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Police brutality, law enforcement, and crime: Evidence from Chicago

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ABSTRACT

It is a popular belief that police brutality incidents increase crime either by causing retaliation (i.e. rioting) or depolicing. But, these incidents may also deter crime, which makes the sign of the effect of brutality and crime ambiguous. In this paper, I build a simple model that highlights this theoretical ambiguity and provides guidance on how to use the joint effects of brutality on crime and arrests to distinguish between these three mechanisms: retaliation, depolicing, and deterrence. Using data on excessive force complaints in Chicago from 2011 to 2015, I exploit variation in the timing and location of serious excessive force incidents to estimate the effect of police brutality on crime rates and arrests rates within Chicago. I find that communities that experience serious brutality incidents experience a 2.1% increase in total crime in the month following the incident. These local crime rate increases are roughly five times larger when the victim is black and the officer is white (i.e. when incidents are *racially charged*). Racially charged incidents also result in large short-term increases in arrest rates (especially for violent crimes). These results are inconsistent with deterrence at the local level and highlight that the joint criminogenic and enforcement response to police brutality varies substantially by the racial composition of those involved. In addition, I also document some evidence of small post-incident city-wide declines in crime and arrests, highlighting the possibility that different mechanisms may matter at different scales of analysis. Contrary to public perception, I do not any find clear evidence of depolicing.

1. Introduction

There is a growing sense that police brutality is a problem in America (McLaughlin, 2015). Every year the list of high-profile brutality incidents manages to grow, claiming victims such as Michael Brown, Tamir Rice, Eric Garner, Walter Scott, Freddie Gray, and, more recently, George Floyd (among countless others).[2] These incidents have resulted in the emergence of activist groups like Black Lives Matter, caused protests reminiscent of the civil rights era, and generated innumerable news articles analyzing nearly every aspect of police misconduct. Despite all this attention, little writing has focused on how these incidents have impacted crime and policing. And, the work that has addressed this has focused on singular incidents rather than studying whether these incidents generally have a systematic effect. In this paper I intend to fill these gaps by examining how and why crime and policing respond locally to brutality incidents.

The idea that police brutality may affect crime started to circulate in the media after a few criminologists documented an increase in homicide rates in major US cities in the months following Michael Brown's murder. In particular, select journalists popularized the idea that Brown's death (and the subsequent social unrest) caused the uptick in murders (Gold, 2015; MacDonald, 2016a; Lopez, 2016; MacDonald, 2016b,c). This informal theory became known as the "Ferguson Effect" (Byers, 2014; MacDonald, 2015). One of the earliest articulations of a potential mechanism that could explain the Ferguson Effect came

[2] Michael Brown, an 18-year-old black teen, was shot to death by a white officer on August 9, 2014, in Ferguson, Missouri. Brown's death caused unrest and protests in multiple U.S. cities (McLaughlin, 2014). Tamir Rice, a 12-year-old black boy, was shot to death by a white officer for possession of a toy pistol on November 25, 2014, in Cleveland, Ohio (Dewan and Oppel, 2015). Eric Garner, a 43-year-old black man, was choked to death by a white officer on suspicion that he was selling cigarettes illegally on July 17, 2014, in Staten Island, New York Baker et al. 2015. Walter Scott, a 50-year-old black man, was shot to death by a white officer for reportedly stealing an officer's Taser (a claim that was contradicted by video evidence) on April 4, 2015, in North Charleston, South Carolina (Michael and Apuzzo, 2015). Freddie Gray, a 25-year-old black man, died from spinal injuries he sustained during a forceful arrest on April 25, 2015, in Baltimore, Maryland Graham (2015). George Floyd, a 46-year-old black man, was murdered by a white officer (Derek Chauvin) who knelt on Floyd's neck and back for 9 min and 29 s (McGreal, 2021).

from Rahm Emmanuel, Chicago's mayor at the time. Mayor Emmanuel argued that the scrutiny society placed on police after Michael Brown's murder had caused officers to pull "back from the ability to interdict... [because] they [didn't] want to be a news story themselves" and because "they [didn't] want their career ended early" (Byrne, 2015). On another occasion Emmanuel elaborated, stating "we have allowed our police department to get fetal, and it is having a direct consequence" (Agrawal, 2015). Translated into the language of economic theory, Emmanuel believed that if brutality incidents cause police to become timid, the expected costs of crime will fall, which could cause crime to rise. Throughout this paper, I refer to this mechanism as the *Depolicing Effect.*

Depolicing, however, is only one of multiple mechanisms to consider when studying the impact of police brutality on crime. A second potential mechanism is retaliation, by which I mean brutality could cause social unrest that manifests as increased crime (via rioting or related behaviors). And a third potential mechanism is deterrence, by which I mean a brutality case could deter would-be criminals from committing crimes because the possibility of being attacked by a police officer becomes more salient. Because these channels to do not all imply effects of the same sign, the overall effect of police brutality on crime is ambiguous. Thus, the true relationship between police brutality and crime is ultimately an empirical question.

In this paper, I formalize these channels into a simple model of how police and criminal behavior respond to brutality incidents.[3] I then use predictions from this theory to interpret estimates of the effect of brutality on crime. The basic ideas behind the model are threefold. First, due to an increase in scrutiny, officers will tend to reduce policing effort in response to a brutality incident. Second, civilians (potential criminals) will find crime more appealing as a mechanism of expression frustration but less appealing insofar as brutality incidents increase the perceived likelihood of becoming the victim of excess force, making the overall effect on the supply of crime ambiguous. And third, arrest rates are increasing in policing effort and increasing in the supply of crime. This framework yields two major predictions. (1) If brutality decreases crime, then the Deterrence Effect dominates whereas if brutality increases crime, then the Retaliation Effect and/or the Depolicing Effect dominate. (2) If crime rates increase and arrest rates decrease, then the Depolicing Effect dominates the Retaliation Effect.

For the empirical analysis, I use officer complaint data on brutality incidents in Chicago to estimate the relationship between police brutality and crime by exploiting the temporal and spatial variation of these incidents. It is important to note that these incidents are generally not nationally publicized incidents, making this the first systematic empirical evidence on the impact of local brutality incidents. First, I estimate the effect of brutality on citywide crime by comparing crime rates shortly before and after an incident using a repeated first difference approach. Second, I estimate whether the local effect of these incidents by estimating the effect on crime within the community in which the incidents occurred using a difference-in-differences approach. Finally, I determine whether these effects vary by the racial composition of the victim and officer.

At the city-level, my results suggest that within a month after a serious brutality incident, total crime decreases by 1% and arrests decrease by 1.7%, which is consistent with a Deterrence Effect. But, within a community where an incident occurred there is a 2.1% increase in total crime and no change in arrests the month after an incident, which is consistent both with a Depolicing and Retaliation Effect. Together, these results suggest that different underlying mechanisms may dominate at different proximities from the incident.[4]

When focusing on incidents with black victims and white officers (henceforth called "racially charged" incidents) the magnitude of the local impacts dramatically increases. The month after one of these incidents occurs, there is a 10.5% local increase in total crime, 11.3% local increase in violent crime, 14.7% local increase in property crime, a 13.1% increase in overall arrests and a 31% percent local increase in violent arrests.

Altogether, these estimates cast doubt on the possibility of a local Deterrence Effect of police brutality on crime. The increased magnitude in these effects when incidents are "racially charged" provides evidence that the joint criminogenic and enforcement response to police brutality varies substantially by the racial composition of those involved. Though the "racially charged" estimates are technically consistent with both the Retaliation and Depolicing Effects, I argue that the relatively large magnitude of the arrest effects (especially for violent crime) are more consistent with retaliation than depolicing. Furthermore, it is important to note that I find no clear evidence of depolicing, which is in sharp contrast to the media discussion of brutality. Finally, these results provide evidence that local brutality incidents (as opposed to nationally publicized estimates) impact crime and policing in ways that should be accounted for in criminal justice policy.

This paper is most closely related to other work studying the effect of singular high-profile police brutality incidents on crime rates. This literature includes multiple papers studying the impact of the Michael Brown murder on crime (Rosenfeld, 2016; Gross and Mann, 2017; Pyrooz et al., 2016; Shjarback et al., 2017). Of these papers, Pyrooz et al. (2016) uses the most credible econometric methods and they find no significant difference in overall crime trends before and after the event in major US cities. The paper in this literature most closely related to ours is (Shi, 2009), who studies the impact of a 2001 police shooting (and subsequent riot) in Cincinnati using an event-study design. Shi (2009) finds evidence that the incident simultaneously increased crime rates and reduced arrest rates. The author also develops a simple theoretical framework that allows them to interpret their results as a depolicing effect. This paper adds to this literature in three ways. First, I extend and generalize the theoretical framework from (Shi, 2009) to allow for three potential mechanisms through which brutality may effect crime. Second, I leverage 49 serious excessive force incidents, which allows me to circumvent identification associated with using one event (e.g. contemporaneous events invalidating causal claims). Using multiple events also allows me to examine whether the racial composition of the brutality incident changes the impact of the incident on crime and arrests. And, third, this paper is the first to examine whether brutality incidents have local (community-level) impacts, instead of the usual city-level analysis.

Another related study is (Devi and Fryer, 2020), which exploits staggered federal investigations of police departments to study the effect of these investigations on crime rates and police engagement in the city whose police department is being investigated. Devi and Fryer (2020) find evidence that these investigations reduce the quantity of policing and increase homicides and felonies. This paper uses a similar empirical strategy as (Devi and Fryer, 2020), but instead focuses on local effects of brutality, not investigations.

Finally, this paper is also related to the broader literature on racial inequality and law enforcement. The biggest topics this literature are

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[3] Though mechanisms other than the three discussed in this paper may be relevant, I focus on these because they are commonly suggested in academic literatures in economics & criminal justice as well as in the public media. For reviews of research on depolicing, see Rushin and Edwards (2016) and Shjarback et al. (2017). For reviews of research on criminal deterrence see Chalfin and McCrary (2017). Finally, both journalistic (see Poon and Patino 2020) and academic research (see Shi, 2009; Pyrooz et al., 2016) have documented riot responses to police brutality, which are generally linked to increases in crime (DiPasquale and Glaeser, 1998). For a more targeted and detailed discussion of this and related research, see the contribution to literature portion of the introduction.

[4] For further discussion of how the city-level and community-level results are related and perhaps interfere with each other, see Section 6.1.

pull-over rates, sentencing, and minority representation in police departments. In all three contexts, most find evidence of racial discrimination and inequality. Research focusing on pull-over rates generally finds that police disproportionately stop black drivers (Norris et al., 1992; Dharmapala and Ross, 2004; Antonovics and Knight, 2009; Heaton, 2010). Research focusing on sentencing generally find that minorities are not only more likely to receive a sentence, but that they tend to receive longer sentences (Mustard, 2001; Anwar et al., 2012; Rehavi and Starr, 2012; Alesina and La Ferrara, 2014). Lastly, though legal changes have increased minority representation in police departments (McCrary, 2007), there is evidence that additional minority officers could reduce discrimination (Donohue and Levitt, 2001; West, 2018). More recently, Fryer (2019) analyzed empirical evidence of racial bias in use of force, finding strong evidence of officer bias in non-lethal use of force. This paper expands this literature by asking whether criminal behavior and law enforcement reacts differently to brutality incidents committed by the racial composition of those involved in the incident.

In Section 2, I develop a theoretical framework that formalizes the Depolicing Effect, Retaliation Effect, and Deterrence Effect channels into a simple model of how police and criminal behavior respond to brutality incidents and derive testable predictions that allow me to distinguish between these channels. In Section 3, I discuss the two main data sources I use for these analyses: complaints data from the Citizen's Police Data Project (CPDP), City of Chicago crime data, and community level Census data for the city of Chicago. In Section 4, I describe the repeated single differences and differences in differences approaches I use to estimate the impact of a serious police brutality incident on crime. In Section 5, I summarize the empirical estimates and show evidence of a negative effect of brutality on crime and arrests at the city-level, but a positive effect at the community-level, especially when incidents are "racially charged". In Section 6.2, I interpret the results using the predictions the theoretical framework. Finally, in Section 7, I summarize the findings and contribution of the paper and conclude with a discussion of the policy relevance of my findings.

2. Theoretical framework

Police Brutality could impact crime rates through many channels. In this paper, I focus on the three most salient channels: the Depolicing, Retaliation, and Deterrence Effects. As described in the introduction, the Depolicing Effect predicts that officers will reduce policing effort in response to scrutiny after a serious brutality incident. This decreases the expected cost of committing a crime, which increases the crime rate. Retaliation, on the other hand, is when frustration with police misconduct causes protests, riots, or other disruptive behaviors, which could manifest as increases in crime rates. Due to the history of racial tension In the United States, this particular channel is most often discussed in relation to "racially charged" black-victim-white-officer brutality incidents, which too frequently cause protests and riots. Lastly, brutality incidents may deter crime by causing them to believe the expected cost of being caught is higher than before the brutality incident. Such an effect would reduce their incentive to commit crimes. In the remainder of this section, I build a simple model that formalizes each of these channels, and outlines how I might use data on arrests and crime rates to distinguish between them empirically.

2.1. Model set up

Generalizing from Shi (2009), assume that police officers' utility $u_o \in \mathbb{R}$ is a function of the effort they put into their job $e \in \mathbb{R}$ and the cost they incur per unit of misconduct $M \in \mathbb{R}$. Furthermore, assume $u_o(e, M)$ is supermodular in e and $-M$, meaning that the return to effort diminishes as M increases. Applying the key theorem on monotone comparative statics in Topkis (1978), this implies that e is decreasing in M (i.e., officers engage in less effort when the perceived costs to them of committing misconduct increase).

For the subset of civilians who may commit crime, let their utility $u_c \in \mathbb{R}$ be a function of how much crime they commit $c \in \mathbb{R}$, the per unit "retribution" payoff to crime $r \in \mathbb{R}$, the perceived likelihood of getting caught while committing a crime $p_c \in (0, 1)$, and the perceived likelihood of getting brutalized when committing a crime $p_b \in (0, 1)$. In this framework, r should be interpreted as the psychological utility associated with retaliatory crime that may be a signal or protest or frustration. Furthermore, assume that $u_c(c, r, -p_c, -p_b)$ exhibits supermodularity in each pair (c, r), $(c, -p_c)$, and $(c, -p_b)$. In words, the utility return to crime increases in r and decreases in both p_c and p_b. The decreasing returns to p_c and p_b, then reflect the higher per unit expected cost of crime associated with a greater likely hood of facing legal repercussions and physical repercussions at the hands of an officer. Also, let p_c be an increasing function in officer effort e. This merely presupposes that if officers try harder, then the likelihood of getting caught when committing a crime goes up.[5]

Another element of the model is arrest rates, which is a function of choice variables e and c. Specifically, assume that arrests $a(e, c)$ increases in both e and c. Because arrests can be observed in the data, we will use this simple relationship to derive testable predictions that will allow me to differentiate between mechanisms. I will return to this later.

The final element of this model is police brutality. Let $B \in [0, \infty]$ represent the intensity of a recent police brutality incident. Police brutality can directly influence $M(B)$, $r(B)$, and $p_b(B)$. In particular, all three parameters are non-decreasing in B. Concretely, this means that if a community experiences a police brutality incident, the officers' expect the punishment to be harsher (or more likely), which increases M. In addition, the incident increases the return to retaliatory crime r, as the brutality incident may be seen as unjustified. Finally, the brutality incident may make marginal criminals more cognizant of the possibility of getting brutalized if they commit crime, which increases the perceived likelihood of a brutality incident p_b.

With this set up, I am now in a position to derive the three predictions that come from this framework.

2.2. The depolicing effect

Consider an increase in B that strictly increases M but effects no other parameters. For simplicity, denote this by an increase of B from $B^0 = 0$ to $B^1 = 1$. By supermodularity of $u_o(e, -M(B))$ in e and $-M$, any increase in M decreases the optimal level of effort e. Thus, $e^*(M(0)) > e^*(M(1))$.

This decrease in optimal effort effects the decision to commit crime as well. Recall that $p_c(e)$ is an increasing function of e. Thus, $p_c(e^*(M(0))) > p_c(e^*(M(1)))$, meaning that the decrease in officer effort decreases the expected likelihood of getting caught by an officer. Finally, the supermodularity of u_c in c and $-p_b$, means that any decrease in p_b will increase c. Symbolically, we conclude that $c^*\left(p_c(e^*(M(0)))\right) < c^*\left(p_c(e^*(M(1)))\right)$. This the first theoretical result Proposition 2.

Proposition 1 (*The Depolicing Effect*)**.** *An increase in police brutality that only increases the expected cost of misconduct will increase the crime rate.*

2.3. The retaliation effect

Now consider an increase in B, again from $B^0 = 0$ to $B^1 = 1$, that increases r, but no other parameters. Thus, $r(0) < r(1)$. By supermodularity of u_c in c and r, any increase in r will increase the optimal rate of crime, giving $c^*(r(0)) < c^*(r(1))$. This very simple implication of my framework is the second theoretical result.

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[5] To simplify notation, I only denote parameters as functions of relevant variables when it is essential for an explanation.

Proposition 2 (*The Retaliation Effect*). *An increase in police brutality that only increases the retribution payoff of crime will increase the crime rate.*

2.4. The deterrence effect

Now consider an increase in B, again from $B^0 = 0$ to $B^1 = 1$, that increases p_b, but no other parameters. Thus, $p_b(0) < p_b(1)$. By supermodularity of u_c in c and $-p_b$, any increase in p_b will decrease the optimal rate of crime, giving $c^*(p_b(0)) > c^*(p_b(1))$. This simple implication of my framework is the third theoretical result.

Proposition 3 (*The Deterrence Effect*). *An increase in police brutality that only increases the perceived likelihood of brutalization by an officer will decrease the crime rate.*

2.5. The ambiguity of an overall effect

Now consider an increase in B, again from $B^0 = 0$ to $B^1 = 1$, that increases all three parameters M, r, and p_b. Combining Propositions 1–3, we can show the general ambiguity in the overall impact on crime. Formally, the total effect, represented by Δc, can be decomposed into the following three components:

$$\Delta c^* = c^*(p_c(e^*(M(1))), r(1), p_b(1)) - c^*(p_c(e^*(M(0))), r(0), p_b(0)) \tag{1}$$

$$\approx \underbrace{\left[c^*\left(p_c(e^*(M(1)))\right) - c^*\left(p_c(e^*(M(0)))\right) \right]}_{\text{Depolicing Effect }(+\,)}$$

$$+ \underbrace{\left[c^*(r(1)) - c^*(r(0)) \right]}_{\text{Retaliation Effect }(+\,)} + \underbrace{\left[c^*(p_b(1)) - c^*(p_b(0)) \right]}_{\text{Deterrence Effect }(-)}.$$

Each component is analogous to the three previous theoretical results, which is why we can sign them; The Depolicing Effect and the Retaliation Effect both predict increases in crime rates, while the Deterrence Effect predicts a decrease. This implies that the sign of the total effect is ambiguous, which is the fourth theoretical result.

Proposition 4 (*Ambiguity of Police Brutality's Overall Impact on Crime*). *An increase in police brutality that simultaneously increases the expected cost of misconduct, the retribution payoff of crime, and the perceived likelihood of brutalization by an officer may increase or decrease crime rates.*

2.6. Empirically distinguishing between mechanisms

Below I describe the three empirical implications of the theoretical framework derived in the previous subsections. These three empirical predictions will guide and make sense of the empirical analysis discussed in sections 5 and 6.

2.6.1. Deterrence vs. Depolicing and retaliation effects

Though Proposition 4 provides an ambiguous prediction for the impact of a local brutality incident on crime, it simultaneously provides a way to determine whether the Deterrence Effect or a combination of the Depolicing Effect and the Retaliation Effect dominates. Specifically, if a police brutality incident increases crime, then the theory implies that the magnitude of the Depolicing Effect and the Retaliation Effect is larger than the magnitude of the Deterrence Effect. But, if police brutality decreases crime, then my theory implies that the magnitude of the Deterrence Effect is larger than the sum of the magnitudes of the Depolicing Effect and the Retaliation Effect. This provides the first empirical implication of the theory that I will use to guide and make sense of my analysis.

Empirical Implication 1: *If police brutality incidents increase crime rates, then the Deterrence Effect does not dominate the Depolicing Effect and the Retaliation Effect.*

2.6.2. Retaliation vs. Depolicing effect

In this theoretical framework, the impact of police brutality and crime can provide evidence that rules out the dominance of the Deterrence Effect, but is agnostic about the relative magnitudes of the Depolicing and Retaliation Effects. Arrest rates, however, can help distinguish between the these two effects. Recall that arrests a are an increasing function in e and c. Also, note that, while the Retaliation Effect and the Depolicing Effect both cause an increase in crime, only the Depolicing Effect lowers officer effort. Because $a(e, c)$ is increasing in both arguments, only the Depolicing Effect is consistent with a non-increasing arrest rate following a brutality incident. This provides a direct test for the presence of the Depolicing Effect.

Empirical Implication 2: *If police brutality incidents increase crime rates and do not increase arrest rates, then the Depolicing Effect must be a contributing mechanism.*

Less formally, large increases in arrest rates relative to crime rates will tend to be more consistent with the Retaliation Effect because, unlike the Depolicing Effect, there is no downward pressure on arrests through decreased officer effort when Retaliation Effect occurs. This reasoning about the relative plausibility of these mechanisms will play a key role in the results discussion in Section 6.2.[6]

3. Data

In this paper, I use three datasets: officer complaint data from the Citizen's Police Data Project, reported crime data from the City of Chicago, and community-level demographic data from the U.S. Census Bureau.

3.1. Citizen's police data project (CPDP)

The CPDP dataset includes information about over 56,000 complaints against Chicago Police officers, from 2002–2008 and 2011–2015.[7] For each complaint, the CPDP data include the type of misconduct, an incident identification number, the incident date, the investigation start date,[8] the name of the chief investigator who was assigned the

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[6] The finding in Devi and Fryer 2020 that crime rates increase in the aftermath of federal investigations gives some reason to think that local investigations after each incident might drive positive crime effects. In addition, except for two cases, all serious cases used in this paper have corresponding investigations that start on the same day or the day following the incident, which makes the independent effects of the investigation and the incident impossible to distinguish. Like Devi and Fryer 2020, I view investigations as most likely causing depolicing, which would predict a disproportionate increase in crime relative to arrests due to a reduction in policing effort. Thus, one way of viewing our test between depolicing and retaliation is that evidence in favor of depolicing is also in favor or some form of investigation impact (i.e. investigations, then, can be interpreted as another form of scrutiny).

[7] I restrict my analysis to 2011–2015 because of the gap in the complaints data. The CPDP claim that these data include all information about police misconduct that the Chicago Police Department have released to the public. These data also incorporate the Bond and Moore datasets. The Bond dataset lists the 662 Chicago Police officers who received more than ten complaints between May 2001 and May 2006. The Moore dataset contains information about the 185 officers who received more than five excessive force complaints between May 2001 and May 2006.

[8] The Independent Police Review Board Authority (IPRA) investigates each complaint made against an officer. Chicago instituted the IPRA to replace the Office of Professional Standards in 2007 "in response to concerns about how allegations of police misconduct were being investigated" (City of Chicago, 2016).

Table 1

Complaint level summary statistics.

Statistic	N	Mean	Median	St. Dev.	Min	Max
Days Until Reported	54	19.167	0	130.461	0	959
Investigation Length (Days)	54	632.667	505.5	347.134	127	1,530
Male Officer	54	0.833	1	0.376	0	1
Black Officer	54	0.241	0	0.432	0	1
Hispanic Officer	54	0.296	0	0.461	0	1
White Officer	54	0.463	0	0.503	0	1
# Complainants	54	2.278	2	0.656	2	4
# Male Complainants	54	1.574	2	1.057	0	4
# Female Complainants	54	0.704	0	0.964	0	3
# Black Complainants	54	0.519	0	1.023	0	4
# Hispanic Complainants	54	0.407	0	0.813	0	2
# White Complainants	54	1.352	2	1.152	0	4

Notes: Based on CPDP complaint data. Each of the 54 complaints are derived from the 49 serious excessive force incidents used as treatment variables throughout this paper. There are more complaints than incidents because incidents often result in complaints against more than one officer.

complaint, the name of the officer who was accused of misconduct, the age and race of the complainant,[9] and, occasionally, the investigation and police board hearing reports[10]

The CPDP data include over 9600 excessive force complaints. Based on results from complaint investigations, the CPDP divides complaints into five categories: unfounded, exonerated, not sustained, sustained, and disciplined.[11] Unfounded means that the investigator found the complaint to be fallacious; exonerated means that the investigator found that the officer's actions were appropriate; not sustained means that the investigator found that the evidence was insufficient to pass judgment on the complaint; sustained means that the investigator found evidence for disciplinary action, and disciplined means that the officer was reprimanded, suspended, or separated from the department. It is uncommon for officers to be disciplined; the most disciplined misconduct type is "Unbecoming Conduct", which has a 22% discipline rate (see Fig. 2). Use of Force, which is the second largest category of misconduct, only has a 3% discipline rate.

I call an excessive force incident "serious" if its investigation was sustained and if the officer was disciplined.[12] Thus defined, there are 123 serious incidents, 55 of which occur from 2011 through 2015. I drop an additional six incidents for idiosyncratic reasons[13]; this paper focuses on analyzing the effects of these 49 incidents on local crime.

For summary statistics about the officer complaints from these 49 incidents, see Table 1. The majority of the officers accused in these cases are male and a plurality are white. Complainants are also disproportionately male and white.

For the sake of the plausibility of each mechanism through which brutality may impact crime, it is important that these incidents were reported relatively soon after they occurred. This is because it would be unlikely that any agents (particularly police precincts) would react to an incident that they were unaware of. Table 1 shows that the median number of days it takes to report these incidents is zero, meaning that, in general, these incidents are reported quickly. In fact, in only three cases are these incidents reported after the incident date.

Because these data include when and where these serious incidents occurred, I can exploit their timing and location to causally identify their effects on local crime rates. Fig. 3 show the distribution of excessive force cases over space by the race of the victim and the officer. One important note is that in this period (at the time when I collected the data), there are only 7 black victim serious incidents and black-victim-white-officer incidents, which is a key limitation of this study.

With the CPDP data, there are a few potential sources of bias. As with any dataset, these data contain minor entry errors,[14] which, assuming they are random, will attenuate my coefficient estimates. A more serious problem would be if the serious brutality incidents in these data are not representative. This would be the case if there were serious brutality incidents that no one complained about. It is challenging to rule this out explicitly, but underreporting should bias my estimates toward zero, making them conservative.

3.2. City of Chicago crime data

The City of Chicago crime data include reported crimes that occurred in Chicago from 2001 to 2016,[15] containing over six million crimes. The data contain 22 variables for each crime including an incident identification number, the date of the crime, multiple measures of the location of the crime, the type of crime committed, a description of what occurred, and whether or not there was an arrest. The data do not include the race of the perpetrator or the victim.[16] In this paper, I only use crime data from 2009 to 2015, to better overlap with the time-period from which I have police misconduct data. As shown in Fig. 1, crime is steadily decreasing in Chicago over this period.

These data are subject to misreporting, which may affect the results. Misreporting will bias my estimates if brutality incidents systematically affect society's tendency to report crimes. For instance, if people distrust the police after a brutality case, then they may be less likely to complain. This would overstate any negative effects I find and understate any positive effects. Because I cannot measure this, it is worth keeping in mind when interpreting my results.[17]

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[9] It is important to note that I use race of complainant as a proxy for race of the victim. I do this because the CPDP data report race of the complainant for every serious incident. Furthermore, in the subsample of complaints where information about the race of the victim is available, whether the complainant is black and the victim is black have a simple correlation coefficient of .752. Though I do not include a table with this correlation, I can reproduce it upon request.

[10] To access the investigation and police board hearing reports, one must request them. After a report has been requested, the CPDP make the report available on their website. These reports contain detailed characteristics about the victim in the incident. I have requested reports for 49 of the excessive force complaints since 2011 for which the investigations have been sustained, but the data is not yet available.

[11] There is a sixth category entitled "other", on which the CPDP do not elaborate. I reached out to them about this but they have yet to respond.

[12] I call the sustained and disciplined incidents serious because the IPRA found them worthy of a close investigation. If the police protect their own (eg. Chin and Wells 1997, Skolnick 2002), this categorization is likely to underestimate the number of severe incidents and attenuate my estimates. But, future work should explore alternative ways of identifying serious incidents (e.g. using details from cases instead).

[13] I drop six serious brutality cases because either they occur outside of Chicago or there was relevant data missing about the incident that I could not find.

[14] On their website, the CPDP show an investigation end date that was inaccurate on their site and had to be corrected. Though they spotted the error, this exemplifies the type of mistakes that may exist in the data.

[15] In my analysis, I drop all reported crime data from 2001 to 2011 because the excessive force data had a gap between 2008 and 2011.

[16] Because the data does not include the race of the perpetrator I cannot estimate the effect of brutality incidents on minority crime rates compared to white crime rates. This is limiting because brutality could reasonably affect minorities differently. A more thorough racial analysis would be a useful extension of this paper.

[17] Though these data are an attempt at a comprehensive crime dataset over a 15 year period, the disclaimer on the site admits that

> These crimes may be based upon preliminary information supplied to the Police Department by the reporting parties that have not been verified. The preliminary crime classifications may be changed at a later date based upon additional investigation and there is always the possibility of mechanical or human error.
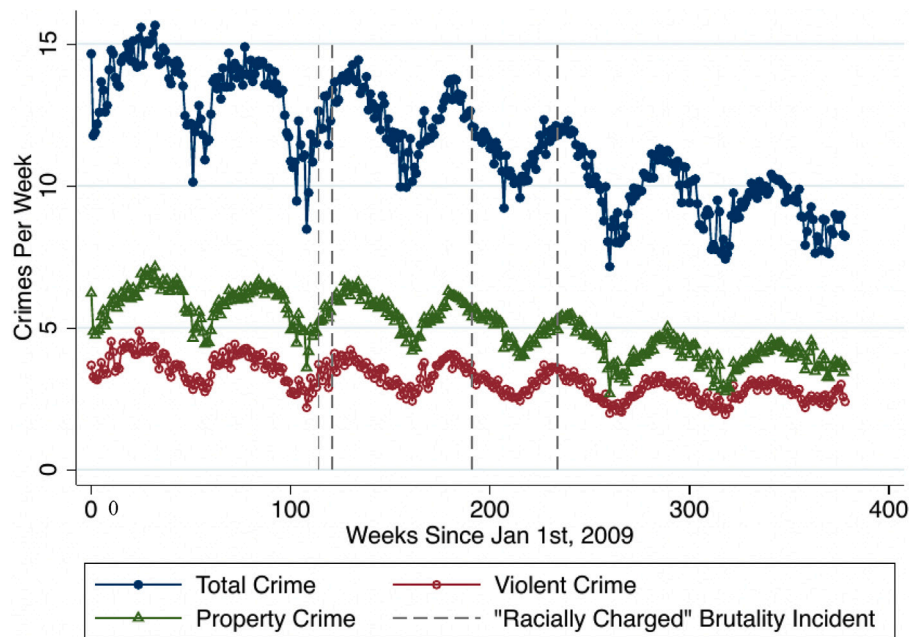
Fig. 1. Weekly crimes in Chicago by type since (2009–2015).
Note: This figure displays the number of crimes in Chicago by type by week from 2009 to 2015. These data come from the City of Chicago Crime database. Dashed horizontal lines indicate the weeks when a brutality incident with a white officer and black victim occurs.

3.3. Census data

In the 1920's the Social Science Research Committee at the University of Chicago divided Chicago up into community areas that have since been recognized by The Census Bureau. In total, there are 77 of these communities, and they are the primary unit of analysis used in this paper. I use publically available Census data to access community-level characteristics. These include median income, the proportion of residents who graduated high school, poverty rate, the proportion of residents who graduated college, the number of residents who are black, the number of residents who are Hispanic, and the number of residents who are white. The Census Bureau does not collect these data every year, so I linearly interpolate (and extrapolate) them to fill in all missing years from 2011–2015.

4. Empirical strategy

Guided by the predictions of the theoretical framework laid out in Section 2, I now turn to an empirical analysis of the effects of police brutality on crime and arrests. I begin with exploratory analysis at the city-level, where I estimate effects using a repeated single difference design. This approach attempts to recover systematic crime and arrest rate differences in all of Chicago in the weeks following a serious brutality incident relative to weeks before. I also analyze city-level impacts of police brutality by racial composition of the incidents as well. I label all city-level analysis as "exploratory" because of the inherent limitations of the identification strategy to credibly recover causal estimates. In particular, because city-level estimates rely solely on before–after comparisons, for a causal interpretation they require that the timing of serious brutality incidents are unrelated to any other events that might influence crime an arrest rates in Chicago. Though

this is an unrealistic assumption, I include the analysis to highlight what is missed by focusing purely on city-level analysis, as has been the focus of much previous research on this topic.

Next, I turn to my primary empirical approach: a staggered difference-in-differences (DiD) strategy that exploits variation in the exact timing and location of serious brutality incidents in Chicago. Conceptually, this strategy uses other communities within Chicago as a control group to compare with communities that are "treated" by serious police brutality. This DiD strategy requires the following four assumptions: (1) the police brutality incidents must not have been determined by the outcome of interest (i.e. no reverse causality), (2) the treatment and control groups exhibit parallel trends in the outcome, (3) the composition of intervention and comparison groups is stable, and (4) there are no spillover effects.[18] The most essential of these assumptions is the Parallel Trends Assumption, which, though not formally testable, will be assessed by looking at average differences in treatment and control trajectories prior to serious brutality estimates using event-study-style figures. These figures will also allow me to assess the plausibility that pre-period changes in crime may have caused the serious brutality incident, which would be a violation of the no reverse causality assumption.

For the staggered DiD empirical strategy, in cases where communities are treated multiple times, I only estimate the impact of the first incident in that community, which allows me to circumvent the challenges associated with multiple treatments. Thus, rather than using treatments from all 49 serious excessive force incidents, I only use the 29 first occurrences in my data. Luckily, only one of the black-victim-white-officer incidents is a repeat incident, so this does not severely limit our race-composition specific analysis much more than the original sample limitations on these cases did. Furthermore, because the outcome variable is a count of daily crimes or arrests, all regression equations in this section are estimated using a Poisson regression

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Therefore, the Chicago Police Department does not guarantee (either expressed or implied) the accuracy, completeness, timeliness, or correct sequencing of the information and the information should not be used for comparison purposes over time. (City of Chicago Website).

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[18] These four assumptions are in addition to the (often unstated) *Exchangeability* and *Positivity* assumptions required to estimate any causal effect. Furthermore, assumptions (3) and (4) are both implications of the Stable Unit Treatment Value Assumption (SUTVA), which is also required for any causal estimate.
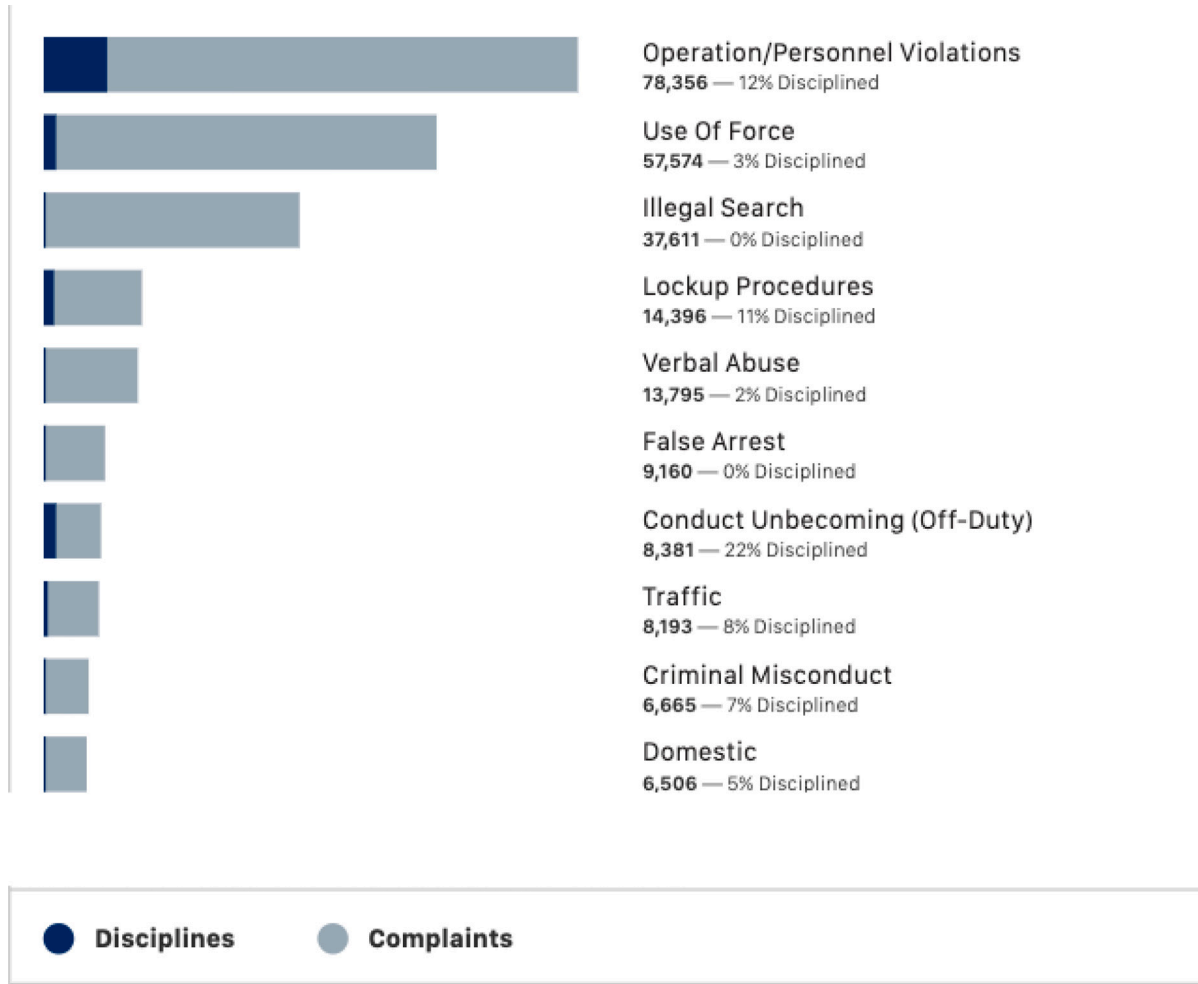
Fig. 2. Top ten complaint types by discipline.
Note: This figure displays the top 10 complaint types against officers by whether the complaint lead to the officer getting punished. Data cover complaints from 1988–2018. This figure and these data come from the Citizens Police Data Project complaints database (available at https://data.cpdp.co/data/D3J3aQ/citizens-police-data-project).

model. Thus, coefficients of interest represent (roughly) the percent change in the daily number of crimes (or arrests) per-community within a month after a serious brutality incident occurred (aka the incidence rate ratio between the "treated" and "control" time periods).

An additional threat to identification using staggered DiD designs arises from treatment effect heterogeneity within units over time, which can cause bias because earlier treated units serve as controls for later treated communities. I discuss this issue further in Section 4.2.1 and outline an augmentation of my identification strategy that only uses never treated units to avoid this problem.

4.1. City-level analysis

I start by analyzing the impact of serious police brutality incidents on crime and arrests across Chicago. II do this by using a repeated single differences design that estimates the impact of being within a month after any of the 49 serious incidents on the number of daily crimes and arrests in Chicago by type. The specific model I estimate is

$$C_{idwmy} = B_0 + \delta\, Post\, Brutality_{dwmy} + X_{iy}B + \alpha\tau_{wmy} + z_w + u_y + \epsilon_{idwmy}. \quad (2)$$

C_{idwmy} represents the number of crimes (i.e. violent crimes, property crimes, or total crimes) or arrests (i.e. violent arrests, property arrests, or total arrests) that occurred in community i on day d in week w in month m in year y. In the baseline specification, the variable of interest, $Post\, Brutality_{dwmy}$, is a dummy that is equal to one if the

date is within 30 days after a serious brutality incident occurred.[19] Thus The variable X_{iy} represents a vector of year-varying community characteristics including the median income, percent of residents who graduated high school, the percent of black residents, the percent of white residents, and the percent of Hispanic residents. Additionally, I include various fixed effects to control for community-specific or time-specific heterogeneity that may be correlated with when a brutality incident occurred and the crime rate. Specifically, z_w represents a week fixed effect, u_y represents a year fixed effect, and τ_{wmy} represents a time trend. For the sake of transparency, I estimate this model in stages, first with only community controls, then adding week and year fixed effects, then adding a time trend, and finally adding quarter by year fixed effects.

To assess parallel trends, I also estimate this regression replacing $Post\, Brutality_{dwmy}$ with a set of indicator variables for each week from three weeks before to four weeks after the each incident. The corresponding model is

$$C_{idwmy} = B_0 + \sum_{t=-3}^{4} \delta^t Week\, Brutality^t_{dwmy} + X_{iy}B + \alpha\tau_{wmy} + z_w + u_y + \epsilon_{idwmy}. \quad (3)$$

---

[19] Because the average length of time between serious incidents was roughly 4.2 weeks, extending the treatment window beyond 30 days reduces the size of the implicit control group (days outside of the treatment period) too much for a valid comparison. Thus, I limit the treatment periods to 30 days after the incident.
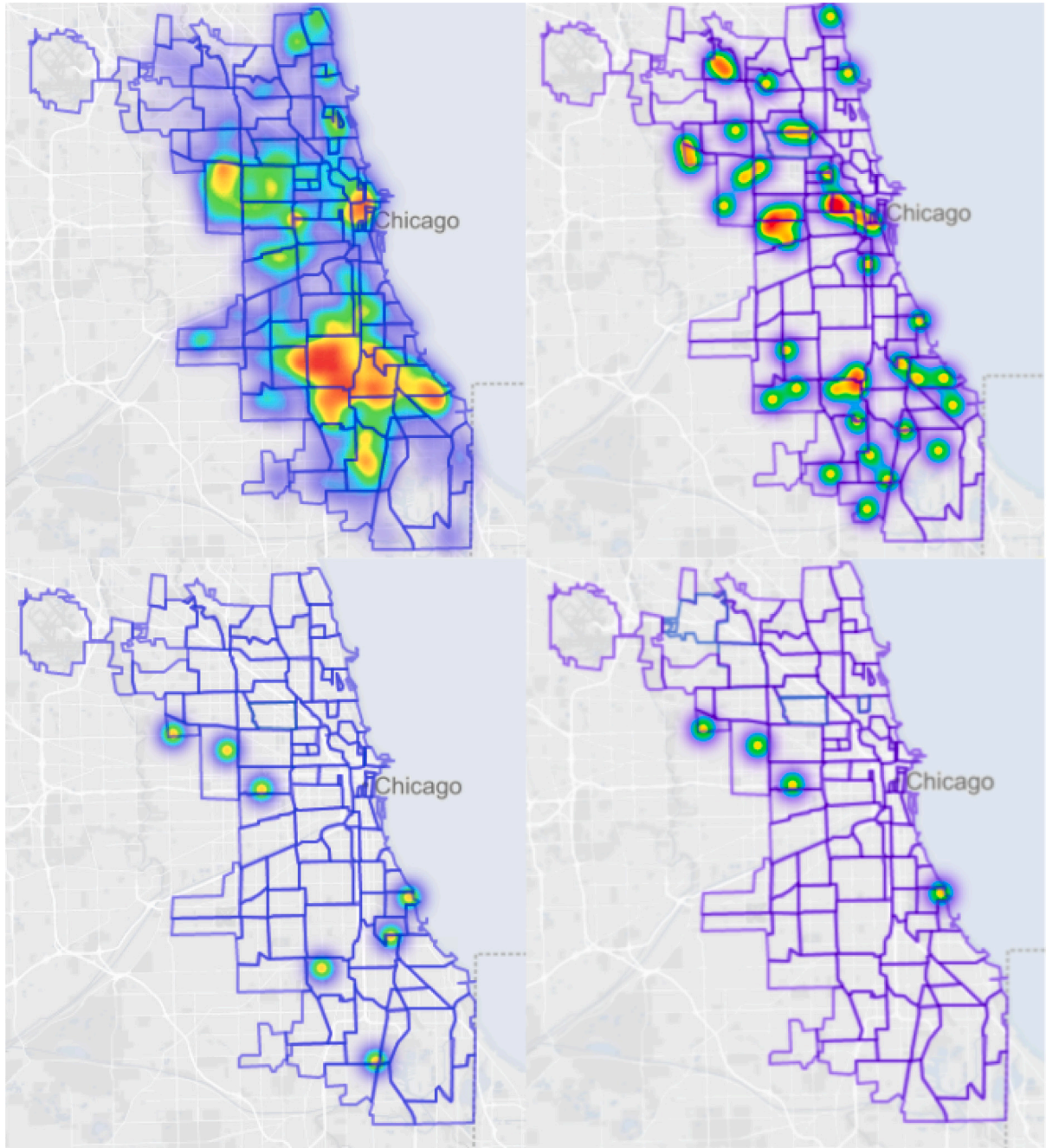
Fig. 3. Heat maps of excessive force cases by type in Chicago (2011–2015).

Note: This figure displays four heat maps for four types of excessive force complaints. Clockwise, the four complaint types are (1) all complaints, (2) complaints that resulted in officer discipline, (3) complaints that resulted in officer discipline with black complainants, and (4) complaints that resulted in officer discipline with black complainants and an accused white officer. Number of cases ranges from red (most cases) to yellow to green to blue (least cases). Data cover complaints from 2011–2015. This figure and these data come from the Citizens Police Data Project complaints database (available at https://data.cpdp.co/data/D3J3aQ/citizens-police-data-project).

where $WeekBrutality^t_{dwmy}$ is an indicator for whether it is t weeks relative to a brutality incident. Following in the standard in the literature, flexible treatment dummies are assessed relative to the period just before treatment (i.e. $t = -1$), meaning that the "impact" on the city at this time is normalized to zero.

To analyze differences in effects by Race, I also estimate the following two specifications:

$$C_{idwmy} = B_0 + \theta_1 BV_{dwmy} + \theta_2 OB^{-BV}_{dwmy} + X_{iy}B + \alpha\tau_{wmy}$$
$$+ z_w + u_y + \epsilon_{idwmy},$$ (4)
$$C_{idwmy} = B_0 + \theta_3 BVWO_{dwmy} + \theta_4 OB^{-BVWO}_{dwmy} + X_{iy}B + \alpha\tau_{wmy}$$

$$+ z_w + u_y + \epsilon_{idwmy}.$$ (5)

These models estimate the effects on crime and arrests of being within 30 days after brutality incidents allowing effects to differ by the race of officer and victim. Thus, BV_{dwmy} is a dummy variable that is one if it is within 30 days after a brutality incident where the victim was black. Similarly, $BVWO_{dwmy}$ is a dummy variable that is one if it is within 30 days after a brutality incident where the victim was black and the officer was white, which I refer to as "racially charged" incidents. The coefficient θ_1, then, can be interpreted as the impact of black victim incidents while θ_3 can be interpreted as the impact of racially charged incidents. OB^{BV}_{dwmy} and OB^{BVWO}_{dwmy} are controls for being within a month

of another police brutality incident, excluding black victim and racially charged incidents respectively.[20]

To assess parallel trends I also estimated this regression replacing BV_{dwmy} and $BVWO_{dwmy}$ with sets of indicator variables for each week from three weeks before to four weeks after the each incident type of incident (analogous to Eq. (3)). As will be discussed in the results section, the main city-wide race-specific specifications (Eqs. (4) and (5)) yields no significant results, so, for brevity, I do not present results from this parallel trends robustness check.

4.2. Community-level analysis

Next I estimate a difference-in-differences model to assess the average effect of brutality incidents on crime within the community in which they occur by comparing affected regions to others where no incident occurred. The specific model I estimate is

$$C_{idwmy} = B_0 + \sum_{t=\{1,2+\}} \Delta^t PostBrutality_{idwmy}^t + X_{iy}B + \alpha\tau_{iwmy}$$
$$+ \mu_i + z_w + u_y + \epsilon_{idwmy}. \tag{6}$$

Relative to Eq. (2), there are two main differences. First, I have added a control ($PostBrut_{idwmy}^{2+}$) for being greater than 1 month after a serious brutality incident within the community in which it occurred. This limits the implied control units to be community by day cells before a serious police brutality incident occurred in these data. Second, the main variable of interest, $PostBrualiyt_{idwmy}^1$, now has an i subscript, but is otherwise exactly the same. The i subscript is needed because I now assume that only the community in which the brutality incident occurred is affected. Thus, the added i signifies that $PostBrutality_{idwmy}^1$ now varies at the community-time level, which allows untreated communities across the city to be leveraged as control units.[21] Because the treatment varies at the community-level in this model, I can also include community fixed effects μ_i, which transforms this model into a form of two-way fixed effects difference-in-difference estimator. I also add community-specific trends τ_i, month-by-year fixed effects, community by year fixed effects, and community by quarter by year fixed effects (all of which I sequentially add and discuss in the results section). This final set of fixed effects allows me to go beyond the traditional time and unit fixed effects in DD models by restricting comparisons to relative changes in crime (and arrests) within a community within a particular quarter of a year.

To assess parallel trends, I also estimate this regression adding indicator variables for each week from three weeks before to four weeks after each incident. The corresponding model is

$$C_{idwmy} = B_0 + \sum_{t=-3}^{4} \Delta^t WeekBrutality_{idwmy}^t + \Delta^{5+} PostBrutality_{idwmy}^{5+}$$
$$+ X_{iy}B + \alpha\tau_{iwmy} + \mu_i + z_w + u_y + \epsilon_{idwmy}. \tag{7}$$

where $WeekBrutality_{idwmy}^t$ is an indicator for whether it is t weeks relative to a brutality incident in community i. Following in the standard in the literature, flexible treatment dummies are assessed relative to the period just before treatment (i.e. $t = -1$), meaning that the "impact" on the community at this time is normalized to zero.

To analyze differences in local effects by Race, I also estimate the following specifications:

$$C_{idwmy} = B_0 + \Theta_1 BV_{idwmy} + \Theta_2 OB_{idwmy}^{-BV} + \Delta^{5+} PostBrutality_{idwmy}^{5+}$$
$$+ X_{iy}B + \alpha\tau_{iwmy} + z_w + u_y + \epsilon_{idwmy}, \tag{8}$$

$$C_{idwmy} = B_0 + \Theta_3 BVWO_{idwmy} + \Theta_4 OB_{idwmy}^{-BVWO} + \Delta^{5+} PostBrutality_{idwmy}^{5+}$$
$$+ X_{iy}B + \alpha\tau_{iwmy} + z_w + u_y + \epsilon_{idwmy}. \tag{9}$$

The main difference between the city-level race-specific specifications (Eqs. (4) and (5)) and these community-level specifications (Eqs. (8) and (9)) is the i subscript on the variables of interest, which signifies. In this analysis, I default to the most stringent set of controls, including community-specific trends, month-by-year fixed effects, community by year fixed effects, and community by quarter by year fixed effects.

To assess parallel trends I also estimated this regression replacing BV_{idwmy} and $BVWO_{idwmy}$ with sets of indicator variables for each week from three weeks before to four weeks after the each incident type of incident (analogous to Eq. (7)). For concision, I omit this final regression equation from the text.

4.2.1. Robustness to panel ("TWFE") difference-in-difference identification concerns

Much research in applied econometrics have called into question the validity of two-way-fixed-effects estimators in recent years (for a recent survey see De Chaisemartin and d'Haultfoeuille 2022). The main problem in these papers arises from treatment effect heterogeneity within units over time, which can cause bias because earlier treated units serve as controls for later treated communities (Cunningham, 2021). To address this concern, I follow a methodology developed in Cengiz et al. 2019 and construct separate data sets for each treated unit where I eliminate all ever-treated units to make sure I am only comparing to control units that have not been treated. I then leverage this methodology as an additional robustness check for the key results in the paper.

In my setting, 29 of the 77 communities in Chicago are ever treated, leaving 48 communities to use as clean controls. Thus, I create 29 separate datasets, one for each of the treated communities, where I remove all observations besides those with the treated community and those in never treated communities. I then estimate "stacked" treatment effects whereby I add a robustness group index that identifies each of the 29 datasets, append the 29 datasets, and estimate the above regressions but add fixed effects for the robustness group indicator to make sure that comparisons only come from within each robustness group.[22] This allows me to recover average impacts over all treatments estimated only off of variation between treated units and never treated units, which circumvents the identification concerns raised in various papers.

This solution, however, raises additional concerns. For instance, what if never treated units are systematically different than ever treated units? To address this, I apply the above methodology both for the main regressions showing the one month local impacts and the event-study regressions that allow me to assess parallel trends. Ultimately, whether or not using never-treated units as controls makes sense will amount to whether the parallel trends assumption appears to hold in the data.

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[20] Unfortunately, my measure of victim race is not perfect. The CPDP has data on the race of the person who complains about the brutality event, usually not the actual victim. I argue that this is a reasonable proxy because often the victim is the complainer or a family member of the complainer or a friend of the complainer. The race of a person and any family member is highly correlated, though this correlation is weaker for friends. Further, there is data on the race of the victim for some of the incidents, but not most.

[21] One might be concerned that using other Chicago community areas as control units violates the no spillovers assumption required for a causal interpretation of Δ^1. In general, the most commonly discussed type of spillover where adjacent areas experience a treatment effect of the same sign, this will tend to underestimate the effects recovered. But, even in the case of spillovers of different signs (e.g. if crime were to be pushed into other communities), I include all communities, rather than restricting comparisons to neighboring communities (like in a border-pairs design), in order to reduce the chance that spillover effects are large in magnitude relative to the true treatment effects.

[22] When estimating impacts of racially charged incidents, I keep only robustness groups whose treated community experienced a racially charged police brutality, which is only three of the robustness groups.

Table 2
City-level effect of brutality on crime and arrests.

Panel A: Crime					
Dependent Variable	(1)	(2)	(3)	(4)	(5)
Total Crime	0.009***	−0.004	0.006*	−0.010***	−0.007**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Violent Crime	−0.019***	−0.015***	−0.004	−0.014***	−0.010*
	(0.005)	(0.004)	(0.005)	(0.005)	(0.005)
Property Crime	0.040***	0.002	0.010**	−0.006	−0.004
	(0.005)	(0.004)	(0.005)	(0.005)	(0.005)
Panel B: Arrests					
Dependent Variable	(1)	(2)	(3)	(4)	(5)
Total Arrests	0.012	−0.010*	0.003	−0.021***	−0.017***
	(0.009)	(0.006)	(0.006)	(0.006)	(0.006)
Violent Arrests	−0.021**	−0.014	−0.005	−0.014	−0.011
	(0.010)	(0.009)	(0.009)	(0.010)	(0.010)
Property Arrests	0.023	0.027**	0.037***	0.009	0.011
	(0.015)	(0.012)	(0.012)	(0.013)	(0.013)
Week of Year FE	no	yes	yes	yes	yes
Year FE	no	yes	yes	yes	–
Quarter by Year FE	no	no	no	yes	yes
Community Trends	no	no	yes	no	yes
N	198 336	198 336	198 336	198 336	198 336

Notes: Based on the City of Chicago reported crime data and CPDP complaint data. Each column represents estimates of δ from separate Poisson regressions of Eq. (2). Standard Errors, corrected for clustering at the community-level, are in parentheses.
* Statistically significant at the 10% level.
** Statistically significant at the 5% level.
*** Statistically significant at the 1% level.

5. Results

In this section, I summarize the results from my analysis. Recall that because the outcome variable is a count of daily crimes or arrests, all regression equations in this section are estimated using a Poisson regression model. Thus, coefficients of interest represent (roughly) the percent change in the daily number of crimes (or arrests) per-community within a month after a serious brutality incident occurred (aka the incidence rate ratio between the "treated" and "control" time periods). For clarity, all effects reported in the text of this section are calculated by exponentiating coefficients of interest so that they represent the exact incident rate ratios (IRR) rather than approximations and then taking the difference between the IRR and one to get a percentage change interpretation.

5.1. City-level results

In this subsection, I describe the results from all city-level repeated single differences estimates (from various versions of regression Eqs. (2), (4), and (5)). For clarity, I have split this description into two parts: first on the impact of all serious incidents and second on the impact of incidents with particular racial compositions of the victim and officer.

5.1.1. Effects of all serious brutality incidents
The main city-level results are presented in Tables 2, A1, and A2. Table 2 presents estimates of δ (the coefficient on an indicator for being within 1 month after a serious brutality incident) from various versions of Eq. (2). Each cell in this table represents an estimate of δ from a separate regression with a unique combination of controls and dependent variable. Panel A contains estimates where the dependent variables are crimes by type (total crime, violent crime, and property crime) while panel B contains estimates where the dependent variables are arrests by type (total arrests, violent arrests, and property arrests).

Estimates from my preferred specification are in column 5, which includes fixed effects for week and quarter by year as well as a time trend. This specification shows that total crime diminished by 0.7%

on average in the city of Chicago one month after a serious brutality incident. This total crime decrease was matched by a total arrest decrease of %1.7.

To examine the robustness of these estimates, I also estimate a version of Eq. (2) that includes seven dummy variables for being three weeks before to four weeks after the incident. This allows me to examine parallel trends in a visual event-study shown in Fig. 4. Panel A depicts estimates of the time dummy variables of interest in this regression with total crime as the dependent variable while in Panel B total arrests are the dependent variable. In Panel A, if anything, there is a positive pretrend in total crime that is reversed in the week following a brutality incident, which suggests that the negative crime estimates in column 5 of panel A of Table 2 are valid.[23] Similarly, In Panel B, there is a flat pretrend in total arrest rates which becomes negative in the weeks following a brutality incident in Chicago, which also supports the validity of the negative arrest estimates in column 5 of panel B of Table 2.

5.1.2. Effects of black victim and "racially charged" incidents
Table A1 presents results in the same form as Table 2, but instead presents estimates of δ_1 (the coefficient on an indicator for being within 1 month after a serious brutality incident where the victim was black), from various specifications of Eq. (4). When we restrict to the effect of these incidents, there is no significant effect of brutality on crime or arrests in the preferred specification (column 5). But, nearly all of the coefficients are negative, which may provide some evidence of a deterrence effect (more on interpreting these results using the model in Section 6.2).

Finally, Table A2 presents results in the same form as Tables 2 and A1, but instead presents estimates of δ_3, which gives the overall impact of "Racially Charged" incidents, from various specifications of Eq. (4). When we restrict to the effect of "Racially Charged Incidents", there is

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[23] An alternative interpretation is that the increase in crime in the pre-period suggests that rises crime may cause police brutality incidents, which could signal reverse causality. Even this case, however, is consistent with police brutality having a subsequent negative impact on crime.
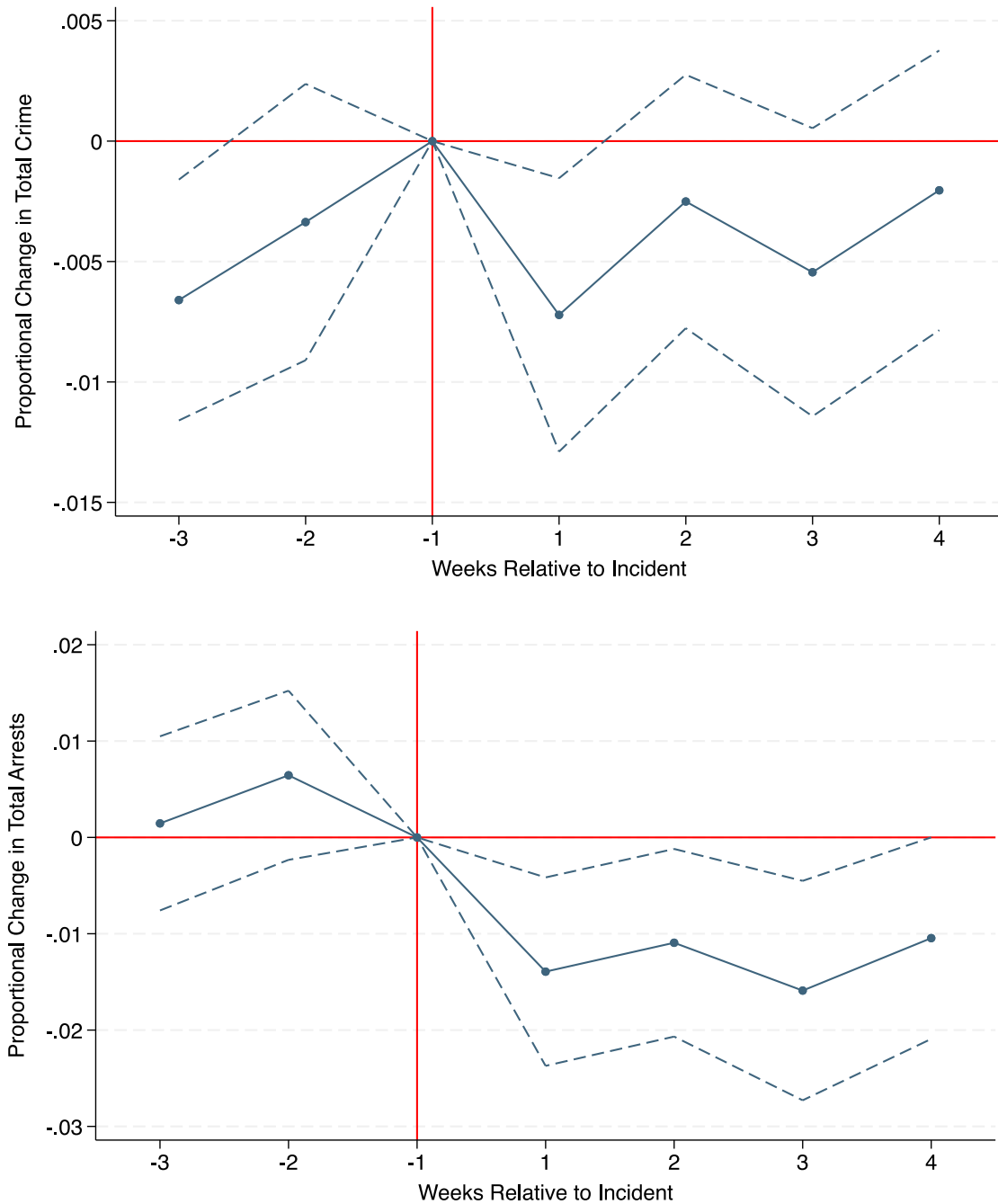
Fig. 4. City-level impact of brutality incidents on crimes/arrests (week-level).
Note: This event study plots coefficients from regressions in which an outcome (number of crimes or number of arrests in Chicago) is regressed on week dummies (defined relative to when a brutality incident occurred) using a Poisson model. For the full regression equation see Eq. (2) in Section 4 of the paper.

no significant effect of brutality on crime and a significant negative impact on arrests in the preferred specification (column 5). Though all of the crime coefficients are negative, the negative coefficients on arrests are of a much larger magnitude, which provides weak evidence of a city-wide depolicing effect for these cases in particular (more on interpreting these results using the model in Section 6.2).

5.2. Community-level results

In this subsection, I describe the results from all community-level difference-in-differences estimates (from various versions of regression Eqs. (6), (8), and (9)). Like for the city-level results, I have split this description into two parts: first on the impact of all serious incidents and

second on the impact of incidents with particular racial compositions of the victim and officer.

5.2.1. Effects of all serious brutality incidents

The main community-level results are presented in Tables 3, A3, and 5. Similar to the city-level results tables, Table 3 presents estimates of δ (the coefficient on an indicator for being within 1 month after a brutality incident within the community where it occurred) from various versions of Eq. (6). Each cell in this table represents an estimate of δ from a separate regression with a unique combination of controls and dependent variable. Panel A contains estimates where the dependent variables are crimes by type (total crime, violent crime, and property

Table 3
Local effect of brutality on crime and arrests.

Panel A: Crime							
Dependent Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Total Crime	0.230**	0.002	0.003	0.026***	0.019*	0.023*	0.021**
	(0.100)	(0.010)	(0.010)	(0.009)	(0.010)	(0.012)	(0.010)
Violent Crime	0.181	0.014	0.017	0.029**	0.028*	0.030*	0.031*
	(0.118)	(0.015)	(0.015)	(0.014)	(0.016)	(0.017)	(0.017)
Property Crime	0.271***	0.017	0.017	0.035***	0.030*	0.018	0.016
	(0.084)	(0.021)	(0.021)	(0.017)	(0.017)	(0.014)	(0.014)
Panel B: Arrests							
Dependent Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Total Arrests	0.259**	−0.017	−0.014	0.023	0.014	0.029	0.028
	(0.130)	(0.019)	(0.019)	(0.015)	(0.016)	(0.019)	(0.018)
Violent Arrests	0.186	0.022	0.027	0.044	0.034	0.034	0.034
	(0.119)	(0.027)	(0.028)	(0.028)	(0.032)	(0.038)	(0.036)
Property Arrests	0.351***	−0.027	−0.029	−0.008	−0.021	−0.017	−0.018
	(0.117)	(0.033)	(0.034)	(0.036)	(0.039)	(0.044)	(0.045)
Community FE	no	yes	yes	yes	yes	yes	yes
Mo x Yr FE	no	yes	yes	yes	yes	yes	yes
Week of Yr FE	no	no	yes	yes	yes	yes	yes
Comm x Yr FE	no	no	no	yes	yes	–	–
Comm x Quarter x Yr FE	no	no	no	no	no	yes	yes
Community Trends	no	no	no	no	yes	no	yes
N	198 336	198 336	198 336	198 275	198 275	197 340	197 340

Notes: Based on the City of Chicago reported crime data and CPDP complaint data. Each column represents estimates of δ from separate Poisson regressions of Eq. (6). Standard Errors, corrected for clustering at the community-level, are in parentheses.
* Statistically significant at the 10% level.
** Statistically significant at the 5% level.
*** Statistically significant at the 1% level.

crime) while panel B contains estimates where the dependent variables are arrests by type (total arrests, violent arrests, and property arrests).

Estimates from my preferred specification are in column 7, which includes fixed effects for community, month by year, week, and community by quarter by year as well as community-specific time trends. Contrary to the city-wide effect of all brutality incidents, this community-level specification yields a statistically significant 2.1% one-month increase in total crime and a marginally significant 3.1% one-month increase in violent crime. Thus, the local impact of brutality on crime appears to have the opposite sign as its city-wide impact. Column 7 of panel B show that there were no significant changes in local arrests, though all of the coefficients are positive.

I examine the robustness of these community-level estimates two ways: (1) by re-estimating these impacts restricting control comparisons to never-treated units[24] and (2) by estimating a version of Eq. (6) that includes seven dummy variables for being three weeks before to four weeks after the incident. Results from the first robustness exercise can be seen in Table 4, which has the same structure as Table 3. Conclusions from this table are qualitatively similar (i.e. one-month increases in total crime and violent crime), but these effects are not longer statistically significant at conventional levels. The second robustness exercise allows me to examine parallel trends in a visual event-study shown in Fig. 5. Panel A depicts estimates of the time dummy variables of interest in this regression with total crime as the dependent variable while in panel B violent crime are the dummy variable. In Panel A, there is weak evidence for a slight positive pretrend. Furthermore, the increase in total crime is concentrated in the third week after the incident. In panel B, there is a flat pretrend in violent crime rates and, like in panel A, the effects are concentrated three weeks after the incident. Altogether,

these event study provide no evidence invalidating the estimates in Table 3, though the lagged impact on both total and violent crimes admits no obvious explanation.

I conclude by exploring the geography of these local effects by expanding the treated unit to the district-level. To do this, I estimate the same model, expanding treated units out to districts, which are larger geographic regions that encompass communities. Results from this exercise can be found in Figure A6. This analysis shows that the positive impacts of brutality incidents largely disappears at the district level, which is consistent with the idea that the impacts are locally concentrated.

5.2.2. Effects of black victim and "racially charged" incidents

Table A3 presents results in the same form as the previous results tables, but instead presents estimates of Δ_1 (the coefficient on an indicator for being within 1 month after a brutality incident where the victim was black within the community where it occurred) from various specifications of Eq. (8). When we restrict to the effect of these incidents, the only statistically significant effect is a 5.8% increase in violent crime. When I examine the robustness of this estimate in the corresponding event study figure, however, there is evidence of a pretrend that is strong enough to call into question the validity of the effect.

Finally, Table 5 presents results in the same form as the previous tables, but instead presents estimates of Δ_3 (which gives the overall impact of "Racially Charged" incidents) from various specifications of Eq. (8). When we restrict to the effect of "Racially Charged" incidents, we see statistically significant and large positive impacts on total, violent, and property crimes as well as total and violent arrests. More specifically, there was a 10.5%, 11.3%, and 14.7% increase in total crime, violent crime, and property crime, respectively. Furthermore, there was a 13.1% and 31% increase in total arrests and violent arrests, respectively.

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[24] See Section 4.2.1 for a more detailed discussion of how this is comparison is executed.

Table 4
Local effect of brutality on crime and arrests [TWFE Robust].

Panel A: Crime							
Dependent Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Total Crime	0.228**	−0.000	0.000	0.024**	0.016	0.020	0.019
	(0.097)	(0.011)	(0.011)	(0.010)	(0.011)	(0.014)	(0.012)
Violent Crime	0.181	0.013	0.015	0.029*	0.027	0.029	0.030
	(0.115)	(0.017)	(0.017)	(0.016)	(0.018)	(0.021)	(0.020)
Property Crime	0.267***	0.014	0.014	0.035*	0.030	0.019	0.017
	(0.083)	(0.023)	(0.023)	(0.019)	(0.019)	(0.017)	(0.016)
Panel B: Arrests							
Dependent Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Total Arrests	0.260**	−0.020	−0.018	0.014	0.004	0.020	0.020
	(0.123)	(0.020)	(0.019)	(0.016)	(0.017)	(0.022)	(0.022)
Violent Arrests	0.190	0.016	0.019	0.043	0.032	0.029	0.025
	(0.116)	(0.031)	(0.032)	(0.034)	(0.038)	(0.046)	(0.044)
Property Arrests	0.349***	−0.039	−0.039	−0.005	−0.019	−0.014	−0.015
	(0.107)	(0.037)	(0.037)	(0.039)	(0.041)	(0.045)	(0.045)
Community FE	no	yes	yes	yes	yes	yes	yes
Mo x Yr FE	no	yes	yes	yes	yes	yes	yes
Week of Yr FE	no	no	yes	yes	yes	yes	yes
Comm x Yr FE	no	no	no	yes	yes	–	–
Comm x Quarter x Yr FE	no	no	no	no	no	yes	yes
Community Trends	no	no	no	no	yes	no	yes
N	3 492 325	3 492 325	3 492 325	3 490 617	3 490 617	3 468 271	3 468 271

Notes: Based on the City of Chicago reported crime data and CPDP complaint data. Each column represents estimates of δ from separate Poisson regressions of Eq. (6). These estimates are "Robust" in the sense that they only use never treated communities as controls to eliminate identification concerns that arise from potentially heterogeneous treatment effects over time. Standard Errors, corrected for clustering at the community-level, are in parentheses.

* Statistically significant at the 10% level.

** Statistically significant at the 5% level.

*** Statistically significant at the 1% level.

Table 5
One month local effect of "racially charged" brutality on crime and arrests.

Panel A: Crime							
Dependent Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Total Crime	0.664	0.080**	0.067**	0.084***	0.087***	0.128***	0.100***
	(0.435)	(0.032)	(0.030)	(0.023)	(0.025)	(0.023)	(0.020)
Violent Crime	0.496	−0.000	−0.001	0.061**	0.087**	0.137***	0.107***
	(0.438)	(0.027)	(0.034)	(0.027)	(0.038)	(0.033)	(0.031)
Property Crime	0.614	0.203***	0.184***	0.156***	0.155***	0.162***	0.137***
	(0.408)	(0.025)	(0.028)	(0.016)	(0.016)	(0.032)	(0.030)
Panel B: Arrests							
Dependent Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Total Arrests	0.929**	0.061	0.034	0.082*	0.097**	0.145***	0.123***
	(0.474)	(0.079)	(0.071)	(0.043)	(0.047)	(0.034)	(0.045)
Violent Arrests	0.514	0.132***	0.134**	0.192***	0.221***	0.317***	0.270***
	(0.475)	(0.051)	(0.053)	(0.053)	(0.058)	(0.048)	(0.041)
Property Arrests	0.879**	0.026	−0.001	0.057	0.059	0.099	0.118
	(0.445)	(0.156)	(0.157)	(0.151)	(0.166)	(0.141)	(0.126)
Community FE	no	yes	yes	yes	yes	yes	yes
Mo x Yr FE	no	yes	yes	yes	yes	yes	yes
Week of Yr FE	no	no	yes	yes	yes	yes	yes
Comm x Yr FE	no	no	no	yes	yes	–	–
Comm x Quarter x Yr FE	no	no	no	no	no	yes	yes
Community Trends	no	no	no	no	yes	no	yes
N	198 336	198 336	198 336	198 275	198 275	197 340	197 340

Notes: Based on the City of Chicago reported crime data and CPDP complaint data. Each column represents estimates of Δ_3 from separate Poisson regressions of Eq. (9). Standard Errors, corrected for clustering at the community-level, are in parentheses.

* Statistically significant at the 10% level.

** Statistically significant at the 5% level.

*** Statistically significant at the 1% level.

Table 6

Local effect of "racially charged" brutality on crime and arrests [TFWE Robust].

Panel A: Crime							
Dependent Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Total Crime	0.369***	0.024**	0.021**	0.044***	0.038***	0.062***	0.046***
	(0.087)	(0.010)	(0.011)	(0.010)	(0.010)	(0.013)	(0.009)
Violent Crime	0.375***	0.022	0.022	0.072***	0.080***	0.121***	0.090***
	(0.104)	(0.017)	(0.018)	(0.018)	(0.017)	(0.015)	(0.013)
Property Crime	0.469***	0.112***	0.108***	0.102***	0.102***	0.085***	0.073***
	(0.074)	(0.017)	(0.018)	(0.012)	(0.013)	(0.029)	(0.025)
Panel B: Arrests							
Dependent Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Total Arrests	0.335***	−0.009	−0.009	0.015	0.010	0.033***	0.029**
	(0.097)	(0.022)	(0.022)	(0.015)	(0.014)	(0.011)	(0.013)
Violent Arrests	0.454***	0.068**	0.075***	0.123***	0.121***	0.148***	0.110**
	(0.107)	(0.028)	(0.029)	(0.041)	(0.041)	(0.041)	(0.056)
Property Arrests	0.404***	0.056	0.053	0.076*	0.066	0.077*	0.085*
	(0.131)	(0.058)	(0.059)	(0.042)	(0.047)	(0.046)	(0.047)
Community FE	no	yes	yes	yes	yes	yes	yes
Mo x Yr FE	no	yes	yes	yes	yes	yes	yes
Week of Yr FE	no	no	yes	yes	yes	yes	yes
Comm x Yr FE	no	no	no	yes	yes	–	–
Comm x Quarter x Yr FE	no	no	no	no	no	yes	yes
Community Trends	no	no	no	no	no	no	yes
N	374 227	374 227	374 227	374 044	374 044	371 655	371 655

Notes: Based on the City of Chicago reported crime data and CPDP complaint data. Each column represents estimates of Δ_3 from separate Poisson regressions of Eq. (9). These estimates are "Robust" in the sense that they only use never treated communities as controls to eliminate identification concerns that arise from potentially heterogeneous treatment effects over time. Standard Errors, corrected for clustering at the community-level, are in parentheses.

* Statistically significant at the 10% level.

** Statistically significant at the 5% level.

*** Statistically significant at the 1% level.

As I did when examining the local effect of all serious brutality incidents, I examine the robustness of race-specific community-level estimates two ways: (1) by re-estimating these impacts restricting control comparisons to never-treated units[25] and (2) by estimating versions of Eqs. (8) and (9) that includes seven dummy variables for being three weeks before to four weeks after the incident. Results from the first robustness exercise can be seen in Table 6, which has the same structure as Table 5. Conclusions from this table are qualitatively similar (i.e. total crime, violent crime, arrests, and violent arrests all spike the month after the incident) and all these effects remain statistically significant at conventional levels. But, the magnitudes of these effects are meaningfully smaller, with reductions in coefficient size that range from 16% to 76%. The second robustness exercise allows me to examine parallel trends in a visual event-study shown in Figs. 6, A2, and A1. In each figure, Panel A depicts estimates of the time dummy variables of interest in this regression with a crime outcome as the dependent variable while in Panel B the corresponding arrests outcome is the dependent variable.

I first examine the robustness of the total crime and total arrests results in Table 5. In Panel A of Fig. 6 I present event-study style evidence of a mild positive pretrend in total crime, which suggests that the positive effects on total crime in column 7 of Table 5 are somewhat overstated. In Panel B, there is a negative pretrend in total arrest rates which reverses in the weeks following a brutality incident in Chicago, which supports the validity of the positive arrest estimates in column 7 of panel B of Table 5 (though it is possible that this is driven by an idiosyncratic value the period before treatment, so this interpretation is somewhat speculative).

A similar assessment of the validity of the violent crime and arrest results can be conducted. In Panel A of Figure A2 there is a positive pretrend in violent crime, which suggests that the positive effects on total crime in column 7 of Table 5 are overstated. Furthermore, the impact of brutality on crime appears to be lagged by two weeks. In Panel B, there is a flat pretrend in violent arrest rates which become sharply positive in the two weeks immediately following a "racially charged" incident. This supports the validity of the positive violent arrest estimates in column 7 of panel B of Table 5.

I also assess the robustness of the property crime and arrest results. In Panel A of Figure A1 there is a positive (insignificant) pretrend in property crime, which suggests that the positive effects on property crime in column 7 of Table 5 are somewhat overstated. In Panel B, there is a flat (or somewhat negative) pretrend in property arrest rates which ultimately becomes positive two weeks after a "racially charged" incident. This supports the validity of the positive (but insignificant) property arrest estimates in column 7 of panel B of Table 5.

To this point, the most robust findings have been the short run local impact of racially charged brutality incidents on total crime and arrests. Thus, I perform a few additional robustness exercises on these estimates. First, I reestimate these event-study-style figures using only the "clean" never-treated controls (which I call "TWFE robust" estimates). Second, I vary whether I am weighting by community population in both my original ("main") event-study estimates and the TWFE robust estimates. The results from these exercises are shown in Fig. 7. In general, the patterns from the main specifications are unaltered by these additional robustness checks. The main difference is that the magnitude of the post-event impacts are generally reduced, but still significant, when restricting comparisons to "clean" controls.[26]

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[25] See Section 4.2.1 for a more detailed discussion of how this is comparison is executed.

[26] In the appendix, I also produce TWFE robust event-study-style figures for violent crime/arrests and property crime/arrests results. Results can be found
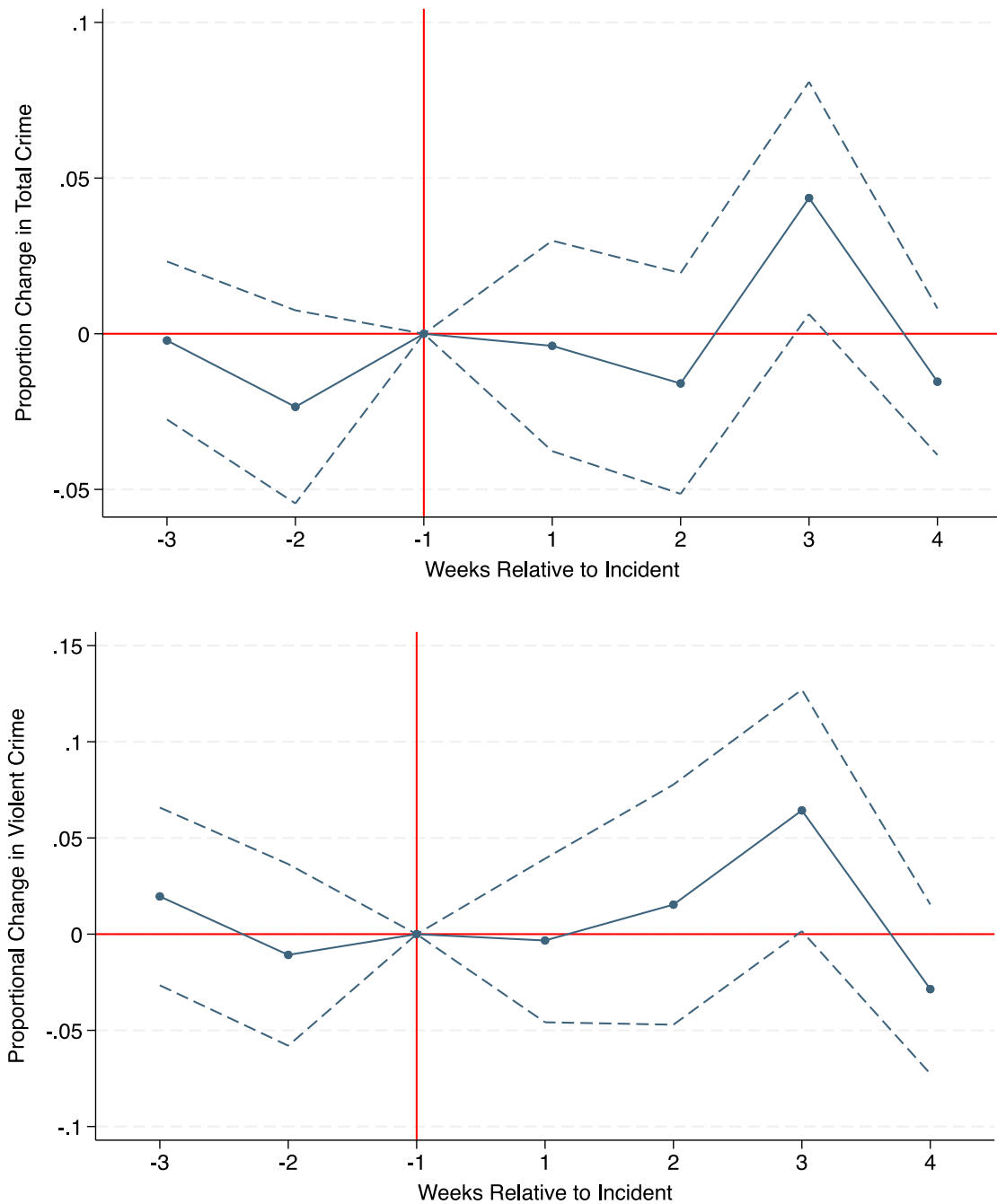
Fig. 5. Local impact of brutality incidents on total/violent crime (week-level).
Note: This event study plots coefficients from regressions in which an outcome (number of crimes or number of violent crimes within a community) is regressed on week dummies (defined relative to when a brutality incident occurred within a community) using a Poisson model. For the full regression equation see Eq. (6) in Section 4 of the paper.

I conclude by exploring the geography of these local effects by expanding the treated unit to the district-level. To do this, I estimate the same model, expanding treated units out to districts, which are larger geographic regions that encompass communities. Results from this exercise can be found in Figure A5. This analysis shows that the positive impacts of racially charged brutality incidents vanishes at the

district level, which is consistent with the idea that the impacts are in fact local.

6. Interpretation and mechanisms

6.1. Interpreting city-level vs. Community-level estimates

Because the city-level and community-level models are different, they ought to be interpreted in a qualitatively different way. The city-level estimates leverage a repeated single differences design, effectively calculating the city-wide average crime and arrest rate at different

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in Figures A4 and A3. Results generally amplify conclusions drawn from main event study figures were discussed for these results.
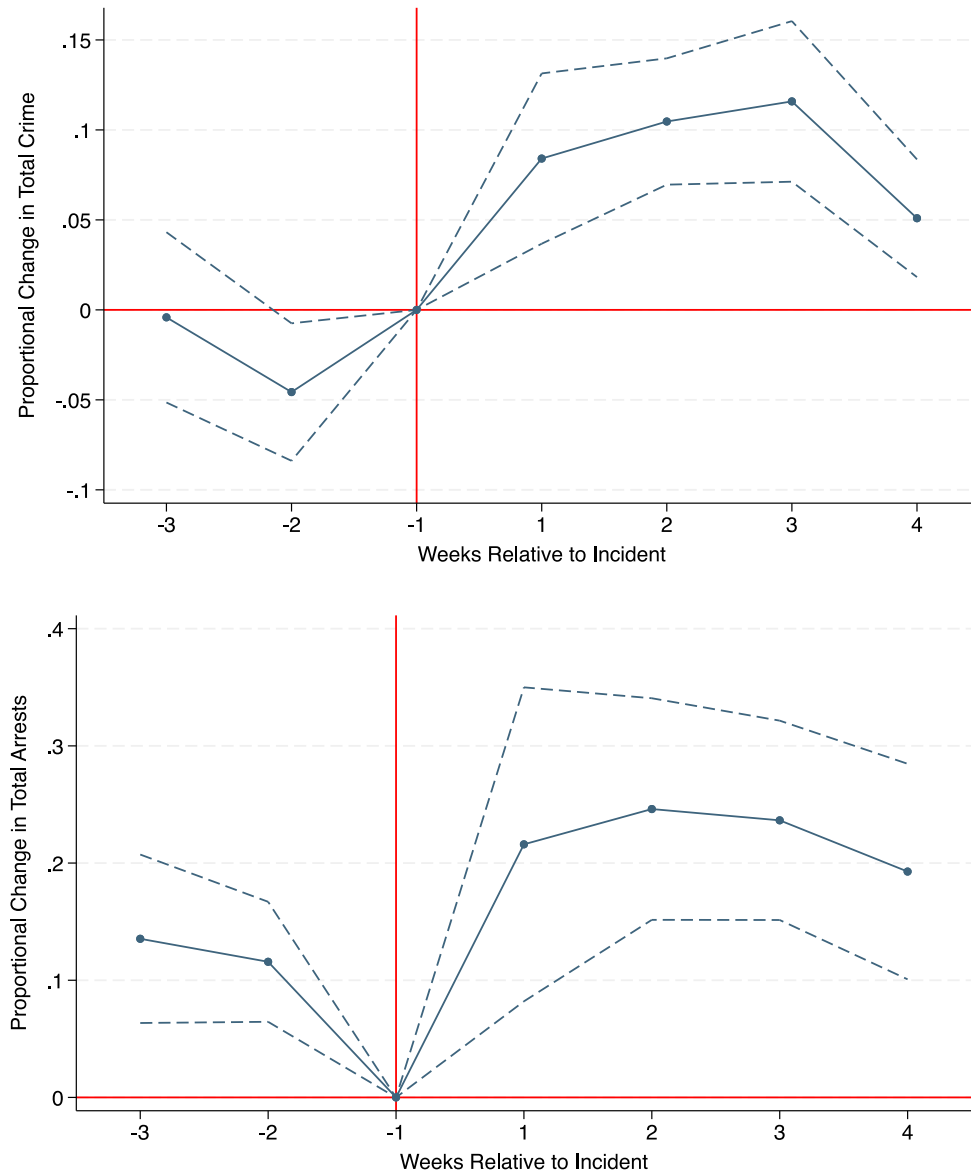
Fig. 6. Local impact of racially charged incidents on total crime/arrests (week-level).
Note: This event study plots coefficients from regressions in which an outcome (number of total crimes and total arrests within a community) is regressed on week dummies (defined relative to when a brutality incident with a black complainant and white officer occurred within a community) using a Poisson model. For the full regression equation see Eq. (9) in Section 4 of the paper.

periods relative to a serious police brutality incident. The only significant results from this model suggest that there is a systematic dip in city-wide crime rates the month after a serious brutality incident, and that there is little variation in this dip by racial composition of the incident (though the dip is insignificant when focusing on black victim or racially charged incidents). These results should be taken with caution because this identification strategy is inherently limited: other factors effecting Chicago at the same times will tend to bias the impact estimates.

The community-level estimates, however, leverage variation in the timing and location of serious brutality incidents. Though identification challenges with these designs have been pointed out (for a recent survey, see De Chaisemartin and d'Haultfoeuille 2022), the estimates from this specification are likely more credible because they implicitly use untreated communities, within a similar time-frame as when the treatments occur, as control units. Thus, all results from this model

should be interpreted as impacts of police brutality *relative to other, untreated communities*. The key findings from the community-level models are local increases in crimes and arrests for serious brutality in general, that get amplified when limiting focus to racially charged incidents.

The joint city-level and community-level estimates are consistent with a series of interpretations about underlying mechanisms. For example, the city-level estimates may mean that every community experiences a common deterrent effect, but that in communities that experience the brutality incident, retaliation is a stronger mechanism than in other communities, which results in a positive treatment coefficient in the community models. Or, the deterrent effect may only occur in untreated units, while only retaliation effect occurs in treated communities. Though we cannot definitively rule out these stories, it is important to keep in mind that the city-level estimates are small (and less well-identified) relative to the community-level estimates. Thus, any story where the city-level deterrent effects are either attributed to each community evenly, even leaving out the treated community,
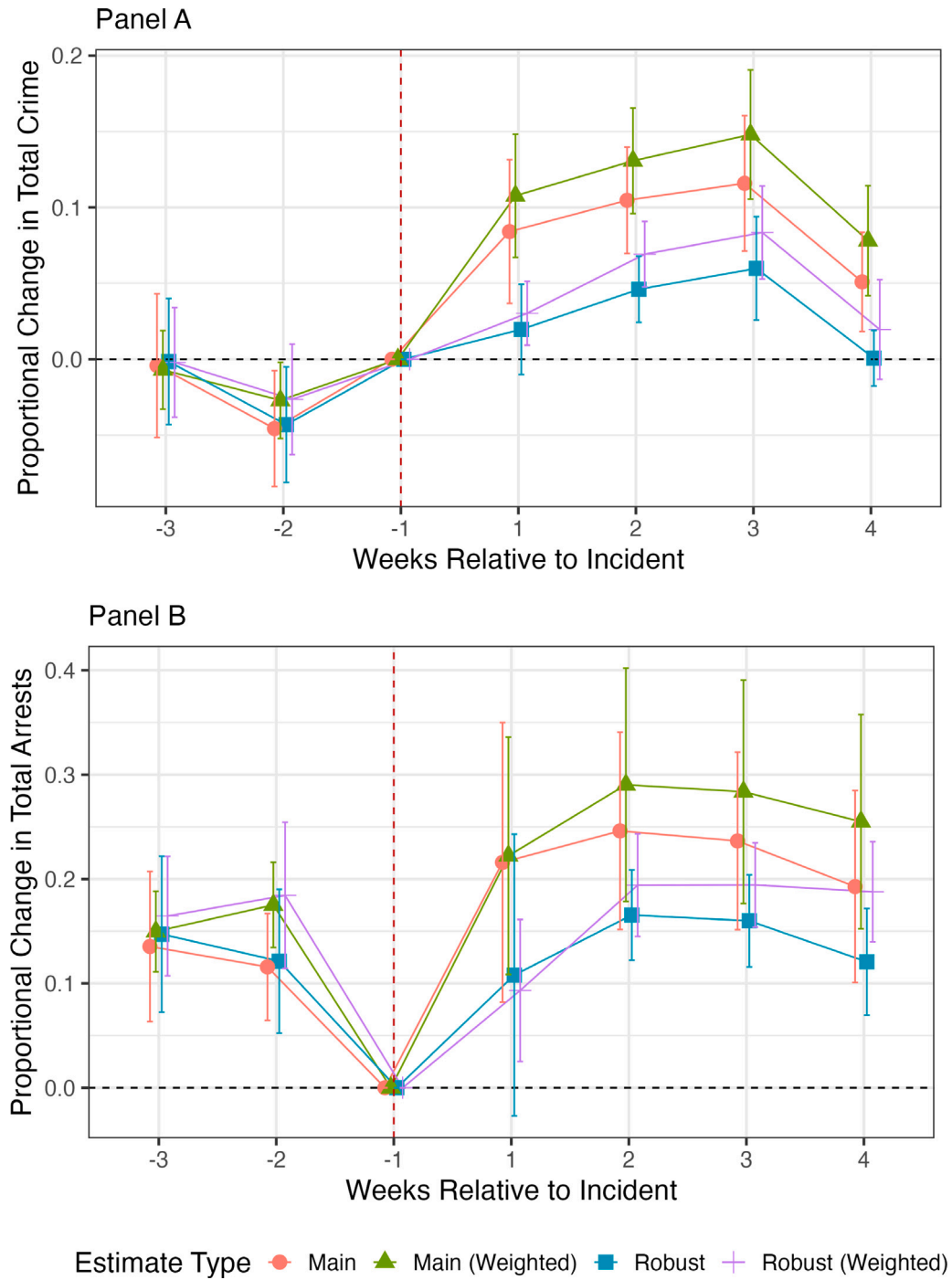
Panel A

Panel B

Fig. 7. Local impact of racially charged incidents on total crime & arrests [TWFE robust].

Note: These event study plots show coefficients from regressions in which an outcome (i.e. number of total crimes (Panel A) and total arrests (Panel B within a community) is regressed on week dummies (defined relative to when a brutality incident with a black complainant and white officer occurred within a community) using a Poisson model. In each Panel results from four versions of these regressions are depicted. "Robust" estimates are estimated using only never treated communities as controls to eliminate identification concerns that arise from potentially heterogeneous treatment effects over time. "Weighted" estimates weight regressions using community population. For the full regression equation see Eq. (9) in Section 4 of the paper.

cannot fully explain the differences in treated and untreated units. This is particularly true for the racially charged estimates, where community effect magnitudes are particularly large.[27]

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[27] More specifically, if the city-level results are taken at face-value the local effect estimates will be overestimated. Given the relatively small magnitude of the city-wide estimates, none of the significant coefficients discussed in this paper can be fully explained by this. But, in the case of the community-level effect of any incident, this could meaningfully reduce the coefficient estimate from (roughly) .021 to .014, which would make this coefficient insignificant. Effects of "racially charged" incidents, however, would be inconsequentially altered.

6.2. Assessing relative importance of mechanisms

In Section 2 I used a simple model to derive empirical predictions that differentiate the deterrence, retaliation, and depolicing mechanisms. The first prediction was that only deterrence is consistent with a negative impact of brutality on crime. The second prediction was that only depolicing is consistent with a positive brutality impact on crime and a negative brutality impact on arrests. In the following subsections, I discuss which mechanisms are most consistent with the empirical evidence presented in Section 5.

6.2.1. Deterrence

At the city-level, the only significant impacts of serious brutality on crime are negative effects on total crime (see column 5 of Table 2). This provides evidence that, at the city-level, the typical serious brutality incident as a slight deterrent effect on crime. It is important to note that, however, that the city-level results are more subject to identification problems than the community-level results, so this conclusion should be seen as tentative.

6.2.2. Depolicing vs. Retaliation

At the community-level all crime and arrest effects are positive. This means that there is no clean way of distinguishing between the Depolicing Effect and the Retaliation Effect using the second empirical prediction from Section 2. This is particularly true when analyzing the impacts of all serious incidents and black victim incidents, where the significant positive crime impacts are matched with insignificant but positive arrest coefficients (see Table 3, A3).

For "racially charged" incidents, however, the evidence appears more consistent with retaliation than depolicing. The main reason is that depolicing will tend to reduce officer effort and, therefore, arrest rates. Though depolicing is consistent with increased crime rates if the supply of crime simultaneously increases, we should not expect this increase to be higher than the increase in crime rates. But, if we examine the results, the magnitude of the arrest increases are larger (and more robust) than the crime estimates. Though not a perfect test, this is suggestive that policing effort did not decline after these incidents. Thus, for "racially charged" incidents, retaliation is more supported by the empirical evidence.[28]

The final takeaway from this analysis is that the largest community level impacts occur after "racially charged" incidents. This suggests that, whatever the underlying mechanism, the extent of the crimino-genic and law enforcement responses to these incidents appears to be much larger for "racially charged" incidents. Furthermore, based on Census data, communities where at least one "racially charged" brutality incident occurred are 32 percentage points more black and have a 9 percentage point higher poverty rate than other communities within Chicago. This is consistent with a retaliation narrative because it is more plausible that a white officer brutalizing a black victim will cause retaliation in black neighborhoods. This also implies that the cost of additional crime from these incidents may be born by dispro-portionately black and poor residents, which, if true, has important distributional implications.

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[28] Another potential mechanism explored in Ang et al. (2021) is that police brutality may reduce civilian cooperation with police and, therefore, crime reporting. Because there is no simple way to measure civilian cooperation in my data, I do not explore this mechanism in the paper, but if true, this would tend to understate the local positive effect of brutality on crime and arrests. Whether or not this mechanism is partially operative does not obviously inform the comparison between depolicing and retaliation since it would effect both crime and arrests in the same direction and (likely) the same magnitude.

7. Conclusion

Over the last decade, high-profile police brutality incidents have shocked the nation and shifted attention to the causes and conse-quences of police brutality. In this paper, I presented a framework and empirical evidence to inform a potential consequence of police brutal-ity: its effect on crime. From my results, there are four main takeaways. First, serious police brutality incidents that do not necessarily receive national media attention seem to have important local effects on crime and arrests rates. Second, looking only at city-level responses (like most previous work does) misses differences in local impacts of these incidents at closer proximity. In this case, while there appears to be a reduction in crime overall in Chicago after a brutality incident, crime in the community where it occurred actually seems to increase. Third, the racial composition of brutality cases matters for the magnitude of the local crime and arrest rate impacts. In particular, "racially charged" incidents cause much larger increases in crime and arrest rates locally than other types of brutality incidents. And, fourth, I find no strong evidence of depolicing causing crime increases in the after math of these events, which is in sharp contrast to the media attention on this mechanism.

Throughout the paper, I have attempted to make clear the imper-fections of this context and my analysis. In particular, it is hard to rule out that spillovers affect the community-level estimates or that the timing of incidents is related to changes in (perhaps unobserved) criminal activity. Despite this, the results discussed in the paper are highly suggestive that police brutality impacts local crime in ways that have previously been unexplored, even if they can be improved in various ways. In my opinion, the aspect of the paper that is most important for future research to improve on is the procedure for classifying police brutality incidents as "serious". In particular, I use whether or not the investigation associated with the case was sustained or disciplined to determine how serious the incident is. Ultimately, this excludes brutality incidents that are serious along other dimensions (such as the details of the incident), which could be equally important to consider. Secondly, though this is the first paper to my knowledge to use multiple serious incidents to explore heterogeneous impacts by the racial composition of incidents, I am only able to leverage four black-victim-white-officer incidents, which limits the extent to which I can say this is a general pattern for "racially charged" incidents.

Finally, the results from this study have at least three potential policy implications. First, this paper provides new evidence that there are indirect consequences of brutality incidents on crime that dispro-portionately harm those local to brutality. This gives an additional reason to prevent these incidents via incentives (harsher punishment for misconduct) or new technologies (e.g. body cameras). Second, be-cause this paper fails to find clear evidence of depolicing, this suggests that there may only be a limited kind of brutality incident that requires adjustment of police incentives to get them to do their jobs in the aftermath. Finally, the substantial increase in the magnitude of the effects for "racially charged" incidents suggests that matching officers to the race of those they are policing may be a way of minimizing the likelihood of large crime responses to brutality incidents.

CRediT authorship contribution statement

Kadeem Noray: Conceptualization, Data curation, Formal analy-sis, Investigation, Methodology, Visualization, Writing – original draft, Writing – review & editing.

Appendix A. Supplementary data

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