. The state of emergency extended police powers significantly. The riots had all but petered out by the beginning of the third week of November. However, Parliament approved an extension of the state of emergency, and out of caution, it lasted until early January.

For our purposes, this chronology of events begs a series of questions. First, does rioting influence other types of criminal behavior? Second, do enhanced police powers deter criminal behavior?
Third, do law enforcement forces display unusual productivity when endowed with such extra powers? Intuition alone does not help making qualitative predictions, since for each question at least two effects could work in opposite directions. Essentially, those are empirical questions dealing with the measurement of distinct treatment effects.

To that end, I first apply the classic linear regression model to monthly, county level crime data. That benchmark framework possesses the advantages of convenience, tractability, and its yields conclusions that are easy to interpret; yet, it relies on a number of strong assumptions. Additionally, in the present case, the effect I try to capture is substantially heterogeneous across counties.

To address this issue, I turn to the non-parametric methodology developed by Manski and Pepper (2017). It is well-known that the cost of a non-parametric approach is a high threshold for finding unambiguous effects. Bounds on treatment response are often uninformative in this regard, which accounts for their lack of popularity among social scientists and policy makers alike (Manski, 2013). However, this flexible approach is well suited to impute counterfactuals and draw inference in the context of a single extreme event. It uses a spectrum of assumptions of varying identifying power, making transparent the tradeoff between the strength of assumptions and the credibility of the results they yield. To wit, rather than making invariance assumptions asserting or implying that specified features of treatment response are constant across space or time it makes weaker assumptions positing bounded variations. A simple example of such weaker assumption is that within an area that experienced rioting, the difference in outcomes between the month of October and the counterfactual outcome for November 2005 is bounded by the minimum and maximum of the same difference observed between those two months over the years prior to the riots in that area. I follow and expand upon this reasoning to examine what happened in Seine-Saint-Denis
county, which experienced the highest level of rioting, using Gironde as the comparison group of choice given that it was the most populated urban county not to be affected by the state of emergency.

I complement this quantitative approach with qualitative insights derived from a couple of semi-structured interviews. I conducted the first one with Mr. Jacques Meric, head of the Seine Saint Denis police in 2005-2006, and currently overseeing community safety for the greater Paris area. I conducted the second one with Mr. Bernard Pasqualini, head of public safety for Seine Saint Denis during that same period, hence immediately under Mr. Meric’s authority, and now retired.³ To give a more concrete idea of their responsibility levels, Mr. Meric was having daily briefings with then Minister of the Interior Nicolas Sarkozy while Mr. Pasqualini was coordinating police operations on the ground. Their viewpoints thus necessarily overlap but also complement each other. Quantitative researchers often analyze events the main actors of which are still alive and capable of providing e.g., a contextualization, a confirmation, an alternative interpretation, or a disambiguation of empirical results. Indeed, Mr. Meric’s and Mr. Pasqualini’s firsthand institutional knowledge, direct involvement, and experience added significant value to the analysis of the raw data.

To summarize, the evidence confirms that the response was heterogeneous. Generally, the riots induced a substantial increase in opportunistic thefts, such as violent robbery in public. Career criminals did not seize upon the riots to commit more crimes requiring planning. Overall, the state of emergency did not deter crime. Quite the opposite, the number of drug related infractions dropped throughout the emergency period, suggesting a deliberate strategy of appeasement. Violent crimes increased substantially almost immediately after the riots had ended and persisted at higher than pre-riot levels for months. This is the first empirical demonstration of an
empowering effect among criminals following urban unrest. Conversely, evidence of opportunism by the police is scant. The narrative that emerges from the analysis of the data accords with Messrs. Meric’s and Pasqualini’s independent accounts.

The remainder of the article is structured as follows. Section 2 provides a brief review of the relevant literature. Section 3 provides a summary of the events. Section 4 describes the data. Section 5 introduces the research design and empirical methodology. Section 6 discusses the results. Section 7 concludes.

2. Literature Review

This paper expands on two strands of literature. First, it connects to the criminal decision-making process, criminal opportunism, and deterrence. An analogous research into criminal spillovers was performed in Gould and Stecklov (2009) in the context of terror attacks. The context in which this paper is situated relates to the incentives provided by a rioting environment. Previous empirical research on the economics of riots has focused on factors that can help explain the occurrence of rioting (e.g., DiPasquale and Glaeser (1998)) or on their longer-term consequences. (e.g., Collins and Margo (2004, 2007)). Within this literature, the present paper is close to Bell et al. (2014) which shows a significant drop in crimes across London in the six months after the 2011 riots, consistent with a deterrence effect from tougher sentencing on the rioters. Although it is not the focus of their article, beyond the expected pattern of property destruction and arson that characterized the riots, they report a significant increase in burglaries and incidents of violence against persons during the riots. As the authors point out, there is a dearth of empirical evidence on the more immediate response of crime to riots, and beyond, on the impact of riots on subsequent crime patterns.
Another segment of the literature that this paper relates to is the response of criminal activity to changes in policing. This scholarship is vast (Nagin, 2013) and has taken advantage of various original institutional and contextual changes that can be credibly construed as exogenous shocks affecting criminals’ environment. However, the estimated elasticities display a wide range, depending on the study and the type of crime. One recent summary is found in Chalfin and McCrary (2015):

> Overall, the literature has reached a consensus that increases in police manpower reduce crime, at least for a population-weighted average of U.S. cities. With respect to police deployments and tactics, the literature supports the idea that crime responds to a visible police presence in hot spots; supports the idea that a pulling levers strategy advertising deterrence reduces crime; and provides mixed evidence that proactive policing strategies such as broken windows and disorder policing reduce crime.

The present paper extends this line of research by considering a different type of qualitative shock on police powers, that is, the temporary availability of extrajudicial law enforcement tools, e.g., warrantless searches and seizures, administrative house arrests, and curfews, to name the most important aspects of the French emergency powers. The issue of police opportunism, i.e., conduct that may be described as technically lawful yet ethically questionable, is virtually unexplored.

Finally, from a methodological standpoint, this piece relates to work that strives to analyze treatment response within a non-parametric framework. Partial identification analysis of treatment effects from observational data was initiated in Manski (1990). The theory behind the bounded variations framework was first developed in Manski and Pepper’s (2000) study of monotone instrumental variable assumptions. The present paper builds most closely on Manski and Pepper’s (2013) analysis of the deterrent effect of the death penalty, and Manski and Pepper’s (2017)
assessment of the effect of right-to-carry gun laws on crimes, both of which applied some forms of the bounded-variation assumptions used here. Other applications to evaluate criminal-justice policy include Manski and Nagin (1998) and Siddique (2013).

3. The Riots and Immediate Aftermath

The general context is one of rapid degradation of the social climate in French immigrant populated suburbs, marked by an escalation of tensions with the police in the period of September and October 2005. On October 27, three French youths of Malian, Kurdish, and Tunisian descent sought refuge in an electrical substation as they fled the police in the Parisian suburban town of Clichy-sous-Bois (Seine Saint Denis county). They got electrocuted. Two of them died, the third one was severely injured. Local protesters took to the streets the next day. In the period of October 28th-31st, disturbances were circumscribed to Clichy-sous-Bois. However, on October 31st, a tear gas grenade that the police had sent to disband demonstrators landed in front of a mosque. A rumor spread that the grenade had exploded inside, triggering further anger, especially because this was still the Ramadan period. This moment corresponds to the turmoil becoming a nationwide phenomenon, the bulk of which taking place in impoverished, immigrant-dense suburban housing projects around major cities. Vandalism exploded. Most of the rioters were unemployed youths. The movement had no structure, no organization, no leader, and there were no claims associated with it. These events can be compared to a watered-down version of the 1968 “Holy Week Uprising” in the United States, or more recently, to the 2011 London riots.

Then Minister of the Interior Nicolas Sarkozy declared a “zero tolerance” policy towards urban violence and police reinforcement was dispatched to confront the rioters.
Chirac announced a state of emergency in those areas affected by rioting on November 8th, pursuant to a 1955 law that was passed in response to terrorist attacks that had plagued France in the midst of the Algerian War. In most cases, specific zones within counties, corresponding to urban areas, were targeted. Some counties were declared emergency areas in their entirety (see Appendix A). Notably, the whole of Île-de-France (Paris and its seven surrounding counties) was included, although Paris itself did not experience any significant unrest. According to Mr. Meric, the state of emergency was not motivated by an operational necessity but was rather driven by political considerations internal to the government. Mr. Pasqualini spoke of the state of emergency as a diversion tactic: such dramatization was intended to persuade public opinion that the government was addressing the crisis with full might.

The state of emergency can only be declared by cabinet for an initial period of twelve days, within a specified geographical perimeter. Any extension must be approved by Parliament. The state of emergency is an administrative regime allowing local authorities to impose curfews, house arrests, bar individuals from certain areas, conduct both daily and nightly warrantless searches and seizures, and ban public gatherings. Despite the announcement and police reinforcement, violence continued. Yet, by the end of the second week of November the riots were receding. The rioters had caused over €200 million in damage as they had torched nearly 9,000 cars and dozens of buildings, daycare centers, and schools. The monthly rate of arson per 100,000 population peaked at 86 in Seine Saint Denis. Next was Val d’Oise, a distant second with 58. Prior to November 2005, that rate had never reached 14 in Seine Saint Denis (See Graph 1), and 10 in Val d’Oise.10

[Insert Graph 1]
During those two weeks, the police arrested close to 3,000 rioters; 126 police and firefighters were injured, and there were three fatalities. The arrests led, in almost all cases, to suspended sentences or to sentences of at most a few months of imprisonment.

By November 14\textsuperscript{th}, cabinet decided an extension of the state of emergency, ostensibly out of precaution. On November 15\textsuperscript{th}, the National Assembly (lower House) approved a three-month extension by a large majority. So did the Senate on the next day. On November 20\textsuperscript{th}, Prime Minister Dominique de Villepin announced tightened controls on immigration, which was purported to be a determining factor in the rioting. Jacques Chirac lifted the state of emergency on January 4\textsuperscript{th}.\textsuperscript{11}

The use of emergency powers was abundantly criticized at the time among human rights advocates, legal scholars, and magistrates as disproportionate. Several petitions to the \textit{Conseil d'État}, the highest administrative court for proceedings involving the state, were filed against the government to suspend the implementation of the emergency powers, or, in the alternative, to enjoin the president to reconsider his position. All petitions were rejected.\textsuperscript{12} While the concern of these lawsuits was the undue restriction on fundamental freedoms, only anecdotal evidence of police abuse of the emergency powers surfaced in the news.\textsuperscript{13}

Although the underlying causes of the riots are of no direct concern to this paper I briefly present here the consensus view among those who have studied these events.\textsuperscript{14} Scholars have emphasized hostility towards the police and state institutions in general, as well as, more systematically, the marginalization of immigrant youth of African and North African descent, perceptions of social and economic exclusion and discrimination. Beyond that, they have questioned the capacity of the French Republic to respond to these challenges while maintaining its distinctive model of and formal commitment to assimilation and social integration irrespective of color or creed. More
concretely, Patrick Buisson, who was Nicolas Sarkozy’s political adviser, explains that the riot must be understood as a reaction against the chokehold being imposed on those suburbs’ underground economy: during the first nine months of 2005, Mr. Sarkozy had been derailing drug trafficking to build a “tough against crime” record for his upcoming presidential campaign.\textsuperscript{15} As for the police, according to Mr. Meric, there is a dearth of internal reports, analyses, and audits concerning how the events were handled, consistent with the authorities’ unease over the whole episode.\textsuperscript{16} The conclusions emerging from the present paper provide additional reasons for this.

4. Data

To evaluate the impact of rioting and emergency powers on criminal activity and policing, I use monthly, county level data on criminal cases. The data are overseen by the *Observatoire National de la Délinquance et des Réponses Pénales* (National Monitoring Center of Delinquency and Penal Responses, henceforth ONDRP). As its name suggests, the ONDRP, created in 2004, is a public and independent bureau in charge of the collection, use and dissemination of data on criminal offenders’ activities and on responses made to address those.

The ONDRP compiles evidence obtained by the police and gendarmerie units, that is offences and crimes brought to the attention of those agencies or uncovered by them. These data (commonly referred to as the “4001 state” in France) apply exclusively to those facts being processed by the public prosecution office, following either a complaint or a police investigation for the more serious cases. The data therefore exclude all contraventions (i.e., minor infractions punishable by, at most, a fine) and traffic offences, as well as complaints registered in police and gendarmerie log books (“main courante”) and not judicially processed. Records of law violations are tallied and assigned to 103 categories. The public access series starts in January 1996.
To compute rates per 100,000 inhabitants, I use yearly population statistics provided by the National Institute for Statistics and Economic Studies (INSEE), available at the county level. I directly study county-year-month crime rates: 100,000 times the ratio of the number of reported crimes to the county population. I do not consider data beyond 2014 because the state of emergency declared in November 2015 following the terrorist attacks changes the environment radically. Tables 1a and 1b present summary statistics for the subset of crimes analyzed in depth in this paper.

[Insert Table 1]

The ONDRP data base allows to get a get a reasonable picture of, firstly, the number of filed crime complaints (whether elucidated or not) and, secondly, to measure the activity of the police and gendarmerie units. It is well known, however, that police statistics in general provide only a rough estimate of crime (Vollaard and Hamed, 2011). The French statistics are not immune to this critique (Aubusson et al., 2003; leBouillonec and Quentin, 2013). Criminologists complement official records with victimization survey data. The offences typically found in victimization surveys are burglaries, car thefts, theft from motor vehicles, wreckage of motor vehicles, simple theft or violence and assault (including most of the minor incidents that would almost always not make it to a formal complaint recorded in police statistics, let alone a judicial procedure). 17

A key question regarding any crime data is to what extent they reflect the underlying criminal activity as opposed to the intensity of policing. According to the ONDRP disclaimer, with respect to a variety of offences for which there is no natural or legally constituted victims (offences related to narcotic drugs, labor code, immigration, environmental protection, pimping, etc.), the number of recorded cases in the database tracks the activity of the security forces and demonstrates the intensity of their efforts to identify infringements and apprehend the alleged perpetrators, as
opposed to changes in crime per se. Note that those categories of criminal activity would not appear in victimization surveys and the ONDRP data are therefore the only available source.

A specific concern a priori in the present context is that unsuccessful searches would not come up in the data. Therefore, if the police used emergency powers to conduct a significantly larger number of extrajudicial searches, and yet most of them turned unproductive, one could conclude erroneously that the data do not support a finding of opportunism. Finally, under-reporting (which would appear more plausible than over-reporting, if any\textsuperscript{18}) would imply a bias toward finding uninformative results.

5. Research Design and Methodological Framework

The timing and sequence of the events ensure the exogeneity of the treatment with respect to types of criminal behavior unrelated to rioting. The circumstances rule out the hypothesis that the riots were a decoy for a concerted looting and sacking enterprise – which would alter the interpretation of economic (or property) crimes as being opportunistic by-products of the riots. In fact, the analysis of the data can be even more conclusive than the sociological or historical approach to address this concern: one can check for evidence of an increase in those types of crime which necessitate a minimum of planning, such as burglary.

With respect to rioting spillovers, we have a combination of two effects, i.e., rioting and the virtually inevitable, immediate police response to it. To which is added the possible effect of emergency powers in the second half of the rioting period. The fact that the additional police powers were in force in December alleviates disentangling the impact of the two treatments. To
rule out catch-up effects, I also consider the combined November + December count as an alternative outcome of interest in some cases.

For exposition purposes in this section, I frame the problem as that of inferring the spillover effect from rioting in the month of November, but the analysis proceeds in a similar way for the problem of assessing deterrence or opportunism from the police in the month of December. I focus on crime rates in the Seine Saint Denis and Gironde counties (1.5 and 1.4 million inhabitants respectively in 2005). As noted above, the choice of Seine Saint Denis is motivated by the fact that this county experienced, by far, the highest level of rioting, and therefore offers the most promising chance of finding unambiguous spillover effects in a non-parametric framework. Conversely, Gironde is the most populated urban county (comprising the city of Bordeaux) that was plagued by such negligible unrest that the French government decided not to add Gironde to the list of counties where emergency powers would take effect.19 The ONDRP confirms that arson in Gironde remained at a relatively low rate.20 It is therefore a natural comparison group against which to analyze the patterns found in Seine Saint Denis. I also consider the other areas affected by the emergency powers to investigate unusual crime spillover patterns in November as well as the intensity of policing in December – bearing in mind that police opportunism could take place irrespective of previous rioting levels. I first formally define the empirical question and the selection problem, and introduce invariance and bounded variation assumptions.

5.1 Average Treatment Effects and the Selection Problem.

Consider the problem of inferring the average treatment effect (ATE) of a riot on the rate of commission of a specified crime in a specified month in any given county. I follow the literature
by assuming that treatment response is individualistic. That is, riots in county i have no direct
effect on crime in county j, but through a contagion process they may give rise to riots in county j
which then contribute to crime in that county.\(^{21}\) For simplicity, I omit covariates from the
following presentation.
For each county j and month d there are two potential outcomes, \(Y_{jd}(1)\) and \(Y_{jd}(0)\). The former
outcome is counterfactual if county j did not experience a riot in month d while the latter is
counterfactual if the county did experience a riot. The fact that the data only reveal one of the
two mutually exclusive outcomes constitutes the selection problem. Here, there are two mutually
exclusive treatments: \(t = 1\) denotes a riot took place and \(t = 0\) denotes a riot did not take place.
Thus, the ATE boils down to:

\[
(\text{I}) \quad ATE_{jd} = Y_{jd}(1) - Y_{jd}(0)
\]

5.2. Invariance Assumptions

5.2.1 Simple examples using two groups

To address the selection problem the conventional practice has been to make assumptions
that point-identity counterfactual mean outcomes and, by implication, the ATE. These
assumptions, often implicit, must assert an invariance of some kind. To illustrate in a simple
setting, consider inference on the impact of rioting on the pickpocketing rate in Seine Saint Denis
(SSD). (I) can be rewritten as:

\[
(\text{II}) \quad ATE_{Nov \ 2005, SSD} = Y_{SSD, Nov \ 2005 \ (1)} - Y_{SSD, Nov \ 2005 \ (0)}.
\]
The data reveal $Y_{SSD, \text{Nov 2005}} (1) \equiv Y_{SSD, \text{Nov 2005}}$, but they do not reveal the counterfactual November 2005 pickpocketing rate in Seine Saint Denis, $Y_{SSD, \text{Nov 2005}} (0)$.

Table 2 displays the pickpocket rates per 100,000 inhabitants in Seine Saint Denis and in the county of Gironde in November of 2004, October of 2005, and November of 2005.

[Insert Table 2]

These data may be used to compute simple estimates of $Y_{SSD, \text{Nov 2005}} (0)$ under alternative invariance assumptions. Those chosen here are arguably the most intuitive but are not exhaustive. In all the following, the element (0) is dropped for the notation of all outcomes not taking place in November 2005 or outcomes taking place in Gironde, since there is no ambiguity.

1. Month-to-month time invariance: $Y_{SSD, \text{November 2005}} = Y_{SSD, \text{October 2005}}$

2. Year-to-year time invariance: $Y_{SSD, \text{November 2005}} = Y_{SSD, \text{November 2004}}$

3. Month-to-month change invariance:

   $Y_{SSD, \text{November 2005}} - Y_{SSD, \text{October 2005}} = Y_{SSD, \text{November 2004}} - Y_{SSD, \text{October 2004}}$

4. Inter-counties invariance: $Y_{SSD, \text{November 2005}} = Y_{Gironde, \text{November 2005}}$

5. Year-to-year DID invariance:

   $Y_{SSD, \text{November 2005}} - Y_{Gironde, \text{November 2005}} = Y_{SSD, \text{November 2004}} - Y_{Gironde, \text{November 2004}}$

6. Month-to-month DID invariance:

   $Y_{SSD, \text{November 2005}} - Y_{Gironde, \text{November 2005}} = Y_{SSD, \text{October 2005}} - Y_{Gironde, \text{October 2005}}$
(7) Triple Difference invariance (DDD)  

\[ Y_{SSD, \text{November 2005}} - Y_{SSD, \text{October 2005}} = Y_{Gironde, \text{November 2005}} - Y_{Gironde, \text{October 2005}} \]

The following table indicates the ATE corresponding to each of those assumptions:

[Insert Table 3]

While the estimates are mutually exclusive, it is worth noticing that they are all strictly positive, suggesting that pickpocketing did increase in Seine Saint Denis because of the riots.

Each estimate appropriately measures the effect of the rioting on the pickpocketing rate in SSD if its corresponding invariance assumption holds. For example, the DID invariance estimate is correct under the assumption that, in the absence of riots, Seine Saint Denis and Gironde would have experienced the same change in pickpocketing between October 2005 and November 2005, or alternatively, between November 2004 and November 2005. Obviously, the differences in the resulting ATE show that these invariance assumptions cannot hold jointly. Indeed, it may be that none of them does. Thus, the ATE may equal none of the values listed above. Given that the data only approximately support some of those assumptions when applied to earlier years this is more likely than not.

5.2.2 Conventional approach using multiple groups

The literature evaluating the effect of exogenous shocks on a given outcome with pooled cross sections uses all the geographical units and time periods available rather than two of each. It has been common to assume a linear model with a homogeneous treatment response embedding, at a minimum, local fixed effects and time fixed effects.
Consider a model of the form:

\[(III) \quad Y_{jde}(t) = \theta \cdot t_{jde} + \sum \alpha_j + \sum \gamma_d + \sum \beta_e + \epsilon_{jde}. \]

Where \(\alpha_j, \gamma_d, \beta_e\) are county fixed effects, month fixed effects and year fixed effects respectively.

Here treatment \(t = 1\) denotes the presence of a riot and \(t = 0\) otherwise.\(^{22}\) The parameter \(\theta\) is the treatment effect, which does not vary with \(j, d,\) or \(e\) and measures the effect of rioting on a particular crime \(Y\). Thus, the model assumes that riots have a homogeneous effect on crime rates across counties. This fixed effects model permits variation in the crime rate across counties through the composite additive intercept \(\alpha_j + \gamma_d + \beta_e + \epsilon_{jde}\). The unobserved random variable \(\epsilon_{jde}\) is a random county-time interaction assumed to have mean zero conditional on each realized value of the treatment. The assumption of a homogeneous response point-identifies the treatment effect: it can be shown to be, in substance, a generalization on the invariance DID model with two groups and two periods, and the same caveats apply. The DID model implicitly relies on the assumption that observations in untreated units act as plausible “controls” for observations in treated units, after “factoring out” local and time fixed effects, and other available variables such as local time trends.

A problem with the linear model in general, but more specifically here, is that of heterogeneous effects. The French riots were widely heterogeneous with respect to the magnitude of the destruction they engendered, and there is no reason to believe that various police units in affected areas would go after the same types of crime, if any. The raw data suggest threshold effects for criminal spillovers.\(^{23}\) One possibility to alleviate the problem is to flexibly interact the dummy variable coding for riots with a function capturing the intensity of the riots, for example with a quadratic polynomial function of the recorded incidents of arson.
Finally, consider that random sampling assumptions are implicit in the inferential methods used to estimate the standard errors of parameters in the linear models prevalent in the literature. Random sampling assumptions, however, are hard to justify when considering geographical units as units of observation,\textsuperscript{24} that is, when the data correspond to a census. Because there is no sampling process that would be intuitive in the present application\textsuperscript{25} one must think of the error term as measurement error and make a heuristic assumption of normality, independence and identical distribution of the residuals.\textsuperscript{26} The standard errors can, however, be safely clustered by county given that there are nearly a hundred of those, which is well above the minimum recommended by the robust clustering literature (Colin and Cameron, 2017).

5.3. Bounded Variation Assumptions

Manski and Pepper (2013, 2017) show how bounded variation restrictions, by weakening invariance assumptions, provide an intuitive and simple way to improve the credibility of empirical findings. This methodology illuminates the sensitivity of inferences to different identifying restrictions. It is well suited for the in-depth analysis of isolated events. In the present application, I use the history of observed data on crime counts per population to motivate a particular set of bounded variation assumptions and explore the inferences that arise from those.

Bounded variation assumptions cannot point-identify the effects of rioting or the effects of emergency powers. Instead, they partially identify them, yielding bounds rather than point estimates. These bounds are not confidence intervals – they do not express statistical imprecision created by sampling variability. Rather, they convey the ambiguity created by the selection problem. Within the bounded variation framework, I perform a finite-population analysis that
views *counties* as the population of interest, rather than seeing them as realizations of some sampling process.

Essentially, to define a plausible range for the unobserved counterfactual crime rates that enter the definition of the ATE, I choose from historical precedent “worst case scenarios.” In other words, I select the most extreme outcomes found in the data which would run against finding a non-ambiguous ATE.

Unlike Manski and Pepper (2017), I do not derive bounds based on the absolute value of past differences. Looking at the absolute value of past differences is compelling when motivating the methodological approach because one can choose a parameter that defines a given bounded assumption, and the bounds on the average treatment effect vary with that parameter in a transparent (and symmetric) way. This approach shows for which value of the parameter the results start becoming ambiguous. However, Manski and Pepper (2017) go one step further in their application and derive bounds that are almost always wider than what actual “worst-case” precedent-based assumptions warrant.

To illustrate, take the month-to-month time bounded variation assumption. Formally, for Manski and Pepper (2017), this would translate as an assumption of the form \( |Y_{\text{November 2005}}(0) - Y_{\text{October 2005}}| \leq \delta \), where \( \delta \) is a parameter that can be increased to weaken invariance assumption (1). Imposing a \( \delta \) that makes the difference of interest at least equal to the maximum of earlier November / October absolute value differences amounts to

\[
(IV) \quad |Y_{\text{November 2005}}(0) - Y_{\text{October 2005}}| \leq \max_{T \leq 2004} |Y_{\text{November } T} - Y_{\text{October } T}|.
\]

This almost always leads to wider bounds than assuming:
Inequality (V) amounts to a stronger assumption but is arguably more straightforward to justify than (IV). Inequality (V) simply asserts that the counterfactual crime rate in November 2005 must not have been so extreme as to make the counterfactual October / November 2005 difference smaller (larger) than the smallest (largest) October / November differences observed in the past. Because the month of November 2005 is an obviously bad yardstick to measure the productivity of police efforts relative to December 2005, I will focus on October / December differences, as the case may be, and the rest of the analysis will follow mutatis mutandis.

Inequality (V) illustrates the approach for the month-to-month bounded variation assumption – that is, applied to assumption (1) from section 5.2.1. All other invariance assumptions can be weakened similarly using Min and Max counterparts. In addition, I include the simplest bounded variation assumption (which, by construction, does not correspond to any plausible invariance assumption) whereby: Min T ≤ 2004 \{ Y_{November T} - Y_{October T} \} ≤ Y_{November 2005} (0) - Y_{October 2005} ≤ Max T ≤ 2004 \{ Y_{November T} - Y_{October T} \}.

The main question is whether the bounds yield a non-ambiguous sign for the ATE, and if so, whether the magnitude of the ATE is meaningful. The robustness of the analysis derives from a comparison of the bounds for each bounded variation assumption that is being exploited, just like a sensitivity analysis stems from running various specifications in conventional parametric settings and then comparing the resulting point estimates and significance levels.

These bounded variation assumptions can be weakened in several ways. First, the approach just mentioned could be misleading in the presence of outliers, either from actual events that are
unlikely to reoccur or because of coding mistakes. Min- and Max-based estimators are the most sensitive to outliers. Therefore, a natural step is to substitute Min and Max values with some order statistics (or some percentiles, say 5% and 95%). In the following, one of the bounded variation assumptions chosen will replace Min and Max values with the second and (n-1)th order statistics of the historical distribution corresponding to each counterfactual quantity. Another way to weaken the assumption is to consider that the preferred range for the counterfactual crime rate of November 2005 must be constructed based on close, not distant history. Therefore, instead of taking the Min and Max values from the entire pre-riot period, another bounded variation assumption will be based on Min and Max values from January 2000 to October 2005. Mechanically, both sets of bounds may only be tighter.

5.4 Interviews with Jacques Meric and Bernard Pasqualini

The meetings with Mr. Meric and Mr. Pasqualini were face to face semi-structured interviews which I conducted in the summer of 2016. Both lasted an hour and followed the same protocol. I first asked them to provide an account of the riots and of their aftermath throughout the state of emergency based on their recollection of the events. After recording their initial answers, I shared with them the result of my quantitative analysis and, using non-directive probes or prompts, asked them to provide further remarks. I also collected their insights with respect to the gaps in the analysis left by missing or unavailable data. In my judgment, both accounts were candid, free of complacency or self-serving motives. In the following, I use Mrs. Meric’s and Pasqualini’s observations throughout the discussion of the results for Seine Saint Denis county.
6. Discussion of Results

6.1 Effect of rioting on criminal activity

From a first pass examination of the raw data, certain crimes appear to change significantly in November 2005 in the rioting areas, for example “Violent Robbery against Women in Public Without a Weapon.”\textsuperscript{30} The following sections apply the linear regression and non-parametric bounded variation methodology to those outcomes and present only some of the most salient results in detail. It should be emphasized that none of the categories of crime analyzed in this section (and in fact none overall, other than arson) experienced any noticeable change in Gironde using the within-county bounded variation assumptions corresponding to invariance assumptions (1)-(3) of section 5.2.1, thus providing additional validation of its suitability as comparison group. Further, the bounded variation framework applied to Gironde against adjacent, mostly rural counties shows no detectable increase for categories of crime other than arson.

6.1.1 Across counties

The most flagrant criminal spillover pattern associated with the riots is the rise of opportunistic thefts. To illustrate, Table 4 shows that the riots are associated with a substantial increase of unarmed, street level, violent robbery.

[Insert Table 4]

The average effect measured by the simple dummy is significant at the 1\% level in specifications (1) and (2). It corresponds to more than a tripling of the average rate. The effect is proportional to rioting intensity at the 5\% level (specifications (3) and (4)). The quadratic parametrization of rioting intensity (specifications (5) and (6)) captures the pattern well, since the joint significance of the coefficients falls below the 5\% or 1\% levels, confirming the presence of threshold effects.
When considering rioting intensity, the coefficient for the presence of riots now corresponds to an intercept and bears no economic signification.

It is hardly surprising that in a state of chaos thieves would target passersby. This pattern is confirmed by the analysis of the parallel outcome “Violent Robbery in Public against Women Without a Weapon.” The fact that the results for each category match closely provides converging lines of evidence for the conclusion of riot-induced, opportunistic, criminal spillovers. There is also evidence of an increase in “Assault and Battery,” and of additional incidents corresponding to the two categories covering “Threat or blackmail.”

While these results are intuitive, one could not fully predict which type of crime would experience an increase that can be credibly linked to the riots among the numerous categories listed in the ‘4001 state.’ Even if those patently riot-related thefts belong to the opportunistic kind, not all opportunistic thefts were similarly exacerbated. For instance, there is no evidence of increase in shoplifting (if anything, the opposite); yet, that may be attributed to shops closing because of the rioting, and the reporting of petty thefts may also be less reliable in that period. The evidence with respect to car thefts is weak; yet, this could be explained by the fact that so many cars were burned, thus could not be plundered, or that their owners found a way to shelter them from burning and, by implication, looting. Still, other types of crime that could include an opportunistic dimension in this context, such as “Other Simple Thefts against Persons in Public Premises,” cannot be traced to the riots under any specification.

That none of the forms of theft requiring planning (armed robbery, burglary, etc.) shows any significant increase confirms that the riots were a spontaneous phenomenon and not some elaborate smokescreen designed by organized crime to engage in more illegal behavior. In fact, some
negative effects can be detected for “Burglary of Main Residence” and “Other Burglaries” suggesting that the riots may have hampered more sophisticated criminal pursuits.

Notably, the recorded number of incidents related to certain drug offenses, namely “Simple Use of Drugs,” “Use and Resale of Drugs,” “Other Drug Related Violations,” as well as “Pimping” drops significantly. That the police would slow down their fight against pimping in the middle of riots is understandable, and there is no similar effect to be detected in the month of December. However, the fact that records of drug incidents experienced a similar decrease in December, as discussed below, helps disentangling whether this was caused by a real decrease in the use/resale of drugs, the riots impairing police work, a sign of placation, or a combination thereof.

Similarly, we observe a significant, substantial drop in the number of incidents in the category of “Sexual Harassment and other Sexual Aggressions against Minors” in both the month of November and December. This result is more problematic to interpret since such crimes are reported by victims. At face value, then, it would appear as if those crimes did go down. However, when we bring together the context of violence (including against women, as described above), the fact that crimes must be judicially processed to enter the data base, that the results mirror what we observe for drugs, and that the age range of the victims would likely relate to the profile of the riot perpetrators (15-25 year-old males), we cannot but contemplate the unsettling hypothesis that the authorities may have been purposely more lax with respect to these crimes. However, there is no evidence that the reduction in the count of sexual assaults and sexual harassments against minors was caused by an escalation of those crimes into rapes, since there is no significant increase in rapes against minors.
6.1.2 Effect of the Rioting in Seine Saint Denis

In Table 5, focusing on the same outcome of “Violent Robbery against other Persons Without a Weapon”, even the weakest set of assumptions (row A) gives an unambiguous sign, and the ranges are close to each other. The intersection of all the bounds is non-empty. To give an idea of the magnitude of the effect, the November 2005 rate in Seine Saint Denis represents about twice the average of previous November rates.

[Insert Table 5]

Again, the same conclusion holds for “Violent Robbery against Women Without a Weapon.” The evidence regarding battery, however, is mixed. Contrary to the average result across counties, the bounds all clearly point toward a surge in pickpocketing for Seine Saint Denis, confirming the results of Table 3, and the effect corresponds to a 40% increase relative to the average of earlier November months.

The narrative underlying those results accords with Mr. Meric’s recollection that rioting gave rise to various types of opportunistic fleecing. Given the nature of the available data, whether the perpetrators were the rioters themselves is unknown. Mr. Meric could not confirm one way or another. Mr. Pasqualini was more confident in lumping rioters and opportunistic thieves together. However, he noted that criminal spillovers were not on the police radar and no instructions were given to counter those specifically.

According to Messrs. Meric and Pasqualini, career criminals’ activities – especially those involving drugs – were hindered by the disturbances. Both rejected the hypothesis that career criminals could have benefited from the riots as some sort of decoy. At the same time, career criminals knew that the youths – drug dealers’ customers for the most part – needed to express
their anger through violence. They also needed to keep up with their peers from other cities, as well as attract publicity as a badge of honor. This situation, according to Mr. Meric, led to a crime pattern that is consistent with those actors’ conflicting incentives and pecking order, albeit untestable with the data at hand. Essentially, violence was coming and going within the county, like a shockwave moving from one area to another, in order to give the local youth an opportunity to partake in violent protest. Yet, in each suburban town, after a few days of venting, career criminals got the mobsters to stop interfering with their business and successfully managed to put a lid on the unrest to resume their illegal activities. To a large extent, then, riot control was the work of career criminals on the ground more than the result of police effort. Mr. Pasqualini, however, held that such control was tacit and did not meet the level of active supervision.

6.2 Effect of Emergency Powers on Criminal Activity and Police Efforts (December 2005)

To reiterate, the idea underlying this part of the analysis is that the riots has cooled off by mid to late November but the emergency powers persisted (until early January). As in the previous section, the investigation focuses on those crime categories which by simple inspection display large enough variations that they deserve thorough examination but I present only some of the most salient results in detail. For regression purposes, the intensity of rioting is imputed from the month of November to analyze residual criminal spillover effects.

6.2.1 Across counties

The riot induced pattern of violent robbery in public, both against female victims and others, remained at a significantly higher than average level, and regression coefficients (or joint tests of coefficients) are all statistically significant at the 1% level for those crimes in the month of
December. Strikingly, violent robbery against women escalated further as the increase measured is close to double the increase of November, itself already significant.

Plausibly, thieves must have construed the absence of tangible signs of increased policing in relation to the emergency powers, let alone police raids, as a more permissive environment in which to maneuver. This is evidenced by the increase of incidents in several categories which are characterized as less opportunistic and/or more serious on the criminal scale than the types of theft that the riots boosted in the preceding month. This pattern was missed by earlier studies since those focused on the rioting period exclusively.

The data reveal an increase in armed robberies, both against financial establishments and against commercial and industrial establishments. I present the results for the latter as they display a pattern consistent with heterogeneous effects.

[Insert Table 6]

The average effect is insignificant (columns (1) and (2)), but it is significantly proportional to the intensity of rioting at the 1% level (columns (3)-(6)). Other categories exhibiting the same pattern are “Other Thefts with a Bladed Weapon” and “Violent Thefts Without a Weapon against Individuals at their Residence.”

For crimes the measurement of which is indicative of police effort, there is strong evidence of a decrease of the number of incidents related to narcotic drugs, as mentioned earlier. Table 7 presents results with respect to “Simple Use.”

[Insert Table 7]
The magnitudes are similar to those found for the month of November. The attitude of the police with respect to drug crime echoes the comments made about drug traffickers being responsible for the de-escalation. Arguably, there may have been some reciprocity at play. Consistent with this pattern, the data also reveal a decrease in the number of violations of the regulations governing tobacco and alcohol.

As mentioned earlier, there is a decrease in the number of incidents for “Sexual Harassment and other Assaults against Minors” following that already observed in November. Further, there is also a significant and substantial decrease in the count for other categories of sex crimes, i.e., “Adult Rape” and “Sexual Harassment and other Sexual Assaults against Adults.” Again, at face value, this should mean a reduction in the actual number of sex crimes committed. However, in the context of a general escalation of violent criminal behavior, particularly against women, this interpretation is difficult to defend against the competing hypothesis of laxness. Finally, no category of crime suggests an increase in police productivity.

6.2.2 Seine Saint Denis

In this part of the analysis, recall that the bounds derived from comparisons across different months (specifications (1), (3), (6), and (7)) are based on December/ October changes. In Seine Saint Denis, emergency powers in force in December can be credibly linked to cracking down on illegal commercial practices, false advertising, and gouging. The small number recorded for that offence in November does not necessarily point to a December catch up effect given that for this category of crime the monthly average is similarly small. Although the absolute magnitude of the effect is small (an increase of about one unit per 100,000 population), the December 2005 rate corresponds to six to seven times the average of the pre-2005 December rates. The result is robust enough that one reaches a similar conclusion by analyzing the combined November/ December rate against
earlier November/December rates (and Gironde combined November/December rates), and even December 2005 alone against previous combined November/December counts using the same methodology. It also holds when considering the full 1996-2014 period, as Table 8 shows.

[Insert Table 8]

It is not possible to know whether this was an opportunistic move by the police unrelated to the rioting, or whether this phenomenon corresponds to action against gouging by shopkeepers who may have tried to take advantage of the actual or perceived shortages incurred during the riot. Another relevant piece of evidence here is that the same category of “Unlawful Pricing, False Advertising and Unlawful Competitive Behavior” underwent a similar spike in the neighboring Hauts de Seine county in the month of November. Neither Mr. Meric nor Mr. Pasqualini was capable of providing any explanation for this pattern.

Ordinary crime remained at a high level. We find some of the average escalation effects mentioned earlier exacerbated in Seine Saint Denis. ATE bounds are again uniformly strictly positive for “Violent Robbery against Women in Public Without a Weapon,” and mostly strictly positive for “Violent Robbery against other Victims in Public Without a Weapon,” and for “Pickpocketing.”

Regarding more serious crimes, all but one of the ATE bounds are strictly positive for “Armed Robbery against Financial or Commercial Establishments”, and all but three are positive for “Other Thefts with a Bladed Weapon” and “Violent Thefts Without a Weapon against Individuals at their Residence”. Those represent changes of +46%, +22%, and +220% relative to the average of prior December months respectively. None of them appears to be result of a catch-up effect.

[Insert Tables 9, 10, 11]
In fact, these crime categories, as well as others, remained unusually high into March, receding slowly to pre-riot levels afterwards for some, while others (such as “Battery,” “Threat or Blackmail with the Purpose of Extortion”) seem to have switched to a higher cruising regime, consistent with the higher post-riotting level of arson already documented. Again, the data do not tell whether those additional thefts bore the stamp of career criminals or signaled the debuts of freshly emboldened rioters on the aggravated robbery scene. This persistently higher level of crime, which subsisted during the period of the emergency powers and beyond, may be contrasted with the swift police and judicial response to the 2011 London riots (Bell et al., 2014).

Mr. Meric did perceive such a bolstering effect, although he could not ascertain the hypothetical transition from rioter to hardened criminal. All the same, his impression at the time was that, from the perspective of criminals, lawlessness had prevailed on the ground, fueling a diffuse appetite for increased territorial control. Mr. Pasqualini, on the other hand, was more confident in identifying former rioters under the age of majority as responsible for the escalation.34

If the data do not reveal evidence of the opportunistic use of emergency powers, it is in fact the result of a deliberate strategy of non-inflammation. Pacification was the main objective. Mr. Meric reiterated that busting crime at large was on nobody’s mind at the time, that the prefect did not give him instructions to use emergency powers to strike crime unrelated to the riots, and that in any event such use would have been contrary to the spirit of the law. He unambiguously rejected the idea of a windfall. Mr. Pasqualini concurred with this view and added that in any event – and to his later regret, he did not have at his disposal the additional manpower needed to conduct searches and seizures. As he recalls, the only exceptional power used was the imposition of local curfews, with the caveat that those had already been enacted in an ad hoc way before November 8th.
For Mr. Meric, the disinhibition of criminals paralleled the inhibition of the authorities: the police were given orders to just be prepared to intervene in case of new incidents. The tragedy that had triggered the unrest was on everyone’s minds, and the priority was to avoid any further blunders. Besides, Mr. Pasqualini stressed the low morale of his men resulting from imposed self-restraint and the perceived impunity of the young rioters after three weeks of nerve racking urban guerilla warfare. This climate translated into operational paralysis, tunnel vision, and a lack of proactivity and anticipation.

6.2.3 Paris November / December 2005

Paris was not affected by any significant rioting. Recall the emergency powers were put in place there out of precaution. Paris therefore provides a distinct opportunity to analyze the effect of emergency powers net of indirect, possibly contaminating rioting effects. Despite a few intriguing observations, one cannot make a compelling case that the emergency powers led to an increase in the caseload of judicial procedures and one must therefore reject the hypothesis that a by-product of the emergency powers was higher police efficiency.

7. Conclusion

Growing economic inequalities, as well ethnic tensions caused by large, uncontrolled immigration movements, make it likely that urban riots will keep occurring in many Western countries (Badiou, 2012). Likewise, additional powers that are given to the police to combat terrorism appear to be here to stay or to be deployed intermittently in the foreseeable future. To better understand some of the less immediate consequences of those new fixtures, I analyze the spillover effects of the 2005 French riots and follow-up emergency powers on criminal activity. I complement the
traditional regression framework with a novel, non-parametric bounded variation framework (Manski and Pepper, 2017) to measure counterfactual outcomes in the area most affected by the riots. The evidence shows that several categories of opportunistic thefts, some violent in nature, increased substantially. Conversely, crimes that require a minimum amount of planning did not. More serious violent crimes increased markedly more than two weeks after the riots had ended, despite the state of emergency still being in force, and persisted at higher than pre-riot levels for months. The decrease in the number of drug related recorded infractions over the entire period of the emergency powers in the riot areas points to permissiveness as the chosen mode of conflict resolution. One possible explanation for the parallel decrease in the recorded count of sex crimes, in line with this interpretation, is that the authorities prosecuted fewer of those. This narrative accords with the account of those events provided by Mr. Jacques Meric and Mr. Bernard Pasqualini, respectively head of the Seine Saint Denis police and head of Seine Saint Denis public safety during the riots, whom I interviewed.

If, as Chalfin and McCrary (2013) argue, the central empirical issue confronting the literature is not whether police affect crime, but the extent to which police reduce violent crime, then this paper shows that the French state of emergency giving extrajudicial powers to the police did not produce deterrence. This finding is consistent with the paucity of evidence with respect to opportunism by the police in that period, although some targeted crackdown on crime unrelated to rioting appears plausible here and there. Importantly, the combined quantitative and qualitative evidence presented in this paper suggest that the relatively fainthearted response of the authorities, which in some cases may be interpreted as turning a blind eye on some criminal pursuits, triggered an emboldening effect among young hoodlums, inspiring them to proceed to the next level on the criminal spectrum within a matter of weeks. It is of course a matter of speculation whether a
massive roundup and incapacitation of rioters or the sweeping use of the extrajudicial apparatus applied to ordinary criminality – along the logic of Sherman’s (1990) “residual deterrence” following police crackdown – would have changed outcomes. Be that as it may, the contrast with the reaction of the British authorities following the London 2011 riots is striking.

**Bibliography**


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Graph 1: Arson per 100,000 Population, Seine Saint Denis
Appendix A

Areas where Articles 6, 8, 9 and 11(1) of the April 3rd 1955 Act
(State of Emergency Powers) can be Implemented

<table>
<thead>
<tr>
<th>COUNTIES (#)</th>
<th>Areas affected</th>
</tr>
</thead>
<tbody>
<tr>
<td>Alpes-Maritimes (06)</td>
<td>Nice ; Saint-Laurent-du-Var.</td>
</tr>
<tr>
<td>Bouches-du-Rhône (13)</td>
<td>Marseille.</td>
</tr>
<tr>
<td>Côte-d’Or (21)</td>
<td>Dijon ; Chenôve ; Longvic.</td>
</tr>
<tr>
<td>Eure (27)</td>
<td>Evreux ; Gisors.</td>
</tr>
<tr>
<td>Haute-Garonne (31)</td>
<td>Toulouse ; Colomiers ; Blagnac.</td>
</tr>
<tr>
<td>Loiret (45)</td>
<td>Orléans.</td>
</tr>
<tr>
<td>Meurthe-et-Moselle (54)</td>
<td>Nancy ; Vandœuvre-lès-Nancy.</td>
</tr>
<tr>
<td>Moselle (57)</td>
<td>Metz ; Woippy.</td>
</tr>
<tr>
<td>Nord (59)</td>
<td>L’ensemble des communes de la communauté urbaine de Lille-Métropole.</td>
</tr>
<tr>
<td>Oise (60)</td>
<td>Méru ; Creil ; Nogent-sur-Oise.</td>
</tr>
<tr>
<td>Puy-de-Dôme (63)</td>
<td>Clermont-Ferrand.</td>
</tr>
<tr>
<td>Bas-Rhin (67)</td>
<td>Strasbourg ; Bischheim.</td>
</tr>
<tr>
<td>Haut-Rhin (68)</td>
<td>Mulhouse.</td>
</tr>
<tr>
<td>Rhône (69)</td>
<td>Lyon ; Vénissieux.</td>
</tr>
<tr>
<td>Seine-Maritime (76)</td>
<td>Rouen ; Le Havre.</td>
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<td>All areas</td>
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<tr>
<td>Yvelines (78)</td>
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<td>Somme (80)</td>
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<td>Vaucluse (84)</td>
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<td>Seine-Saint-Denis (93)</td>
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<tr>
<td>Val-de-Marne (94)</td>
<td>All areas</td>
</tr>
<tr>
<td>Val-d’Oise (95)</td>
<td>All areas</td>
</tr>
</tbody>
</table>

Source: Décret n°2005-1387 du 8 novembre 2005 relatif à l'application de la loi n°55-385 du 3 avril 1955
Table 1a
Summary Statistics for France, 1996-2014
Counts per 100,000 population

<table>
<thead>
<tr>
<th>Category</th>
<th>Min</th>
<th>Max</th>
<th>Mean</th>
<th>Std</th>
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<tr>
<td>Violent robbery against women in public w/o a weapon</td>
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<td>Pickpocketing</td>
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<td>169</td>
<td>8.3</td>
<td>12.1</td>
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<tr>
<td>Illegal commercial practices, false advertising, and gouging</td>
<td>0</td>
<td>170</td>
<td>0.1</td>
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<tr>
<td>Armed robbery against industrial/commercial establishments</td>
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<td>10.9</td>
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<td>0.5</td>
</tr>
<tr>
<td>Other thefts with a bladed weapon</td>
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<td>0.8</td>
<td>1.1</td>
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<tr>
<td>Violent thefts w/o a weapon against individuals at their residence</td>
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<td>10.1</td>
<td>0.2</td>
<td>0.3</td>
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<tr>
<td>Simple Use of Narcotic Drugs</td>
<td>0</td>
<td>136</td>
<td>12.8</td>
<td>8.3</td>
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Table 1b
Summary Statistics for Seine Saint Denis, 1996-2014
Counts per 100,000 population

<table>
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<tr>
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<th>Min</th>
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<td>56.1</td>
<td>27.5</td>
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### Table 2
Pickpocketing rate per 100,000 inhabitants in Seine Saint Denis and Gironde

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<td>Gironde</td>
<td>17.26</td>
<td>23.66</td>
<td>17.2</td>
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Table 3
Estimates of the Average Treatment Effect of the Riots for Pickpocketing in Seine Saint Denis using alternative Invariance Assumptions (1)-(7)

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<td></td>
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<td>9</td>
<td>7.5</td>
<td>16.9</td>
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<td>10.6</td>
<td>13</td>
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Table 4: OLS Regression with dependent variable: Violent robbery against other victims in public w/o a weapon

<table>
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<td>0.002</td>
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<td>(6×10^-4)**</td>
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<td></td>
<td>5.5×10^-6</td>
<td>5.4×10^-6</td>
<td>9×10^-6</td>
<td>9×10^-6</td>
<td>(2.5×10^-6)**</td>
<td>(2.4×10^-6)**</td>
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<tr>
<td>d_Riot × Arson^2</td>
<td>(5×10^-6)**</td>
<td>(5×10^-6)**</td>
<td>(5×10^-6)**</td>
<td>(5×10^-6)**</td>
<td>(5×10^-6)**</td>
<td>(5×10^-6)**</td>
</tr>
<tr>
<td>Adjusted R^2</td>
<td>0.87</td>
<td>0.90</td>
<td>0.87</td>
<td>0.90</td>
<td>0.87</td>
<td>0.90</td>
</tr>
<tr>
<td>County × Trend</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>N</td>
<td>21,888</td>
<td>21,888</td>
<td>21,888</td>
<td>21,888</td>
<td>21,888</td>
<td>21,888</td>
</tr>
</tbody>
</table>

All regressions control for year, month, and county fixed effects. Robust standard errors clustered by county. d_Riot is a dummy coding for counties where and when riots took place. Arson is measured as the number of private property items recorded as being torched in the month of November 2005. It proxies rioting intensity. *: 10% statistical significance **: 5% statistical significance ***: 1% statistical significance
Table 5: Non-parametric bounds on the Average Treatment Effect
Violent robbery against other victims in public w/o a weapon (Seine Saint Denis)

<table>
<thead>
<tr>
<th></th>
<th>(S) Simplest</th>
<th>(I) Month to Month</th>
<th>(II) Year to Year</th>
<th>(III) Monthly Change</th>
<th>(IV) Inter county</th>
<th>(V) Year to Year DID</th>
<th>(VI) Month to Month DID</th>
<th>(VII) DDD</th>
</tr>
</thead>
<tbody>
<tr>
<td>(A) Min-Max</td>
<td>[10, 28]</td>
<td>[6, 12]</td>
<td>[9, 17]</td>
<td>[2, 13]</td>
<td>[7, 24]</td>
<td>[4, 15]</td>
<td>[3, 15]</td>
<td>[5, 13]</td>
</tr>
<tr>
<td>(B) 2nd &amp; (n-1)th order</td>
<td>[11, 26]</td>
<td>[7, 12]</td>
<td>[9, 17]</td>
<td>[2, 13]</td>
<td>[11, 22]</td>
<td>[6, 15]</td>
<td>[7, 14]</td>
<td>[6, 10]</td>
</tr>
<tr>
<td>(C) Min-Max post 1999</td>
<td>[10, 14]</td>
<td>[6, 12]</td>
<td>[9, 17]</td>
<td>[2, 13]</td>
<td>[7, 15]</td>
<td>[4, 15]</td>
<td>[3, 14]</td>
<td>[5, 8]</td>
</tr>
</tbody>
</table>

Column (S) corresponds to the simplest bounded variation assumption where the counterfactual outcome is bounded by the past extreme values of the outcome in the same month. Columns (1)-(7) correspond to the bounded variation assumptions that are the counterparts of the invariance assumptions (1)-(7) of section 5.2.1. Rows (A)-(C) correspond to bounded variation assumptions using Min- and Max-based bounds, second and (n-1)th order statistics-based bounds, both covering 1996-2005, and Min- and Max-based bounds for the 2000-2005 period respectively. All numbers rounded to the nearest integer.
Table 6: OLS Regression with dependent variable: Armed Robbery against Commercial and Industrial Establishments

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>d_EmergencyNoRiot</td>
<td>$10^{-4}$</td>
<td>$10^{-4}$</td>
<td>$3\times10^{-4}$</td>
<td>$3\times10^{-4}$</td>
<td>$10^{-4}$</td>
<td>$10^{-4}$</td>
</tr>
<tr>
<td></td>
<td>($10^{-4}$)</td>
<td>($10^{-4}$)</td>
<td>($10^{-4}$)</td>
<td>($10^{-4}$)</td>
<td>($2\times10^{-4}$)</td>
<td>($2\times10^{-4}$)</td>
</tr>
<tr>
<td>d_EmergencyNoRiot × Arson</td>
<td>0.01</td>
<td>0.01</td>
<td>-0.014</td>
<td>-0.013</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>($0.004$)***</td>
<td>($0.004$)***</td>
<td>($0.01$)</td>
<td>($0.01$)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>d_EmergencyNoRiot × Arson$^2$</td>
<td></td>
<td></td>
<td>0.33</td>
<td>0.32</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>($0.11$)***</td>
<td>($0.1$)***</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Joint statistical significance of d_EmergencyNoRiot × Arson and d_EmergencyNoRiot × Arson$^2$</td>
<td>***</td>
<td>***</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Adjusted $R^2$</td>
<td>0.43</td>
<td>0.46</td>
<td>0.43</td>
<td>0.46</td>
<td>0.43</td>
<td>0.46</td>
</tr>
<tr>
<td>County × Trend</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>N</td>
<td>21,888</td>
<td>21,888</td>
<td>21,888</td>
<td>21,888</td>
<td>21,888</td>
<td>21,888</td>
</tr>
</tbody>
</table>

All regressions control for year, month, and county fixed effects. Robust standard errors clustered by county. d_EmergencyNoRiot is a dummy coding for the month of December 2005 in counties where riots took place in November. Arson is measured as the number of private property items recorded as being torched in the month of November 2005. It proxies rioting intensity.

*: 10% statistical significance
**: 5% statistical significance
***: 1% statistical significance
## Table 7: OLS Regression with dependent variable: Simple Use of Narcotic Drugs

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>d_EmergencyNoRiot</td>
<td>-0.002</td>
<td>-0.003</td>
<td>-9×10^{-4}</td>
<td>-8×10^{-4}</td>
<td>-0.006</td>
<td>-0.006</td>
</tr>
<tr>
<td></td>
<td>6×10^{-4}***</td>
<td>(6×10^{-4})***</td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.002)***</td>
<td>(0.002)***</td>
</tr>
<tr>
<td>d_EmergencyNoRiot × Arson</td>
<td>-0.05</td>
<td>-0.06</td>
<td>0.3</td>
<td>0.31</td>
<td>(0.04)</td>
<td>(0.04)</td>
</tr>
<tr>
<td>d_EmergencyNoRiot × Arson^2</td>
<td>-4.6</td>
<td>-4.7</td>
<td>(1.28)***</td>
<td>(1.34)***</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Joint statistical significance of d_EmergencyNoRiot × Arson and d_EmergencyNoRiot × Arson^2</td>
<td>***</td>
<td>**</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Adjusted R^2</td>
<td>0.54</td>
<td>0.62</td>
<td>0.54</td>
<td>0.62</td>
<td>0.54</td>
<td>0.62</td>
</tr>
<tr>
<td>County × Trend</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td></td>
<td></td>
</tr>
<tr>
<td>N</td>
<td>21,888</td>
<td>21,888</td>
<td>21,888</td>
<td>21,888</td>
<td>21,888</td>
<td>21,888</td>
</tr>
</tbody>
</table>

All regressions control for year, month, and county fixed effects. Robust standard errors clustered by county. d_EmergencyNoRiot is a dummy coding for the month of December 2005 in counties where riots took place in November. Arson is measured as the number of private property items recorded as being torched in the month of November 2005. It proxies rioting intensity. In columns (3) and (4), the joint test of the null hypothesis for d_EmergencyNoRiot and d_EmergencyNoRiot × Arson is rejected at the 1% level.

*: 10% statistical significance  
**: 5% statistical significance  
***: 1% statistical significance
Table 8: nonparametric bounds on the Average Treatment Effect
Unlawful commercial practices, false advertising, and gouging (Seine Saint Denis)

<table>
<thead>
<tr>
<th></th>
<th>(S) Simplest</th>
<th>(I) Month to Month</th>
<th>(II) Year to Year</th>
<th>(III) Monthly Change</th>
<th>(IV) Inter county</th>
<th>(V) Year to Year DID</th>
<th>(VI) Month to Month DID</th>
<th>(VII) DDD</th>
</tr>
</thead>
<tbody>
<tr>
<td>(A) Min-Max</td>
<td>[0.4, 0.9]</td>
<td>[0.6,1]</td>
<td>[0.7,1.2]</td>
<td>[0.5,1.2]</td>
<td>[0.6,1]</td>
<td>[0.7,1.2]</td>
<td>[0.5,1]</td>
<td>[0.5, 1.3]</td>
</tr>
<tr>
<td>(B) 2nd &amp; (n-1)th order</td>
<td>[0.6, 0.9]</td>
<td>[0.7,1]</td>
<td>[0.8,1.2]</td>
<td>[0.7,1]</td>
<td>[0.7,1]</td>
<td>[0.9,1.1]</td>
<td>[0.7,1]</td>
<td>[0.5, 1.2]</td>
</tr>
<tr>
<td>(C) Min-Max post-1999</td>
<td>[0.9, 0.9]</td>
<td>[0.8,1]</td>
<td>[0.9,1]</td>
<td>[0.7,1]</td>
<td>[0.9,1.1]</td>
<td>[0.9,1.1]</td>
<td>[0.8,1]</td>
<td>[0.6, 1.2]</td>
</tr>
<tr>
<td>(D) Min-Max 1996-2014</td>
<td>[0.4, 0.9]</td>
<td>[0.6,1]</td>
<td>[0.7, 1.2]</td>
<td>[0.5, 1.2]</td>
<td>[0.6,1]</td>
<td>[0.7, 1.2]</td>
<td>[0.5,1]</td>
<td>[0.4, 1.3]</td>
</tr>
</tbody>
</table>

Column (S) corresponds to the simplest bounded variation assumption where the counterfactual outcome is bounded by the past extreme values of the outcome in the same month. Columns (I)-(VII) correspond to the bounded variation assumptions that are the counterparts of the invariance assumptions (1)-(7) of section 5.2.1. Rows (A)-(C) correspond to bounded variation assumptions using Min and Max-based bounds, second and (n-1)th order statistics-based bounds, both covering 1996-2005, and Min and Max-based bounds for the 2000-2005 period. Row (D) corresponds to Min and Max-based bounds covering the entire period 1996-2014.
Table 9: nonparametric bounds on the Average Treatment Effect
Armed robbery against industrial or commercial establishments (Seine Saint Denis)

<table>
<thead>
<tr>
<th></th>
<th>(S) Simplest</th>
<th>(I) Month to Month</th>
<th>(II) Year to Year</th>
<th>(III) Monthly Change</th>
<th>(IV) Inter county</th>
<th>(V) Year to Year DID</th>
<th>(VI) Month to Month DID</th>
<th>(VII) DDD</th>
</tr>
</thead>
<tbody>
<tr>
<td>(A) Min-Max</td>
<td>[0.1, 0.9]</td>
<td>[0°, 1.8]</td>
<td>[0.4, 1.7]</td>
<td>[-0.5, 2.8]</td>
<td>[0.6, 1.4]</td>
<td>[0.6, 1.6]</td>
<td>[0.5, 1.8]</td>
<td>[0°, 2.7]</td>
</tr>
<tr>
<td>(B) 2nd &amp; (n-1)th order</td>
<td>[0.6, 0.9]</td>
<td>[0.8, 1.4]</td>
<td>[0.7, 1.2]</td>
<td>[0.5, 1.7]</td>
<td>[0.8, 1.2]</td>
<td>[0.7, 1.5]</td>
<td>[0.7, 1.6]</td>
<td>[0.5, 2.4]</td>
</tr>
<tr>
<td>(C) Min-Max post 1999</td>
<td>[0.7, 0.9]</td>
<td>[1.1, 1.3]</td>
<td>[0.7, 1.2]</td>
<td>[1.3, 1.4]</td>
<td>[0.8, 1.4]</td>
<td>[0.6, 1.5]</td>
<td>[1.3, 1.8]</td>
<td>[1.1, 1.8]</td>
</tr>
</tbody>
</table>

Column (S) corresponds to the simplest bounded variation assumption where the counterfactual outcome is bounded by the past extreme values of the outcome in the same month. Columns (I)-(VII) correspond to the bounded variation assumptions that are the counterparts of the invariance assumptions (1)-(7) of section 5.2.1. Rows (A)-(C) correspond to bounded variation assumptions using Min- and Max-based bounds, second and (n-1)th order statistics-based bounds, bounds, both covering 1996-2005, and Min- and Max-based bounds for the 2000-2005 period respectively.
Table 10: Nonparametric based bounds on the Average Treatment Effect
Other thefts with a bladed weapon (Seine Saint Denis)

<table>
<thead>
<tr>
<th>(S) Simplest</th>
<th>(I) Month to Month</th>
<th>(II) Year to Year</th>
<th>(III) Monthly Change</th>
<th>(IV) Inter county</th>
<th>(V) Year to Year DID</th>
<th>(VI) Month to Month DID</th>
<th>(VII) DDD</th>
</tr>
</thead>
<tbody>
<tr>
<td>(A) Min-Max</td>
<td>[0.1, 1.9]</td>
<td>[1.7, 5]</td>
<td>[0.3, 2.9]</td>
<td>[1.7, 3.8]</td>
<td>[0.1, 2.4]</td>
<td>[0.1*, 3.2]</td>
<td>[-0.3, 5]</td>
</tr>
<tr>
<td>(B) 2nd &amp; (n-1)th order</td>
<td>[0.2, 1.8]</td>
<td>[1.9, 3.7]</td>
<td>[1.1, 2.6]</td>
<td>[2, 3.7]</td>
<td>[0.7, 2]</td>
<td>[1.6, 2.9]</td>
<td>[1.8, 3.9]</td>
</tr>
<tr>
<td>(C) Min-Max post 1999</td>
<td>[0.3, 1.9]</td>
<td>[1.7, 5]</td>
<td>[1.7, 2.9]</td>
<td>[3, 3.7]</td>
<td>[0.7, 2]</td>
<td>[1.6, 3.2]</td>
<td>[1.9, 4.7]</td>
</tr>
</tbody>
</table>

Column (S) corresponds to the simplest bounded variation assumption where the counterfactual outcome is bounded by the past extreme values of the outcome in the same month. Columns (I)-(VII) correspond to the bounded variation assumptions that are the counterparts of the invariance assumptions (1)-(7) of section 5.2.1. Rows (A)-(C) correspond to bounded variation assumptions using Min- and Max-based bounds, second and (n-1)th order statistics-based bounds, both covering 1996-2005, and Min- and Max-based bounds for the 2000-2005 period respectively.
Table 11: nonparametric based bounds on the Average Treatment Effect

Violent thefts without a weapon against individuals at their residence (Seine Saint Denis)

<table>
<thead>
<tr>
<th></th>
<th>(S) Simplest</th>
<th>(I) Month to Month</th>
<th>(II) Year to Year</th>
<th>(III) Monthly Change</th>
<th>(IV) Inter county</th>
<th>(V) Year to Year DID</th>
<th>(VI) Month to Month DID</th>
<th>(VII) DDD</th>
</tr>
</thead>
<tbody>
<tr>
<td>(A) Min-Max</td>
<td>[0.2, 0.5]</td>
<td>[0.3, 0.8]</td>
<td>[0.3, 0.7]</td>
<td>[0.2, 0.7]</td>
<td>[0.3, 0.9]</td>
<td>[0.3, 0.9]</td>
<td>[0, 1.1]</td>
<td>[-0.2, 0.8]</td>
</tr>
<tr>
<td>(B) 2nd &amp; (n-1)th order</td>
<td>[0.2, 0.5]</td>
<td>[0.3, 0.7]</td>
<td>[0.3, 0.6]</td>
<td>[0.3, 0.7]</td>
<td>[0.3, 0.7]</td>
<td>[0.3, 0.8]</td>
<td>[0.3, 0.8]</td>
<td>[-0.1, 0.6]</td>
</tr>
<tr>
<td>(C) Min-Max post 1999</td>
<td>[0.3, 0.5]</td>
<td>[0.3. 0.7]</td>
<td>[0.3, 0.7]</td>
<td>[0.5, 0.7]</td>
<td>[0.3, 0.9]</td>
<td>[0.3, 0.9]</td>
<td>[0+, 1.1]</td>
<td>[-0.2, 0.8]</td>
</tr>
</tbody>
</table>

Column (S) corresponds to the simplest bounded variation assumption where the counterfactual outcome is bounded by the past extreme values of the outcome in the same month.

Columns (I)-(VII) correspond to the bounded variation assumptions that are the counterparts of the invariance assumptions (1)-(7) of section 5.2.1. Rows (A)-(C) correspond to bounded variation assumptions using Min- and Max-based bounds, second and (n-1)th order statistics-based bounds, both covering 1996-2005, and Min- and Max-based bounds for the 2000-2005 period respectively.
Endnotes

1 In the U.S., concerns over police abuse following the *Patriot Act* have been numerous. In particular, the “Sneak and Peek” method of investigation has been used to spy on drug suspects and immigrants according to the American Civil Liberties Union and the Electronic Frontier Foundation – see e.g., Bloss (2007).

2 By ambiguous or uninformative I denote those bounds that encompass zero, which means that one cannot even sign the treatment effect.

3 A (translated) transcript of the interviews is available upon request.

4 Kepel ((2015 : 35).

5 See maps, pictures, and graphs on https://commons.wikimedia.org/wiki/Paris_suburb_riots

6 Ethnographic studies of soccer hooliganism find evidence of rioters rioting for the thrill of the riot (Buford, 1991; Wilkinson, 2009).

7 For a comparative analysis of American and French recent race riots, see Schneider (2014).

8 The government’s response in November 2005 (and later) was amplified by a wide range of commentary that attempted to link the rioting to illegal immigration, Muslim separatism, and polygamous practices, since most of the rioters were second generation immigrant youths.

9 This happened during the armed independence movement in the distant territory of New Caledonia in the mid-1980s. See Beaud and Guérin-Bargues (2016) for an in-depth presentation of the state of emergency in France.

10 That is, from the time data are available. Note that the graph suggests that the riots had persistent effects. Especially troubling is the post 2005 frequency of spikes, only one of which corresponds to the anniversary of the riots (November 2006).
11 http://www.assemblee-nationale.fr/12/rapports/r2675.asp

12 Conseil d’État N° 286835  (November 21st 2005).


14 See http://riotsfrance.ssrc.org/ for a review.


16 Except for Mr. Pasqualini who relates his experience of the 2005 events in his professional autobiography (Pasqualini, 2013).

17 Here, however, the French victimization surveys would not help for two reasons: from 1996 to 2003, those surveys were conducted in January, May, and October, and in 2004-2005, only the January and October waves were conducted. Further, given the sample size of some 11,000 individuals for the entire metropolitan France, a representative sample would not be guaranteed at the county level.

18 This would happen if e.g., the riots hamper victims’ ability to file complaints, or if the riots make police services congested and thus, practically, less accessible, etc. In terms of police opportunism, the police would presumably have an incentive not to trigger suspicion of unethical behavior. There is no intuitive reason why the police would report fake evidence when endowed with extra powers not intended to be used to deal with ordinary crime.

19 As is often the case in non-experimental settings, there is no particular compelling “control group” that would be a basis for an invariance assumption, i.e., same environments and pre-riot trajectories between treatment and control group. All urban areas were affected to some extent. On the surface, the two counties appear similar. Gironde had a 9.1% unemployment rate in the last quarter of 2005 versus 11.9% for Seine Saint Denis. The proportion of men age 20-39 out the
total Gironde population was 14% in 2005 versus 15% for Seine Saint Denis. However, although the French government does not collect ethnic statistics, it is well known that Bordeaux harbors a quantitatively negligible proportion of immigrants from North and Sub Saharan Africa, which were the main two groups represented among rioters (Lagrange and Oberti, 2006). To the extent that there was some limited disturbance in Gironde, it should produce a bias against finding any effect in Seine Saint Denis and other rioting areas.

20 The rate of arson in Gironde rose from about 3 to 12 per 100,000 inhabitants, hence about 14% of what prevailed in Seine Saint Denis in the same period.

21 One possibility that the literature on hot spot policing had to deal with is that criminals may just move from one neighborhood to another, in which case such spillover effects can challenge the validity of control groups chosen in a quasi-natural experiment setting. In the present case, the concern is alleviated to the extent that spillovers would have to involve criminal migration across county lines, something that is implausible for most crimes, especially if the counties are geographically far apart. In addition, it is even more difficult to criticize the no-spillover assumption with respect to police opportunism, since the police in non-emergency powers jurisdictions are unlikely to behave differently because of the emergency powers in place in other jurisdictions.

22 That is, 24 counties including counties where only the main urban center is declared in a state of emergency – see Table A. I exclude Paris because it did not experience riots but was put under the state of emergency out of precaution.

23 Isolating the most extreme events of Saint Saint Denis would amount to creating a dummy variable taking value one for one observation only, and the statistical inference that may be drawn from such a variable is known to be unreliable (Correia, 2015).
Abadie et al. (2014) raise a similar concern insofar as state level analysis “creates a problem for interpreting the uncertainty in the parameter estimates. If the parameters of interest are defined in terms of observable variables defined on units in the population, then if the sample is equal to the population there should be no uncertainty and standard errors should be equal to zero.”

Following Manski and Pepper (2017), one would have to view the existing state of France as “the sampling realization of a random process defined on a super-population of alternative nations.” That is, one would have to come up with a random process generating actual French history as one among a set of possible histories that could have generated alternative county level-monthly crime rates. See e.g., Cochran (1977) and Deaton (1997) for an exposition of the distinction between finite-population and super-population analysis.

To keep the discussion short, I do not introduce the semi-parametric version of the linear regression model which does not rely on any other assumption than a conditional mean of zero of the error term and is developed in an earlier version of this paper, available upon request.

Intuitively, the longer the past being considered, the weaker the assumption. To make an analogy, suppose one randomly draws a sample of n observations \{X_1, ..., X_n\} from a population in which values are assumed to have a continuous probability distribution. Then the probability that the next observation \(X_{n+1}\) will be the largest is \(1/(n + 1)\), since all observations have equal probability of being the maximum. In the same way, the probability that \(X_{n+1}\) will be the smallest is \(1/(n + 1)\). Again, this is just an analogy since the historical data cannot be construed as being a sample drawn from any plausible single distribution, much less randomly so.

Conversely, they can also be strengthened by considering multiples of the maximum and minimum.
Over the twenty year history and ninety five counties, a few values look suspicious. Outliers are more distortionary here than in a conventional linear model since the bounds are precisely derived from extreme values of the data.

This corresponds to a specific category of crime in the French statistics.

“Other Drugs related Offences” includes a variety of offences, in particular incitement towards consumption and/or trafficking of narcotic drugs, bogus prescriptions, etc. See http://www.inhesj.fr/sites/default/files/ga_38.pdf

This would be consistent with the argument advanced by Bleich et al. (2010) that when confronting ethnic riots western states balance repression and accommodation in keeping with a social control perspective.

With a handful of isolated exceptions, there is no detectable pattern where one could suspect increased police efforts within a specific county across categories of crime or with respect to a particular category of crime across counties.

The French judicial system is particularly protective of minors and ill-equipped to prosecute them. A majority of young rioters had therefore been rapidly released after their arrest.