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Civil unrest, emergency powers, and spillover effects: 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. In the affected areas the government declared a state of emergency which gave the police extrajudicial powers. 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 opportunistically to bust crime unrelated to the riots. I supplement linear regressions with a non-parametric bounded-variation assumptions framework combined with a synthetic control approach, and interviews I conducted with two of the events' key actors. Criminals did not take advantage of the riots to commit more crimes requiring planning. However, the riots triggered a surge of violent thefts. The state of emergency did not result in a decrease in delinquency. Several clues suggest a strategy of appeasement. Meanwhile, some serious crimes increased immediately after the riots ended, suggesting an emboldening effect. Evidence of police opportunism is scant.

1. Introduction

Urban riots have been on the rise worldwide in the last decade (Banks and Wilson, 2019).[1] Liberal democracies are not immune to this trend, resulting in a forced reassessment of the appropriate means to maintain public order (Kingshott, 2014; Waddington, 2014). While the focus is generally on understanding the causes of those phenomena – for example, the marginalization of minority groups (Holdo and Bengtsson, 2019), and on finding ways to prevent them – such as redistribution policies (Justino, 2008), less attention has been 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 occurrence 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

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[1] To give but one recent example, in October of 2019, the people of Santiago, Chile took to the streets to protest a raise in the Metro fare. This soon led to the worst unrest in decades, with over 20 people reported dead by mid-November and numerous state buildings being set ablaze and/or vandalized (Bremmer, 2019).

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police may seize those powers to crack down on crime unrelated to rioting or terrorism.[2] At issue, here, is whether criminals and police forces behave differently when the parameters of the environment in which they operate undergo some unexpected 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 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 expanded 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?

To summarize, my findings indicate that the riots led to a 20% increase in violent thefts in public relative to non-rioting counties over the average of previously recorded November rates. Career criminals did not seize upon the riots to commit more crimes requiring planning. Crime rates other than arson did not decline during the period of emergency powers that followed the riots. Much the opposite, violent thefts in public continued unabated after the riots ended. In Seine Saint Denis, the epicenter of the riots, more serious crimes soared. For instance, in December 2005, violent weaponless thefts at residences increased there by 120% over the average of previously recorded December rates. This exemplifies a singular doubling down effect among criminals following urban unrest. Conversely, evidence of opportunism by the police is scant.

Some of these results are in line with what was found about the contemporaneous impact of riots on low-level crime during the 2011 London riots (Bell et al., 2014). However, the aftermath effects between the British and French episodes diverge: whereas tougher sentencing created deterrence in the former case, the French experience of aggravated violence is consistent with the de-policing strategy analyzed by Shi (2009). This paper therefore shows that while suspicion of police abuse was misplaced by critics of the emergency powers, the existence of such emergency powers on paper without tangible signs of increased policing or sentencing proved ineffective in reducing crime and may instead have triggered a sense of impunity.

To support a causal interpretation of these results, I first apply the classic difference-in-difference approach within a fixed effects linear regression model to monthly-, county-level crime data. To which I supplement a series of placebo (also called falsification) tests. Next, because the effect I try to capture is substantially heterogeneous across counties, I turn to the non-parametric methodology developed by Manski and Pepper (2018). 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,[3] which accounts for their lack of popularity among social scientists and policy makers alike (Manski, 2013). However, this new approach is particularly well suited to draw causal 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*. To give a simple example: within an area that experienced rioting, the difference in outcomes between the month of October and the counterfactual outcome for November 2005 is assumed to be 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 by far the highest level of rioting. However, while Manski and Pepper (2018) essentially used one, heuristically chosen comparison unit in their study, I use the synthetic control method (Abadie and Gardeazabal, 2003; Abadie et al. 2010, 2015) to construct an objective counterfactual that best matches pre-November 2005 Seine Saint Denis from all counties that did not experience emergency powers. This simple refinement addresses the main caveat that could be levelled at the bounded variation framework in its original version. Note that the regression approach applied to France as a whole may be construed as a first-order test of the external validity of the non-parametric case study of Seine Saint Denis county.

I complement this quantitative approach with qualitative insights garnered through 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. To give a 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 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

[2] 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).

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

results. Indeed, Mr. Meric's and Mr. Pasqualini's firsthand institutional knowledge, direct involvement, and experience add significant value to the analysis of the raw data. Notably, the narrative that emerges from the quantitative analysis of the Saint Denis data accords with Messrs. Meric's and Pasqualini's independent accounts.

2. Literature review

This paper situates itself within the broader context of the resurgence of urban violence in the twenty-first century (Beall, 2007; Bhavnani et al., 2014). It expands on several strands of literature. At the most general level, it connects to the criminal decision-making process and criminal opportunism. Gary Becker's rational choice theory of crime (Becker, 1968) posits that individuals weigh the expected costs and benefits of committing crimes. This theory encompasses crimes which are not premeditated but can be characterized instead as a response to an immediate opportunity presenting itself (Akers, 1990; Cornish and Clarke, 1986). Gould and Stecklov (2009) performed an analysis of such criminal incentives in the context of terror attacks.

In a similar vein, this paper relates to the incentives provided by a rioting environment. Bell et al. (2014), analyzing the 2011 London riots, report a significant increase in burglaries and incidents of violence against persons. Morrell et al. (2011) surveyed youths who participated in looting during these riots: they discovered that some motivations to engage in crime included the lure of instant thrill, as well as the chance to get even with police for real and/or perceived incidents of discrimination and/or brutality.[4] In 2018–2019 France, the infiltration of Black Bloc activists (Dupuis-Déri, 2019) and looters during yellow vest demonstrations has blurred the line between hard line political protest and full blown riots, leading to debates over the balancing of upholding civil liberties with effective deterrence (den Heyer, 2020).

With respect to the medium to long-term effects of rioting on crime, Bell et al. (2014) show a significant drop in crimes across London in the six months after the riots, consistent with a deterrence effect from tougher sentencing on the rioters. However, they acknowledge a dearth of empirical evidence on the impact of riots on subsequent crime patterns. An important exception is Shi (2009) who found that felony crime surged in Cincinnati after the 2001 riot. This was the result of so-called de-policing, to avoid further criticism of racial profiling. Along a similar line, Shjarback et al., 2016 conducted a comprehensive analysis of state of Missouri local law enforcement agencies after the 2014 Ferguson riot. Overall, they found little to support a post-Ferguson change in the crime rates they tracked.

In turn, a related segment of the literature 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 (2017):

> 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; (…) 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 legal literature has extensively conceptualized emergency powers within the rule of law paradigm (Gross and Aoláin, 2006). Further, criminologists have analyzed how legally ambiguous police tactics, when used to disperse rioters, can have the ancillary effect of infringing upon civil rights and can potentially escalate violence (Baker 2016; Davies and Dawson 2015; Masterson 1988; Waddington and Wright 2008). However, the issue of police opportunism, i.e., conduct that may be described as technically lawful yet ethically questionable,[5] has only been studied to a limited extent in e.g., Vanuatu (Mitchell, 2011) and Brazil (Alves and Evanson, 2011). This phenomenon is yet to be explored in the Western context.[6]

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 present paper builds most closely on Manski and Pepper's (2013) analysis of the deterrent effect of the

[4] Other studies have focused on gang activities during riots. For example, Harding (2012) discovered that multiple London based gangs coordinated a truce during the 2011 riots, allowing rival gang members "hood passes" to go through each other's territories. Similarly, during the 1992 Los Angeles riot, rival gangs forged a truce which led to a decrease in the number of gunshot victims admitted in trauma centers (Ordog et al., 1993).

[5] The literature distinguishes between *outcome seekingmisconduct and opportunisticmisconduct*. According to Small and Watson's (1999) typology, "outcome seeking misconduct" means misconduct which occurs in support of or is aimed at achieving identifiable policing objectives. This type of misconduct is similar to the type of conduct often associated with the term "noble cause corruption." "Opportunistic misconduct" means misconduct that is unrelated to any specific policing objective. This type of misconduct is often associated with the term "corruption." In this paper, opportunism by the police will refer to outcome seeking zeal, though not necessarily misconduct

[6] On the other hand, there is growing evidence on how riots influence police deterrence strategies post-conflict, including, controversially, predictive policing through social media analysis and "tension monitoring" (Binder 2016; Williams et al., 2013), and over-policing of disenfranchised communities – which can increase resentment between the public and the state (Bleich et al., 2010; Côté-Lussier 2013; King 2013), but also protect vulnerable populations (Bohlken and Sergenti, 2010).

death penalty, and Manski and Pepper's (2018) 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

Dancygier (2010) suggests that conflicts involving immigrant groups can be usefully grouped as either immigrant-native or immigrant-state conflicts.[7] The 2005 French riots squarely fall in the latter category. The background leading to the riots is characterized by a degradation of the social climate in French immigrant populated suburbs between 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 as a result. Two of them died, the third one was severely injured. Local protesters took to the streets the next day. During the period of October 28th–31st, disturbances were circumscribed to Clichy-sous-Bois. However, on October 31st, a tear gas grenade launched by the police to disband demonstrators landed in front of a mosque. A rumor alleging that the grenade had exploded inside the mosque spread, triggering further anger, especially because it occurred during Ramadan.[8] 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.[9] Vandalism exploded. The movement had no structure, no organization, no leader, and there were no claims associated with it.[10] These nation-wide events are reminiscent of a watered-down version of the 1968 "Holy Week Uprising" in the United States.[11] More recently, they can be compared with the 2011 London riots (which actually took place over several cities) with respect to scale, ethnic composition, and criminal activity.[12] One notable difference being that, in both cases, the French riots lasted for more than twice as long.

Nicolas Sarkozy declared a "zero tolerance" policy towards urban violence and police reinforcement was dispatched to confront the rioters. President Jacques 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 those terrorist attacks that had plagued France during the Algerian War.[13] 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 entirety of Île-de-France (Paris and its seven surrounding counties) was included, although Paris proper did not experience any significant unrest.

The state of emergency can only be declared by the cabinet for an initial period of twelve days, within a specified perimeter. Any extension must be approved by Parliament. This administrative regime allows 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 torching nearly 9000 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, and 10 in Val d'Oise.[14].

During those two weeks, the police arrested close to 3000 rioters; 126 police and firefighters were injured, and there were three fatalities.

On November 14th, however, cabinet decided to extend the state of emergency, ostensibly out of precaution. On November 15th, the National Assembly (lower House) approved a three-month extension by a large majority. So did the Senate on the next day. On November 20th, Prime Minister Dominique de Villepin announced tightened controls on immigration,

[7] Immigrant-native conflicts involve sustained confrontation between immigrant communities and the local population. Immigrant-state conflicts are perpetrated by immigrant populations against state actors out of frustration at real or perceived societal disadvantages due to their ethnic status (Bleich et al. 2010; Dancygier, 2010).

[8] Kepel (2015 : 35).

[9] See maps, pictures, and graphs on https://commons.wikimedia.org/wiki/Paris_suburb_riots and a detailed report by Cazelles et al. (2007).

[10] This is consistent with ethnographic studies of soccer hooliganism (Buford, 1991; Wilkinson, 2009).

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

[12] Rioters were ethnically heterogeneous – youth of North and Sub Saharan African descent were the main two groups represented among them (Lagrange and Oberti, 2006; Mucchielli 2009; Kawalerowicz and Biggs, 2015). Unlike other immigrant-state conflicts which experienced pockets of racially charged violence – illustrated in e.g., the 1992 Los Angeles riots and the 2009 Solomon Island riots (Atkinson, 2009; Katz, 2016; Oh, 2009) – the French riots did not specifically target property owned by certain ethnic groups (Jobard, 2009; Moran and Waddington, 2016). Although Body-Gendrot (2007) disputes the use of the term riots to characterize the events, it has become standard in the literature.

[13] 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.

[14] 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 and magnitude of spikes, only one of which corresponds to the anniversary of the riots (November 2006).

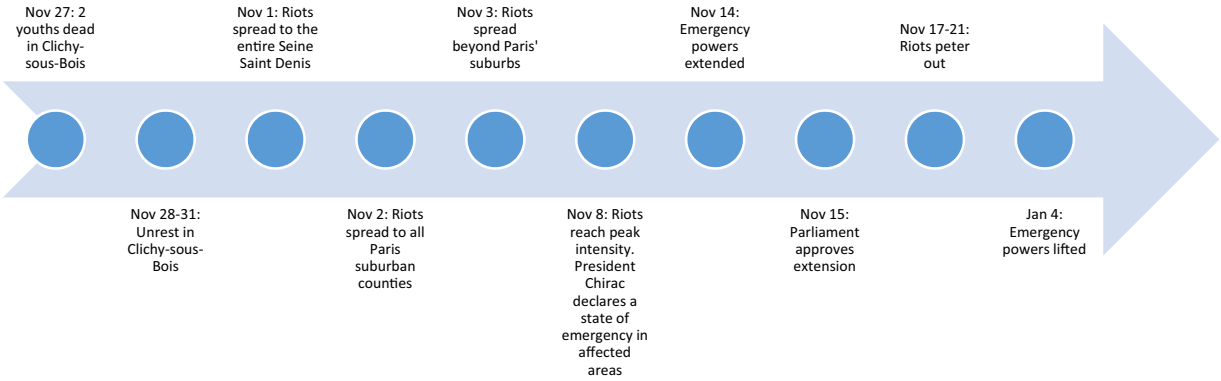

Fig. 1. Timeline of the events.

which was purported to be a determining factor in the rioting.[15] Jacques Chirac lifted the state of emergency on January 4th.[16] (Fig. 1).

The use of emergency powers was abundantly criticized among human rights advocates, legal scholars, and magistrates as disproportionate (Lebreton, 2007). Several petitions to the *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.[17] 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.[18]

Overall, Bleich et al. (2010) found that the French state response exhibited characteristics of *high repression* stemming from the combination of the state of emergency, saturated policing in "hot spot" neighborhoods and excessive official rhetoric against rioters. Yet, they point out that the French government also utilized a *medium accommodation* strategy through the allocation of additional resources to riot torn communities and the establishment of youth employment programs for low-income communities; this two-pronged approach was rather unique when compared to previous state responses in Western Europe (Moran and Waddington, 2016).

Although the underlying causes of the riots are of no direct concern to this paper, I briefly present the consensus view among those who have studied these events.[19] Scholars have emphasized hostility towards the police and state institutions in general. They have focused on the marginalization of immigrant youth of sub-Saharan and North African descent in the form 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. In contrast, Patrick Buisson, who was Nicolas Sarkozy's political advisor, explains that the riot may be understood as a reaction against the chokehold being imposed on those suburbs' underground economy: during the first ten months of 2005, Mr. Sarkozy had been building a "tough against crime" record for his upcoming presidential campaign.[20] As for the police, according to Mr. Meric, there is a dearth of internal reports, analyses, and audits concerning how the events happened and were handled, consistent with the authorities' unease over the whole episode.[21] 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). The ONDRP, created in 2004, is a public and

[15] While the concrete effects of the riots on French immigration policy were modest at best, Hernández (2010) argues that they were one of the contributing factors in passing the controversial House Bill 4437 in the United States. Republicans used the French ethnic unrest to highlight the alleged risks associated with first- and second-generation immigration from Mexico; noting that the French riots were a result of second-generation immigrants failing to integrate into their host nation's society. Similarly, controversial immigration laws were passed after ethnic riots in Rome (Selmini, 2016) and South Africa (Hammerstad, 2012).

[16] http://www.assemblee-nationale.fr/12/rapports/r2675.asp.

[17] Conseil d'État N° 286835 (November 21st 2005).

[18] http://www.lemonde.fr/societe/chat/2005/11/16/etat-d-urgence-un-risque-pour-la-democratie_710605_3224.html.

[19] See http://riotsfrance.ssrc.org/ for a review.

[20] Buisson (2016): 72–75. This is corroborated by immigrant and ethnic minority inhabitants of Clichy-sous-Bois reporting frustration and resentment towards "oppressive" drug and street crime legislation (Schneider, 2014).

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

Table 1a
Summary Statistics for French counties (unweighted), Counts per 100,000 population.

	Min	Max	Mean	Std. Dev
Violent Robbery in Public w/o a Weapon	0	95	7.4	9.9
Drug Trafficking	0	40.6	0.7	1.2
Armed Robberies	0	11.7	0.4	0.6
Use and Resale of Drugs	0	45.8	2.1	2.1
Racing and Gaming	0	126.3	0.03	0.9
Pickpocketing	0	168.9	8.3	12.1
Violent Theft w/o a Weapon against Individuals at their Residence	0	10.1	0.2	0.3

Statistics are calculated: 1996-2005.

Table 1b
Summary Statistics for Seine Saint Denis, Counts per 100,000 population.

	Min	Max	Mean	Std. Dev
Violent Robbery in Public w/o a Weapon	14.8	94.1	53.4	18
Drug Trafficking	0.6	7.9	2.5	1.3
Armed Robberies	0.2	5.2	1.4	0.7
Use and Resale of Drugs	0	7.6	1.2	1
Racing and Gaming	0	3.5	0.1	0.4
Pickpocketing	12.8	46.4	26.2	6.2
Violent Theft w/o a Weapon against Individuals at their Residence	0	0.9	0.3	0.2

Statistics are calculated: 1996-2005.

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 investigated by the public prosecution office. Thus, they exclude all contraventions (i.e., minor infractions punishable by, at most, a fine) and traffic offences, as well as nominal complaints registered in police and gendarmerie log books ("*main courante*") and not judicially processed. Records of law violations have been consistently tallied and assigned to 103 categories since inception. The public access series starts in January 1996.

To each crime corresponds a specific accounting. For example, for a homicide, the unit is the victim. For non-deadly assault, it is the complainant. For use of drugs, it is the number of perpetrators involved, etc. Therefore, the 4001 state collects "units of crime" ("*faits constatés*") that correspond to e.g., procedures, people, or objects, depending on which type of crime is being considered.

To compute rates per 100,000 inhabitants at the county level, I use yearly population statistics provided by the National Institute for Statistics and Economic Studies (INSEE). I directly study county-year-month crime rates: 100,000 $\times$ (number of reported crimes / county population). Tables 1a and 1b present summary statistics for the subset of crimes analyzed in this paper.

The ONDRP data base allows to 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.[22] A key question is the extent to which the data 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 perpetrators, as opposed to changes in crime per se. This distinction will play an important role in what follows.

One concern 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.

Another concern is that the riots could have induced a change in the reporting of certain crimes. While there is no way to remove it conclusively, the literature provides some reassurance. In their analysis of the 1967 Cincinnati race riots, Chamlin and Cochran (2000) tested the "fear-of-crime" hypothesis to identify whether there was a significant increase in violent crime reporting during the riot. Overall, they found no significant increase in victim-initiated crime reporting. Analyzing

[22] Police statistics in general provide a lower estimate of crime (Vollaard and Hamed, 2012). 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). 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.

victimization survey data, Levitt (1998) explored the hypothesis that when the size of the police force increases, changes in the perceived likelihood that a crime will be solved may lead a higher fraction of crimes to be reported to the police. He produces evidence that such reporting bias is small. In the context of stolen goods, Draca et al. (2019) similarly find that the potential biases from endogenous reporting are minimal. Bellégo and Drouard (2019) ask whether the pacification of favelas in Rio de Janeiro may have increased the reporting behavior of individuals because they may have perceived the likelihood of a crime being solved to be higher, or they may trust the institutions more. After making some assumptions to adjust for possible reporting bias their results do not change significantly for assault and robbery.[23]

Conversely, under-reporting, which would happen if e.g., the riots hampered victims' ability to file complaints, or if the riots made police services congested and thus, practically, less accessible[24] would imply a bias toward finding uninformative results.

5. Research design and methodological framework

The timing and sequence of the events rule out the hypothesis that the riots were a decoy for a concerted looting and sacking enterprise. With respect to rioting spillovers, we grapple with a conflation of offsetting effects, i.e., rioting and the inevitable, immediate police response to it, as well as the possible effect of emergency powers in the second half of the rioting period (both of which should dampen any spillover effects[25]). However, the fact that emergency powers were in force throughout December, well after the riots had ended, alleviates disentangling the impact of those two treatments. To rule out catch-up effects, I also considered 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 net spillover effect from rioting in the month of November, but the analysis proceeds in a similar way for the problem of assessing aftermath effects or opportunism from the police in the month of December – bearing in mind that police opportunism could (and, if any, should) take place irrespective of previous rioting levels.

I first focus on crime rates in Seine Saint Denis. 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, and specifically for our purposes, the bounded variation framework. The drawback of this approach is that it becomes quickly intractable when dealing with multiple comparison units simultaneously – illustrating the so-called "curse of dimensionality" plaguing all non-parametric testing. Further, as is often the case in non-experimental settings, there is no compelling comparison unit displaying the same environments and pre-riot trajectories as in the treatment unit. Hence, rather than heuristically choosing a single county that did not experience emergency powers as a comparison unit based on some common characteristics (say, population size), I construct a synthetic version of pre-November 2005 Seine Saint Denis following the strategy pioneered by Abadie et al. (2003, 2010, and 2015).[26]

The variable used to build synthetic Seine Saint Denis county is the outcome of interest itself, for all October and November months before November 2005 (for the analysis of the November riots) and for all October and December months before December 2005 (for the analysis of the emergency powers in December),[27] in all counties that did not experience emergency powers. Given that the bounded variation framework is solely outcome data driven, i.e., it does not posit a "true model" that is a function of covariates, the use of lagged values of the outcome variable is the only consistent choice.[28] Note that the making of the synthetic county thus created necessarily changes for each outcome variable considered.

5.1. Treatment effects and the selection problem

Consider the problem of inferring the treatment effect (TE) 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,

[23] In addition, the interviews of Mrs. Pasqualini and Meric suggest that the police intervention was anything but vigorous. In turn, trust in the institutions would have arguably decreased because of police restraint.

[24] In terms of police opportunism, the police would presumably have an incentive not to trigger suspicion of unethical behavior. There is no reason why the police would report made-up evidence when endowed with extra powers not intended to deal with ordinary crime.

[25] If police units were redeployed from non-rioting counties to rioting counties, this could only lead to an increase of criminal activities in non-rioting counties, and therefore the treatment effect, when measured by comparison to non-rioting counties would be biased downwards. However, if any police forces were transferred, it would have been specialized riot units (*CRS*) which have no bearing on ordinary criminality.

[26] Importantly, to the extent that there were some limited disturbances in all major urban areas, a comparison of Seine Saint Denis and of its synthetic version should produce a bias against finding any spillover effect in Seine Saint Denis.

[27] This is motivated by the fact that the invariance model and bounded variation analysis that follow only use those months. Accordingly, the mean squared prediction error is also minimized over those months. The conclusions are qualitatively unchanged when the mean squared prediction error is minimized over the entire pre-treatment period. I use the entire pre-treatment period to construct the synthetic Seine Saint Denis time series in the graphs.

[28] Kaul et al. (2018) state that researchers could "deliberately decide to construct their synthetic control unit based on past values of the outcome variables alone, because, e.g., there is no theoretical foundation for using covariates or simply because there are no covariates available due to data restrictions.

Table 2
Pickpocketing rate per 100,000 inhabitants in Seine Saint Denis and Synthetic Seine Saint Denis.

	November 2004	October 2005	November 2005
Seine Saint Denis	25.1	30.04	34.1
Synthetic Seine Saint Denis	21.17	25.58	30.05

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.[29] 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 TE boils down to:

$$TE_{jd} = Y_{jd}(1) - Y_{jd}(0) \tag{I}$$

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 TE. 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:

$$TE_{SSD,Nov2005} = Y_{SSD,Nov2005}(1) - Y_{SSD,Nov2005}(0). \tag{II}$$

The data reveal $Y_{SSD,\ Nov\ 2005}(1) \equiv Y_{SSD,\ Nov\ 2005}$, but they do not reveal the counterfactual November 2005 pickpocketing rate in Seine Saint Denis, YSSD, Nov 2005 (0).

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

These data may be used to compute simple point estimates of YSSD, 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 synthetic Seine Saint Denis (SSSD), since there is no ambiguity.

(1) Month-to-month invariance: $Y_{SSD,\ November\ 2005}(0) - Y_{SSD,\ October\ 2005} = 0$
(2) Year-to-year invariance: $Y_{SSD,\ November\ 2005}(0) - Y_{SSD,\ November\ 2004} = 0$
(3) Month-to-month change invariance:
$[Y_{SSD,\ November\ 2005}(0) - Y_{SSD,\ October\ 2005}] - [Y_{SSD,\ November\ 2004} - Y_{SSD,\ October\ 2004}] = 0$
(4) Inter-counties invariance: $Y_{SSD,\ November\ 2005}(0) - Y_{SSSD,\ November\ 2005} = 0$
(5) Year-to-year DID invariance:
$[Y_{SSD,\ November\ 2005}(0) - Y_{SSSD,\ November\ 2005}] - [Y_{SSD,\ November\ 2004} - Y_{SSSD,\ November\ 2004}] = 0$
(6) Month-to-month DID invariance:
$[Y_{SSD,\ November\ 2005}(0) - Y_{SSSD,\ November\ 2005}] - [Y_{SSD,\ October\ 2005} - Y_{SSSD,\ October\ 2005}] = 0$
(7) Triple Difference invariance (DDD):
$[Y_{SSD,\ November\ 2005}(0) - Y_{SSD\ October\ 2005}] - [Y_{SSD,\ November\ 2004} - Y_{SSD,\ October\ 2004}]$
$- \{[Y_{SSSD,\ November\ 2005} - Y_{SSSD\ October\ 2005}] - [Y_{SSSD,\ November\ 2004} - Y_{SSSD,\ October\ 2004}]\} = 0$

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

[29] 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 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. Also, note that the ethnic nature of the riots and their lack of political motivation can explain why we did not observe an influx of foreign hooligans, unlike what is now common in the trail of violent protests surrounding e.g., WTO or G20 meetings. Baudains et al. (2013) analyzed the demographic characteristics of the 2011 London rioters. They find that youth were more likely to riot close to home or in areas where they have established a social connection (e.g., a school or mall); they seldom ventured beyond these boundaries. One explanation was the need to secure escape routes should they meet resistance from authorities.

Table 3
Point Estimates of the Treatment Effect for Pickpocketing in Seine Saint Denis using Invariance Assumptions (1)–(7).

(1)	(2)	(3)	(4)	(5)	(6)	(7)
4.1	9	7.3	4.1	0.1	−0.4	0.5

(1) Month-to-month invariance assumption.
(2) Year-to-year invariance assumption.
(3) Month-to-month change invariance assumption.
(4) Inter-counties invariance assumption.
(5) Year-to-year DID invariance assumption.
(6) Month-to-month DID invariance assumption.
(7) Triple Difference invariance (DDD) assumption.
See text of Section 5.2.1 for details.

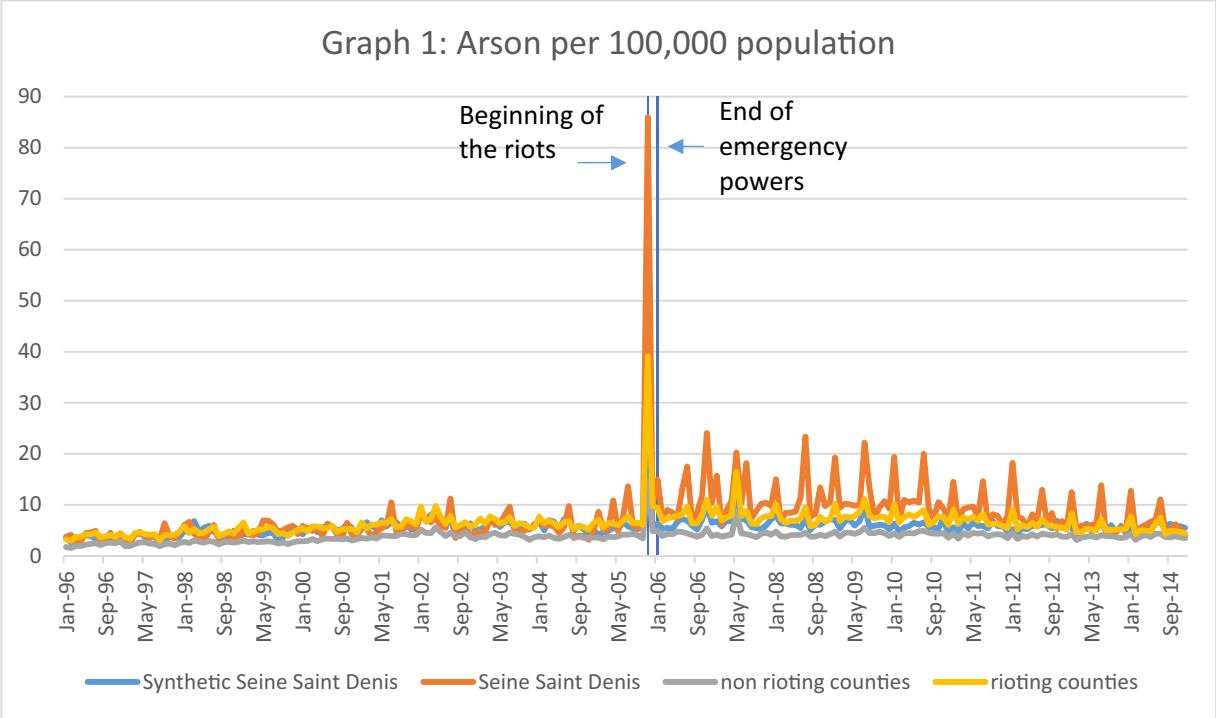

Fig. 2. Notes: Arson combines two variables, arson of public premises and arson of private premises. Synthetic SSD was constructed based on all outcome lags prior to November 2005 among non-rioting counties (see Kaul et al., 2018 for details on using outcome lags as predictors).

Not all of them are strictly positive, so we cannot conclude with confidence 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 its synthetic counterpart 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 TE show that these invariance assumptions cannot hold jointly.

To assess this idea further, Fig. 2 plots the left-hand side of those equations for years up to 2004 inclusive (i.e., these are the historical errors associated with making such invariance assumptions), as a proportion of the November pickpocketing rate in Seine Saint Denis. To interpret this graph, the closer the values to zero, and the more random their behavior around zero, the more plausible the invariance assumptions are. As expected, Invariance Assumption (4) is the least plausible, with corresponding error values always positive and the largest. Yet, even the errors associated with the various DID assumptions, while fluctuating seemingly randomly around zero, exhibit, in some years, large enough magnitudes to make them unreliable for accurately point estimating the TE in 2005. It is precisely that shortcoming that the bounded variation framework addresses in Section 5.3.

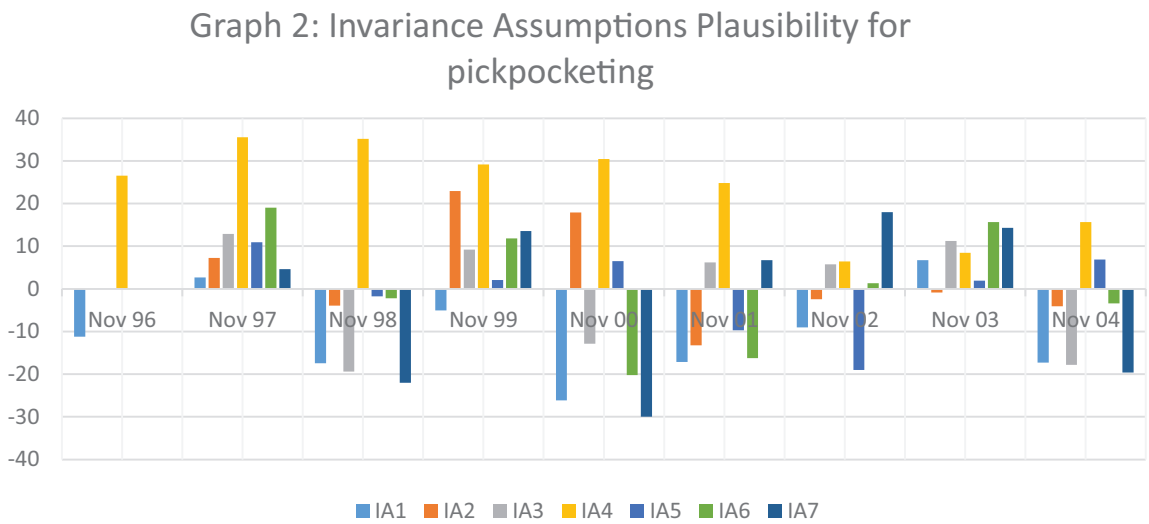

Fig. 3. This graph shows the errors associated with Invariance Assumptions (IA) (1)–(7) if those were applied to earlier years, as a proportion of the pickpocketing rate in Seine Saint Denis in November. For 1996, by construction, only IA2 and IA4 are defined. Synthetic SSD was constructed based on all November and October outcome lags prior to November 2005 among non-rioting counties.

5.2.2. Conventional approach using multiple control groups

Models evaluating the effect of exogenous shocks on a given outcome with pooled cross sections typically use all the geographical units and time periods available rather than two of each.

Consider linear models of the form:

$$Y_{ij} = \theta \cdot D_{riots\ ij} + \Sigma\, \alpha_i \times D_{1i} + \Sigma\ \beta_j \times D_{2j} + \varepsilon_{ij} \tag{IIIa}$$

$$Y_{ij} = \theta \cdot D_{riots\ ij} + \gamma\ D_{riots\ ij} \times Arson_{ij} + \delta Arson_{ij} + \Sigma\alpha_i \times D_{1i} + \Sigma\beta_j \times D_{2j} + \varepsilon_{ij} \tag{IIIb}$$

Where D_{riots} is a dummy variable denoting the occurrence of the riots,[30] $D_{riots} \times$ Arson measures rioting intensity, D_{1i}, D_{2j} are county and year-month-level time fixed effects respectively, and ε_{ij} is the standard error term.[31] Both models generalize the DID invariance assumption introduced earlier to more than two units and two periods. Further specifications with local linear time trends capture secular patterns unfolding within counties.

One may also include lags of the explanatory variables following a strategy found in e.g., Autor (2003).[32] These models correspond to a thought experiment whereby one posits that riots took place at earlier times in those counties where they did take place. The expectation, with such placebo tests, is that there should be no effect of those counterfactual events on crime.

Regarding ε, note that a random sampling assumption is harder to justify when considering geographical units as units of observation.[33] Because there is no sampling process that would be intuitive in the present application[34] one should rather think of the error term as measurement error and make a heuristic assumption of normality, independence and identical distribution of the residuals.[35] The standard errors can, however, be safely clustered by county given that there are ninety-six of those in metropolitan France, which is well above the minimum recommended by the robust clustering literature (Cameron and Miller, 2017).

[30] 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.

[31] In Model (b), the variable D_{riots} is an intercept and the coefficient has no proper economic interpretation.

[32] In the following, the placebos will be tested for October 2005 and November 2004 – and their interaction with the contemporaneous arson rates in type (b) specifications.

[33] 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."

[34] Following Manski and Pepper (2018), 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."

[35] To keep the discussion short, I do not introduce a semi-parametric version of the OLS model developed for the problem at hand in an earlier version of this paper, available upon request. That semi-parametric model relies on two assumptions: a) a conditional mean of zero of the error term and, and b) positing that the counterfactual observation $Y_{Nov\ 2005}(0)$ is bounded by the value of its least square estimate plus and minus the Max and Min values (or other extreme points of the distribution) of the OLS pre-November 2005 residuals. It captures the idea that the counterfactual quantity $Y_{Nov\ 2005}(0)$ would not have deviated from an OLS prediction more than the actual observations least consistent with the model.

5.3. Bounded-variation assumptions

Manski and Pepper (2013, 2018) show how bounded-variation assumptions 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 when a natural experiment framework is unavailable. 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 inference that arises from those.

Bounded-variation assumptions cannot point-identify TEs. 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 assumptions 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 TE, I conservatively choose from historical precedent "worst case scenarios." In other words, I select the most extreme outcomes in the data which would run against finding a non-ambiguous TE. Loosely speaking, this flips the logic of invariance assumptions on its head: the larger the historical errors associated with a given invariance assumption (see Graph 2), the more convincing the finding of a TE if the corresponding bounds provide an unambiguous response.

Parting a second time with Manski and Pepper (2018), however, my preferred specification does 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. Yet, Manski and Pepper (2018) go one step further 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 (2018), this would translate as an assumption of the form $|Y_{\text{November 2005}}(0) - Y_{\text{October 2005}}| \leq \delta$, where δ is a parameter that can be increased to weaken invariance assumption (1). Imposing a δ that makes the difference of interest at least equal to the maximum of earlier November / October absolute value differences amounts to

$$|Y_{\text{November 2005}}(0) - Y_{\text{October 2005}}| \leq \text{Max}_{T\leq 2004}|Y_{\text{November}T} - Y_{\text{October}T}|. \tag{IV}$$

This almost always leads to wider bounds than assuming:

$$\text{Min}_{T\leq 2004}\{Y_{\text{November}T} - Y_{\text{October}T}\} \leq Y_{\text{November2005}}(0) - Y_{\text{October2005}} \leq \text{Max}_{T\leq 2004}\{Y_{\text{November}T} - Y_{\text{October}T}\} \tag{V}$$

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.[36] 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 bound-based identification assumption (which, by construction, does not correspond to any plausible invariance assumption) whereby: $\text{Min}_{T \leq 2004} Y_{\text{November } T} \leq Y_{\text{November 2005}}(0) \leq \text{Max}_{T \leq 2004} Y_{\text{November } T}$. This alternative assumption posits that the counterfactual outcome $Y_{\text{November 2005}}(0)$ would not have been smaller than the minimum or larger than the maximum values of the corresponding outcome observed in the same month in prior years.

The main question is whether the bounds yield a non-ambiguous sign for the TE, and if so, whether the magnitude of the TE 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.[37] First, the approach just mentioned could be misleading in the presence of outliers – Min- and Max-based estimators being the most sensitive to outliers. Consequently, 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 $(n\text{-}1)^{th}$ 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 closer history. Therefore, instead of taking the Min and Max values from the entire pre-riot period, another

[36] 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.

[37] Conversely, they can also be strengthened by considering multiples of the maximum and minimum; or by adopting the original approach pursued by Manski and Pepper, as shown below.

Table 4
OLS regressions effects of the November 2005 riots.

	Violent Robbery in Public w/o Weapon				Use & Resale of Drugs			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
D_Riots	2.64	−6.38	1.75	−5.59	−0.7	−0.72	−0.56	−0.53
	(1.4)*	(3.22)*	(1.14)	(2.57)**	(0.31)**	(0.46)	(0.31)*	(0.51)
D_Riots × Arson		0.21		0.18		4×10^{-3}		-3×10^{-4}
		(0.11)*		(0.09)**		(0.01)		(0.01)
Arson		0.08		0.06		-5×10^{-3}		-8×10^{-4}
		(0.03)***		(0.02)***		(0.01)		(0.01)
October placebos significant	No	No	No	No	No	No	No	No
November placebos significant	No	No	No	No	No	No	No	No
Time Trends	No	No	Yes	Yes	No	No	Yes	Yes
Adjusted R^2	0.87	0.94	0.95	0.95	0.15	0.15	0.17	0.17
N	6816	6816	6816	6816	6816	6816	6816	6816

All crime rates defined as (100,000 × crime count)/county population. All regressions control for time (year/month) fixed effects and county fixed effects. Robust standard errors clustered by county (96 clusters). Period considered: January 2000-November 2005. D_Riots is a dummy coding for those counties where riots took place in November 2005. D_Riots × Arson is a measure of rioting intensity. Note that when including D_Riots × Arson, D_Riots becomes an intercept and there is no proper economic interpretation of the coefficient.
"Placebos significant" corresponds to including dummy variables corresponding to those counties that experienced riots in November 2005 coded as 1 for October 2005 and November 2004 (and those dummies interacted with arson at those times, in even-numbered columns). "No" means none of the corresponding coefficients are significant at the 5% level, either individually or jointly.
* 10% statistical significance.
** 5% statistical significance.
*** 1% statistical significance.

bounded-variation assumption will be based on Min and Max values from October 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. Appendix B uses Mrs. Meric's and Pasqualini's observations to supplement the discussion of the quantitative results for Seine Saint Denis county.

6. Discussion of results

6.1. Effect of rioting on criminal activity (November 2005)

The following subsections analyze those crimes which, from a first pass examination of the raw data, appear to rise disproportionately in November 2005 in the rioting areas. It is worth noting that none of the categories of crime analyzed experienced any noticeable change in the different synthetic Seine Saint Denis constructs using the within-county bounded-variation assumptions corresponding to invariance assumptions (1)–(3) of Section 5.2.1, thus providing additional validation of their suitability as comparators.

6.1.1. Across counties

One criminal spillover pattern associated with the riots is the rise of violent thefts (Fig. 4). To illustrate, Table 4 tests the hypothesis that the riots led to an increase of unarmed, street level, violent robbery.[38]

The average effect (Column 1) represents a relative increase of nearly 2 incidents per 100,000 population. This effect, however, is statistically insignificant in a model containing county specific linear time trends (Column 3). However, there is a robust, statistically significant positive correlation between violent thefts in public and rioting intensity (Columns 2, 4),

[38] This variable aggregates two variables in the ONDRP, namely "Violent Robbery in Public Without a Weapon committed against Women" and "Violent Robbery in Public Without a Weapon committed against Others". Note that it is the latter which drives the result.

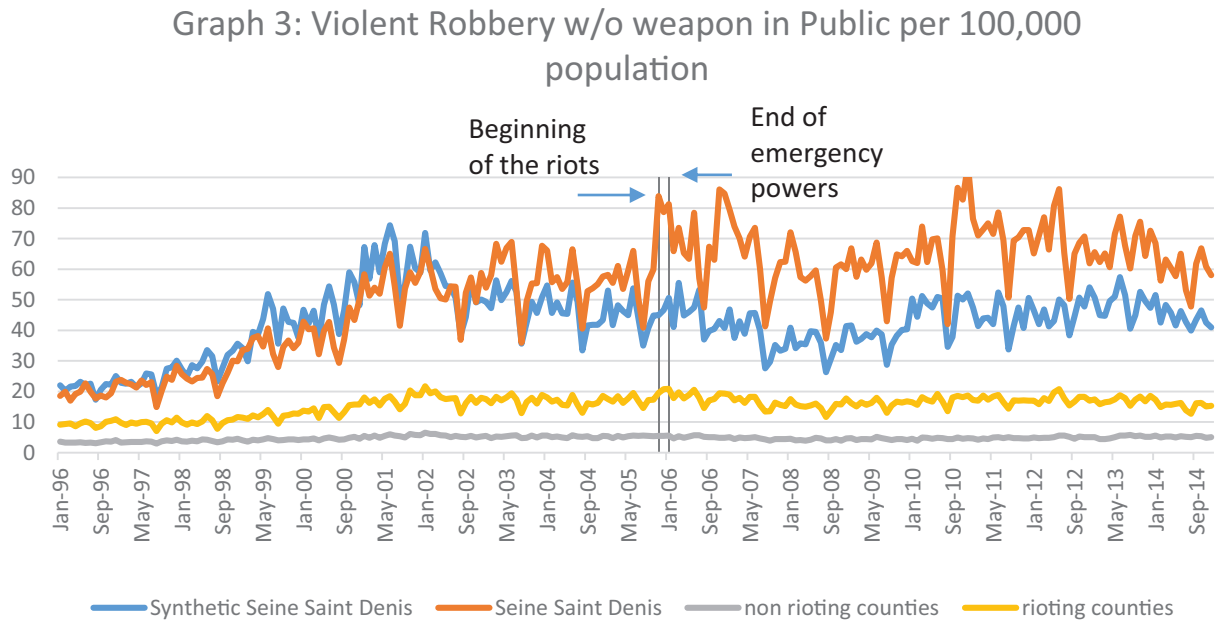

Fig. 4. Note: Violent Robbery w/o a Weapon combines two variables, i.e., against women specifically, and against others. Synthetic SSD was constructed based on all outcome lags prior to November 2005 among non-rioting counties.

suggesting the presence of a threshold effect. The coefficient shows an average five percentage point in arson leading to one extra percentage point in such violent robbery.

It is difficult to tease out a purely opportunistic effect of the riots from a reduction of the attachment to civil society or the rule of law.[39] Further, one could not accurately 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.' 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 general underreporting of petty theft. Still, other types of crime of the same caliber, such as "Other Simple Thefts in Private Premises," cannot be traced to the riots under any specification. Among the list of unsophisticated crimes, "Trespassing," "Assault and Battery," and "Threat or Blackmail" experience slight increases relative to non-rioting counties over previous November rates, but regression results are mixed. Those forms of theft requiring planning (such as "Burglary of Industrial/Commercial/Financial Premises") show no significant increase, confirming that the riots were a spontaneous phenomenon and not some elaborate smokescreen designed by organized crime to engage in more illegal behavior.[40]

The police may have decided to revisit priorities. Columns 5 and 7 show a statistically significant relative decrease of approximately 0.6 use/ resale of drugs related incident per 100,000 population. Columns 6 and 8 show that the effect is not proportional to rioting intensity. Over the period considered, the regression coefficient conflates two effects: an absolute decrease of 14% in counties affected by the riots, together with an increase of 28% in unaffected areas.

That the police would slow down their fight against drug use in the middle of riots is understandable. It echoes a similar decrease with respect to gaming and racing law violations, discussed later. An alternative interpretation suggested by Messrs. Meric and Pasqualini (see Appendix) is that routine, low level drug transactions were hampered by the riots. The reasons for the increase in unaffected counties are less clear: it could be that if the supplying of drugs had become more difficult in rioting counties traffic was diverted to other areas, or that the higher alertness of police forces across the country led to more arrests.

6.1.2. Effect of the rioting in Seine Saint Denis

In Table 5, focusing on the outcome of "Violent Robbery in Public Without a Weapon", the benchmark specification (row A) yields an unambiguous sign, and the union of the bounds yields a TE interval ranging from 15 to 65 extra criminal acts per 100,000 population. The intersection of all the bounds is non-empty.

To give another idea of the magnitude of the effect, the November 2005 rate in Seine Saint Denis represents a 70% increase relative to its synthetic counterpart over all previously recorded November rates (+45% since 2000).[41]

[39] For, if the latter holds, it suggests that the effects of the riots on crime may have persisted after the riots ended, which the data support.

[40] This paragraph draws from unreported results, available upon request.

[41] This is similar to the findings reported for the 1992 Los Angeles riots where street level violent robbery surged, however, these robberies were often conducted with firearms due to their ease of access (Tierney and Baldassare, 1994).

Table 5
Non-parametric bounds on the treatment effect violent robbery in public w/o a weapon per 100,000 (Seine Saint Denis, November 2005).

	(S) Simplest	(I) Month to Month	(II) Year to Year	(III) Monthly Change	(IV) Inter county	(V) Year to Year DID	(VI) Month to Month DID	(VII) DDD
(A) Min-Max	[28, 65]	[20, 32]	[17, 35]	[15, 29]	[26,56]	[15, 29]	[18, 26]	[20, 28]
(B) 2nd & (n-1)th order	[29, 60]]	[23, 28]	[20, 29]	[18, 27]	[27, 49]	[20, 27]	[20, 24]	[21, 25]
(C) Min–Max 2000–2005	[28, 41]	[23, 32]	[17, 35]	[15, 27]	[26, 28]	[15, 29]	[23, 26]	[20, 25]
(D) Manski-Pepper	[28, 83]	[16, 32]	[17, 41]	[15, 31]	[26, 84]	[15, 40]	[17, 28]	[17, 28]

Crime rates defined as (100,000 × crime count)/county population.
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. Row (D) provides bounds using the approach followed by Manski and Pepper (2018). All numbers rounded to the nearest integer.

Other crimes, including more serious ones where a planning component is involved, do not exhibit convincing escalation patterns.

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 that local emergency powers persisted (until early January).

6.2.1. Across counties

To analyze the effects of the emergency powers over the entire country, I modify models (III) (a) and (b) slightly in the following way:

$$Y_{ij} = \theta\, D_{\text{emergency, no riot ij}} + \gamma\, D_{\text{riots ij}} + \Sigma\alpha_i \times D_{1i} + \Sigma\, \beta_j \times D_{2j} + \varepsilon_{ij} \tag{VIa}$$

$$\begin{aligned} Y_{ij} = {} & \theta\, D_{\text{emergency, no riots ij}} + \gamma\, D_{\text{riots ij}} + \zeta\, D_{\text{riots ij}} \times \text{Arson}_{ij} + \delta \text{Arson}_{ij} \\ & + \mu D_{\text{emergency, no riots ij}} \times \text{Arson}_{ij-1} + \lambda \text{Arson}_{ij-1} + \Sigma\alpha_i \times D_{1i} + \Sigma\beta_j \times D_{2j} + \varepsilon_{ij} \end{aligned} \tag{VIb}$$

Where $D_{\text{emergency, no riot}}$ is a dummy taking a value of 1 if the county is under emergency powers in December.[42] In the second model, arson is lagged to capture the rioting intensity of the previous month.[43]

We first observe that the pattern of violent robbery in public remained at a significantly higher than average level in counties under emergency powers (Columns 1, 3), and the regression coefficients are statistically more significant than in November and reveal a substantively similar increase.[44] Further, violent robbery in public in December is significantly positively correlated with the intensity of rioting in the preceding month in those counties (Columns 2, 4), echoing the similar result found for the month of November. Plausibly, thieves may 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.

With respect to incidents which may be 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, one notable feature concerns armed robberies, (merging the categories of "Armed Robbery of Commercial/Industrial premises" "Armed Robbery of Financial premises," and "Armed Robberies of Cash Transport") (Fig. 5). Columns (5) and (7) exhibit a statistically significant TE.[45] However, the effect is not a function of rioting intensity (Columns (6) and (8)). It is worth noting that the positive coefficients measure a relative increase: armed robberies decreased globally in December 2005, however significantly less so in those counties which had experienced riots. Further, because of the negative significant coefficient in November, one cannot rule out a catch-up effect.

[42] For the study of opportunistic crimes and police's permissiveness, Paris is treated as a non-emergency county, since what matters for that purpose is whether rioting occurred in the preceding month. Conversely, for the study of the police active use of emergency powers, Paris is treated as an emergency county.

[43] The placebos used here now correspond to counterfactuals in October 2005, November 2004, and December 2004 in specification (a), as well as interactions with contemporaneous arson for October 2005 and November2004, and with the interaction of November arson 2004 and December 2004 in specification (b).

[44] Although the December 2004 placebo was found to be statistically significant at the 5% level in specifications (1) and (3), the coefficient is three times smaller. The test of equality between the December 2004 and December 2005 coefficients is rejected at the 10% level. This result must be likely attributed to the disturbances that traditionally plague large city suburbs on New Year's Eve (Collet, 2006).

[45] Again, the December 2004 placebo was found to be statistically significant at the 5% level in specification (7), likely for the same reason previously mentioned. While the coefficient is smaller, the test of equality between the coefficients is not rejected. The significance of the October 2004 placebo in column (5) is more puzzling; however, this time the coefficient was negative, and it does not appear in the other specifications.

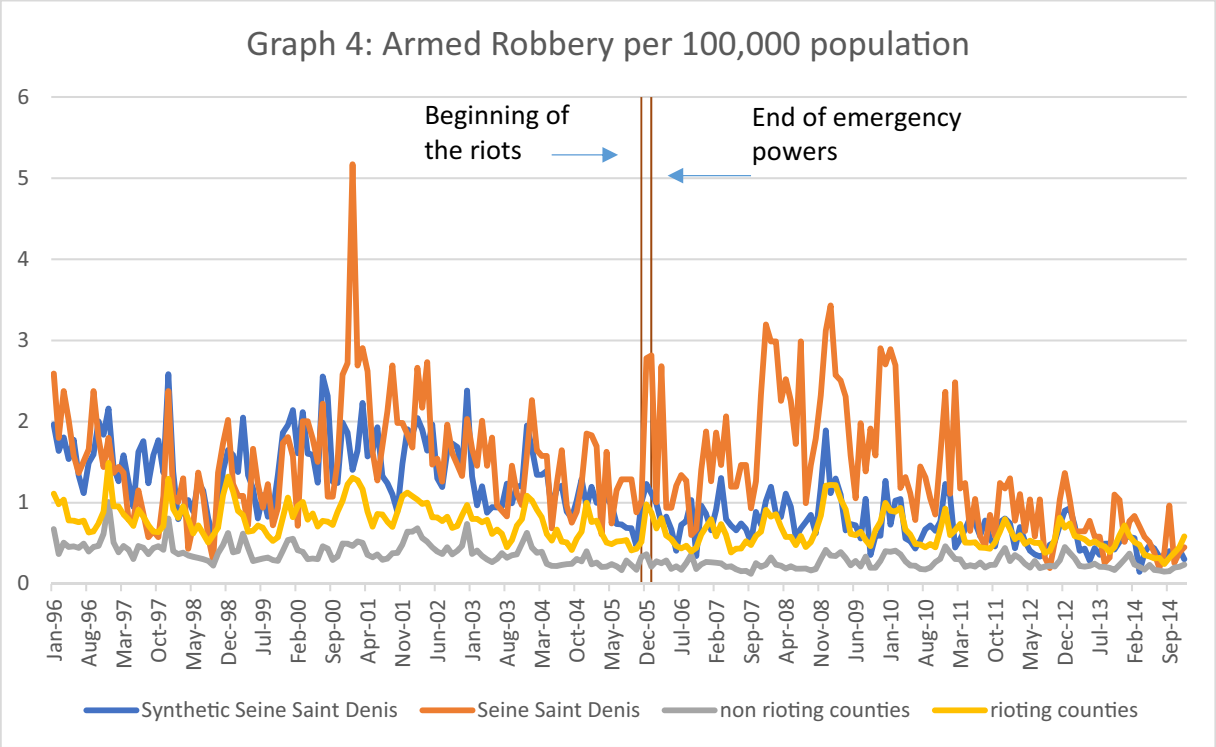

Fig. 5. Note: Armed Robbery combines three variables, i.e., "Armed Robbery of Commercial/Industrial premises" "Armed Robbery of Financial premises," and "Armed Robberies of Cash Transport". Synthetic SSD was constructed based on all outcome lags prior to November 2005 among non-rioting counties. The January 2001 outlier is not a coding error. There was an unprecedented wave of armed robberies in Seine Saint Denis that month and it made the news, see: http://www.leparisien.fr/seine-saint-denis-93/les-braqueurs-ecument-le-departement-22-01-2001-2001907393.php.

In any event, it appears that the emergency powers had no deterrent effect – at least not enough to counterbalance the erosion of the rule of law from the month of November.

For crimes the measurement of which is indicative of police effort, there is evidence of significant decrease in the number of incidents related to racing and gaming law violations (Columns 1–4), echoing an earlier finding of the same magnitude for the month of November. Incidents related to drug use (unreported) also drop, however not statistically significantly relative to the rest of the country that experienced a parallel pattern. The decrease in the number of "recreative infractions" over the entire period of the emergency powers in the riot areas, and to some extent elsewhere as well, points to permissiveness as the chosen mode of conflict resolution and, plausibly, prophylaxis.[46]

A more important question is the effect of emergency powers on "Drug Trafficking and Sale without Use" (Columns 5–8). This is because the fight against drug trafficking arguably benefits from the possibility of warrantless search and seizure enabled by the emergency powers.[47] The significant increase displayed in Column (5) is not robust to the introduction of county specific trends (Column 7). In any event, it turns out that the relative increase being measured is driven by the drop characterizing the rest of the country that month. This result is, therefore, inconclusive with respect to a claim of police opportunism.[48]

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. Ordinary crime remained at a high level. We find escalation effects exacerbated in Seine Saint Denis. Bounds on the TE are again consistently strictly positive for "Violent Robbery in Public Without a Weapon." The range for the TE across all bounds in the benchmark specification spans [4, 55].

[46] 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.

[47] This observation is confirmed ex post by the backhanded use of emergency powers to bust drug trafficking following the 2015 terrorist attacks in Paris. See http://www2.assemblee-nationale.fr/static/14/lois/communication_2016_03_30.pdf.

[48] Paris was not affected by any significant rioting but was under emergency powers throughout the period. Paris therefore provides an opportunity to analyze the effect of emergency powers net of indirect, possibly contaminating effects. Despite some intriguing patterns (for example, the rate of tax evasion cases is the highest in December 2005 when considering the entire 1996-2005 period), overall, the conclusion of the bounded variation framework applied to Paris in December 2005 is that the emergency powers did not lead to a clearly perceptible increase in the caseload of judicial procedures and one must therefore reject the hypothesis of higher police efficiency.

Table 6
OLS regressions.

(a)	Violent Robbery in Public w/o a Weapon				Armed Robberies			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
D_Riot	2.66	−6.38	1.8	−5.51	−0.23	0.14	−0.1	0.18
	(1.44)*	(3.19)**	(1.14)	(2.53)**	(0.1)**	(0.17)	(0.08)	(0.17)
D_Riots × Arson		0.21		0.17		−0.02		−0.02
		(0.1)**		(0.08)**		(6×10^{-3})***		(6×10^{-3})***
D_Emergency, no Riot	3.86	−4.65	2.97	−3.72	0.2	0.03	0.34	0.06
	(1.21)***	(2.82)	(1.02)***	(2.04)*	(0.1)**	(0.2)	(0.1)***	(0.24)
D_Emergency, no Riot × $Arson_{-1}$		0.23		0.19		-2×10^{-3}		-5×10^{-4}
		(0.1)**		(0.08)**		(6×10^{-3})		(7×10^{-3})
Arson		0.08		0.06		0.01		0.01
		(0.03)***		(0.02)**		(5×10^{-3})**		(5×10^{-3})**
$Arson_{-1}$		0.01		−0.02		0.01		0.01
		(0.02)		(0.03)		(4×10^{-3}) ***		(4×10^{-3})***
October Placebos significant	No	No	No	No	Yes	No	No	No
November Placebos significant	No	No	No	No	No	No	No	No
December Placebos significant	Yes	No	Yes	No	No	No	Yes	No
Time Trends	No	No	Yes	Yes	No	No	Yes	Yes
Adjusted R^2	0.94	0.94	0.95	0.95	0.55	0.57	0.55	0.58
N	6912	6912	6912	6912	6912	6912	6912	6912
(b)	Racing and Gaming				Drug Trafficking / Resale w/o Use			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
D_Riot	−0.03	−0.01	−0.02	−0.03	−0.05	0.12	−0.07	0.22
	(0.02)**	(0.02)	(0.02)	(0.03)	(0.09)	(0.21)	(0.1)	(0.24)
D_Riots × Arson		10^{-5}		10^{-3}		-3×10^{-3}		-9×10^{-3}
		(7×10^{-4})		(6×10^{-4})*		(8×10^{-3})		(8×10^{-3})
D_Emergency, no Riot	−0.04	−0.02	−0.03	−0.04	0.18	0.18	0.08	−0.05
	(0.02)***	(0.03)	(0.01)**	(0.02)*	(0.08)**	(0.2)	(0.08)	(0.15)
D_Emergency, no Riot × $Arson_{-1}$		-8×10^{-4}		3×10^{-4}		2×10^{-3}		4×10^{-3}
		(10^{-3})		(9×10^{-4})		(6×10^{-3})		(4×10^{-3})
Arson		-10^{-3}		-10^{-3}		-2×10^{-3}		2×10^{-3}
		(6×10^{-4})**		(6×10^{-4})**		(5×10^{-4})		(4×10^{-3})
$Arson_{-1}$		4×10^{-4}		2×10^{-4}		-4×10^{-3}		-10^{-3}
		(9×10^{-4})		(9×10^{-4})		(4×10^{-4})		(3×10^{-3})
October Placebos significant	No	No	No	No	No	No	No	No
November Placebos significant	No	No	No	No	No	No	No	No
December Placebos significant	No	No	No	No	No	No	No	No
Time Trend	No	No	Yes	Yes	No	No	Yes	Yes
Adjusted R^2	0.06	0.06	0.09	0.09	0.51	0.51	0.53	0.53
N	6912	6912	6912	6912	6912	6912	6912	6912

All crime rates defined as (100,000 × crime count)/county population. All regressions control for time (year/month) fixed effects and county fixed effects. Robust standard errors clustered by county (96 clusters). Period considered: January 2000-December 2005. D_Riots is a dummy coding for those counties where riots took place in November 2005. D_Riots × Arson is a measure of rioting intensity. D_Emergency, no Riot is a dummy coding for December 2005 for those counties where riots took place in November plus Paris. D_ Emergency, no Riot × $Arson_{-1}$ is a measure of rioting intensity in the preceding month. Note that when including D_ Emergency, no Riot × $Arson_{-1}$, D_Emergency, no Riot becomes an intercept and there is no proper economic interpretation of the coefficient. The same applies to D_Riots and D_Riots × Arson.
"October Placebos significant" corresponds to including a dummy variable corresponding to those counties that experienced emergency powers in November-December 2005 coded as 1 for October 2005. Same for November and December 2004 placebos. In even-numbered columns October 2005 and November 2004 placebos are interacted with contemporaneous arson, and December 2004 is interacted with November 2004 arson "No" means none of the corresponding coefficients are significant at the 5% level, either individually or jointly.

* 10% statistical significance.
** 5% statistical significance.
*** 1% statistical significance.

The December 2005 rate represents a 65% increase relative to the synthetic control over the average of previously recorded December months (+43% since 2000). This pattern was missed by earlier studies since those focused on the rioting period exclusively.

Regarding more serious crimes, all the TE ranges are strictly positive for "Violent Thefts Without a Weapon against Individuals at their Residence" in the benchmark specification. The corresponding ranges span an interval of [0.1, 0.6]. While apparently modest in absolute terms this increase reflects an average change of +79% relative to the synthetic control over previously recorded December months (+70% since 2000).

As well, "Other Thefts with a Bladed Weapon" and "Armed robberies" experienced substantial increases in absolute terms, and relative to their synthetic counterparts over the average of previously recorded December rates, with no evidence a catch-up effect. However, for these two outcomes, two or more bounds yield an ambiguous result. These crime categories,

Table 7
Nonparametric bounds on the treatment effect violent robbery in public w/o a weapon per 100,000 (Seine Saint Denis, December 2005).

	(S) Simplest	(I) Month to Month	(II) Year to Year	(III) Monthly Change	(IV) Inter county	(V) Year to Year DID	(VI) Month to Month DID	(VII) DDD
(A) Min-Max	[11, 55]	[6, 19]	[7, 31]	[4, 23]	[11, 52]	[18, 43]	[7, 22]	[13, 38]
(B) 2nd & (n-1)th order	[20, 50]	[14, 19]	[12, 21]	[11, 19]	[18, 47]	[20, 25]	[14, 20]	[17, 29]
(C) Min–Max 2000–2005	[11, 23]	[6, 19]	[7, 31]	[4, 23]	[11, 29]	[18, 43]	[7, 22]	[13, 38]
(D) Manski Pepper	[11, 79]	[6, 31]	[7, 35]	[4, 26]	[11, 79]	[11, 43]	[7, 34]	[7, 38]

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. Row (D) provides bounds using the approach followed by Manski and Pepper (2018).

Table 8
Nonparametric bounds on the treatment effect violent theft w/o a weapon at people's residence per 100,000 (Seine Saint Denis, December 2005).

	(S) Simplest	(I) Month to Month	(II) Year to Year	(III) Monthly Change	(IV) Inter county	(V) Year to Year DID	(VI) Month to Month DID	(VII) DDD
(A) Min-Max	[0.2, 0.5]	[0.3, 0.6]	[0.3, 0.6]	[0.1, 0.6]	[0.3, 0.5]	[0.4, 0.6]	[0.3, 0.6]	[0.2, 0.6]
(B) 2nd & (n-1)th order	[0.2, 0.5]	[0.3, 0.6]	[0.3, 0.6]	[0.1, 0.6]	[0.4, 0.5]	[0.4, 0.5]	[0.3, 0.5]	[0.2, 0.6]
(C) Min–Max 2000–2005	[0.3, 0.5]	[0.3, 0.6]	[0.3, 0.6]	[0.1, 0.6]	[0.4, 0.5]	[0.4, 0.6]	[0.3, 0.5]	[0.3, 0.6]
(D) Manski Pepper	[0.2, 0.6]	[0^+, 0.6]	[0.3, 0.6]	[0^+, 0.6]	[0.3, 0.5]	[0.4, 0.6]	[0.3, 0.6]	[0.2, 0.6]

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. Row (D) provides bounds using the approach followed by Manski and Pepper (2018).

as well as others, remained unusually high into March, receding to pre-riot levels afterwards for some, while others (such as "Assault and Battery," or "Threat or Blackmail") seem to have switched to a higher cruising regime, consistent with the higher post-rioting 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.[49]

This persistently higher level of crime, which subsisted during the period of the emergency powers and beyond, suggests external validity insofar as it echoes a similar pattern detected after the Cincinnati riots in 2001 (Shi, 2009), and more generally resonates with the consequences of de-policing strategies observed elsewhere (Heaton, 2010; MacDonald, 2017), even though the French authorities did not publicly endorse changes in their law enforcement practice. Conversely, the French treatment of the riots may be contrasted with the swift police and judicial response to the 2011 London riots (Bell et al., 2014).

7. Conclusion

Urban violence is a symptom of fractures running deep in society: growing economic inequalities, austerity measures, political repression, as well ethnic tensions, make it likely that riots will reoccur at a high frequency across the globe (Badiou, 2012; Clover, 2016; Clement, 2019). The yellow vest protest movement that sprang all over France in October 2018 illustrates this trend.[50] Likewise, additional powers 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 a year-month-/county-level fixed effects difference-in-difference regression framework applied to the entire country with a novel, non-parametric bounded-variation assumptions methodology (Manski and Pepper, 2018), combined for the first time with the synthetic control approach (Abadie et al., 2003, 2010, and 2015), to study the area most

[49] This paragraph draws from unreported results, available upon request.

[50] Incidentally, there is a debate on the origins of the high level of criminality observed after October 2018 – which the methods used in this paper could help settle. Some, like police union representatives and the French government, blaming the phenomenon on the yellow vest movement, while others, like the ONDRP's head and French criminologist Xavier Raufer, exercise more caution when assigning any causal connection, see for example: https://www.lexpress.fr/actualite/societe/pourquoi-il-est-complique-de-lier-la-hausse-de-la-delinquance-aux-gilets-jaunes_2072482.html; https://www.atlantico.fr/decryptage/3570301/criminalite–pourquoi-attribuer-la-hausse-de-la-criminalite-aux-gilets-jaunes-releve-de-la-mauvaise-foi-gouvernementale-xavier-raufer.

affected by the riots. With respect to measuring criminal spillovers or the deterrence effects of various policies, this methodology can be applied to other granular data sets such as the British Crime Report Information System, or the American Uniform Crime Reports. Applications need not be circumscribed to criminal matters though, and the potential for studying the consequences of localized exogenous shocks where standard parametric methods would be impractical is large.

The evidence presented in this paper shows that the riots resulted in a substantial increase in impulsive, unsophisticated, violent thefts in public. Conversely, crimes that require a minimum amount of planning did not soar. Violent thefts in public remained abnormally high more than two weeks after the riots ended, despite the state of emergency still being in force, and persisted at higher than pre-riot levels for months. Meanwhile, more serious violent crimes increased markedly in Seine Saint Denis, which was the epicenter of the riots. This narrative is consistent with the accounts of those events provided by Mr. Jacques Meric and Mr. Bernard Pasqualini, heads of the Seine Saint Denis police and head of Seine Saint Denis public safety respectively, during the riots, whom I interviewed. The evidence of opportunistic, targeted crackdown on crime unrelated to rioting during the period of emergency powers remains inconclusive at best.

If, as Chalfin and McCrary (2017) 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 – at least not enough to counterbalance the longer-run effects of the riots. 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 lighter criminal pursuits (e.g., related to drug use and gaming), likely triggered an emboldening effect among young hoodlums, inspiring them to proceed to the next level on the criminal spectrum within a matter of weeks.[51]

Yet, this is not to suggest that 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 exemplified the optimal response. On the one hand, there is evidence that repressive conflict management strategies are generally successful against rioters (Verma, 2007; King, 2013). Brown (2015), analyzing separate events from Chicago in the early 1990s, showed the substantial effect of increasing police presence on property damage and looting incidents. In the context of the 2011 London riots, Baudains et al. (2013) noted that police were able to effectively deter rioters by impeding their ability to mobilize in specific areas through blockades and increasing their presence in "hot spots". On the other hand, such policing techniques can potentially exacerbate violence; this is especially relevant during crises where ethnic minority rioters meet with predominantly white police forces (Bleich et al., 2010; Lieberson and Arnold, 1965; Steidly and Ramey, 2019). Hinkle and Weisburd (2008) advance that strategies based on repression alone make communities less likely to cooperate with police and contribute to a breakdown in the relationship between ethnic minorities and police (Howell, 2016; Thompson, 2015; Klien, 2012; Moran and Waddington, 2016).

A few limitations of this work must be acknowledged. First, from an operational standpoint, as Mr. Pasqualini pointed out, the lack of coordination among rioters would likely not repeat itself nowadays because of the advent of smartphones and social media. In turn, we should not expect to observe the same criminal patterns uncovered here, or lack thereof, during and following a riot.[52]

Second, the absence of opportunistic behavior with respect to the emergency powers that characterized the police in 2005 is not to be taken for granted. A nation-wide state of emergency was declared following the November 2015 terrorist attacks in Paris. Within weeks, Human Rights Watch and Amnesty International released reports signaling police abuse.[53] The media also related more stories pointing in that direction than in 2005.[54] Third, the present research analyzed the behaviors of criminals and the police during a riot and a period of emergency powers. It did not touch on the larger branch of literature devoted to the factors that explain the occurrence of riots (Olzak and Shanahan, 1996; Olzak et al., 1996; DiPasquale and Glaeser, 1998)), their modeling as adaptive contagion processes (Davies et al., 2013), their longer-term consequences (Collins and Margo, 2007), or the various public policy measures that have been crafted to address the causes of riots (Iris, 1983; Bleich et al., 2010). Finally, the specificity of the French context may question the generalizability of the findings. France remains officially in denial of the ethnic dimension of its social fractures, including those that culminate in mass scale violence. This is because France has been trying to preserve the fiction of a color-blind, indivisible country

[51] This latter result may be contrasted with the spillover effects of other criminal justice policies along an extensive margin, such as displacement (e.g., Tabarrok and Helland (2009), Bronars and John (1998)), or, as suggested here, along an intensive margin (e.g., Iyengar (2008); Caetano and Maheshri (2018)).

[52] However, these new technologies can also benefit police services during riots. Schneider and Trottier (2012) and Rizza et al. (2014) provide evidence that social media and smartphones enabled ordinary citizens to assist the police during the 2011 Vancouver riot. This was done through tagging and sharing videos, images, and audio recordings; leading to the identification and arrest of multiple rioters. The same phenomenon of "citizen-government" collaborative strategies of riot reduction was observed during the 2011 London riot (Gasco et al., 2017; Panagiotopoulos et al., 2014).

[53] https://www.hrw.org/fr/news/2016/02/03/france-abus-commis-dans-le-cadre-de-letat-durgence; http://www.amnesty.fr/Nos-campagnes/Liberte-expression/Dossiers/Etat-urgence-une-efficacite-contestable-et-contestee-17401

[54] Notably, criticism over police excessive use of the emergency powers arose across the political spectrum, e.g.: http://rue89.nouvelobs.com/2015/11/21/1955-2015-avons-a-craindre-letat-durgence-262204; http://www.lepoint.fr/societe/les-abus-de-l-etat-d-urgence-25-11-2015-1984649_23.php; http://tempsreel.nouvelobs.com/societe/etat-d-urgence/20151127.OBS0323/carte-etat-d-urgence-panorama-des-abus-et-mesures-controversees.html; http://www.lemonde.fr/les-decodeurs/article/2015/11/23/les-petites-bavures-de-l-etat-d-urgence_4815803_4355770.html; http://www.huffingtonpost.fr/2015/11/26/etat-urgence-carte-plaintes-controverses_n_8653882.html In one such stories, a judge struck down conduct that the police explicitly tried to justify invoking the state of emergency: http://delinquance.blog.lemonde.fr/2015/11/25/etat-durgence-un-controle-didentite-legalement-non-identifie/

grounded on universal, republican principles (Bourdeau et al., 2007). French policy, therefore, operates under the ideological constraint that there is nothing between the individual and the nation. Conversely, the liberal tenets of other countries, such as the U.S. or the U.K., could allow the state, in theory, to be more receptive to communities expressing group claims, including race-based grievances, and formulate answers accordingly.[55]

Declarations of Competing Interest

None.

Appendix A

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

COUNTIES (#)	Areas affected
Alpes-Maritimes (06)	Nice ; Saint-Laurent-du-Var.
Bouches-du-Rhône (13)	Marseille.
Côte-d'Or (21)	Dijon ; Chenôve ; Longvic.
Eure (27)	Evreux ; Gisors.
Haute-Garonne (31)	Toulouse ; Colomiers ; Blagnac.
Loiret (45)	Orléans.
Meurthe-et-Moselle (54)	Nancy ; Vandoeuvre-lès-Nancy.
Moselle (57)	Metz ; Woippy.
Nord (59)	L'ensemble des communes de la communauté urbaine de Lille-Métropole.
Oise (60)	Méru ; Creil ; Nogent-sur-Oise.
Puy-de-Dôme (63)	Clermont-Ferrand.
Bas-Rhin (67)	Strasbourg ; Bischheim.
Haut-Rhin (68)	Mulhouse.
Rhône (69)	Lyon ; Vénissieux.
Paris (75)	Paris.
Seine-Maritime (76)	Rouen ; Le Havre.
Seine-et-Marne (77)	All areas
Yvelines (78)	All areas
Somme (80)	Amiens.
Vaucluse (84)	Avignon.
Essonne (91)	All areas
Hauts-de-Seine (92)	All areas
Seine-Saint-Denis (93)	All areas
Val-de-Marne (94)	All areas
Val-d'Oise (95)	All areas

Source : Décret n°2005–1387 du 8 novembre 2005 relatif à l'application de la loi n°55–385 du 3 avril 1955.

Appendix B

Interviews with Mr. Meric and Mr. Pasqualini

November 2005

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 narrative emerging from the quantitative results accords with Mr. Meric's recollection that rioting gave rise to various types of 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 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

[55] Yet, the record in those countries reveals a different type of hindrance – that is, an apprehension of social phenomena based on purely material factors, therefore within the realm of economic policy – that does not address the ethnic component of the conflict head-on. After the 1992 Los Angeles riots the response of the Bush administration focused on reducing poverty levels through "weed and seed projects" (where run-down neighborhoods would be reconstructed) or through educational initiatives; the concerns of the African American community regarding police misconduct was ignored (Jacobs, 1993; Rich, 1993). That pattern repeated itself in the U.K. where the racial tensions underlying the 2011 riot were arguably not fully appreciated by the government; rather, policy responses targeted poverty and youth gang violence (Angel, 2012; Heap and Smithson, 2012). It is too early to tell whether the reflections emerging from the 2014 Ferguson riots constitute a paradigm shift.

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.[56] 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.

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Mr. Meric did perceive 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.[57]

If the Seine Saint Denis data do not reveal evidence of the opportunistic use of emergency powers, it appears to be the result of a deliberate strategy of non-inflammation. Appeasement was the main objective. Mr. Meric reiterated that busting crime at large was not on his 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.

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[56] This description is strikingly consistent with the epidemiological modeling of the riots provided in Bonnasse-Gahot et al. (2018).

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

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