# THE CAUSAL EFFECT OF POLICE BRUTALITY ON LOCAL CRIME: EVIDENCE FROM CHICAGO

by

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# ABBREVIATIONS

BV – Black Victim

BVWO – Black Victim and White Officers

CPDP – Citizens Police Data Project

IPRA – Chicago's Independent Police Review Authority

OLS – Ordinary Least Squares

## ABSTRACT

Using recently digitized complaints made about Chicago police officers released by the Citizen's Police Data Project, I estimate the effect of police brutality on short-term local crime. My empirical strategy uses conditionally random variation in the timing and location of serious excessive force incidents within Chicago to identify the causal relationship between these incidents and local crime. I also explore how this relationship changes with proximity to where a brutality incident occurred, the race of the victim, and the race of the offending officer. I find that, within a month after a brutality incident occurred, the average incident is associated with a one percent decrease in citywide violent crime. Within a community where an incident occurred, however, there is a three percent increase in violent crime and a two percent increase in total crime. Expanding out to the district, the total crime effect disappears, and the violent crime effect diminishes to two percent but remains positive. I also find that if the victim of a brutality incident is black, the community-level effect on violent crime increased to four percent, the district-level effect on violent crime increased to seven percent, and property crime increased by six percent at the district-level. If the offending officer is white and the victim is black (a.k.a. if the incident is potentially racially charged), there is a ten percent increase in total crime and an 18 percent increase in property crime within a month after the incident, both at the community-level.

# INTRODUCTION

There is a growing sense that police brutality is getting worse in America (McLaughlin, 2015). New names of brutality victims like Michael Brown, Tamir Rice, Eric Garner, Walter Scott, and Freddie Gray appear in newspaper headlines monthly.[1] The formation of groups like Black Lives Matter and protests reminiscent of the civil rights era have made it clear that there is still tension between police and minorities, and this tension may have unintended consequences.

A few months after Michael Brown's murder, criminologists noticed an increase in homicide rates in major U.S. cities; news outlets quickly popularized the idea that Brown's death caused the uptick in murders (cf. Mac Donald, 2016abc; Lopez, 2016; Gold, 2015). This informal theory became known as the "Ferguson Effect" (Mac Donald, 2015). Chicago's mayor, Rahm Emmanuel, has proposed an explanation for the supposed Ferguson Effect. Emmanuel claims that the scrutiny society placed on police after Michael Brown's murder has caused officers to pull "back from the ability to interdict... [because] they don't want to be a news story themselves" and because "they don't want their career ended early" (Byrne, 2015). Furthermore, Emmanuel believes that "we have allowed our police department to get fetal, and it

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[1] 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 & 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 (Schmidt & 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).

is having a direct consequence" (Agrawal, 2015). If brutality incidents cause police to become timid, the expected costs of crime will fall. It is also possible that police brutality incidents incite social unrest, which manifests as criminal retaliation. Like the Ferguson effect, this should increase crime, ceteris paribus. On the other hand, after a brutality incident, some potential criminals may be deterred from committing crimes because they become more cognizant of the risk of a brutal police reaction. Because the direction of the overall effect is theoretically ambiguous, the relationship between police brutality and crime is ultimately an empirical question.

Using officer complaint data on brutality incidents in Chicago, I estimate the relationship between police brutality and crime by exploiting the temporal and spatial variation of these incidents. First, I estimate the effect of brutality on citywide crime by comparing crime rates shortly before and after an incident. Second, I estimate whether the effect depends on proximity to the brutality incident by estimating the effect on the surrounding community and district. Finally, I determine whether these effects vary by the race composition of the victim and officer.

Results suggest that, within a month after the average brutality incident occurred, violent crime decreases by one percent. Within a community where an incident occurred, however, there is a three percent increase in violent crime and a two percent increase in total crime. Expanding out to the district, the total crime effect disappears, and the violent crime effect diminishes to two percent but remains positive. I also find that if the victim of a brutality incident is black, the community-level effect on violent crime increases to four percent, the district-level effect on violent crime increases to seven percent, and property crime increases by six percent at the district-level. If the offending officer is white and the victim is black (a.k.a. if the incident is potentially racially charged), there is a ten percent increase in total crime and an 18 percent increase in property crime within a month of the incident, both at the community-level.

# REVIEW OF LITERATURE

Becker (1968) originated the idea that, even in the context of committing crime, people weigh the expected costs and benefits of their actions. Though there are multiple reasons to believe that the costs and benefits associated with criminal behavior change after a police brutality incident, only Pyrooz et al. (2016) and Shi (2009) have examined the causal relationship between police brutality and crime.[1] Shi (2009) finds evidence that a 2001 police shooting (and subsequent riot) in Cincinnati increased crime rates through reduced policing effort. Pyrooz et al. (2016) use fixed and random effects models to estimate the effect of the Michael Brown shooting on crime trends in major U.S. cities and, unlike Shi, they do not find a significant relationship. While Shi (2009) and Pyrooz et al. (2016) address relevant and crucial issues, they only analyze a single brutality incident, which makes it difficult to rule out that other concurrent events confound their estimates.[2]

I circumvent each of these issues by using daily crime data and exploiting 49 serious excessive force incidents. With high-frequency crime data, I can estimate the average effect of brutality incidents on crime in the following day or week. Furthermore, by including various fixed effects, my empirical strategy allows me to control for area-specific and time-specific heterogeneity that may otherwise bias my results. Also, by including area-specific time trends I am able to control for changes in community characteristics (i.e. population, racial composition, income, etc.) that evolve at a constant rate. Lastly, given the geographic detail of the data, I can

[1]Additionally, Rosenfeld (2015) and Rosenfeld (2016) are two frequently referenced descriptive studies that show an increase in homocide rates in Baltimore after Michael Brown's murder.

[2]Also, Pyrooz et al. (2016) cannot estimate immediate shocks to crime (within the day or week of the Brown shooting) because they use monthly crime rates. Furthermore, though they control for seasonality with month fixed effects, Pyrooz et al. (2016) fail to control for time-specific shocks (they omit season by year fixed effects) and region-specific shocks (they omit region fixed effects).

examine whether the effect of brutality varies with proximity to the incident, which previous studies have not been able to assess.

Racial inequity is another important concern related to police brutality. When studying racial inequity, economists often care about the role of taste-based vs. statistical discrimination. Taste-based discrimination refers to racial bias that is motivated by an individual's preferences. Statistical discrimination refers to racial bias that is motivated by an individual's attempt to use race as a proxy for other characteristics.[3] The literature on whether taste-based or statistical discrimination is more prevalent is inconclusive; depending on the context, some find that statistical discrimination dominates (Ewens et al., 2014; Bertrand & Mullainathan, 2003; Baert & De Pauw, 2014) while others find that statistical discrimination dominates (Anwar & Fang, 2012; Fryer & Levitt, 2004). The subset of discrimination literature that addresses crime and law enforcement primarily covers pull-over rates, sentencing, and minority representation in police departments. In all three contexts, most find evidence of racial discrimination and inequity. [4] Only Fryer (2016) analyzes racial discrimination in police use of force. Fryer (2016) finds that, while officers use non-lethal force disproportionately more on minorities, officers use lethal force disproportionately less on minorities.[5]

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[3]For example, a store owner who dislikes black people, and thus refuses to serve them exhibits taste-based discrimination. But, if that store owner, based on past interactions, believes that black people probably will not want his product, and thus refuses to serve them, he exhibits statistical discrimination.

[4]Research focusing on pull-over rates generally finds that police disproportionately stop black drivers (Norris et al., 1992; Dharmapala & Ross, 2004; Antonovics & Knight, 2009; Heaton, 2009). 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 (Starr & Rehavi, 2012; Mustard, 2001; Alesina & Ferrara, 2011; Amwar et al., 2010). Lastly, though legal changes have increased minority representation in police departments (McCrary, 2007), there is evidence that additional minority officers could reduce discrimination (Donohue & Levitt, 2001; West, 2015).

[5]Fryers finding, that, conditional on pertinent factors, police officers are less likely to use lethal force on minorities compared to whites, surprised many, including Fryer himself (Bui & Cox, 2016).

Though Fryer (2016) analyzed racial bias in police use of force, there are other important race-based questions that surround police brutality. If crime rates respond to police brutality, do they respond differently by race of the victim or the officer? In a society fraught with racial tension like the United States, different responses could occur for a few reasons. Perhaps information about brutality incidents with minority victims and white officers spread to more people because the media pays more attention to them. Or, perhaps police departments change their law enforcement strategies more after an officer brutalizes a minority. In this thesis, I examine the evidence for differential effects of police brutality incidents by the race of the victim and the officer by separately estimating the effect of all brutality incidents, black-victim incidents, and black-victim-white-officer incidents.

## THEORETICAL JUSTIFICATION

In this paper, I attempt to causally identify the effect of police brutality incidents on crime rates in Chicago. I also hope to comment on whether the Ferguson Effect is a property of all brutality incidents in Chicago, whether people retaliate in response to police brutality in Chicago, or whether brutality is primarily a deterrent to crime in Chicago. Each of these potential effects has theoretical justification. As I mentioned in the introduction, the Ferguson Effect predicts that officers will reduce policing effort in response to scrutiny after a serious brutality incident. This decreases the risk of committing a crime, which increases the incentive to commit a crime. Retaliation, on the other hand, occurs because of frustration with police misconduct. A Retaliation Effect could manifest as protests, riots, or other disruptive behaviors. These retaliatory actions are associated with crimes like vandalism, public disturbance, public indecency, and interfering with a public officer. Furthermore, racial tension may stimulate racially charged retaliation to black-victim-white-officer incidents. Lastly, brutality incidents may deter crime. Perhaps police brutality scares potential criminals, leading them to believe the expected cost of being caught is higher than before the brutality incident; this would reduce their incentive to commit crimes. Also, this effect might be heterogeneous across types of crime, deterring crimes with a high likelihood of encountering police the most. In the remainder of this section, I will demonstrate that each of these potential effects are derivable within a utility maximization framework.

### The Ferguson Effect

To reiterate, the Ferguson Effect predicts that the expected cost of police misconduct increases after a police brutality incident due to public scrutiny. If we assume, as in Shi (2009), that officer's utility functions are increasing in their wage, decreasing in the expected cost of making a mistake, decreasing in the policing effort, and increasing in the number of arrests, then the following utility function sufficiently represents officer preferences:

$$U_{officer}(e) = w - A(e) * r * (p_m * M + p_c * p_i * p_g * P) + R[A(e)] - \frac{1}{2}e^2,$$

where $e$ is policing effort,$w$ is wage, $A(e)$ is the number of arrests as an increasing function of $e$, $r$ is the rate of officer misconduct, $p_m$ is the probability that a particular mistake gets media attention, $M$ is the personal cost of a mistake getting media attention, $p_c$ is the probability that there is a complaint filed against the officer, $p_i$ is the probability that a complaint leads to an investigation, $p_g$ is the probability of being found guilty of misconduct in an investigation, $P$ is the cost of the punishment itself, and $R[A(e)]$ is an additional reward for arrests which is an increasing function of arrests. Note that the final term, $e^2$, implies that there is an increasing cost to police effort for the officer. Also, unlike Shi (2009), I separate the potential costs of misconduct into the personal cost of a news outlet publicizing the mistake and the cost of the punishment the police department inflicts after an investigation.[1] The terms $p_i * p_g * P$ and $p_m * M$ are both elements of oversight that we expect to increase after a brutality incident. The officer objective then is to maximize this function:

$$\max_e w - A(e) * r * (p_m * M + p_c * p_i * p_g * P) + R[A(e)] - \frac{1}{2}e^2.$$

[1] I separate these costs primarily because, even if there is no formal investigation, there are still costs imposed on the officer if the media exposes his/her mistake or publicly inveighs him/her.

To solve, derive the first order condition and solve for e to get

$$e = -A'(e) * r * (p_m * M + p_c * p_i * p_g * P) + R'[A'(e)].$$

Assuming that the second-order condition for a maximum hold, this gives the optimal level of e. Now derive the second-order condition

$$-A''(e) * r * (p_m * M + p_c * p_i * p_g * P) + R''[A''(e)] - 1 < 0.$$

Now we must take derivatives of the first order condition with respect to the two oversight terms $p_i * p_g * P$ and $p_m * M$.[2] This gives

$$\frac{\partial e}{\partial (p_i * p_g * P)} = \frac{A'(e) * r * p_c}{-A''(e) * r * (p_m * M + p_c * p_i * p_g * P) + R''[A''(e)] - 1} < 0$$

and

$$\frac{\partial e}{\partial (p_m * M)} = \frac{A'(e) * r}{-A''(e) * r * (p_m * M + p_c * p_i * p_g * P) + R''[A''(e)] - 1} < 0.$$

The denominators of both of these terms are equivalent to the second-order condition, which means both denominators are negative. Further, the $A(e)$ is increasing in e, so the numerator is positive. Thus, both of these partial derivatives are negative, meaning that an increase in oversight due to a brutality incident should decrease policing effort. In turn, this affects criminal behavior by reducing the expected cost of committing a crime. Mathematically, assume that a potential criminal's utility function is increasing in crime rents and decreasing in the likelihood of being arrested.

[2] Note that e is a function of the other parameters, though the generality of this utility function does not allow me to solve for $e(r, p_m, M, p_c, p_i, p_g, P)$ explicitly.

The following function represents these assumptions:

$$U_{pcriminal}(c) = R(c) - c * p_A[A(e)] * S$$

Where c is the rate of committing crime, R(c) are the rents from a crime as a function of c, $p_A[(A(e)]$ is the probability of being arrested, and S is the punishment if caught committing a crime. The criminal's maximization problem is the following:

$$\max_c R(c) - c * p_A[A(e)] * S.$$

The first order condition is

$$R'(c) - p_A[A(e)] * S = 0$$

and the second-order condition is

$$R''(c) < 0.$$

If we take the derivative of the first order condition with respect to policing effort $e$, we get

$$\frac{\partial c}{\partial e} = \frac{p'_A[A(e)] * S * A'(e)}{R''(c)} < 0$$

The denominator is equivalent to the second-order condition, which means that it is negative. Because the arrest rate A is an increasing function in the policing effort, the entire partial derivative is negative. Thus, a marginal decrease in policing effort increases the optimal crime rate.

## The Retaliation Effect

The Retaliation Effect is a criminal response to police brutality caused by frustration. I model this by augmenting the rents from crime to include rents from emotional release or being part of a cause. Thus, the following utility function can represent potential criminal preferences:

$$U_{pcriminal}(c) = R(c) + g * c - c * p_A[A(e)] * S$$

where the g represents the rate of return from retaliatory crime. Using comparative statics, we can show that

$$\frac{\partial c}{\partial g} = \frac{g}{R''(c)} > 0.$$

Thus, if a police brutality crime increases the return from retaliatory crime by making retaliation cathartic, then crime rates should increase.

## The Deterrence Effect

Lastly, the Deterrence Effect is the hypothesis that criminals perceive the expected cost of punishment to be higher after a police brutality incident. Thus, the parameter of interest is S. If we take the potential criminal maximization problem again we have

$$\max_c R(c) - c * p_A[A(e)] * S$$

with first order condition

$$R'(c) - p_A[A(e)] * S = 0.$$

If we assume the second-order conditions hold and solve for the marginal change of $c$ attributable to a change in $S$ we get

$$\frac{\partial c}{\partial S} = \frac{p_A[A(e)]}{R''(c)} < 0$$

If the second-order condition holds the denominator is negative. Because the numerator is just a nonzero probability, we know $p_A \in (0, 1]$. Thus, if this theory holds, then a police brutality incident should decrease crime rates.

# DATA

In this thesis, 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 and district level demographic data from the U.S. Census Bureau.

## 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.[1] For each complaint, the CPDP data include the type of misconduct, an incident identification number, the incident date, the investigation start date,[2] the name of the chief investigator who was assigned the complaint, the name of the officer who was accused of misconduct, the age and race of the complainant,[3] and, occasionally, the investigation and police board hearing reports[4]

[1]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.

[2]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).

[3]It is important to note that in the racial analysis, 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 incident I exploit. Furthermore, in the subsample of complaints where information about the race of 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 with this correlation in this thesis, I can produce such information upon request

[4]To access the the investigation report and the police board hearing report they must be requested. 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 the 55 excessive force complaints since 2011 for which the investigations have been sustained, but the data is not yet available.

The CPDP data include over 9,600 excessive force complaints. Based on results from complaint investigations, the CPDP divides complaints into five categories: unfounded, exonerated, not sustained, sustained, and disciplined.[5] 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. I call an excessive force incident "serious" if its investigation was sustained or if the officer was disciplined.[6] Thus defined, there are 123 serious incidents, 55 of which occur from 2011 through 2015. I drop an additional six incidents for idiosyncratic reasons[7]; this thesis focuses on analyzing the effects of these 49 incidents.

These data allow me to track down when and where these serious incidents occurred. Thus, I exploit their timing and location to causally identify their effects on local crime rates. Figures 4.1–4.4 show the distribution of excessive force cases over space by race of victim and officer.

With the CPDP data, there are a few potential sources of bias. As with any dataset, these data contain minor entry errors,[8] which, assuming they are random, will attenuate my coefficient estimates. A more serious problem would be if the

[5] 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.

[6] I call the sustained and disciplined incidents serious because the IPRA found them worthy of a close investigation. If the police protect their own (cf. Chin & Wells, 1997; Skolnick, 2002), this categorization is likely to underestimate the number of severe incidents and attenuate my estimates.

[7] 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.

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

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 would be difficult to rule this out explicitly, but underreporting should bias my estimates toward zero, making them conservative.

## City of Chicago Crime Data

The City of Chicago crime data include reported crimes that occurred in Chicago from 2001 to 2016,[9] 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 race of the perpetrator or victim.[10]

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.[11]

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[9] I drop all reported crime data from 2001 to 2011 because the excessive force data had a gap between 2008 and 2011.

[10] That I cannot access the race of the perpetrator or victim is limiting because I cannot analyze the effect of brutality incidents on minority crime rates and compare to effects on white crime rates. It is reasonable to think that these cases would affect racial minorities differently, which means a more thorough racial analysis would be a good extension of this thesis.

[11] 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. Therefore, the Chicago Police Department does not guarantee (either expressed or implied) the accuracy,*

## Census Data

I use census data to examine community characteristics. These include median income, proportion of residents who graduated high school, poverty rate, proportion of residents who graduated college, number of residents who are black, number of residents who are hispanic, and 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. See Panel B of Table 4.1 for a summary of these variables.

*completeness, timeliness, or correct sequencing of the information and the information should not be used for comparison purposes over time.* (City of Chicago Website)

Table 4.1: Community Summary Statistics

| | Mean | Min | Max |
|---|---|---|---|
| *Panel A: Daily Crimes* | | | |
| Violent Crime | 3.89 | 0 | 48 |
| Total Crime | 13.89 | 0 | 517 |
| Property Crime | 4.69 | 0 | 47 |
| Assault | 0.84 | 0 | 15 |
| Homocide | 0.02 | 0 | 6 |
| Theft | 2.89 | 0 | 45 |

| | Mean | Min | Max |
|---|---|---|---|
| *Panel B: Demographic Characteristics* | | | |
| Total Population | 36,045 | 2,550 | 117,527 |
| Black Population | 11,994 | 19 | 105,369 |
| Hispanic Population | 10,280 | 12 | 75,613 |
| White Population | 11,335 | 6 | 76,134 |
| Median Income | 51,274 | 10,896 | 99,333 |
| Poverty Rate | 0.23 | 0.01 | 0.6 |
| Fraction High School Grad | 0.78 | 0.37 | 1 |
| Fraction College Grad | 0.27 | 0.03 | 0.88 |

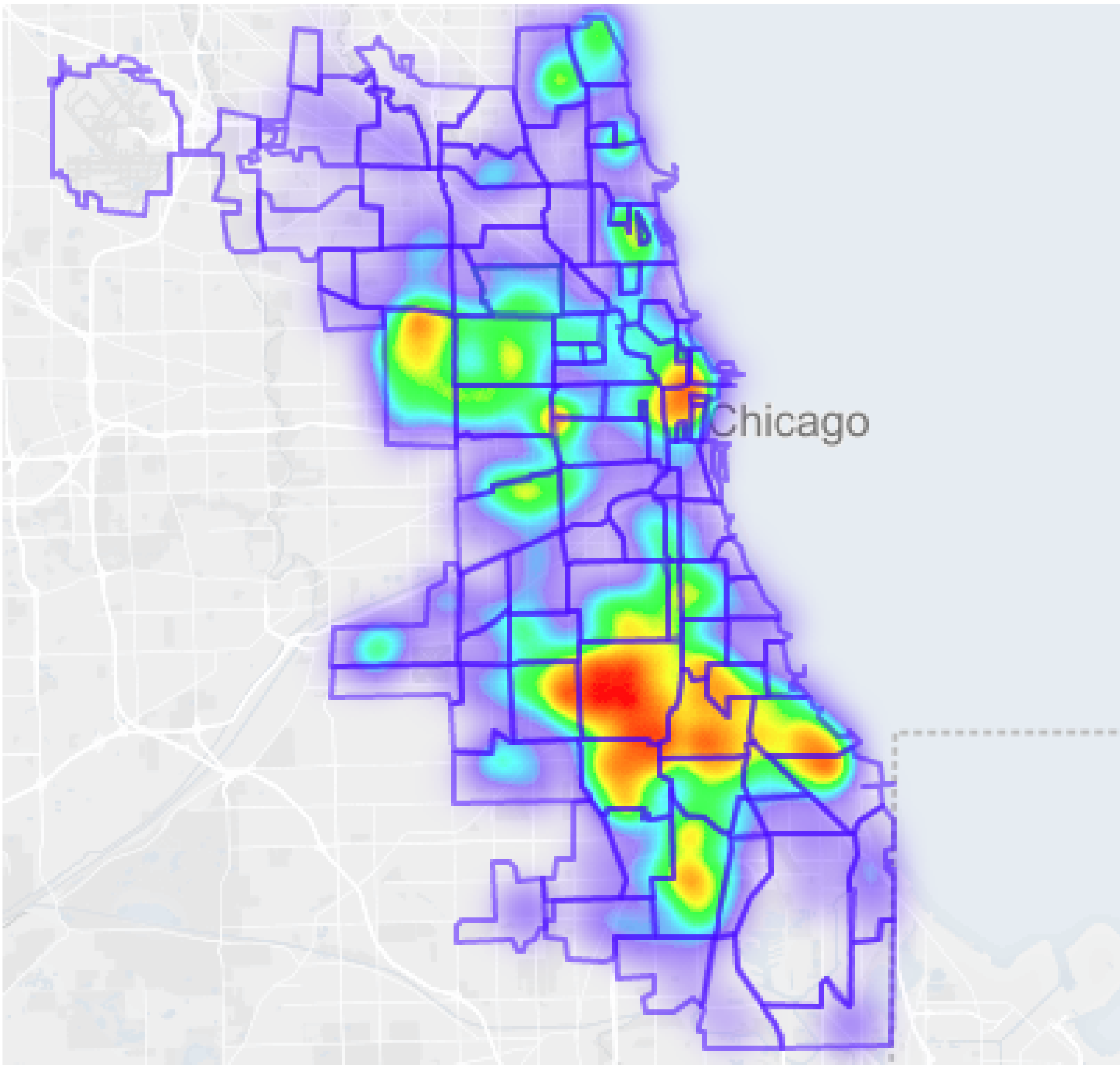


Figure 4.1: Map of All Excessive Force Incidents From 2002 to 2015

Note: This map depicts the locational variation of the 9608 excessive force incidents that occurred in Chicago from 2002 to 2008 and 2011 to 2015 in the CPDP data. Red areas had the most incidents, yellow and green areas have had an intermediate number of incidents, and blue areas have had the least number of incidents.

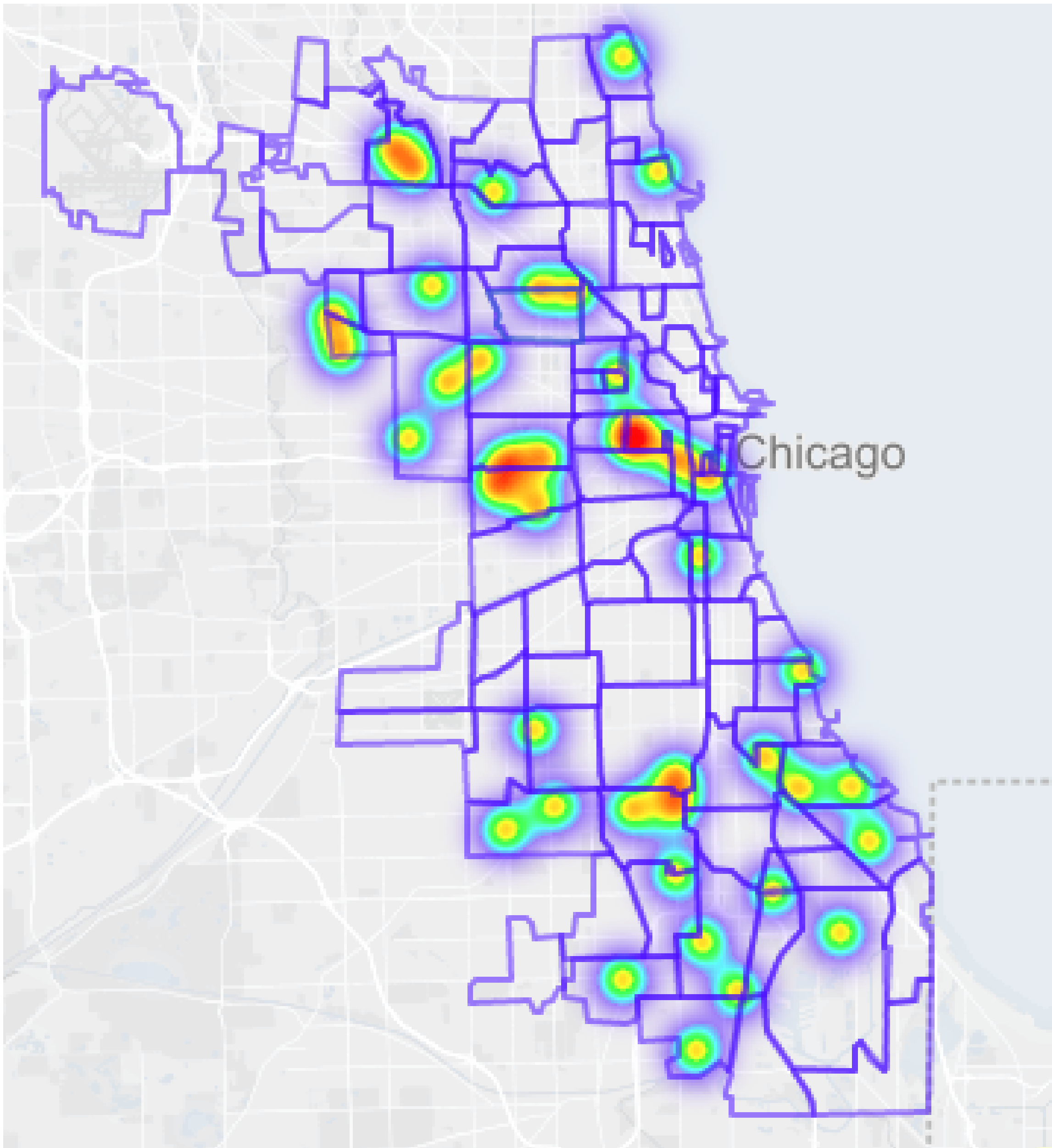


Figure 4.2: Map of All Serious Incidents From 2011 to 2015

Note: This map depicts the locational variation of the 55 serious excessive force incidents that occurred in Chicago from 2011 to 2015 in the CPDP data. Red areas had the most incidents, yellow and green areas have had an intermediate number of incidents, and blue areas have had the least number of incidents.

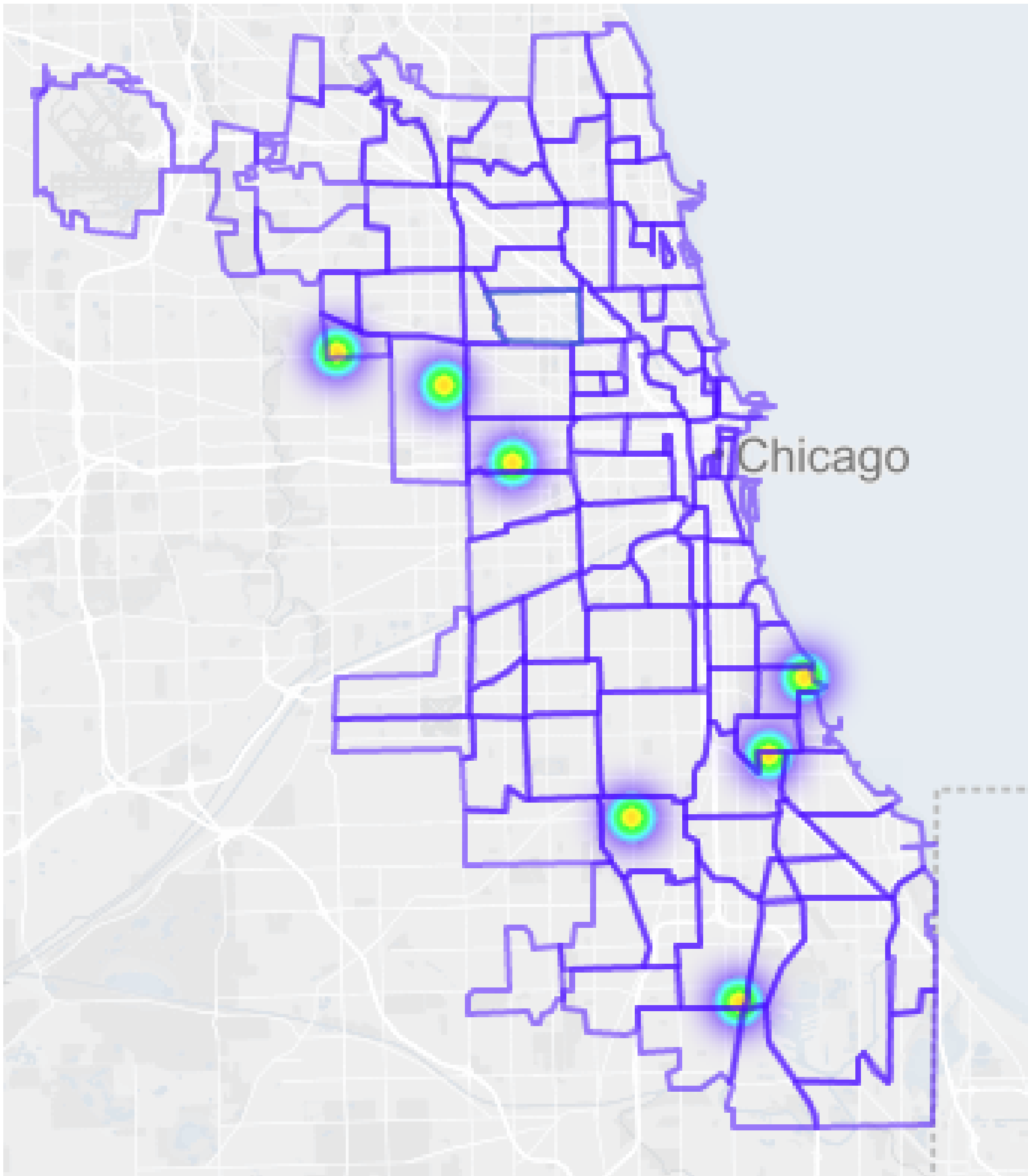


Figure 4.3: Map of All Serious BV Incidents From 2011 to 2015

Note: This map depicts the locational variation of the seven serious excessive force incidents where the complainant was black that occurred in Chicago from 2011 to 2015 in the CPDP data. Red areas had the most incidents, yellow and green areas have had an intermediate number of incidents, and blue areas have had the least number of incidents.

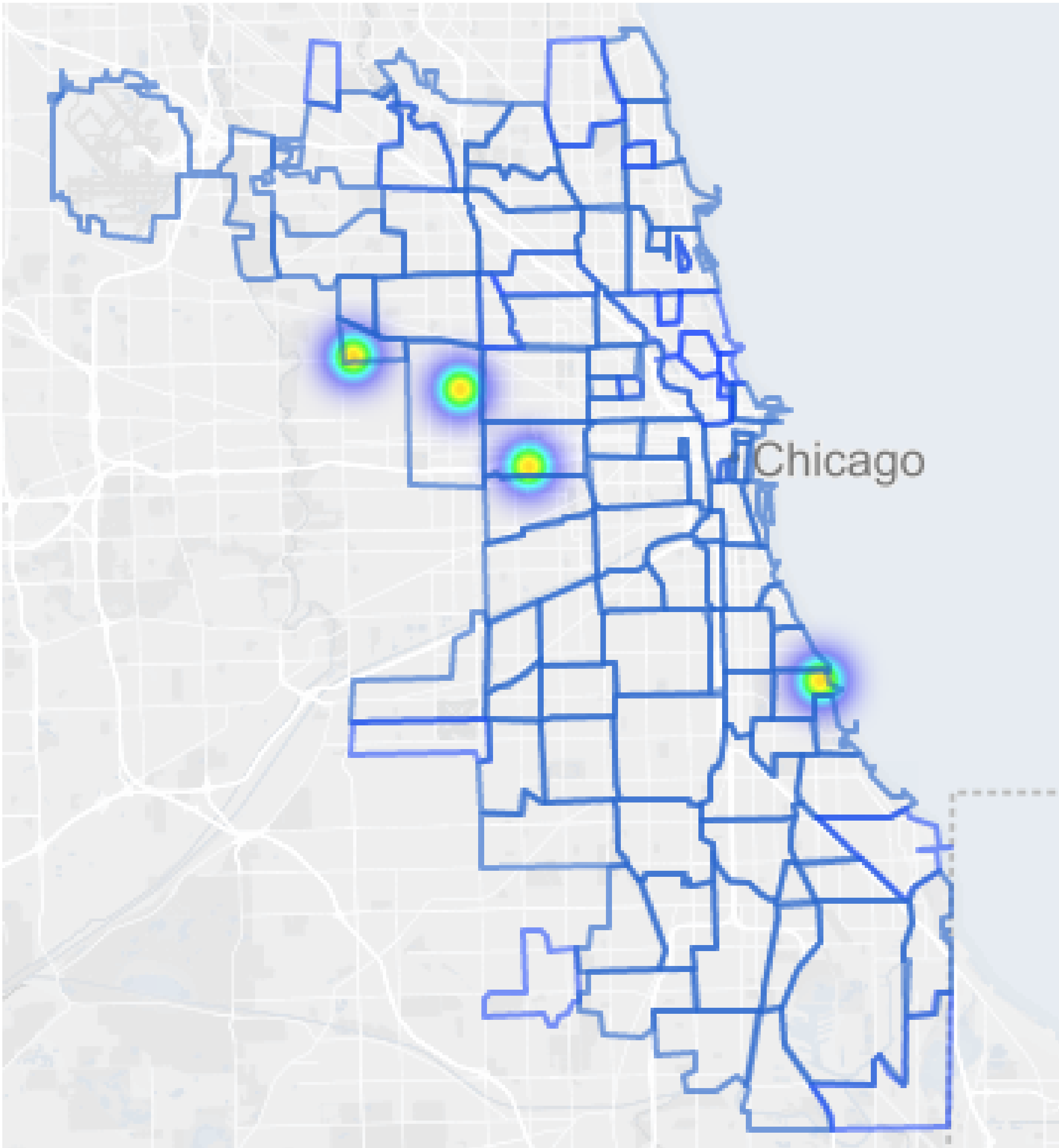


Figure 4.4: Map of All Serious BVWO Incidents From 2011 to 2015

Note: This map depicts the locational variation of the four serious excessive force incidents where the complainant was black and the officer was white that occurred in Chicago from 2011 to 2015 in the CPDP data. Red areas had the most incidents, yellow and green areas have had an intermediate number of incidents, and blue areas have had the least number of incidents.

# EMPIRICAL STRATEGY

In this paper, I use a series of fixed effects models to estimate the effects of police brutality incidents on crime. I estimate this relationship at the city-level, the community level, and at the district level. I hope to capture both the effect on crime attributable to being within a specific number of days after a brutality incident and the effect on crime of being a particular distance away from where the incident occurred. At the city-level, my strategy amounts to a repeated simple difference model. At the community level and district level, it amounts to a difference-in-difference (DD) model.

For causal identification, these models assume that the timing of serious brutality incidents are conditionally random. In other words, it is important that, conditional on controls and fixed effects, police brutality incidents are not predictable in such a way that will people will systematically change their behavior leading up to an incident. To rule out that people can predict these incidents, I estimate the causal effect of being before a brutality incident on crime as well. Additionally, one may worry that estimated changes in crime following a brutality incident may be predicted by preexisting trends. To deal with this, I control for community-specific and district-specific trends.

## Three Models

### City-Wide Model

The city-wide model estimates the average effect of brutality incidents on Chicago crime by taking the mean difference between the average per-day, per-community number of crimes before and after a brutality incident (a.k.a. a repeated simple difference). The specific model I estimate is

$$C_{idwmy} = B_0 + \delta Afterbrut_{dwmy} + X_{iy}B + \mu_i + z_w + u_y + \epsilon_{idwmy}. \tag{5.1}$$

In this equation, $C_{idwmy}$ represents the number of crimes (i.e. violent crimes, property crimes, etc.) that occurred in community $i$ on day $d$ in week $w$ in month $m$ in year $y$. The variable of interest $Afterbrut$ is a dummy that is equal to one if the date is within thirty days after a serious brutality incident occurred. The variable $X_{iy}$ represents a vector of year-varying community characteristics including the median income, percent of residents who graduated highschool, 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, $\mu_i$ represents a community fixed effect, $z_w$ represents a week fixed effect, and $u_y$ represents a year fixed effect. For the sake of transparency, I estimate this model in stages, first without fixed effects, then with community level fixed effects, and finally with month and year fixed effects.

Becaue my outcome variable is a count of daily crimes, I estimate equation 5.1 using a Poisson model. Thus, $\delta$ represents the percent change in the daily number of crimes per community within a month after a serious brutality incident occurred.[1]

Proxiity Model

The proximity model estimates the average effect of brutality incidents on crime within the region (community or district) they occur by comparing affected regions to others where no incident occurred. The model does this by comparing the difference

[1] Additionally, I estimate each of these models using OLS and include the results in appendix tables.

in the number of crimes committed before and after an incident within an affected region to the difference in the number of crimes committed before and after an incident within unaffected region (a.k.a. a difference-in-difference). The specific model I estimate is

$$C_{idwmy} = B_0 + \delta Afterbrut_{idwmy} + X_{iy}B + \mu_i + z_w + u_y + \epsilon_{idwmy}. \tag{5.2}$$

The only difference between equation 5.1 and equation 5.2 is the $i$ subscript on the variable of interest. Unlike the city-wide model, the proximity model assumes that an incident only affects the region in which it occurred, so whether $Afterbrut$ is one depends on the region. Because the treatment varies at the region level in this model, I also include month-by-year fixed effects, region-specific linear trends, and region-specific quadratic trends to control for sharp within-year shocks and predictable changes by region.[2]

City-Wide Race Model

The race model attempts to determine whether the effect of police brutality on crime varies with officer or victim race. To get at this, I re-estimate the city-wide model and separate the incidents into the various race combinations and include variables of interest for all of them. The specific model I estimate is

$$\begin{aligned} C_{idwmy} = B_0 &+ \delta_1 Blackvic_{dwmy} + \delta_2 Blackvic_{dwmy} * Whiteoff_{dwmy} \\ &+ \delta_3 Otherbrut_{dwmy} + X_{iy}B + \mu_i + z_w + u_y + \epsilon_{idwmy}. \end{aligned} \tag{5.3}$$

[2]For the remainder my models I include the same progression of fixed effects.

This model[3] estimates the effects on crime of being within some specified number of days from brutality events allowing effects to differ by the race of officer and victim. Thus, $Blackvic_{dwmy}$ is a dummy variable that is one if it is within thirty days of a brutality incident where the victim was black. Similarly, $Whiteoff_{dwmy}$ is a dummy variable that is one if it is within thirty days of a brutality incident where the officer was white. When I interact $Blackvic$ and $Whiteoff$ I get a variable that is one only when it is within thirty days of a brutality incident where the victim was black and the officer was white (a.k.a. potentially racially charged incidents). The coefficients $\delta_1$ and $\delta_2$ can be interpreted as the causal effect of black victim incidents and the additional effect of adding a white officer to those incidents. I also control for the effect of all other brutality incidents so that I can isolate the race effects.[4]

<u>Proximity Race Model</u>

This combines the proximity model and the race model by allowing each brutality incident only to affect the region where it occured. The model is the following:

$$\begin{aligned}C_{idwmy} = B_0 + \delta_1 Blackvic_{idwmy} + \delta_2 Blackvic_{idwmy} * Whiteoff_{idwmy} \\ + \delta_3 Otherbrut_{idwmy} + X_{iy}B + \mu_i + z_w + u_y + \epsilon_{idwmy}. \quad (5.4)\end{aligned}$$

The only difference between equation 5.3 and equation 5.4 is the $i$ subscript on the variables of interest.

[3]I don't actually include estimates of this model in this thesis, but I explain the model because it logically precedes the final model, the proximity race model.

[4]Unfortunately, my measure of victim race is not perfect. The Citizens Police Data Project 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.

# RESULTS

## City-Wide Model

The first column of Table 6.1 presents Poisson estimates of the relationship between being within a month after a brutality incident and the number of daily crimes. Without controlling for any time-specific or community-specific heterogeneity, being within a month after a brutality incident is associated with a 14 percent increase in total crimes per day, an eight percent increase in violent crimes per day, a 19 percent increase in property crimes per day, and a 15 percent increase in the number of arrests per day (all significant at the one percent level). In columns two through six of Table 6.1 I add various fixed effects. The specification of choice includes fixed effects for community, week, and year because crime rates vary widely both between communities and across time. The estimates from this specification are presented in column three. They suggest that within a month after a brutality incident, the number of daily violent crimes decreases by one percent (significant at the one percent level). This suggests that, though a naive model is consistent with speculation that brutality incidents cause crime to increase, these incidents may decrease violent crime on net. This also suggests that the effect of police brutality is heterogeneous across crimes because not all crimes were deterred.

## Within Community Model

The first column of Table 6.2 presents the Poisson estimates of the relationship between being within a community where a brutality incident occurred less than a month ago on the number of daily crimes relative to other communities. Without any fixed effects, being within an affected community within a month after an incident is associated with a five percent decrease in the number of arrests (significant at the one percent level). When I include community fixed effects, week fixed effects month-by-year fixed effects, and linear trends, brutality incidents are associated with a three percent increase in violent crime (significant at the five percent level). There is weaker evidence that these incidents cause a 1.5 percent increase in total crime and a three percent increase in property crime (both significant at the ten percent level). Because statistically significant effects on violent crime only appear with the inclusion of trends, I specify both linear and quadratic trends to verify that this result is not purely driven by a misspecification of trends. The positive effect of brutality on violent crime is robust to both trend specifications.

### Within District Model

Table 6.5 presents the Poisson estimates of the relationship between being within a district where a brutality incident occurred less than a month ago on the number of daily crimes relative to other districts. Without any fixed effects, being within an affected district within a month after an incident is associated with a five percent increase in the number of total crime and violent crime as well as a seven percent increase in property crime (all significant at the five percent level). When I include district fixed effects, week fixed effects month-by-year fixed effects, and linear trends, brutality incidents are associated with a two percent increase in violent crime (significant at the five percent level). This effect is robust to including linear or quadratic trends in the model.

## Race-Specific Effects

### Within Community Effects

Table 6.3 presents the Poisson estimates of the relationship between being within a community where a black-victim brutality incident occurred less than a month ago on the number of daily crimes relative to other communities.[1] Without any fixed effects, there is no significant relationship between these incidents and crime. But, with community fixed effects, week fixed effects, month-by-year fixed effects, and community-specific linear trends, being within a month after a black-victim brutality incident in an affected community is associated with a four percent increase in the number of violent crimes (significant at the five percent level). This suggests that there is evidence for a local Ferguson or retaliation effect when the victim of police brutality is black. This effect is robust to including linear or quadratic trends in the model.

Table 6.4 presents the additional effect of black victim incidents on crime if the officer is white.[2] When I include community fixed effects, week fixed effects, month-by-year fixed effects, and community-specific linear trends, being within a month after a black-victim-white-officer incident (a.k.a. a potentially racially charged incident) in an affected community is associated with an additional nine percent increase in total crimes (significant at the one percent level), and an additional 18 percent increase in property crimes (significant at the 1 percent level). There is also some evidence of an additional 5-7 percent increase in violent crime, but these results are not robust to both types of trends.

[1] Though it is not mentioned on the table, I also control for days that are within a month after other brutality incidents as well, so as not to confound my estimates with other treatment periods.

[2] It is important to note that there are only four brutality incidents where the victim is white, and the officer is black where the officer was punished in these data. Future work would integrate more potentially racially charged brutality incidents and see whether these results are substantiated.

Within-District Effects

Table 6.6 presents the Poisson estimates of the relationship between being within a district where a black-victim brutality incident occurred less than a month ago on the number of daily crimes relative to other districts.[3] Without any fixed effects, there is a large positive relationship between being within a month after a brutality incident and total crime, violent crime, crimes against officers, and arrests (between 25 and 100 percent increases). With district fixed effects, week fixed effects, month-by-year fixed effects, and district-specific linear trends, being within a month after a black-victim brutality incident in an affected district is associated with a six percent increase in the number of violent crimes (significant at the five percent level) and a five percent increase in the number of property crimes. There is also evidence that these incidents increase total crime by five percent, though the result goes away with quadratic trends. Lastly, there is weak evidence that the number of arrests goes up by eight percent just after a brutality incident (significant at the 10 percent level). This suggests that there is evidence for a local Ferguson or retaliation effect when the victim of police brutality is black.

Table 6.7 presents the additional effect of black victim incidents on crime if the officer is white.[4] When I include district fixed effects, week fixed effects, month-by-year fixed effects, and district-specific linear trends, being within a month after a black-victim-white-officer incident (a.k.a. a potentially racially charged incident) in an affected district is associated with an additional eight percent increase in property crimes (significant at the five percent level).

[3]Though it is not mentioned on the table, I also control for days that are within a month after other brutality incidents as well, so as not to confound my estimates with other treatment periods.

[4]It is important to note that there are only four brutality incidents where the victim is white, and the officer is black where the officer was punished in these data. Future work would integrate more potentially racially charged brutality incidents and see whether these results are substantiated.

Table 6.1: Effect of Brutality on City-Wide Crime

| | (1) | (2) | (3) |
|---|---|---|---|
| Total Crime | 0.138*** | 0.009* | -0.000 |
| | (0.021) | (0.005) | (0.003) |
| Violent Crime | 0.078*** | -0.018*** | -0.011** |
| | (0.019) | (0.005) | (0.005) |
| Property Crime | 0.189*** | 0.027*** | 0.006 |
| | (0.019) | (0.005) | (0.005) |
| Crime Against Officer | -0.072 | -0.050 | 0.022 |
| | (0.049) | (0.037) | (0.038) |
| Gambling | 0.175*** | -0.105** | -0.038 |
| | (0.046) | (0.048) | (0.064) |
| Arrests | 0.151*** | 0.003 | -0.007 |
| | (0.032) | (0.010) | (0.006) |
| Community FE | no | yes | yes |
| Week FE | no | no | yes |
| Year FE | no | no | yes |
| N | 143305 | 143305 | 143305 |

Notes: Based on the City of Chicago reported crime data and CPDP complaint data. Each column represents results from a separate Poisson regression. Standard Errors, corrected for clustering at the district level, are in parentheses. *Statistically significant at the 10% level;**at the 5% level; *** at the 1% level.

Table 6.2: Effect of Brutality on Community Crime

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Total Crime | -0.009 | -0.003 | -0.000 | 0.015* | 0.020** |
| | (0.020) | (0.010) | (0.010) | (0.009) | (0.009) |
| Violent Crime | 0.006 | 0.011 | 0.015 | 0.031** | 0.031** |
| | (0.028) | (0.015) | (0.015) | (0.015) | (0.016) |
| Property Crime | 0.015 | 0.020 | 0.018 | 0.030* | 0.032 |
| | (0.025) | (0.017) | (0.017) | (0.016) | (0.017) |
| Crime Against Officer | -0.174 | -0.116 | -0.058 | -0.045 | -0.020 |
| | (0.115) | (0.113) | (0.108) | (0.110) | (0.114) |
| Gambling | -0.199 | -0.171* | -0.056 | -0.029 | -0.036 |
| | (0.195) | (0.103) | (0.095) | (0.078) | (0.078) |
| Arrests | -0.047* | -0.032 | -0.017 | 0.007 | 0.013 |
| | (0.028) | (0.021) | (0.022) | (0.016) | (0.014) |
| Community FE | yes | yes | yes | – | yes |
| Week FE | no | yes | yes | yes | yes |
| Year FE | no | yes | – | – | – |
| Month by Year FE | no | no | yes | yes | yes |
| Community Specific Trends | no | no | no | yes | yes |
| Community Specific Quadratic Trends | no | no | no | no | yes |
| N | 143305 | 143305 | 143305 | 143305 | 141463 |

Notes: Based on the City of Chicago reported crime data and CPDP complaint data. Each column represents results from a separate Poisson regression. Standard Errors, corrected for clustering at the district level, are in parentheses. *Statistically significant at the 10% level;**at the 5% level; *** at the 1% level.

Table 6.3: Effect of BV Brutality on Community Crime

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Total Crime | -0.129 | -0.033 | -0.011 | -0.005 | 0.011 | 0.024 |
| | (0.126) | (0.046) | (0.025) | (0.022) | (0.022) | (0.027) |
| Violent Crime | -0.154 | 0.001 | 0.004 | 0.016 | 0.035** | 0.041** |
| | (0.106) | (0.045) | (0.012) | (0.015) | (0.013) | (0.015) |
| Property Crime | -0.122 | -0.027 | 0.013 | 0.010 | 0.031 | 0.045 |
| | (0.150) | (0.053) | (0.039) | (0.034) | (0.038) | (0.042) |
| Crime Against Officer | -0.198 | -0.017 | 0.006 | 0.102 | 0.087 | 0.040 |
| | (0.139) | (0.028) | (0.035) | (0.064) | (0.066) | (0.061) |
| Gambling | -0.246 | -0.168*** | -0.036 | 0.114 | 0.094 | -0.002 |
| | (0.156) | (0.035) | (0.061) | (0.080) | (0.089) | (0.093) |
| Arrests | -0.021 | -0.043 | -0.026 | 0.014 | 0.026 | 0.024 |
| | (0.107) | (0.050) | (0.032) | (0.037) | (0.033) | (0.033) |
| Community FE | no | yes | yes | yes | yes | yes |
| Week FE | no | no | yes | yes | yes | yes |
| Year FE | no | no | yes | – | – | – |
| Month by Year FE | no | no | no | yes | yes | yes |
| Community Specific Trends | no | no | no | no | yes | yes |
| Community Specific Quadratic Trends | no | no | no | no | no | yes |
| N | 135096 | 134965 | 133028 | 134965 | 134264 | 133427 |

Notes: Based on the City of Chicago reported crime data and CPDP complaint data. Each column represents results from a separate Poisson regression. Standard Errors, corrected for clustering at the district level, are in parentheses. *Statistically significant at the 10% level;**at the 5% level; *** at the 1% level.

Table 6.4: Effect of BVWO Brutality on Community Crime

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Total Crime | -0.282 | 0.133* | 0.039 | 0.041 | 0.088*** | 0.100*** |
| | (0.234) | (0.069) | (0.051) | (0.033) | (0.030) | (0.032) |
| Violent Crime | -0.513** | 0.143*** | -0.017 | -0.016 | 0.046 | 0.072** |
| | (0.201) | (0.044) | (0.023) | (0.029) | (0.032) | (0.033) |
| Property Crime | -0.229 | 0.223*** | 0.152*** | 0.132*** | 0.182*** | 0.187*** |
| | (0.218) | (0.032) | (0.021) | (0.026) | (0.030) | (0.032) |
| Crime Against Officer | -0.427* | 0.130 | 0.012 | 0.294 | 0.312 | 0.256 |
| | (0.238) | (0.197) | (0.235) | (0.246) | (0.275) | (0.287) |
| Gambling | 0.494 | 0.399 | -0.954** | -0.418 | -0.624 | -0.738 |
| | (0.907) | (0.721) | (0.462) | (0.443) | (0.517) | (0.489) |
| Arrests | -0.124 | 0.061 | 0.002 | 0.058 | 0.092 | 0.086 |
| | (0.318) | (0.126) | (0.091) | (0.071) | (0.063) | (0.057) |
| Community FE | no | yes | yes | yes | yes | yes |
| Week FE | no | no | yes | yes | yes | yes |
| Year FE | no | no | yes | – | – | – |
| Month by Year FE | no | no | no | yes | yes | yes |
| Community Specific Trends | no | no | no | no | yes | yes |
| Community Specific Quadratic Trends | no | no | no | no | no | yes |
| N | 143305 | 143305 | 143305 | 143305 | 143305 | 143305 |

Notes: Based on the City of Chicago reported crime data and CPDP complaint data. Each column represents results from a separate Poisson regression. Standard Errors, corrected for clustering at the district level, are in parentheses. *Statistically significant at the 10% level;**at the 5% level; *** at the 1% level.

Table 6.5: Effect of Brutality on District Crime

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Total Crime | 0.046** | -0.003 | -0.001 | 0.004 | 0.004 |
| | (0.020) | (0.009) | (0.008) | (0.008) | (0.008) |
| Violent Crime | 0.050** | 0.011 | 0.015 | 0.020** | 0.021** |
| | (0.024) | (0.009) | (0.009) | (0.010) | (0.009) |
| Property Crime | 0.068** | 0.004 | 0.005 | 0.001 | 0.001 |
| | (0.028) | (0.013) | (0.013) | (0.013) | (0.014) |
| Crime Against Officer | -0.027 | -0.013 | 0.027 | 0.033 | 0.034 |
| | (0.128) | (0.112) | (0.088) | (0.084) | (0.083) |
| Gambling | 0.118 | -0.174 | -0.066 | -0.052 | -0.066 |
| | (0.188) | (0.141) | (0.134) | (0.123) | (0.115) |
| Arrests | 0.028 | -0.028 | -0.018 | -0.003 | -0.005 |
| | (0.035) | (0.027) | (0.027) | (0.022) | (0.020) |
| District FE | yes | yes | yes | – | yes |
| Week FE | no | yes | yes | yes | yes |
| Year FE | no | yes | – | – | – |
| Month by Year FE | no | no | yes | yes | yes |
| District Specific Trends | no | no | no | yes | yes |
| District Specific Quadratic Trends | no | no | no | no | yes |
| N | 42449 | 42449 | 42449 | 42449 | 42439 |

Notes: Based on the City of Chicago reported crime data and CPDP complaint data. Each column represents results from a separate Poisson regression. Standard Errors, corrected for clustering at the district level, are in parentheses. *Statistically significant at the 10% level;**at the 5% level; *** at the 1% level.

Table 6.6: Effect of BV Brutality on District Crime

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Total Crime | 0.253*** | -0.030 | 0.033 | 0.045*** | 0.051*** | 0.036 |
| | (0.091) | (0.075) | (0.022) | (0.013) | (0.017) | (0.024) |
| Violent Crime | 0.401*** | -0.016 | 0.040* | 0.055*** | 0.061** | 0.065*** |
| | (0.101) | (0.058) | (0.024) | (0.021) | (0.027) | (0.022) |
| Property Crime | 0.033 | -0.050 | 0.044 | 0.050** | 0.051** | 0.059*** |
| | (0.096) | (0.084) | (0.029) | (0.022) | (0.021) | (0.020) |
| Crime Against Officer | 0.335** | 0.014 | 0.066 | 0.139 | 0.096 | 0.028 |
| | (0.163) | (0.139) | (0.153) | (0.166) | (0.178) | (0.188) |
| Gambling | 1.000*** | 0.073 | -0.048 | 0.088 | 0.101 | 0.073 |
| | (0.246) | (0.232) | (0.143) | (0.181) | (0.164) | (0.157) |
| Arrests | 0.419*** | -0.004 | 0.046 | 0.074** | 0.086** | 0.045 |
| | (0.103) | (0.094) | (0.033) | (0.030) | (0.041) | (0.050) |
| District FE | no | yes | yes | yes | yes | yes |
| Week FE | no | no | yes | yes | yes | yes |
| Year FE | no | no | yes | – | – | – |
| Month by Year FE | no | no | no | yes | yes | yes |
| District Specific Trends | no | no | no | no | yes | yes |
| District Specific Quadratic Trends | no | no | no | no | no | yes |
| N | 42449 | 42449 | 42449 | 42449 | 42449 | 42449 |

Notes: Based on the City of Chicago reported crime data and CPDP complaint data. Each column represents results from a separate Poisson regression. Standard Errors, corrected for clustering at the district level, are in parentheses. *Statistically significant at the 10% level;**at the 5% level; *** at the 1% level.

Table 6.7: Effect of BVWO Brutality on District Crime

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Total Crime | 0.134* | 0.122 | -0.014 | -0.008 | 0.011 | 0.003 |
| | (0.077) | (0.120) | (0.058) | (0.036) | (0.034) | (0.045) |
| Violent Crime | -0.005 | 0.079 | -0.104*** | -0.097*** | -0.065 | -0.036 |
| | (0.081) | (0.095) | (0.032) | (0.021) | (0.049) | (0.043) |
| Property Crime | 0.108 | 0.185 | 0.053 | 0.044 | 0.058 | 0.076** |
| | (0.080) | (0.114) | (0.067) | (0.047) | (0.046) | (0.038) |
| Crime Against Officer | -0.042 | 0.233 | -0.017 | 0.217 | 0.203 | 0.108 |
| | (0.205) | (0.238) | (0.317) | (0.311) | (0.343) | (0.375) |
| Gambling | 1.743*** | 0.951* | -0.869*** | -0.429 | -0.481 | -0.420 |
| | (0.509) | (0.566) | (0.329) | (0.328) | (0.312) | (0.362) |
| Arrests | 0.286*** | 0.102 | -0.024 | 0.022 | 0.033 | 0.003 |
| | (0.070) | (0.160) | (0.065) | (0.055) | (0.061) | (0.074) |
| District FE | no | yes | yes | yes | yes | yes |
| Week FE | no | no | yes | yes | yes | yes |
| Year FE | no | no | yes | – | – | – |
| Month by Year FE | no | no | no | yes | yes | yes |
| District Specific Trends | no | no | no | no | yes | yes |
| District Specific Quadratic Trends | no | no | no | no | no | yes |
| N | 42449 | 42449 | 42449 | 42449 | 42449 | 42449 |

Notes: Based on the City of Chicago reported crime data and CPDP complaint data. Each column represents results from a separate Poisson regression. Standard Errors, corrected for clustering at the district level, are in parentheses. *Statistically significant at the 10% level;**at the 5% level; *** at the 1% level.

# ROBUSTNESS AND PERSISTENCE

## Falsification

In each of my models, I included gambling as an outcome variable to provide evidence that my estimates are not spurious. Such a falsification test assumes that police brutality incidents will not causally affect the rate of illegal gambling, which is reasonable if the mechanisms through which police brutality affects crime are limited to the Ferguson effect, retaliation effect, and deterrence effect.[1] In each preferred regression, the change in the number of gambling incidents is not statistically different from zero (for Poisson and OLS), which suggests that my main regression results are not spurious.

## Can People Predict Brutality Incidents?

If officers and potential criminals can predict brutality incidents and, thus, systematically alter their behavior before an incident, this will confound my estimates of the effect of police brutality incidents on crime. To assess the evidence that these incidents are predictable, I present estimates of the relationship between being within a community in which a black-victim-white-officer brutality incident is going to occur in one month, two months, or three months (see column six in Tables 7.1 – 7.3). I do this by adding lead variables to the proximity race model. I only analyze leads for total crime (Table 7.1) and property crime (Table 7.3), the only two types of crime that were significantly affected by black-victim-white-officer incidents in the main

[1] One concern is that police brutality incidents influence where officers patrol (Shi, 2009). For instance, after a brutality incident, police departments may order officers to patrol dangerous areas less to avoid another incident. If these dangerous areas are systematically near, or far away from areas that are prone to gambling violations, we may expect an effect on gambling.

specifications (lead estimates for violent crime are presented in column six of Table 7.2).

The first three estimates in column six of Table 7.1 show that being three, two, or one month before a racially charged brutality incident in a community where an incident occurred is not significantly related to total crime. But, being two months before an incident is associated with a 3.1 percent decrease in daily crime relative to other communities. Thus, the leads provide evidence that the race model is estimating the true effect of police brutality on total crime.

Similarly, the first three estimates in column six of Table 7.2 show that being three, two, or one month before a racially charged brutality incident in a community where an incident occurred is not significantly related to property crime. Similar to total crime, because none of the lead terms are significant, they do not provide evidence that people could predict brutality incidents and respond accordingly.

## Persistence

To estimate the persistence of the effect of racially charged incidents, I also include lag terms that capture the effect of being two or three months after an incident within an affected community. I present these estimates in the first six columns of Tables 7.1 – 7.3. As with the leads, I only estimate the lagged effect on total crime and property crime, the only types of crime that were significantly affected by racially charged brutality incidents.

As presented in the first column of Table 7.1, there is no significant relationship between being a month, two months, or three months after a racially charged brutality incident and local crime when I exclude all fixed effects. In the specification of choice, presented in column five, being within a month after a racially charged brutality incident is associated with an eight percent increase in total crimes and being in the second month after an incident is associated with a four percent decrease in total crimes, relative to other communities (both significant at the one percent level). This suggests that potentially racially charged police brutality incidents may initially increase local crime, but that this spike in crime is displaced from the following month.

In the first column of Table 7.3, as with total crime, there is no significant relationship between being a month, two months, or three months after a racially charged brutality incident and local crime when I exclude all fixed effects. In the specification of choice, presented in column six, being within a month after a racially charged incident is associated with a 17 percent increase in property crimes relative to other communities. The effect then falls effectively to zero two months and three months after the incident. This suggests that there is a sharp and transient effect of these incidents on property crime.

Table 7.1: Persistence of Community Effect: Total Crime

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Three Months Before | | | | | | -0.018 |
| | | | | | | (0.012) |
| Two Months Before | | | | | | -0.009 |
| | | | | | | (0.009) |
| One Month Before | | | | | | -0.002 |
| | | | | | | (0.010) |
| One Month After | 0.067 | -0.003 | -0.005 | -0.002 | 0.014 | 0.012 |
| | (0.068) | (0.020) | (0.011) | (0.011) | (0.009) | (0.010) |
| Two Months After | 0.060 | -0.006 | -0.022 | -0.023 | -0.005 | -0.006 |
| | (0.075) | (0.021) | (0.017) | (0.017) | (0.017) | (0.017) |
| Three Months After | 0.071 | 0.007 | -0.015 | -0.018* | -0.002 | -0.003 |
| | (0.068) | (0.016) | (0.010) | (0.010) | (0.008) | (0.009) |
| After Three Months | 0.195* | -2.768** | 1.265* | 1.281* | 0.756 | 2.715 |
| | (0.116) | (1.267) | (0.761) | (0.776) | (0.975) | (2.616) |
| Community FE | no | yes | yes | yes | yes | yes |
| Week FE | no | no | yes | yes | yes | yes |
| Month FE | no | no | yes | – | – | – |
| Year FE | no | no | yes | – | – | – |
| Month by Year FE | no | no | no | yes | yes | yes |
| Community Specific Trends | no | no | no | no | yes | yes |
| Community Specific Quadratic Trends | no | no | no | no | no | no |
| N | 148379 | 148379 | 148004 | 148379 | 148282 | 147951 |

Notes: Based on the City of Chicago reported crime data and CPDP complaint data. Each column represents results from a separate Poisson regression. Standard Errors, corrected for clustering at the district level, are in parentheses. *Statistically significant at the 10% level;**at the 5% level; *** at the 1% level.

Table 7.2: Persistence of Community Effect: Violent Crime

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Three Months Before | | | | | | -0.028** |
| | | | | | | (0.014) |
| Two Months Before | | | | | | -0.009 |
| | | | | | | (0.014) |
| One Month Before | | | | | | 0.027** |
| | | | | | | (0.011) |
| One Month After | -0.009 | 0.010 | 0.008 | 0.014 | 0.031** | 0.030** |
| | (0.046) | (0.028) | (0.015) | (0.015) | (0.015) | (0.015) |
| Two Months After | -0.016 | 0.005 | -0.024 | -0.018 | -0.002 | -0.002 |
| | (0.045) | (0.026) | (0.019) | (0.019) | (0.017) | (0.018) |
| Three Months After | 0.002 | 0.024 | -0.015 | -0.013 | 0.002 | 0.003 |
| | (0.043) | (0.030) | (0.016) | (0.015) | (0.013) | (0.014) |
| After Three Months | 0.201* | -1.722 | 1.390 | 1.524* | 6.831*** | 7.196*** |
| | (0.111) | (1.327) | (0.848) | (0.900) | (1.490) | (1.471) |
| Community FE | no | yes | yes | yes | yes | yes |
| Week FE | no | no | yes | yes | yes | yes |
| Month FE | no | no | yes | – | – | – |
| Year FE | no | no | yes | – | – | – |
| Month by Year FE | no | no | no | yes | yes | yes |
| Community Specific Trends | no | no | no | no | yes | yes |
| Community Specific Quadratic Trends | no | no | no | no | no | no |
| N | 148379 | 148257 | 146281 | 148257 | 146784 | 146784 |

Notes: Based on the City of Chicago reported crime data and CPDP complaint data. Each column represents results from a separate Poisson regression. Standard Errors, corrected for clustering at the district level, are in parentheses. *Statistically significant at the 10% level;**at the 5% level; *** at the 1% level.

Table 7.3: Persistence of Community Effect: Property Crime

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Three Months Before | | | | | | -0.008 |
| | | | | | | (0.016) |
| Two Months Before | | | | | | 0.007 |
| | | | | | | (0.014) |
| One Month Before | | | | | | 0.002 |
| | | | | | | (0.016) |
| One Month After | 0.148* | 0.028 | 0.021 | 0.019 | 0.031* | 0.032* |
| | (0.086) | (0.027) | (0.016) | (0.017) | (0.017) | (0.019) |
| Two Months After | 0.150* | 0.035 | 0.017 | 0.012 | 0.025* | 0.026* |
| | (0.091) | (0.024) | (0.016) | (0.014) | (0.014) | (0.015) |
| Three Months After | 0.147* | 0.036 | 0.012 | 0.009 | 0.019 | 0.019 |
| | (0.087) | (0.025) | (0.016) | (0.016) | (0.016) | (0.016) |
| After Three Months | 0.190 | -2.541* | 2.005** | 2.089** | -2.832** | 5.961*** |
| | (0.131) | (1.413) | (0.795) | (0.873) | (1.281) | (1.494) |
| Community FE | no | yes | yes | yes | yes | yes |
| Week FE | no | no | yes | yes | yes | yes |
| Month FE | no | no | yes | – | – | – |
| Year FE | no | no | yes | – | – | – |
| Month by Year FE | no | no | no | yes | yes | yes |
| Community Specific Trends | no | no | no | no | yes | yes |
| Community Specific Quadratic Trends | no | no | no | no | no | no |
| N | 148379 | 148378 | 147169 | 148378 | 148002 | 146708 |

Notes: Based on the City of Chicago reported crime data and CPDP complaint data. Each column represents results from a separate Poisson regression. Standard Errors, corrected for clustering at the district level, are in parentheses. *Statistically significant at the 10% level;**at the 5% level; *** at the 1% level.

Table 7.4: Persistence of BV Community Effect: Total Crime

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Three Months Before | | | | | | 0.007 |
| | | | | | | (0.013) |
| Two Months Before | | | | | | -0.008 |
| | | | | | | (0.009) |
| One Month Before | | | | | | 0.008 |
| | | | | | | (0.012) |
| One Month After | -0.121 | -0.078 | -0.009 | -0.003 | 0.008 | 0.010 |
| | (0.101) | (0.048) | (0.026) | (0.023) | (0.023) | (0.023) |
| Two Months After | -0.128 | -0.067** | -0.025 | -0.029 | -0.018 | -0.017 |
| | (0.098) | (0.033) | (0.023) | (0.022) | (0.020) | (0.021) |
| Three Months After | -0.050 | -0.003 | 0.013 | 0.012 | 0.025 | 0.026* |
| | (0.088) | (0.033) | (0.016) | (0.016) | (0.015) | (0.015) |
| After Three Months | 0.196* | -2.725** | 1.292* | 1.310 | 0.788 | 2.645 |
| | (0.117) | (1.258) | (0.779) | (0.797) | (0.970) | (2.627) |
| Community FE | no | yes | yes | yes | yes | yes |
| Week FE | no | no | yes | yes | yes | yes |
| Year FE | no | no | yes | – | – | – |
| Month by Year FE | no | no | no | yes | yes | yes |
| Community Specific Trends | no | no | no | no | yes | yes |
| Community Specific Quadratic Trends | no | no | no | no | no | no |
| N | 148379 | 148379 | 148004 | 148379 | 148282 | 147951 |

Notes: Based on the City of Chicago reported crime data and CPDP complaint data. Each column represents results from a separate Poisson regression. Standard Errors, corrected for clustering at the district level, are in parentheses. *Statistically significant at the 10% level;**at the 5% level; *** at the 1% level.

Table 7.5: Persistence of BV Community Effect: Violent Crime

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Three Months Before | | | | | | -0.005 |
| | | | | | | (0.035) |
| Two Months Before | | | | | | -0.039 |
| | | | | | | (0.033) |
| One Month Before | | | | | | 0.001 |
| | | | | | | (0.016) |
| One Month After | -0.143 | -0.033 | 0.010 | 0.026* | 0.045** | 0.049** |
| | (0.090) | (0.040) | (0.012) | (0.014) | (0.018) | (0.021) |
| Two Months After | -0.206* | -0.079 | -0.066*** | -0.055*** | -0.037** | -0.040** |
| | (0.109) | (0.051) | (0.022) | (0.020) | (0.015) | (0.017) |
| Three Months After | -0.098 | 0.017 | -0.008 | -0.005 | 0.010 | 0.008 |
| | (0.109) | (0.064) | (0.026) | (0.027) | (0.024) | (0.025) |
| After Three Months | 0.199* | -1.700 | 1.408 | 1.537* | 6.837*** | 7.257*** |
| | (0.111) | (1.321) | (0.867) | (0.915) | (1.493) | (1.458) |
| Community FE | no | yes | yes | yes | yes | yes |
| Week FE | no | no | yes | yes | yes | yes |
| Year FE | no | no | yes | – | – | – |
| Month by Year FE | no | no | no | yes | yes | yes |
| Community Specific Trends | no | no | no | no | yes | yes |
| Community Specific Quadratic Trends | no | no | no | no | no | no |
| N | 148379 | 148257 | 146281 | 148257 | 146784 | 146784 |

Notes: Based on the City of Chicago reported crime data and CPDP complaint data. Each column represents results from a separate Poisson regression. Standard Errors, corrected for clustering at the district level, are in parentheses. *Statistically significant at the 10% level;**at the 5% level; *** at the 1% level.

Table 7.6: Persistence of BV Community Effect: Property Crime

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Three Months Before | | | | | | -0.031 |
| | | | | | | (0.029) |
| Two Months Before | | | | | | 0.044*** |
| | | | | | | (0.014) |
| One Month Before | | | | | | 0.011 |
| | | | | | | (0.026) |
| One Month After | -0.119 | -0.086 | 0.017 | 0.017 | 0.034 | 0.029 |
| | (0.129) | (0.056) | (0.043) | (0.039) | (0.040) | (0.037) |
| Two Months After | -0.089 | -0.031 | 0.043 | 0.027 | 0.044 | 0.045 |
| | (0.114) | (0.032) | (0.035) | (0.036) | (0.034) | (0.032) |
| Three Months After | -0.094 | -0.054 | -0.014 | -0.019 | -0.005 | -0.001 |
| | (0.101) | (0.037) | (0.050) | (0.048) | (0.049) | (0.048) |
| After Three Months | 0.194 | -2.556* | 1.979** | 2.069** | -3.028** | 5.782*** |
| | (0.134) | (1.360) | (0.775) | (0.856) | (1.291) | (1.507) |
| Community FE | no | yes | yes | yes | yes | yes |
| Week FE | no | no | yes | yes | yes | yes |
| Year FE | no | no | yes | – | – | – |
| Month by Year FE | no | no | no | yes | yes | yes |
| Community Specific Trends | no | no | no | no | yes | yes |
| Community Specific Quadratic Trends | no | no | no | no | no | no |
| N | 148379 | 148378 | 147169 | 148378 | 148002 | 146708 |

Notes: Based on the City of Chicago reported crime data and CPDP complaint data. Each column represents results from a separate Poisson regression. Standard Errors, corrected for clustering at the district level, are in parentheses. *Statistically significant at the 10% level;**at the 5% level; *** at the 1% level.

Table 7.7: Persistence of BVWO Community Effect: Total Crime

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Three Months Before | | | | | | 0.005 |
| | | | | | | (0.023) |
| Two Months Before | | | | | | -0.013 |
| | | | | | | (0.008) |
| One Month Before | | | | | | 0.003 |
| | | | | | | (0.006) |
| One Month After | -0.239 | 0.079 | 0.044 | 0.049 | 0.086*** | 0.088*** |
| | (0.198) | (0.072) | (0.050) | (0.032) | (0.029) | (0.029) |
| Two Months After | -0.207 | -0.086* | -0.059*** | -0.062*** | -0.036*** | -0.037*** |
| | (0.154) | (0.045) | (0.016) | (0.015) | (0.012) | (0.011) |
| Three Months After | -0.124 | -0.014 | -0.007 | -0.009 | 0.014 | 0.013 |
| | (0.146) | (0.047) | (0.015) | (0.014) | (0.018) | (0.018) |
| After Three Months | 0.195* | -2.726** | 1.291* | 1.309* | 0.368*** | 0.367*** |
| | (0.117) | (1.259) | (0.775) | (0.793) | (0.002) | (0.002) |
| Community FE | no | yes | yes | yes | yes | yes |
| Week FE | no | no | yes | yes | yes | yes |
| Year FE | no | no | yes | – | – | – |
| Month by Year FE | no | no | no | yes | yes | yes |
| Community Specific Trends | no | no | no | no | yes | yes |
| Community Specific Quadratic Trends | no | no | no | no | no | no |
| N | 148379 | 148379 | 148004 | 148379 | 147316 | 147316 |

Notes: Based on the City of Chicago reported crime data and CPDP complaint data. Each column represents results from a separate Poisson regression. Standard Errors, corrected for clustering at the district level, are in parentheses. *Statistically significant at the 10% level;**at the 5% level; *** at the 1% level.

Table 7.8: Persistence of BVWO Community Effect: Violent Crime

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Three Months Before | | | | | | -0.065** |
| | | | | | | (0.028) |
| Two Months Before | | | | | | -0.085** |
| | | | | | | (0.041) |
| One Month Before | | | | | | -0.027 |
| | | | | | | (0.034) |
| One Month After | -0.436*** | 0.090** | -0.023 | -0.001 | 0.034 | 0.050* |
| | (0.152) | (0.043) | (0.021) | (0.026) | (0.028) | (0.029) |
| Two Months After | -0.316 | -0.066 | -0.109*** | -0.090*** | -0.063*** | -0.070*** |
| | (0.193) | (0.051) | (0.024) | (0.021) | (0.010) | (0.007) |
| Three Months After | -0.196 | 0.033 | -0.027 | -0.025 | -0.000 | -0.016* |
| | (0.196) | (0.069) | (0.023) | (0.020) | (0.010) | (0.008) |
| After Three Months | 0.197* | -1.717 | 1.414* | 1.541* | 0.823*** | 0.813*** |
| | (0.111) | (1.328) | (0.858) | (0.908) | (0.003) | (0.003) |
| Community FE | no | yes | yes | yes | yes | yes |
| Week FE | no | no | yes | yes | yes | yes |
| Year FE | no | no | yes | – | – | – |
| Month by Year FE | no | no | no | yes | yes | yes |
| Community Specific Trends | no | no | no | no | yes | yes |
| Community Specific Quadratic Trends | no | no | no | no | no | no |
| N | 148379 | 148257 | 146281 | 148257 | 145597 | 145597 |

Notes: Based on the City of Chicago reported crime data and CPDP complaint data. Each column represents results from a separate Poisson regression. Standard Errors, corrected for clustering at the district level, are in parentheses. *Statistically significant at the 10% level;**at the 5% level; *** at the 1% level.

Table 7.9: Persistence of BVWO Community Effect: Property Crime

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Three Months Before | | | | | | -0.028 |
| | | | | | | (0.028) |
| Two Months Before | | | | | | 0.039 |
| | | | | | | (0.025) |
| One Month Before | | | | | | 0.009 |
| | | | | | | (0.035) |
| One Month After | -0.186 | 0.142*** | 0.151*** | 0.143*** | 0.173*** | 0.164*** |
| | (0.175) | (0.044) | (0.023) | (0.023) | (0.024) | (0.025) |
| Two Months After | -0.171 | -0.049 | 0.005 | -0.004 | 0.022 | 0.023 |
| | (0.171) | (0.043) | (0.029) | (0.034) | (0.040) | (0.036) |
| Three Months After | -0.175 | -0.065 | -0.053 | -0.053 | -0.030 | -0.025 |
| | (0.153) | (0.040) | (0.051) | (0.052) | (0.059) | (0.064) |
| After Three Months | 0.193 | -2.558* | 1.977** | 2.068** | -0.047*** | -0.045*** |
| | (0.134) | (1.362) | (0.774) | (0.856) | (0.008) | (0.010) |
| Community FE | no | yes | yes | yes | yes | yes |
| Week FE | no | no | yes | yes | yes | yes |
| Year FE | no | no | yes | – | – | – |
| Month by Year FE | no | no | no | yes | yes | yes |
| Community Specific Trends | no | no | no | no | yes | yes |
| Community Specific Quadratic Trends | no | no | no | no | no | no |
| N | 148379 | 148378 | 147169 | 148378 | 146418 | 146418 |

Notes: Based on the City of Chicago reported crime data and CPDP complaint data. Each column represents results from a separate Poisson regression. Standard Errors, corrected for clustering at the district level, are in parentheses. *Statistically significant at the 10% level;**at the 5% level; *** at the 1% level.

Table 7.10: Persistence of District Effect: Total Crime

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Three Months Before | | | | | | -0.008 |
| | | | | | | (0.009) |
| Two Months Before | | | | | | -0.002 |
| | | | | | | (0.009) |
| One Month Before | | | | | | -0.003 |
| | | | | | | (0.007) |
| One Month After | 0.215*** | 0.038* | -0.004 | -0.002 | 0.004 | 0.003 |
| | (0.077) | (0.021) | (0.009) | (0.008) | (0.008) | (0.008) |
| Two Months After | 0.208*** | 0.040* | -0.011 | -0.013 | -0.008 | -0.009 |
| | (0.077) | (0.021) | (0.012) | (0.011) | (0.010) | (0.009) |
| Three Months After | 0.206*** | 0.044** | -0.013 | -0.018 | -0.012 | -0.013 |
| | (0.072) | (0.019) | (0.012) | (0.013) | (0.012) | (0.012) |
| After Three Months | 0.033 | -0.193*** | -0.023* | -0.028** | -0.017 | -0.018 |
| | (0.152) | (0.017) | (0.013) | (0.013) | (0.011) | (0.012) |
| District FE | no | yes | yes | yes | yes | yes |
| Week FE | no | no | yes | yes | yes | yes |
| Month FE | no | no | yes | – | – | – |
| Year FE | no | no | yes | – | – | – |
| Month by Year FE | no | no | no | yes | yes | yes |
| District Specific Trends | no | no | no | no | yes | yes |
| District Specific Quadratic Trends | no | no | no | no | no | no |
| N | 46248 | 46248 | 46248 | 46248 | 44322 | 44322 |

Notes: Based on the City of Chicago reported crime data and CPDP complaint data. Each column represents results from a separate Poisson regression. Standard Errors, corrected for clustering at the district level, are in parentheses. *Statistically significant at the 10% level;**at the 5% level; *** at the 1% level.

Table 7.11: Persistence of District Effect: Violent Crime

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Three Months Before | | | | | | -0.023** |
| | | | | | | (0.011) |
| Two Months Before | | | | | | -0.002 |
| | | | | | | (0.014) |
| One Month Before | | | | | | 0.010 |
| | | | | | | (0.012) |
| One Month After | 0.249*** | 0.040* | 0.010 | 0.014 | 0.019* | 0.018* |
| | (0.086) | (0.024) | (0.009) | (0.009) | (0.010) | (0.010) |
| Two Months After | 0.231*** | 0.033 | -0.016 | -0.012 | -0.008 | -0.008 |
| | (0.088) | (0.026) | (0.012) | (0.013) | (0.011) | (0.011) |
| Three Months After | 0.240*** | 0.049** | -0.007 | -0.007 | -0.003 | -0.003 |
| | (0.078) | (0.021) | (0.012) | (0.014) | (0.012) | (0.012) |
| After Three Months | 0.132 | -0.156*** | -0.025** | -0.025** | -0.015 | -0.017* |
| | (0.169) | (0.016) | (0.010) | (0.011) | (0.010) | (0.010) |
| District FE | no | yes | yes | yes | yes | yes |
| Week FE | no | no | yes | yes | yes | yes |
| Month FE | no | no | yes | – | – | – |
| Year FE | no | no | yes | – | – | – |
| Month by Year FE | no | no | no | yes | yes | yes |
| District Specific Trends | no | no | no | no | yes | yes |
| District Specific Quadratic Trends | no | no | no | no | no | no |
| N | 46248 | 42394 | 42394 | 42394 | 42391 | 42391 |

Notes: Based on the City of Chicago reported crime data and CPDP complaint data. Each column represents results from a separate Poisson regression. Standard Errors, corrected for clustering at the district level, are in parentheses. *Statistically significant at the 10% level;**at the 5% level; *** at the 1% level.

Table 7.12: Persistence of District Effect: Property Crime

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Three Months Before | | | | | | -0.002 |
| | | | | | | (0.013) |
| Two Months Before | | | | | | 0.007 |
| | | | | | | (0.015) |
| One Month Before | | | | | | -0.000 |
| | | | | | | (0.011) |
| One Month After | 0.182** | 0.059** | 0.003 | 0.004 | 0.001 | 0.001 |
| | (0.085) | (0.029) | (0.013) | (0.013) | (0.014) | (0.014) |
| Two Months After | 0.194** | 0.075*** | 0.017 | 0.012 | 0.008 | 0.008 |
| | (0.083) | (0.027) | (0.015) | (0.013) | (0.013) | (0.013) |
| Three Months After | 0.173** | 0.056** | -0.009 | -0.011 | -0.015 | -0.015 |
| | (0.078) | (0.024) | (0.016) | (0.015) | (0.015) | (0.015) |
| After Three Months | -0.104 | -0.224*** | -0.021 | -0.016 | -0.017 | -0.017 |
| | (0.159) | (0.020) | (0.020) | (0.021) | (0.019) | (0.019) |
| District FE | no | yes | yes | yes | yes | yes |
| Week FE | no | no | yes | yes | yes | yes |
| Month FE | no | no | yes | – | – | – |
| Year FE | no | no | yes | – | – | – |
| Month by Year FE | no | no | no | yes | yes | yes |
| District Specific Trends | no | no | no | no | yes | yes |
| District Specific Quadratic Trends | no | no | no | no | no | no |
| N | 46248 | 44321 | 44321 | 44321 | 44311 | 44311 |

Notes: Based on the City of Chicago reported crime data and CPDP complaint data. Each column represents results from a separate Poisson regression. Standard Errors, corrected for clustering at the district level, are in parentheses. *Statistically significant at the 10% level;**at the 5% level; *** at the 1% level.

Table 7.13: Persistence of BV District Effect: Total Crime

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Three Months Before | | | | | | 0.008 |
| | | | | | | (0.011) |
| Two Months Before | | | | | | 0.017 |
| | | | | | | (0.023) |
| One Month Before | | | | | | 0.028*** |
| | | | | | | (0.010) |
| One Month After | 0.249** | -0.049 | 0.030 | 0.042*** | 0.052*** | 0.055*** |
| | (0.100) | (0.057) | (0.021) | (0.013) | (0.016) | (0.016) |
| Two Months After | 0.232** | -0.049 | 0.020 | 0.019 | 0.032 | 0.035 |
| | (0.096) | (0.073) | (0.019) | (0.016) | (0.022) | (0.022) |
| Three Months After | 0.228*** | -0.033 | 0.011 | 0.009 | 0.021 | 0.026 |
| | (0.083) | (0.077) | (0.023) | (0.025) | (0.021) | (0.022) |
| After Three Months | 0.169 | -0.185*** | 0.032 | 0.034 | 0.040** | 0.048*** |
| | (0.125) | (0.034) | (0.035) | (0.035) | (0.016) | (0.015) |
| District FE | no | yes | yes | yes | yes | yes |
| Week FE | no | no | yes | yes | yes | yes |
| Month FE | no | no | yes | – | – | – |
| Year FE | no | no | yes | – | – | – |
| Month by Year FE | no | no | no | yes | yes | yes |
| District Specific Trends | no | no | no | no | yes | yes |
| District Specific Quadratic Trends | no | no | no | no | no | no |
| N | 46248 | 46248 | 46248 | 46248 | 44322 | 44322 |

Notes: Based on the City of Chicago reported crime data and CPDP complaint data. Each column represents results from a separate Poisson regression. Standard Errors, corrected for clustering at the district level, are in parentheses. *Statistically significant at the 10% level;**at the 5% level; *** at the 1% level.

Table 7.14: Persistence of BV District Effect: Violent Crime

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Three Months Before | | | | | | -0.022 |
| | | | | | | (0.027) |
| Two Months Before | | | | | | 0.010 |
| | | | | | | (0.028) |
| One Month Before | | | | | | 0.010 |
| | | | | | | (0.030) |
| One Month After | 0.402*** | -0.026 | 0.041* | 0.056*** | 0.059** | 0.058** |
| | (0.109) | (0.042) | (0.022) | (0.019) | (0.024) | (0.026) |
| Two Months After | 0.315*** | -0.081 | -0.012 | -0.010 | -0.004 | -0.005 |
| | (0.111) | (0.068) | (0.024) | (0.025) | (0.023) | (0.026) |
| Three Months After | 0.318*** | -0.044 | -0.006 | -0.012 | -0.006 | -0.005 |
| | (0.100) | (0.089) | (0.025) | (0.027) | (0.019) | (0.021) |
| After Three Months | 0.342*** | -0.159*** | 0.010 | 0.013 | -0.007 | -0.007 |
| | (0.129) | (0.020) | (0.018) | (0.018) | (0.023) | (0.025) |
| District FE | no | yes | yes | yes | yes | yes |
| Week FE | no | no | yes | yes | yes | yes |
| Month FE | no | no | yes | – | – | – |
| Year FE | no | no | yes | – | – | – |
| Month by Year FE | no | no | no | yes | yes | yes |
| District Specific Trends | no | no | no | no | yes | yes |
| District Specific Quadratic Trends | no | no | no | no | no | no |
| N | 46248 | 42394 | 42394 | 42394 | 42391 | 42391 |

Notes: Based on the City of Chicago reported crime data and CPDP complaint data. Each column represents results from a separate Poisson regression. Standard Errors, corrected for clustering at the district level, are in parentheses. *Statistically significant at the 10% level;**at the 5% level; *** at the 1% level.

Table 7.15: Persistence of BV District Effect: Property Crime

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Three Months Before | | | | | | -0.029 |
| | | | | | | (0.037) |
| Two Months Before | | | | | | 0.021 |
| | | | | | | (0.025) |
| One Month Before | | | | | | 0.010 |
| | | | | | | (0.023) |
| One Month After | -0.002 | -0.065 | 0.041 | 0.049** | 0.050** | 0.049** |
| | (0.104) | (0.066) | (0.028) | (0.024) | (0.021) | (0.021) |
| Two Months After | 0.036 | -0.031 | 0.083*** | 0.068*** | 0.069*** | 0.069*** |
| | (0.100) | (0.083) | (0.022) | (0.019) | (0.019) | (0.017) |
| Three Months After | -0.002 | -0.076 | 0.012 | 0.002 | 0.002 | 0.003 |
| | (0.089) | (0.089) | (0.036) | (0.038) | (0.035) | (0.036) |
| After Three Months | -0.168 | -0.253*** | 0.011 | 0.015 | 0.025 | 0.026 |
| | (0.118) | (0.020) | (0.028) | (0.028) | (0.021) | (0.021) |
| District FE | no | yes | yes | yes | yes | yes |
| Week FE | no | no | yes | yes | yes | yes |
| Month FE | no | no | yes | – | – | – |
| Year FE | no | no | yes | – | – | – |
| Month by Year FE | no | no | no | yes | yes | yes |
| District Specific Trends | no | no | no | no | yes | yes |
| District Specific Quadratic Trends | no | no | no | no | no | no |
| N | 46248 | 44321 | 44321 | 44321 | 44311 | 44311 |

Notes: Based on the City of Chicago reported crime data and CPDP complaint data. Each column represents results from a separate Poisson regression. Standard Errors, corrected for clustering at the district level, are in parentheses. *Statistically significant at the 10% level;**at the 5% level; *** at the 1% level.

Table 7.16: Persistence of BVWO District Effect: Total Crime

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Three Months Before | | | | | | 0.014 |
| | | | | | | (0.019) |
| Two Months Before | | | | | | 0.054* |
| | | | | | | (0.030) |
| One Month Before | | | | | | 0.028*** |
| | | | | | | (0.009) |
| One Month After | 0.123 | 0.083 | -0.002 | 0.004 | 0.016 | 0.021 |
| | (0.098) | (0.107) | (0.064) | (0.045) | (0.033) | (0.035) |
| Two Months After | 0.252*** | 0.044 | 0.010 | 0.012** | 0.016 | 0.025 |
| | (0.082) | (0.042) | (0.009) | (0.005) | (0.023) | (0.026) |
| Three Months After | 0.217*** | 0.060 | 0.027 | 0.024 | 0.031 | 0.042* |
| | (0.069) | (0.062) | (0.023) | (0.023) | (0.027) | (0.025) |
| After Three Months | 0.185 | -0.137** | 0.074 | 0.076 | 0.052* | 0.071*** |
| | (0.163) | (0.056) | (0.054) | (0.053) | (0.028) | (0.017) |
| District FE | no | yes | yes | yes | yes | yes |
| Week FE | no | no | yes | yes | yes | yes |
| Month FE | no | no | yes | – | – | – |
| Year FE | no | no | yes | – | – | – |
| Month by Year FE | no | no | no | yes | yes | yes |
| District Specific Trends | no | no | no | no | yes | yes |
| District Specific Quadratic Trends | no | no | no | no | no | no |
| N | 46248 | 46248 | 46248 | 46248 | 44322 | 44322 |

Notes: Based on the City of Chicago reported crime data and CPDP complaint data. Each column represents results from a separate Poisson regression. Standard Errors, corrected for clustering at the district level, are in parentheses. *Statistically significant at the 10% level;**at the 5% level; *** at the 1% level.

Table 7.17: Persistence of BVWO District Effect: Violent Crime

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Three Months Before | | | | | | -0.076*** |
| | | | | | | (0.018) |
| Two Months Before | | | | | | -0.018 |
| | | | | | | (0.042) |
| One Month Before | | | | | | -0.053* |
| | | | | | | (0.028) |
| One Month After | 0.002 | 0.046 | -0.094** | -0.085*** | -0.067 | -0.081* |
| | (0.087) | (0.077) | (0.037) | (0.029) | (0.046) | (0.043) |
| Two Months After | 0.292*** | 0.006 | -0.050*** | -0.045*** | -0.032 | -0.043 |
| | (0.101) | (0.042) | (0.009) | (0.011) | (0.029) | (0.033) |
| Three Months After | 0.252*** | 0.036 | -0.003 | -0.016** | 0.001 | -0.013 |
| | (0.069) | (0.096) | (0.007) | (0.006) | (0.020) | (0.022) |
| After Three Months | 0.284* | -0.145*** | 0.023 | 0.027 | -0.010 | -0.037 |
| | (0.150) | (0.036) | (0.024) | (0.023) | (0.038) | (0.043) |
| District FE | no | yes | yes | yes | yes | yes |
| Week FE | no | no | yes | yes | yes | yes |
| Month FE | no | no | yes | – | – | – |
| Year FE | no | no | yes | – | – | – |
| Month by Year FE | no | no | no | yes | yes | yes |
| District Specific Trends | no | no | no | no | yes | yes |
| District Specific Quadratic Trends | no | no | no | no | no | no |
| N | 46248 | 42394 | 42394 | 42394 | 42391 | 42391 |

Notes: Based on the City of Chicago reported crime data and CPDP complaint data. Each column represents results from a separate Poisson regression. Standard Errors, corrected for clustering at the district level, are in parentheses. *Statistically significant at the 10% level;**at the 5% level; *** at the 1% level.

Table 7.18: Persistence of BVWO District Effect: Property Crime

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Three Months Before | | | | | | -0.060 |
| | | | | | | (0.048) |
| Two Months Before | | | | | | 0.045*** |
| | | | | | | (0.014) |
| One Month Before | | | | | | -0.022 |
| | | | | | | (0.032) |
| One Month After | 0.038 | 0.122 | 0.044 | 0.038 | 0.048 | 0.041 |
| | (0.118) | (0.104) | (0.067) | (0.049) | (0.046) | (0.048) |
| Two Months After | 0.060 | 0.080*** | 0.044** | 0.037** | 0.039* | 0.039** |
| | (0.084) | (0.030) | (0.021) | (0.016) | (0.020) | (0.018) |
| Three Months After | 0.036 | 0.050 | 0.005 | -0.003 | -0.001 | -0.003 |
| | (0.062) | (0.056) | (0.053) | (0.053) | (0.055) | (0.057) |
| After Three Months | -0.278** | -0.262*** | -0.009 | -0.006 | -0.002 | -0.008 |
| | (0.123) | (0.028) | (0.028) | (0.029) | (0.039) | (0.036) |
| District FE | no | yes | yes | yes | yes | yes |
| Week FE | no | no | yes | yes | yes | yes |
| Month FE | no | no | yes | – | – | – |
| Year FE | no | no | yes | – | – | – |
| Month by Year FE | no | no | no | yes | yes | yes |
| District Specific Trends | no | no | no | no | yes | yes |
| District Specific Quadratic Trends | no | no | no | no | no | no |
| N | 46248 | 44321 | 44321 | 44321 | 44311 | 44311 |

Notes: Based on the City of Chicago reported crime data and CPDP complaint data. Each column represents results from a separate Poisson regression. Standard Errors, corrected for clustering at the district level, are in parentheses. *Statistically significant at the 10% level;**at the 5% level; *** at the 1% level.

# MECHANISMS

Though I cannot identify the underlying mechanisms, each theory I mentioned in chapter three[1] predicts that police brutality incidents will affect crime and arrest rates differently. In this section, I explain the empirical results each theory predicts and compare with the results from this thesis.

## Empirical Predictions and Potential Mechanisms

Recall that if there is a Ferguson effect, then, in general, crime rates should increase because officers exert less policing effort. But what about arrest rates? If brutality incidents simultaneously cause a decrease in arrest rates and increase in crime rates, this provides evidence that officers reduced their policing effort which caused crime rates to spike (Shi, 2009). This conclusion relies on the assumption that arrest rates increase in policing effort. Naturally, arrest rates also increase in the number of crimes committed, which suggests that if arrest rates stay constant and crimes increase that there is weak evidence for a Ferguson effect. Thus, requiring that the effect on arrest rates be negative to support a Ferguson effect theory is a conservative restriction.

Like the Ferguson effect, the retaliation effect predicts that that police brutality incidents will increase crime, but by inciting crimes of frustration instead of reducing the expected cost of committing a crime. Retaliation can manifest as riot-related crimes, the majority of which are property crimes, or crimes against police officers. Furthermore, pure retaliation predicts an increase in arrest rates. All else equal, if

[1] Recall that I am interested in the Ferguson effect, retaliation effect, and deterrence effect, as explained in chapter three.

police brutality incidents incite more crime, then, so long as the arrest rates for the types of crime that increase are not zero, then arrests should increase as well.

Unlike the other two effects, the deterrence effect predicts that police brutality incidents will scare marginal criminals and reduce crime. Like the retaliation effect, a pure deterrence effect would change potential criminal behavior, not police behavior. Thus, I expect arrest rates to decrease with crime rates if brutality incidents deter crime.

## Comparing Results with Predictions

In this thesis, I find results consistent with all three potential mechanisms. Results from the city-wide model suggest that brutality incidents decrease violent crimes. This finding is consistent with a pure deterrence effect on violent crime. But, when I estimate the proximity models, the results at both the community and district levels suggest that black-victim incidents cause an increase in property crime and do not alter the arrest rate. These results are most consistent with a retaliation effect because, in theory, if property crime increases, officers would need to reduce policing efforts to keep arrest rates the same. But, because the estimate of the effect on arrest rates is non-negative and relatively imprecise, it is difficult to infer anything from it. Lastly, the proximity race models also suggest that black-victim-white-officer (racially charged) incidents increase property crime while leaving arrest rates unchanged. This is most consistent with the retaliation effect because riot-related crimes are disproportionately property crimes, but because these incidents appear not to affect arrest rates, it is hard to distinguish this from a Ferguson effect.

# CONCLUSION

Recent high-profile police brutality incidents have shocked the nation and shifted our attention to the causes and consequences of police misconduct. Furthermore, the most salient of these brutality incidents were committed by white officers who brutalized black men. In this thesis, I attempted to answer two crucial questions about one aspect of this issue: how do police brutality incidents effect local crime and do these effects vary by the race of the officer and victim? To do this, I estimated the causal effect of serious excessive force incidents on crime rates in Chicago. My results suggest that the average police brutality incident both deters violent crime across Chicago and increases local crime in the community and district in which it occurred. When I focus on the effect of brutality incidents where the victim is black, increases in local crime become more pronounced. Lastly, I find that local crime increases even more after a potentially racially charged incident (black victim and white officer). These results are consistent with th notion that potentially racially charged incidents are felt more strongly because they cause officers to reduce policing effort or incite retaliatory crime.

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# APPENDIX

## OLS TABLES

Table A.1: Effect of Brutality on City-Wide Crime (OLS)

| | (1) | (2) | (3) |
|---|---|---|---|
| Total Crime | 1.177*** | 0.038 | 0.050 |
| | (0.190) | (0.071) | (0.037) |
| Violent Crime | 0.151*** | -0.074*** | -0.012 |
| | (0.037) | (0.017) | (0.014) |
| Property Crime | 0.796*** | 0.135*** | 0.074*** |
| | (0.107) | (0.042) | (0.023) |
| Crime Against Officer | -0.005** | -0.002 | 0.001 |
| | (0.002) | (0.002) | (0.002) |
| Gambling | 0.003*** | -0.004*** | 0.001 |
| | (0.001) | (0.001) | (0.001) |
| Arrests | 0.336*** | -0.028 | -0.024 |
| | (0.080) | (0.034) | (0.020) |
| Community FE | no | yes | yes |
| Week FE | no | no | yes |
| Year FE | no | no | yes |
| N | 143305 | 143305 | 143305 |

*Statistically significant at the 10% level;**at the 5% level; *** at the 1% level.

Table A.2: Effect of Brutality on Community Crime (OLS)

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Total Crime | 3.063*** | 0.371 | 0.332 | 0.320 | 0.178 | 0.300 |
| | (1.050) | (0.417) | (0.337) | (0.335) | (0.301) | (0.308) |
| Violent Crime | 0.570** | 0.130 | 0.114 | 0.110 | 0.128 | 0.127 |
| | (0.217) | (0.164) | (0.133) | (0.133) | (0.151) | (0.153) |
| Property Crime | 1.706*** | 0.366 | 0.337 | 0.316 | 0.227 | 0.258 |
| | (0.601) | (0.245) | (0.207) | (0.206) | (0.198) | (0.194) |
| Crime Against Officer | -0.014 | -0.015 | -0.014 | -0.013 | -0.010 | -0.008 |
| | (0.010) | (0.010) | (0.010) | (0.009) | (0.009) | (0.009) |
| Gambling | 0.013 | -0.003 | -0.003 | -0.003 | -0.007 | -0.004 |
| | (0.015) | (0.009) | (0.009) | (0.009) | (0.008) | (0.008) |
| Arrests | 1.049** | -0.039 | -0.025 | -0.005 | -0.101 | -0.016 |
| | (0.418) | (0.156) | (0.144) | (0.141) | (0.097) | (0.099) |
| Community FE | no | yes | yes | yes | yes | yes |
| Week FE | no | no | yes | yes | yes | yes |
| Year FE | no | no | yes | – | – | – |
| Month by Year FE | no | no | no | yes | yes | yes |
| Community Specific Trends | no | no | no | no | yes | yes |
| Community Specific Quadratic Trends | no | no | no | no | no | yes |
| N | 143305 | 143305 | 143305 | 143305 | 143305 | 143305 |

*Statistically significant at the 10% level;**at the 5% level; *** at the 1% level.

Table A.3: Effect of BV Brutality on Community Crime (OLS)

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Total Crime | 2.178 | -0.251 | -0.040 | 0.039 | 0.028 | 0.387 |
| | (2.949) | (1.388) | (1.177) | (1.125) | (1.170) | (1.397) |
| Violent Crime | 0.480 | 0.116 | 0.151 | 0.178 | 0.341 | 0.416 |
| | (0.674) | (0.431) | (0.338) | (0.324) | (0.463) | (0.509) |
| Property Crime | 0.435 | 0.017 | 0.143 | 0.156 | 0.214 | 0.287 |
| | (1.161) | (0.627) | (0.558) | (0.526) | (0.588) | (0.588) |
| Crime Against Officer | 0.001 | -0.009 | -0.006 | -0.004 | -0.004 | 0.005 |
| | (0.017) | (0.022) | (0.023) | (0.024) | (0.025) | (0.036) |
| Gambling | 0.043 | 0.001 | 0.002 | 0.004 | -0.006 | 0.005 |
| | (0.037) | (0.010) | (0.007) | (0.007) | (0.009) | (0.009) |
| Arrests | 1.913 | 0.046 | 0.126 | 0.192 | -0.110 | 0.172 |
| | (1.472) | (0.541) | (0.485) | (0.488) | (0.315) | (0.581) |
| Community FE | no | yes | yes | yes | yes | yes |
| Week FE | no | no | yes | yes | yes | yes |
| Year FE | no | no | yes | – | – | – |
| Month by Year FE | no | no | no | yes | yes | yes |
| Community Specific Trends | no | no | no | no | yes | yes |
| Community Specific Quadratic Trends | no | no | no | no | no | yes |
| N | 143305 | 143305 | 143305 | 143305 | 143305 | 143305 |

*Statistically significant at the 10% level;**at the 5% level; *** at the 1% level.

Table A.4: Effect of BVWO Brutality on Community Crime (OLS)

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Total Crime | 11.651** | 4.685*** | 3.341** | 3.381** | 3.964*** | 4.413*** |
| | (4.515) | (1.724) | (1.608) | (1.445) | (1.325) | (1.668) |
| Violent Crime | 2.341** | 1.422*** | 0.877** | 0.892** | 1.407*** | 1.559** |
| | (1.052) | (0.446) | (0.405) | (0.367) | (0.528) | (0.599) |
| Property Crime | 3.190** | 2.637*** | 2.089*** | 2.028*** | 2.254*** | 2.186*** |
| | (1.313) | (0.385) | (0.487) | (0.429) | (0.512) | (0.562) |
| Crime Against Officer | 0.070** | 0.009 | -0.000 | 0.008 | 0.014 | 0.041 |
| | (0.031) | (0.039) | (0.040) | (0.040) | (0.041) | (0.058) |
| Gambling | 0.188*** | 0.039** | 0.017 | 0.018 | -0.014 | 0.001 |
| | (0.054) | (0.016) | (0.016) | (0.016) | (0.022) | (0.021) |
| Arrests | 6.972*** | 1.200 | 0.902 | 1.050 | 0.684 | 1.330 |
| | (2.445) | (0.943) | (0.842) | (0.807) | (0.541) | (0.858) |
| Community FE | no | yes | yes | yes | yes | yes |
| Week FE | no | no | yes | yes | yes | yes |
| Year FE | no | no | yes | – | – | – |
| Month by Year FE | no | no | no | yes | yes | yes |
| Community Specific Trends | no | no | no | no | yes | yes |
| Community Specific Quadratic Trends | no | no | no | no | no | yes |
| N | 143305 | 143305 | 143305 | 143305 | 143305 | 143305 |

*Statistically significant at the 10% level;**at the 5% level; *** at the 1% level.

Table A.5: Effect of Brutality on District Crime (OLS)

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Total Crime | 8.661*** | 1.927** | 0.126 | 0.037 | -0.101 | -0.066 |
| | (2.707) | (0.879) | (0.403) | (0.366) | (0.311) | (0.329) |
| Violent Crime | 2.784*** | 0.565* | 0.146 | 0.127 | 0.136 | 0.156 |
| | (0.867) | (0.312) | (0.157) | (0.159) | (0.158) | (0.165) |
| Property Crime | 3.291** | 1.184** | 0.144 | 0.052 | -0.105 | -0.092 |
| | (1.359) | (0.488) | (0.193) | (0.195) | (0.239) | (0.246) |
| Crime Against Officer | 0.029 | -0.005 | -0.003 | -0.002 | 0.000 | 0.001 |
| | (0.022) | (0.021) | (0.019) | (0.017) | (0.015) | (0.015) |
| Gambling | 0.034 | 0.011 | -0.003 | -0.004 | -0.010 | -0.008 |
| | (0.024) | (0.017) | (0.014) | (0.015) | (0.012) | (0.012) |
| Arrests | 2.140*** | 0.433 | -0.133 | -0.090 | -0.120 | -0.116 |
| | (0.744) | (0.395) | (0.299) | (0.301) | (0.261) | (0.241) |
| District FE | no | yes | yes | yes | yes | yes |
| Week FE | no | no | yes | yes | yes | yes |
| Month FE | no | no | yes | – | – | – |
| Year FE | no | no | yes | – | – | – |
| Month by Year FE | no | no | no | yes | yes | yes |
| District Specific Trends | no | no | no | no | yes | yes |
| District Specific Quadratic Trends | no | no | no | no | no | yes |
| N | 42449 | 42449 | 42449 | 42449 | 42449 | 42449 |

*Statistically significant at the 10% level;**at the 5% level; *** at the 1% level.

Table A.6: Effect of BV Brutality on District Crime (OLS)

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Total Crime | 10.281*** | -1.621 | 0.231 | 0.513 | 1.622 | 0.885 |
| | (3.595) | (3.340) | (1.395) | (1.101) | (0.985) | (1.313) |
| Violent Crime | 4.713*** | -0.248 | 0.147 | 0.244 | 0.492 | 0.509 |
| | (1.065) | (0.815) | (0.292) | (0.194) | (0.382) | (0.382) |
| Property Crime | 0.328 | -0.913 | 0.130 | 0.176 | 0.583 | 0.658 |
| | (1.528) | (1.333) | (0.480) | (0.437) | (0.423) | (0.389) |
| Crime Against Officer | 0.062** | -0.002 | 0.000 | 0.010 | 0.006 | -0.011 |
| | (0.023) | (0.026) | (0.025) | (0.027) | (0.031) | (0.048) |
| Gambling | 0.122* | 0.021 | 0.033 | 0.038 | 0.027 | 0.015 |
| | (0.061) | (0.043) | (0.037) | (0.039) | (0.020) | (0.032) |
| Arrests | 5.260*** | -0.010 | 0.584 | 0.817 | 1.129** | 0.247 |
| | (1.530) | (1.464) | (0.822) | (0.815) | (0.514) | (0.968) |
| District FE | no | yes | yes | yes | yes | yes |
| Week FE | no | no | yes | yes | yes | yes |
| Year FE | no | no | yes | – | – | – |
| Month by Year FE | no | no | no | yes | yes | yes |
| District Specific Trends | no | no | no | no | yes | yes |
| District Specific Quadratic Trends | no | no | no | no | no | yes |
| N | 42449 | 42449 | 42449 | 42449 | 42449 | 42449 |

*Statistically significant at the 10% level;**at the 5% level; *** at the 1% level.

Table A.7: Effect of BVWO Brutality on District Crime (OLS)

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Total Crime | 5.944 | 5.262 | 0.636 | 0.723 | 1.982 | 1.384 |
| | (3.522) | (5.030) | (2.942) | (2.268) | (1.870) | (2.485) |
| Violent Crime | 0.039 | 1.040 | -0.713 | -0.676 | -0.169 | 0.212 |
| | (1.017) | (1.291) | (0.738) | (0.548) | (0.753) | (0.712) |
| Property Crime | 1.639 | 2.733 | 0.691 | 0.442 | 0.963 | 1.210 |
| | (1.317) | (1.740) | (1.024) | (0.885) | (0.855) | (0.760) |
| Crime Against Officer | 0.004 | 0.043 | 0.007 | 0.036 | 0.035 | 0.014 |
| | (0.035) | (0.044) | (0.056) | (0.053) | (0.061) | (0.092) |
| Gambling | 0.231*** | 0.106* | 0.036 | 0.040 | 0.015 | 0.006 |
| | (0.044) | (0.054) | (0.059) | (0.064) | (0.044) | (0.062) |
| Arrests | 4.720*** | 1.676 | 0.503 | 0.994 | 1.168 | 0.151 |
| | (1.247) | (2.228) | (1.356) | (1.295) | (0.781) | (1.720) |
| District FE | no | yes | yes | yes | yes | yes |
| Week FE | no | no | yes | yes | yes | yes |
| Year FE | no | no | yes | – | – | – |
| Month by Year FE | no | no | no | yes | yes | yes |
| District Specific Trends | no | no | no | no | yes | yes |
| District Specific Quadratic Trends | no | no | no | no | no | yes |
| N | 42449 | 42449 | 42449 | 42449 | 42449 | 42449 |

*Statistically significant at the 10% level;**at the 5% level; *** at the 1% level.