

Community Engagement with Law Enforcement after High-profile Acts of Police Violence*

Desmond Ang,[†] Panka Bencsik,[‡] Jesse Bruhn[§] and Ellora Derenoncourt[¶]

October, 2023

Abstract

We document a sharp rise in gunshots coupled with declining 911 call volume across thirteen major US cities in the aftermath of the murder of George Floyd. This pattern occurs in both white and non-white neighborhoods, persists beyond the protest movement, and is not accompanied by large declines in police response times. National survey data indicate victims of crime became less likely to report their victimization due to fear of police harassment. Our results suggest that high profile acts of police violence may erode community engagement with law-enforcement, and highlight the call-to-shot ratio as a natural measure of attitudes towards the police.

JEL classification: K4

Keywords: police, crime reporting, use of force, race

*For invaluable help with data collection and research assistance, we thank Fu Jin, Jacob Fabian, Colin Dunkley, and Lauren Fung. We thank Damon Jones and Conrad Miller for their detailed feedback, along with participants at the Brown University junior faculty lunch workshop and the Northeast Labor Symposium for Early Career Economists. We also thank the Orlando Bravo Center for Economic Research for their generous financial support of this project. An earlier version of this paper circulated under the title “Police violence reduces civilian cooperation and engagement with law enforcement.”

[†]Harvard University, Kennedy School of Government, 79 JFK St., Cambridge, MA 02138. Email: desmond_ang@hks.harvard.edu

[‡]Vanderbilt University, 2201 West End Ave, Nashville, TN 37235. Email: pankka.bencsik@vanderbilt.edu

[§]Brown University, 64 Waterman Street, Providence, RI 02912. Email: jesse_bruhn@brown.edu

[¶]Princeton University, Department of Economics, Princeton, NJ 08544 and NBER. Email: ellora.derenoncourt@princeton.edu

In recent years, high-profile acts of police violence against unarmed Black individuals have sparked widespread allegations of racial discrimination and national calls for policing reform. Such events are not unique in American history, nor are their attendant concerns (Cunningham & Gillezeau, 2018, 2021). The 1968 Kerner Commission report attributed nationwide civil unrest to the belief in Black communities in “the existence of police brutality and a double standard of justice and protection.” More generally, scholars have long argued that injustice within the legal system, and in particular police violence, may foster institutional distrust and legal cynicism or estrangement (Archibong & Obikili, 2020; Ba *et al.*, 2021; Bell, 2017; Kirk *et al.*, 2012; Sampson, 2012; Weitzer, 2002).

Central to these concerns is the understanding that civic trust and engagement is critical to many aspects of a well-functioning government. In particular, police departments are highly reliant on community cooperation and assistance to identify, report, and solve crimes. As former New York City police commissioner Bill Bratton stated, “police are most effective when they work in partnership with the community... when they are responding to citizens’ needs and working with citizens on determining priorities” (Bratton, 1997). If use of force degrades citizen trust in the police, these events could have the perverse effect of reducing policing efficacy, increasing crime and ultimately threatening public safety.

This study examines how salient acts of police violence affect community engagement with police. It fills an important gap in the literature on the causal effects of police violence on civilian-police cooperation, which is scarce and finds little consensus (Lerman & Weaver, 2014; Desmond *et al.*, 2016; Zoorob, 2020; Cohen *et al.*, 2019; Desmond *et al.*, 2020; Brantingham *et al.*, 2022).¹ Two studies examining 911 calls in Milwaukee after the 2005 police beating of Frank Jude come to conflicting conclusions, with one claiming decreased call volume from Black neighborhoods and the other finding no effect. A fundamental complication with criminal justice research in general, and this topic in particular, is that nearly all relevant outcomes are subject to selection bias. 911 calls represent the intersection of incidents observed in

¹In related work, Jacome (2021) found that prioritizing violent offenders for immigration enforcement increased crime reporting among Hispanic communities in Dallas.

a community and that community’s willingness to report those incidents to police. Thus, reductions in 911 calls could represent reduced engagement with police or actual reductions in crime, two explanations with drastically different policy implications. In theory, benchmarking changes in 911 call volume to changes in actual crime could help disentangle these competing narratives. In practice, researchers typically only observe crimes that have been reported to or directly witnessed by police, further compounding the issue.

To isolate changes in reporting from actual changes in crime, we pair 911 call volume with data on acoustically detected gunshots. The gunshot data come from a system of fixed-location microphones that cities across the nation have begun to install in order to detect and locate gunfire. While imperfect, the data provide a consistent proxy of local crime that is less reliant on human reporting than other measures. By combining this information with detailed data on 911 calls for service, we are able to observe, for a given gunshot, how likely a community is to call the police. Prior work has leveraged data from acoustic gunshot detection systems to explore how measurement issues related to selective reporting can complicate the interpretation of quasi-experimental research designs in the context of policy evaluation (Carr & Doleac, 2016, 2018).² We build on this insight by developing the call-to-shot ratio as a measure of community engagement and cooperation with police.³ Consistent with this interpretation, we show that survey measures of perceptions of law-enforcement are highly correlated with the call-to-shot ratio, but uncorrelated with total call volume.

Using our call-to-shot measure, we then explore how civilian crime-reporting evolved in the aftermath of high-profile acts of police violence. Examining data for thirteen major cities, we show a sharp drop in the ratio of 911 calls to acoustically detected gunshots immediately after the murder of George Floyd. While gunfire spiked following the killing and remained

²Carr & Doleac (2018) use acoustically detected gunshots to measure gunfire after the introduction of a curfew policy, and in robustness checks validate the core finding by showing that the same observed pattern emerges using 911 calls instead. Carr & Doleac (2016) evaluate how much under-reporting of gun crime occurs by comparing the number of 911 calls to the number of acoustically detected gunshots and find that 911 calls underestimate the *de facto* quantity of shootings.

³Since the initial release of this research as a working paper, it has been cited as the source of the call-to-shot ratio and its development as a measure of police trust by Facchetti (2021).

persistently high through the end of 2020, total calls-for-service volume declined. For the subset of 911 calls we are able to identify as specifically relating to gunshots, we observe a short-run (4-week) increase during the post-killing protest movement followed by a rapid decline. Using either measure of 911 call volume, the net result is a nearly 50% reduction in crime reporting. Notably, this drop is reflected across nearly all sample cities and persists until the end of 2020. We find similar patterns in majority-Black, majority-Hispanic and majority-white neighborhoods.

Importantly, the reporting patterns hold across a range of robustness checks meant to rule out competing theories and concerns. We show that declines are similar in neighborhoods with high versus low reporting rates at baseline suggesting that the drop in the call-to-shot ratio is not the product of a “ceiling” effect on communities’ willingness to report gunfire. We also find little evidence of increased police response times suggesting that de-policing in response to protests is unlikely to explain our main effects. To further corroborate the role of changing civilian behavior, we show robustness to excluding 911 calls that are most likely to be initiated by police. We also find similar effects when benchmarking 911 calls to gun violence casualties and automated alarms, indicating that effects are not driven by measurement error in acoustic gunshot detection data. Finally, we demonstrate robustness to controls for seasonality in 911 calls, pandemic-induced changes in community mobility, and numerous other factors.

To further interrogate the role of declining trust in law enforcement in explaining decreased crime reporting, we leverage detailed data from the National Crime Victimization Survey. Comparing observably similar incidents, we find that individuals victimized after George Floyd’s murder were significantly more likely to report mistrust of law enforcement and fear of police harassment as the main reasons for not reporting the incident to police. We find little evidence of reporting changes due to other reasons (e.g., private concerns). Taken together, these patterns suggest that declines in crime reporting are connected to a decline in police trust over this period.

We conclude by pivoting to two complementary questions raised by declines in reporting

that emerged in the wake of the murder of George Floyd. First, we ask if the patterns we document after Floyd’s murder are unique to that particular moment, or if they extrapolate to other acts of police violence. Examining a sub-sample of cities with historical data, we find large declines in the call-to-shot ratio after the murder of Michael Brown. However, we find little evidence of similar shifts following police murders garnering less public attention, suggesting salience as an important mediator. Second, we ask whether civilian engagement can be rebuilt after the conviction of officers involved in police brutality. To do so, we extend our sample to examine trends through April 21, 2021, when Minneapolis police officer Derek Chauvin was found guilty of murdering George Floyd. Notably, we find little evidence that civilian reporting converges to pre-Floyd rates even after Chauvin’s conviction. Together, our results point to the lasting damage that police violence may have on civilian cooperation and engagement with law enforcement.

1 Police favorability in the wake of George Floyd’s murder

The murder of George Floyd was just one of a string of recent police killings of unarmed Black individuals. Every year, roughly a thousand people die at the hands of American law enforcement officers. Estimates suggest that more than half of these individuals were racial minorities and that roughly 40% did not possess a gun (Washington Post, 2021). While the vast majority of these incidents received little public attention and studies have shown that the social impacts of police killings are often highly geographically-localized (Ang & Tebes, 2021; Ang, 2021), viral footage of Floyd’s death spread rapidly across social and traditional media platforms, sparking nationwide protests and renewed debate about racial bias in policing. Thus, while the incident bears many similarities to other recent incidents, its high visibility allows us to interrogate the impact of police violence on civilian engagement, even among communities with little direct exposure to those types of events.

[Figure I about here.]

Figure I tracks trends in public perceptions of police before and after the killing of George Floyd. The data come from Nationscape and includes a nationally representative sample of 6,250 weekly interviews from January to July 2020. We find a sharp decrease (increase) in the share of respondents holding favorable (unfavorable) views of police after George Floyd’s death. Notably, this is true across racial groups, with similar patterns for white and Asian individuals, those groups who are least likely to be killed by law enforcement, as for Black and Hispanic individuals, those groups who are most likely to experience police violence.

Floyd’s killing merits particular focus for another reason. Derek Chauvin, the Minneapolis police officer who knelt on Floyd’s neck, was convicted of murder in a verdict seen on live television by over 23 million Americans. It is incredibly rare that police officers are charged, much less convicted, even after high-profile use of force incidents. For example, Darren Wilson was not indicted for killing Michael Brown in Ferguson, nor was Daniel Pantaleo charged for killing Eric Garner in New York. Of the roughly 15,000 fatal police killings that have occurred since 2005, only 140 officers were arrested for murder or manslaughter and of those, fewer than 50 were ultimately found guilty (Stinson, 2019). President Biden hailed the conviction as a “giant step forward in the march toward justice in America,” while the Floyd family attorney called it a “turning point in American history for accountability of law enforcement.” Thus, the verdict presents a rare opportunity to assess whether highly-salient examples of officer discipline can repair civilian trust and reporting.

2 Background and data

2.1 Gunshots

Acoustic gunshot detection technology works via an audio recording system designed to capture the time and location of gunshots fired in an area. It is a common tool in a recent movement towards technology-supported policing, and has been implemented in more than 100 cities nationwide. The technology relies on a dispersed set of permanently-mounted sensors located

on buildings across a city. When a shot is fired, the sensors triangulate its location using Geographic Information System (GIS) technology and send a notification to the local police department with the predicted location of the shot.

Acoustic gunshot detection technology is not without controversy. While proponents claim the system can detect even silenced gunfire, there are concerns that it may also falsely classify car backfires, helicopters, and fireworks as gunshots (Carr & Doleac, 2016). Community activists have further questioned how acoustic gunshot detection systems are used, arguing that its alerts may prime officers to incorrectly assume that suspects are armed and that the parent company’s close relationship with police compromises its integrity (Stanley, 2021). However, evaluations suggest that the technology is generally accurate in detecting gunfire: acoustic gunshot detection technology is able to detect 80 to 99.6% of gunshots and to triangulate 91% of detected shots to within 40 feet of the actual location (Goode, 2012; Mazerolle *et al.*, 2000; Irvin-Erickson *et al.*, 2017; Watkins *et al.*, 2002).

Thus, while acoustic gunshot detection reports may include both false positives and false negatives, the data may nonetheless improve our ability to measure changes in violent crime when compared to traditional sources of crime micro-data, all of which rely on human reporting. The relative benefits of acoustic gunshot detection data are especially pronounced in our context. While both community and police reporting may be influenced by public scandals like high-profile police killings (Ba & Rivera, 2019), any measurement error in acoustic gunshot detection data is unlikely to be correlated with the timing of George Floyd’s murder or the resolution of Derek Chauvin’s trial. As corroboration, we show robustness using data on actual gun violence deaths and injuries, which are also unlikely to suffer from the reporting biases affecting lower-level offenses (Addington, 2008).⁴

⁴Data on the date and location of gun violence casualties come from the gunviolencearchive.org, which compiles information from over 7,500 law enforcement, media, government and commercial sources. While the data on acoustically detected gunshots and actual gun violence casualties are highly correlated, they are not 1-to-1 (see appendix figure A.I). The fact that we find similar patterns using either measure is thus reassuring.

2.2 Civilian Crime Reporting

To construct our measure of civilian crime reporting, we combine acoustic gunshot data with data on 911 calls for service that have been routed to police departments. These calls are connected to local dispatch centers, which log the date and location of the call as well as details about the incident being reported. Importantly, our restriction to calls that have been routed to police departments ensures that the data are capturing civilian crime reporting and not reporting related to other types of emergencies.

Our primary sample includes the thirteen cities where we were able to obtain both incident-level acoustic gunshot data and call-level data for 911 calls routed to police for the 2020 calendar year: Baltimore, MD; Cincinnati, OH; Fresno, CA; Glendale, AZ; Washington (DC); Miami, FL; Milwaukee, WI; Minneapolis, MN; New York City, NY; Oakland, CA; Richmond, CA; San Diego, CA; and San Francisco, CA. Together, these cities include major metropolitan areas across the East Coast, West Coast and Midwest encompassing over 15 million residents.

To minimize measurement error in the acoustic gunshot detection data caused by fireworks, we exclude data from New Year’s Eve, New Year’s Day and the Fourth of July. As acoustic gunshot detectors only cover parts of a city, we also restrict the sample to 911 calls initiated from neighborhoods in the local coverage areas.⁵ The final sample contains 12.1 million 911 calls and 55,523 detected shots in 2020.

Because the categorization of calls into crime types differs widely across cities, our preferred outcome is defined as the ratio of total 911 calls to acoustically detected shots in an area-period. However, in robustness analysis, we show similar patterns when examining the ratio of “shots fired”-related calls to gunshots.

⁵Specifically, we examine only 911 calls that are located in Census tracts with at least one acoustically detected gunshot over the sample period.

2.3 Correlation with Police Trust

To examine the relationship between police trust and the call-to-shot ratio, Panel A of Figure II plots call-to-shot ratios in each city against the share of local Nationscape respondents reporting unfavorable views of police.⁶ We find a significant, negative relationship between police trust and call-to-shot ratios. Cities with high shares of unfavorable views of police tend to receive fewer 911 calls per shot than cities with low shares of unfavorable police views. In contrast, we find little relationship between police favorability and 911 call volume. As shown in Panel B, views of police in a city are virtually uncorrelated with the average number of 911 calls made by its resident.⁷

[Figure II about here.]

Together, the results highlight the limitations of using raw 911 call volume as a proxy of community engagement and trust in police, while corroborating the importance of instead benchmarking 911 calls to local crime. Given the scarcity of granular survey data tracking public perceptions of law enforcement, the call-to-shot ratio holds the potential to fill an important gap for researchers by serving as a “revealed preference” measure of police trust.⁸

⁶City call-to-shot ratios and police unfavorability rankings are based on data from January 2, 2020 to March 12, 2020, the day before the Covid-19 National Emergency. As the Nationscape data are identified by respondent Congressional District, we construct city-level police favorability measures by averaging across respondents in districts containing each city. We exclude small cities (i.e., Glendale, Arizona and Richmond, California) that comprise only a small share of any given congressional district, as responses are unlikely to be representative of views in those areas. However, results are robust to their inclusion.

⁷Similar patterns hold over time within cities. Regressing our 911 measures in a city-week on contemporaneous police trust and city fixed effects, we find that a one standard-deviation increase in police unfavorability predicts a 0.05 standard deviation reduction in local call-to-shot ratio (p-value = 0.044) but little change in local call volume ($\beta=0.014$, p-value = 0.659).

⁸Case in point, the Nationscape poll is the only public data source of police perceptions we found that spanned the murder of George Floyd and that included enough temporal and geographic granularity to conduct rigorous empirical research. Even there, the question on police perceptions was removed from the survey during the summer of 2020.

3 Results

3.1 National Trends

To investigate how civilian crime reporting responded to the police killing of George Floyd, Figure III plots the ratio of weekly 911 calls to weekly gunshots over time. Both the numerator and denominator are aggregated across all sample cities.

[Figure III about here.]

The call-to-shot ratio declines by over 50% in the aftermath of the murder. As shown in Panel A of figure III, trends in call rates are relatively flat from the beginning of the calendar year until George Floyd’s murder, with the exception of a level shift downward when the national COVID-19 emergency was declared on March 13, 2020. After George Floyd’s murder, the call-to-shot ratio drops and remains depressed until the end of 2020.

This decline is driven by sudden changes to both the numerator and the denominator of the call-to-shot ratio. Panel B of figure III reveals surging gun-violence, with shots detected more than doubling in the weeks after George Floyd’s death. At the same time, Panel B also shows that while calls were trending up to pre-pandemic levels in the weeks before Floyd’s death, they decrease by nearly 25% in the weeks after the murder and largely remained depressed for the rest of the year. This is consistent with an explicit reduction in the tendency of individuals to reach out to police. That aggregate calls actually fell despite increased gunfire provides further corroboration that civilian engagement and cooperation with law enforcement was eroded after the murder of George Floyd.

[Table I about here.]

To test this effect empirically, Panel A of Table I estimates a time-series regression of the national call-to-shot ratio on a simple post-Floyd indicator.⁹ Data are at the daily-level and

⁹For inference, we account for auto-correlation using Newey-West standard errors (Wooldridge, 2010). However, we find similar levels of significance ($p = 0.031$) using a finite sample inferential procedure based on randomly chosen placebo dates. See appendix figure A.II for more detail.

the sample spans the 73 days between the beginning of the COVID-19 National Emergency and George Floyd’s murder and the 73 days afterwards. Column (1) presents the base model with no controls. To account for potential autocorrelation, Columns (2) and (3) include once and twice lagged dependent variables, respectively. Column (4) further includes a linear time trend to control for existing trends in reporting behavior. To account for changes in community mobility during the pandemic that may affect crime reporting, Column (5) includes controls for the amount of time spent in residences based on Google tracking data. Finally, Column (6) accounts for seasonality patterns in crime reporting by controlling for 911 call volume from the same date in 2019. In all cases, we find a large and significant decrease in civilian crime reporting of greater than 30% after the police murder of George Floyd.

3.2 Robustness

In this section, we discuss potential concerns about measurement error and interpretation, providing empirical tests using alternative samples and outcomes. For each test, regression estimates are shown in Panel B of Table I, while corresponding figures showing the raw time trends are included in the Appendix.¹⁰

Fireworks, “shot-fired” 911 calls, and other sources of measurement error

Community activists have raised important concerns about acoustic gunshot detection technology, including about undue police influence and false positives from fireworks usage, which were prevalent during the summer of 2020 (Tiffany, 2020). To address these concerns, Column (1) of Panel B benchmarks 911 calls to actual gun violence casualties, which are unlikely to be subject to the same reporting biases. We find, if anything, a larger proportional decrease in crime reporting of around 75%.

Given that civilians may call the police for a number of different issues unrelated to gunfire,

¹⁰More precisely, Figures A.III, A.IV, A.V, and A.VI show visual representations of the samples/outcomes explored in columns (1) through (4) of panel B of Table I, while Figure A.VII shows visual representations of the samples explored in columns (5) and (6) of panel B.

we next limit the analysis to 911 calls regarding “shots fired.” We identify these calls from text descriptions of each incident. While this is an imperfect exercise and liable to errors both in how 911 call center operators categorized events and in our ability to identify the appropriate categories in often poorly documented datasets, as shown in Column (2), we continue to find a significant decrease in civilian crime reporting when comparing “shots fired” calls to detected gunshots.

Ultimately, our ideal measure of civilian engagement and cooperation would be the ratio of *all* reported crimes to *all* actual crimes. While acoustic detection systems provide a third-party measure of one type of crime – gunfire – focusing only on changes in “shots fired” calls could misstate the true impact of Floyd’s murder by masking changes in reporting for other, less consequential crimes. Thus, to move closer to our ideal measure, we identify calls for service triggered by automated alarms - such as silent bank alarms, home alarms, and commercial alarms. As these incidents are captured without an individual calling police, they provide a proxy for property crime that is not mediated by human reporting. As shown in Column (3), we continue to find large drops in civilian engagement when including these alarms in our benchmark of local crime.

De-policing and the “Ferguson” effect

Next we explore the possibility that the results are driven by de-policing. The literature exploring the existence of a “Ferguson Effect,” in which public scrutiny of police brutality leads to reduced law enforcement effort which in turn increases crime, has found mixed results (Prendergast, 2001; Shi, 2008; Pyrooz *et al.*, 2016; Wolfe & Nix, 2016; Owens, 2019; Devi & Fryer Jr, 2020).¹¹ While we cannot rule out the possibility that increased gun violence is itself a product of reduced policing effort, it is unlikely that the effects on crime reporting - our primary outcome of interest - are fully explained by changes in policing behavior. In Column (4), we limit our analysis to exclude 911 calls arising from proactive policing efforts, such as

¹¹Related, Cho *et al.* (2023) find police officer deaths lead to reductions in arrest activity but no change in crime.

traffic and pedestrian stops. Focusing only on civilian-initiated 911 calls, we continue to find a significant decrease in crime reporting of over 50%.¹²

Ceiling effects in crime reporting

One potential concern with interpreting our results is that the changes could be driven by “ceiling” effects on reporting. For example, if gunshots are surging in neighborhoods that never reported crime to begin with, we may see a decline in the call to shot ratio even though the underlying tendency to engage with the police did not change. To address this, we split our sample according to baseline crime reporting rates. Columns (5) and (6) examine the post-Floyd change in call-to-shot ratios in census tracts with above- and below-median reporting rates in 2019, respectively. Notably, we find significant declines of over 50% in both cases, suggesting that results are not driven by a ceiling effect on crime reporting.

3.3 Heterogeneity

Neighborhood Racial Composition

Though researchers disagree on whether officers discriminate in the use of force,¹³ Black and Hispanic individuals are far more likely than other groups to experience police violence and to believe that it is an important public issue (AP-NORC, 2015; Edwards *et al.*, 2019). In contrast, rates of civilian-initiated contact with police—such as reporting a crime or approaching the police to seek help for a medical emergency—are highest among white individuals (Davis *et al.*, 2018). Thus, we next explore how the effects of George Floyd’s murder on civilian crime reporting differed across racial groups.

To do so, we geo-code the 911 and gunshot microdata to Census tracts, which we merge with 2015-2019 American Community Survey data to obtain area demographics. We then

¹²In Appendix Figure A.VIII, we also examine trends in average police response times to 911 calls, which we observe for three cities in our sample. We find little change after George Floyd’s murder further suggesting limited changes in policing effort.

¹³For example, see Cesario *et al.* (2019); Knox *et al.* (2020); Hoekstra & Sloan (2020); Lieberman (2020); Ba *et al.* (2021).

calculate call-to-shot rates specific to each tract by, for example, dividing the total number of 911 calls in majority-Black tracts by the total number of gunshots detected in those same areas.

Appendix figure A.IX plots changes in weekly call rates by neighborhood racial composition, normalized to call rates the week prior to Floyd’s killing. While trends in Asian American call rates are noisy due to the scarcity of majority-Asian-American neighborhoods, we find large dips in calls per shot in majority-white, majority-Black and majority-Hispanic areas. Notably, the relative decrease in white call rates is as large, if not larger, than that in Black and Hispanic neighborhoods.

These results stand in contrast to prior research showing that the educational and voting effects of police violence are driven entirely by Black and Hispanic communities (Legewie & Fagan, 2019; Ang, 2021; Ang & Tebes, 2021). Instead, we find that George Floyd’s death significantly reduced crime reporting across a wide range of communities, even those with the highest existing trust and engagement with law enforcement. This is consistent with the trends in police favorability examined earlier as well as broader discussions about the racial reckoning that viral footage of George Floyd’s murder sparked among many white Americans.

City-level variation

Appendix figure A.X disaggregates the trends by city. We focus on the period after the national COVID-19 emergency declaration and plot deviations in call-to-shot ratios, relative to each city’s call ratio during the week before George Floyd’s murder.

We find a pronounced drop in call rates immediately following the incident date in nearly all cities. In Baltimore, Cincinnati, DC, Milwaukee, Minneapolis, New York, Oakland, Richmond and San Francisco, call rates drop nearly 50% in the weeks after George Floyd’s death. The one case where trends are least clear, San Diego, is likely due in part to the city’s small acoustic gunshot detection coverage area, which introduces noise in the denominator (ex: 15% of weeks had 0 or 1 detected shots).

The fact that the effect emerges in cities across the country highlights the broader civic ramifications of Floyd’s killing. While law enforcement agencies are locally-governed institutions, our findings suggest that citizens may view them as part of a larger criminal justice system. As a result, highly visible acts of police violence, like George Floyd’s murder in Minneapolis, may fuel disengagement with local law enforcement agencies even among residents living in geographically and politically disparate areas.

3.4 Trust in Police as a Mechanism

Given the declines in reporting documented in Figure III and the similarly large declines in police favorability documented in Figure I, a natural follow-up question is the extent to which the two patterns are connected. Are declines in reporting after high profile acts of police violence driven by damage done to trust in law enforcement as an institution?

To explore this, we turn to data from the National Crime Victimization Survey (NCVS). NCVS is a large-scale national survey conducted by the Bureau of Justice Statistics. Throughout each year, roughly 160,000 households are interviewed about the frequency and nature of crime victimization incidents experienced in the past six months. Importantly, respondents are asked not only whether they reported a victimization to police but also the reason they did or did not report the incident. As the survey includes information on the month of each incident, this data allows us to explore the potential role of changing perceptions of the police as a mediator for non-reporting following George Floyd’s murder.

[Table II about here.]

In Table II, we examine all NCVS incidents from January 2019 to August 2021 and estimate the change in non-reporting before and after May 2020, accounting for seasonality patterns through calendar month fixed effects. In Panel A, our outcome is an indicator set to 1 for incidents that were unreported due to police mistrust.¹⁴ Column 1 reveals that respon-

¹⁴Specifically, incidents for which the “most important reason” for non-reporting was the belief that “police wouldn’t think it was important enough”, “police would be inefficient, ineffective” or “police would be biased, would harass/insult respondent, cause respondent trouble, etc.”

dents who were victimized after the murder of George Floyd were significantly more likely to decide not to report due to concerns about police, relative to respondents victimized before Floyd’s murder. Point estimates suggest nearly a 20% increase in non-reporting due to mistrust, roughly 1.5 fewer reports per 100 incidents. Notably, estimates are virtually unchanged when including additional controls for victim and incident characteristics (Columns 2 and 3), suggesting that changing selection into the NCVS sample is unlikely to explain our findings.

Meanwhile, in Panel B, we show no effect on crime non-reporting due to the two most commonly-stated reasons in the NCVS (i.e. the belief that the incident was a private or minor concern). Point estimates are near zero across specifications. Like all survey data, the NCVS data may be subject to selective recall and inaccurate reporting. However, the results are consistent with our core call-to-shot ratio findings and suggest that actual crime victims were less likely to report incidents to police due to institutional mistrust following the murder of George Floyd.

4 Discussion

The decline in community engagement engendered by the murder of George Floyd raises a number of important, secondary questions. While it is outside the scope of this paper to provide definitive answers to all of them, they are worth discussing in more detail.

4.1 Will these results generalize to other acts of police violence?

The murder of George Floyd was a highly salient act of police violence that occurred during a global pandemic and hyper-polarized election year. In this context, it is possible that the stark findings documented here are a product of the particular cultural moment that surrounded George Floyd’s murder. Thus, we attempt to explore the external validity of our results in two ways.

First, and most importantly, for two cities in our sample, we are able to construct call-to-shot ratios around the 2014 police killing of Michael Brown, the most nationally salient act of

police violence prior to George Floyd’s murder. Appendix figure A.XI displays these results. As with the George Floyd murder, we document a striking decline in civilian crime reporting coinciding with the date of the incident. This suggests that our results may generalize to other, highly publicized police killings.

Second, we used data from the Mapping Police Violence project and Google Trends to find the five most searched police killings in the year before George Floyd’s murder.¹⁵ Appendix figure A.XII plots the call-to-shot ratio over this time-period with additional vertical lines denoting the dates of these other incidents. Notably, we find no evidence in the time-series that reporting declined in our sample cities.

However, it is important to note that public salience of these events was dwarfed by that of the George Floyd murder and, to a lesser extent, the killing of Michael Brown. Peak search volume for each of the five other incidents never exceeded 1% of the former and 4% of the latter (see Appendix figure A.XIII). Thus, the fact that we find little change in the national call-to-shot ratio could be explained by limited public awareness and does not preclude the existence of more localized effects. Indeed, zooming into incident neighborhoods, Mikdash & Zaiour (2022) find that police shootings in Minneapolis cause declines in shot-reporting on the order of 1-2%.¹⁶

4.2 Did the conviction of Floyd’s murderer repair community engagement?

The final question we explore is whether engagement rebounds after the disciplining of involved officers. We examine this in the context of Derek Chauvin’s conviction for the murder of George Floyd, a rare but highly-visible example of police accountability. Appendix figure A.XIV plots trends in 911 call-to-shot ratios in the weeks before and after the verdict was announced on

¹⁵To be precise, we collected Google Trends search volume over the years 2019-2020 for every name in the Mapping Police Violence database. Then we ranked names from first to last according to which exhibited the largest trough to peak change in search volume over that time period. From there, we excluded incidents where the victim of police violence shared the same name as a celebrity, where the news story that drove the change in search volume was not directly related to the murder itself (e.g. new developments related to an ongoing court case), and where the victim of police violence died while perpetrating a mass shooting.

¹⁶Their analysis was released after the first public working paper of our project.

April 20, 2021.¹⁷ We find little evidence that the decision increased civilian crime reporting.

While many lauded Derek Chauvin’s conviction as a ground-breaking shift in police accountability, these results reinforce the enduring harm that George Floyd’s killing had on public engagement and cooperation with law enforcement. They further suggest that local communities may view issues of police accountability as too endemic to be repaired with a single conviction, no matter how high-profile.

5 Conclusion

Together, our results provide novel insight into the deleterious effects that high-profile acts of police violence may have on civilian crime reporting. These effects are large, persistent and widespread. We find that George Floyd’s murder by Minneapolis police spurred a roughly 50% drop in 911 calls per gunshot, an effect that is mirrored across multiple cities and racial groups and that persisted over time.

In light of these findings, it is natural to wonder the extent to which our findings are applicable to other instances of aggressive policing. While prior research has found corroborating evidence of police violence’s negative impact on community health, these effects tend to be highly-localized. However, existing studies also examine older incidents that occurred before the rise of cell phone cameras, social media platforms, and recent social justice movements. In this light, the far-reaching consequences of George Floyd’s death may well predict the modern aftermath of controversial police killings, though this can only be confirmed with further research. At minimum, our findings suggest that public cooperation with police may have been fundamentally altered by George Floyd’s murder and that any future incidents will be assessed from a new baseline of heightened distrust and skepticism.

¹⁷This includes all sample cities except Washington, DC, for which we only have data through December 31, 2020.

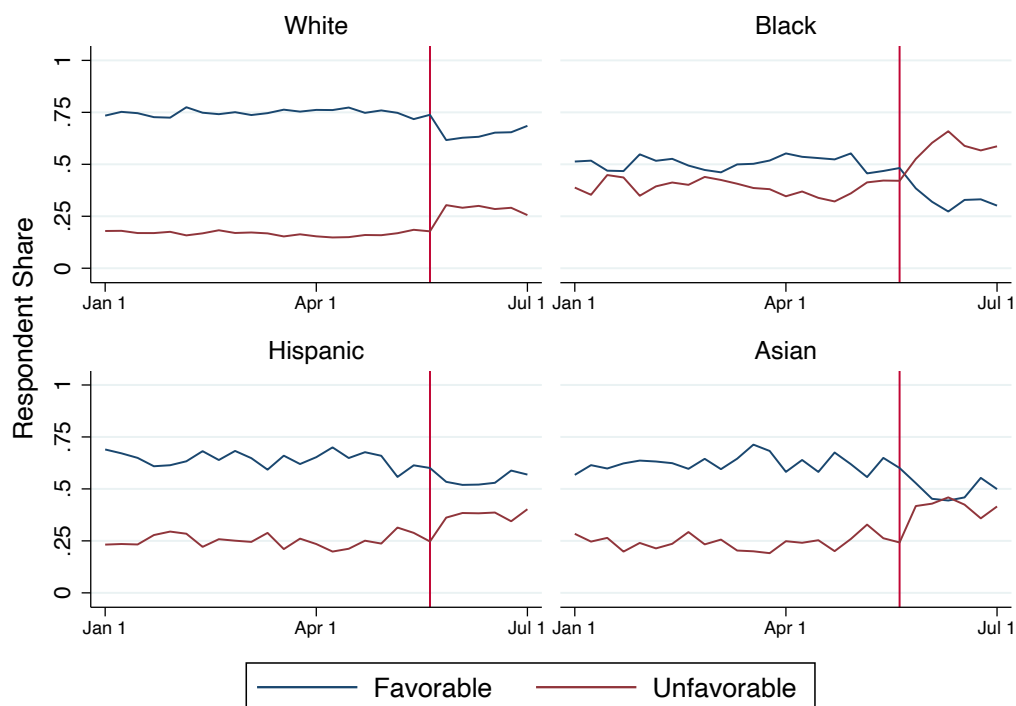
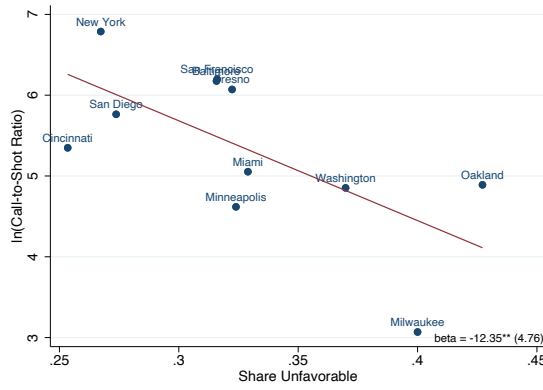
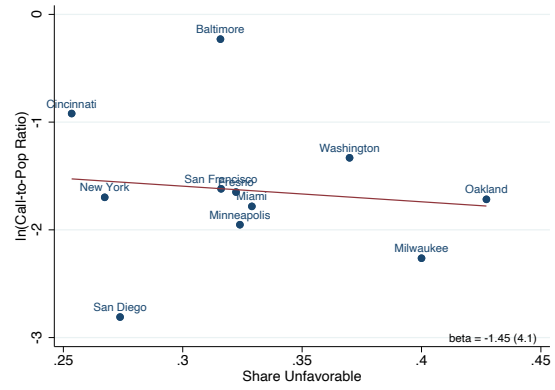


Figure I: Perceptions of Police over Time

Notes: Figure plots weekly police favorability ratings by respondent race. Data come from Nationscape, which include weekly surveys of 6,250 individuals (repeated cross-section). Respondents are asked to rate, on scale of 1 to 5, how favorably they view police. Blue line represents share of respondents with positive views (i.e., 4 or 5 rating). Red line represents share of respondents with negative views (i.e., 1 or 2 rating). Red vertical line represents the week of George Floyd's death.



(a) Calls per Shot



(b) Calls per Resident

Figure II: Predicting police favorability with calls-to-shots

Notes: Panel (a) plots the natural log of the call to shot ratio against average police unfavorability from the Nationascope data for each of the 13 cities in our sample. The slope of the best fit line is -12.35 with standard error of (4.76). Panel (b) is identical to panel (a) except that we replace the call-to-shot ratio with the call-to-population ratio. The slope of this best fit line is -1.45 with a standard error of (4.1).

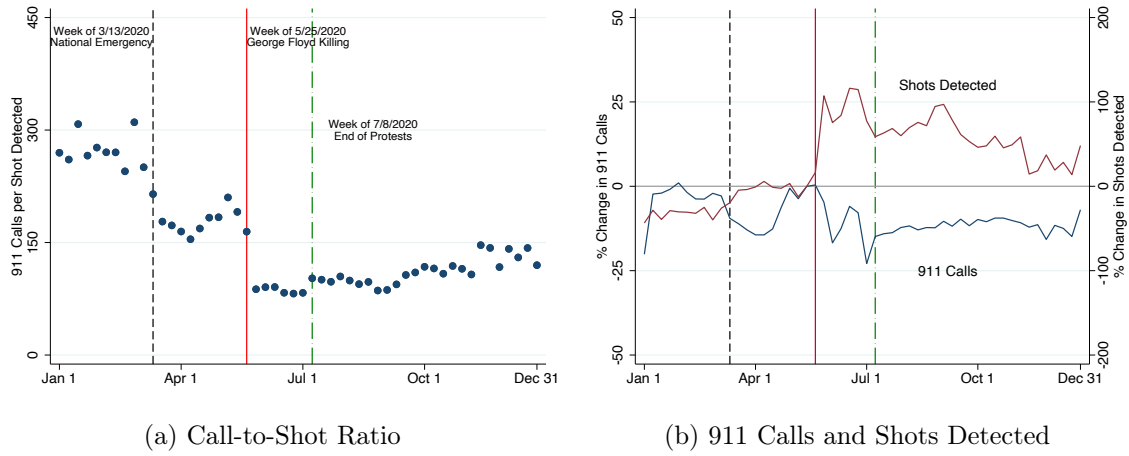


Figure III: Civilian Crime Reporting over Time

Notes: Panel A plots the ratio of 911 calls to acoustically detected shots over time. Calls and shots are aggregated by week across gunshot coverage areas in our thirteen sample cities. Panel B plots the percent change in 911 calls and shots over time, relative to the value of each variable during the week prior to George Floyd's death. Dashed vertical line represents the week the COVID-19 National Emergency was declared. Red vertical line represents the week of George Floyd's death. Dashed green line marks the end of rioting and Black Lives Matter protests which we date using information from the Armed Conflict Location and Event Data (ACLED) project (see appendix figure A.XV).

Table I: Average Effects on Civilian Crime Reporting

Panel A: Alternative Specifications						
	(1)	(2)	(3)	(4)	(5)	(6)
Post Floyd	−94.71*** (8)	−46.38*** (7.77)	−59.48*** (9.93)	−64.62*** (17.56)	−51.93*** (16.84)	−46.67*** (17.9)
$\bar{Y}$	151.44	151.44	151.44	151.44	151.44	151.44
Outcome	Calls/Shots	Calls/Shots	Calls/Shots	Calls/Shots	Calls/Shots	Calls/Shots
Lagged DV	No	Yes	Yes	Yes	Yes	Yes
Twice Lagged DV	No	No	Yes	Yes	Yes	Yes
Time Trend (Days)	No	No	No	Yes	Yes	Yes
Google Mobility	No	No	No	No	Yes	Yes
911 Calls from 2019	No	No	No	No	No	Yes
Panel B: Alternative Samples and Outcomes						
	(1)	(2)	(3)	(4)	(5)	(6)
Post Floyd	−537.55*** (55.28)	−0.23*** (0.07)	−8.65*** (0.92)	−67.35*** (5.3)	−236.21*** (23)	−25.95*** (2.08)
$\bar{Y}$	719.7	1.03	27.71	118.45	351.43	48.55
Outcome	Calls/Casualties	Shots-Fired/Shots	Calls/Shots-and-Alarms	Civilian-Calls/Shots	Calls/Shots	Calls/Shots
Sample	All	All	All	All	Above Median	Below Median

Notes: Table displays results from regressions of the form $Y_t = \beta D_t + \pi X_t + \epsilon_t$ where D_t takes a value of 1 after George Floyd's murder so that β measures the average size of the reporting decline after accounting for controls contained in X_t . The sample contains 147 observations, one for each day spanning the 73 days between the COVID-19 National Emergency on March 13, 2020 and George Floyd's murder on May 25, 2020 plus the 73 afterwards. Panel A present results for models with different sets of controls. Column (1) includes no controls. Column (2) controls for a lagged dependent variable. Column (3) adds a control for a twice lagged dependent variable. Column (4) adds a time trend. Column (5) adds a control for the amount of time spent in places of residence based on Google tracking data. Column (6) adds controls for total 911 call volume as measured on the same date in 2019. Panel B shows results for alternative samples and outcome variables. Column (1) shows results where the denominator is gun violence casualties. Column (2) shows results where the numerator measures calls we can specifically tie to gun shots. Column (3) shows results where the denominator includes shots and automated alarms. Column (4) shows results where the numerator contains only civilian initiated calls. Columns (5) and (6) show results for census tracts that were above or below median reporting rates during the year prior to the murder. The appendix contains figures that show the results in panel B visually. For inference, we account for auto-correlation using Newey-West standard errors (Wooldridge, 2010). However, we find similar levels of significance for the Post-Floyd drop ($p = 0.031$) using a finite sample inferential procedure based on randomly chosen placebo dates. See appendix figure A.II for more detail.

Table II: Average Effects on Victim Non-Reporting by Reason (National Crime Victimization Survey)

	(1)	(2)	(3)
<i>Panel A: Outcome - Non-Report due to Police Mistrust</i>			
Post-Floyd	0.015** (0.005)	0.014** (0.005)	0.015** (0.004)
Mean	0.084	0.084	0.084
<i>Panel B: Outcome - Non-Report due to Private Concern</i>			
Post-Floyd	-0.005 (0.004)	-0.006 (0.004)	-0.005 (0.004)
Mean	0.213	0.213	0.213
Observations	22,757	22,756	22,756
Month Fixed Effects	Yes	Yes	Yes
Victim Characteristics	No	Yes	Yes
Incident Characteristics	No	No	Yes

Notes: Table displays results from regressions of the form $Y_{it} = \beta D_t + \pi X_{it} + \epsilon_{it}$ where D_t takes a value of 1 after George Floyd's murder (from June 2020 onwards) so that β measure the average size of the reporting decline after accounting for controls contained in X_{it} . Sample includes all NCVS crime victimizations from January 2019 to August 2021. Data are at the incident-level and identified to the month. Panel A examines an indicator set to 1 for incidents that were unreported to police because the respondent claimed "police wouldn't think it was important enough", "police would be inefficient," or "police would be biased, would harass/insult respondent, cause respondent trouble, etc." Panel B examines an indicator set to 1 for incidents that were unreported to police because the respondent claimed the incident was a "private or personal matter or took care of it myself or informally" or "minor or unsuccessful crime, small or no loss, recovered property." Column (1) includes calendar month fixed effects. Column (2) includes controls for victim age, gender and race. Column (3) includes controls for crime type and incident location. Standard errors are clustered by month-year and reported in parentheses.

References

- ACLED. *Armed Conflict Location and Event Data*. <https://acleddata.com>. Accessed: 2022-04-01.
- ADDINGTON, LYNN A. 2008. Assessing the extent of nonresponse bias on NIBRS estimates of violent crime. *Journal of Contemporary Criminal Justice*, **24**(1), 32–49.
- ANG, DESMOND. 2021. The effects of police violence on inner-city students. *The Quarterly Journal of Economics*, **136**(1), 115–168.
- ANG, DESMOND, & TEBES, JONATHAN. 2021. Civic responses to police violence. *American Political Science Review*, 1–16.
- AP-NORC. 2015. *Law Enforcement and Violence: the Divide Between Black and White Americans*. Tech. rept.
- ARCHIBONG, BELINDA, & OBIKILI, NONSO. 2020. Prison labor: The price of prisons and the lasting effects of incarceration. *African Economic History Working Paper Series*.
- BA, BOCAR A, & RIVERA, ROMAN G. 2019. The effect of police oversight on crime and allegations of misconduct: Evidence from chicago.
- BA, BOCAR A, KNOX, DEAN, MUMMOLO, JONATHAN, & RIVERA, ROMAN. 2021. The role of officer race and gender in police-civilian interactions in Chicago. *Science*, **371**(6530), 696–702.
- BELL, MONICA C. 2017. Police reform and the dismantling of legal estrangement. *The Yale Law Journal*, 2054–2150.
- BRANTINGHAM, P JEFFREY, MOHLER, GEORGE, & MACDONALD, JOHN. 2022. Changes in public–police cooperation following the murder of George Floyd. *PNAS nexus*, **1**(5), pgac189.
- BRATTON, WILLIAM J. 1997. Cutting crime and restoring order: What America can learn from New York’s finest.
- CARR, JILLIAN, & DOLEAC, JENNIFER L. 2016. The geography, incidence, and underreporting of gun violence: new evidence using ShotSpotter data.

- CARR, JILLIAN B., & DOLEAC, JENNIFER L. 2018. Keep the Kids Inside? Juvenile Curfews and Urban Gun Violence. **100**(4), 609–618. Publisher: MIT Press.
- CESARIO, JOSEPH, JOHNSON, DAVID J, & TERRILL, WILLIAM. 2019. Is there evidence of racial disparity in police use of deadly force? Analyses of officer-involved fatal shootings in 2015–2016. *Social psychological and personality science*, **10**(5), 586–595.
- CHO, SUNGWOO, GONÇALVES, FELIPE, & WEISBURST, EMILY. 2023. *The Impact of Fear on Police Behavior and Public Safety*. Tech. rept. National Bureau of Economic Research.
- COHEN, ELISHA, GUNDERSON, ANNA, JACKSON, KAYLYN, ZACHARY, PAUL, CLARK, TOM S, GLYNN, ADAM N, & OWENS, MICHAEL LEO. 2019. Do officer-involved shootings reduce citizen contact with government? *The Journal of Politics*, **81**(3), 1111–1123.
- CUNNINGHAM, JAMEIN P, & GILLEZEAU, ROB. 2018. Racial differences in police use of force: Evidence from the 1960s civil disturbances. *Pages 217–21 of: AEA Papers and Proceedings*, vol. 108.
- CUNNINGHAM, JAMEIN P, & GILLEZEAU, ROB. 2021. Don't shoot! the impact of historical African American protest on police killings of civilians. *Journal of Quantitative Criminology*, **37**, 1–34.
- DAVIS, ELIZABETH, WHYDE, ANTHONY, & LANGTON, LYNN. 2018. Contacts between police and the public, 2015. *US Department of Justice Office of Justice Programs Bureau of Justice Statistics Special Report*, 1–33.
- DESMOND, MATTHEW, PAPACHRISTOS, ANDREW V, & KIRK, DAVID S. 2016. Police violence and citizen crime reporting in the Black community. *American sociological review*, **81**(5), 857–876.
- DESMOND, MATTHEW, PAPACHRISTOS, ANDREW V, & KIRK, DAVID S. 2020. Evidence of the effect of police violence on citizen crime reporting. *American sociological review*, **85**(1), 184–190.
- DEVI, TANAYA, & FRYER JR, ROLAND G. 2020. *Policing the Police: The Impact of "Pattern-or-Practice" Investigations on Crime*. Tech. rept. National Bureau of Economic Research.
- EDWARDS, FRANK, LEE, HEDWIG, & ESPOSITO, MICHAEL. 2019. Risk of being killed by police use of force in the United States by age, race–ethnicity, and sex. *Proceedings of the National Academy of Sciences*, **116**(34), 16793–16798.

- FACCHETTI, ELISA. 2021. Police Infrastructure, Police Performance, and Crime: Evidence from Austerity Cuts. *Job Market Paper*.
- GOODE, ERICA. 2012. Shots fired, pinpointed and argued over.
- HOEKSTRA, MARK, & SLOAN, CARLY WILL. 2020. *Does race matter for police use of force? Evidence from 911 calls*. Tech. rept. National Bureau of Economic Research.
- IRVIN-ERICKSON, YASEMIN, LA VIGNE, NANCY, LEVINE, NED, TIRY, EMILY, & BIELER, SAMUEL. 2017. What does Gunshot Detection Technology tell us about gun violence? **86**, 262–273.
- JACOME, ELISA. 2021. The Effect of Immigration Enforcement on Crime Reporting: Evidence from Dallas. *Journal of Urban Economics*.
- KIRK, DAVID S, PAPACHRISTOS, ANDREW V, FAGAN, JEFFREY, & TYLER, TOM R. 2012. The paradox of law enforcement in immigrant communities: Does tough immigration enforcement undermine public safety? *The Annals of the American Academy of Political and Social Science*, **641**(1), 79–98.
- KNOX, DEAN, LOWE, WILL, & MUMMOLO, JONATHAN. 2020. Administrative records mask racially biased policing. *American Political Science Review*, **114**(3), 619–637.
- LEGEWIE, JOSCHA, & FAGAN, JEFFREY. 2019. Aggressive policing and the educational performance of minority youth. *American Sociological Review*, **84**(2), 220–247.
- LERMAN, AMY E, & WEAVER, VESLA. 2014. Staying out of sight? Concentrated policing and local political action. *The ANNALS of the American Academy of Political and Social Science*, **651**(1), 202–219.
- LIEBERMAN, CARL. 2020. Variation in Racial Disparities in Police Use of Force. *Industrial Relations Section Working Paper 639*.
- MAZEROLLE, LORRAINE G., FRANK, JAMES, ROGAN, DENNIS, & WATKINS, CORY. 2000. A field evaluation of the ShotSpotter gunshot location system: Final report on the Redwood City field trial.
- MIKDASH, MAYA, & ZAIOUR, REEM. 2022. The impact of police shootings on gun violence and civilian cooperation. *Unpublished manuscript*.

- OWENS, EMILY. 2019. Economic Approach to "De-Policing". *Criminology and Public Policy*, **18**(1), 77–80.
- PRENDERGAST, CANICE. 2001. *Selection and oversight in the public sector, with the Los Angeles Police Department as an example*.
- PYROOZ, DAVID C, DECKER, SCOTT H, WOLFE, SCOTT E, & SHJARBACK, JOHN A. 2016. Was there a Ferguson Effect on crime rates in large US cities? *Journal of criminal justice*, **46**, 1–8.
- SAMPSON, ROBERT J. 2012. *Great american city*. University of Chicago Press.
- SHI, LAN. 2008. Does oversight reduce policing? Evidence from the Cincinnati police department after the April 2001 riot. *Journal of Public Economics*.
- STANLEY, JAY. 2021. *Four Problems with the ShotSpotter Gunshot Detection System*.
- STINSON, PHILIP MATTHEW. 2019. The Henry A. Wallace Police Crime Database. *Bowling Green State University*, 2005–2007.
- TIFFANY, KAITLYN. 2020. The Boom in Fireworks Conspiracy Theories. *The Atlantic*.
- WASHINGTON POST. 2021. Police Shootings Database.
- WATKINS, CORY, MAZEROLLE, LORRAINE, ROGAN, DENNIS P., & FRANK, JAMES. 2002. Technological approaches to controlling random gunfire: Results of a gunshot detection system field test.
- WEITZER, RONALD. 2002. Incidents of police misconduct and public opinion. *Journal of criminal justice*, **30**(5), 397–408.
- WOLFE, SCOTT E, & NIX, JUSTIN. 2016. The alleged “Ferguson Effect” and police willingness to engage in community partnership. *Law and human behavior*, **40**(1), 1.
- WOOLDRIDGE, JEFFREY M. 2010. *Econometric analysis of cross section and panel data*. MIT press.
- ZOOROB, MICHAEL. 2020. Do police brutality stories reduce 911 calls? Reassessing an important criminological finding. *American sociological review*, **85**(1), 176–183.

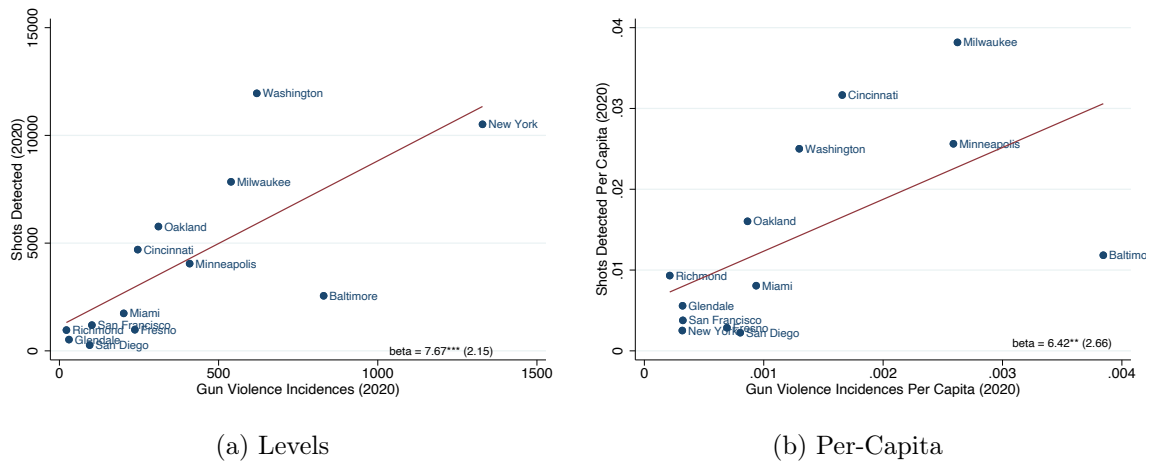


Figure A.I: Correlation Between Acoustically Detected Gunshots and Gun Violence Casualties

Notes: This figure plots the cross-city relationship between the number of acoustically detected gunshots and reported gun violence casualties. Panel (a) presents the relationship in levels. Panel (b) presents the relationship adjusting for population.

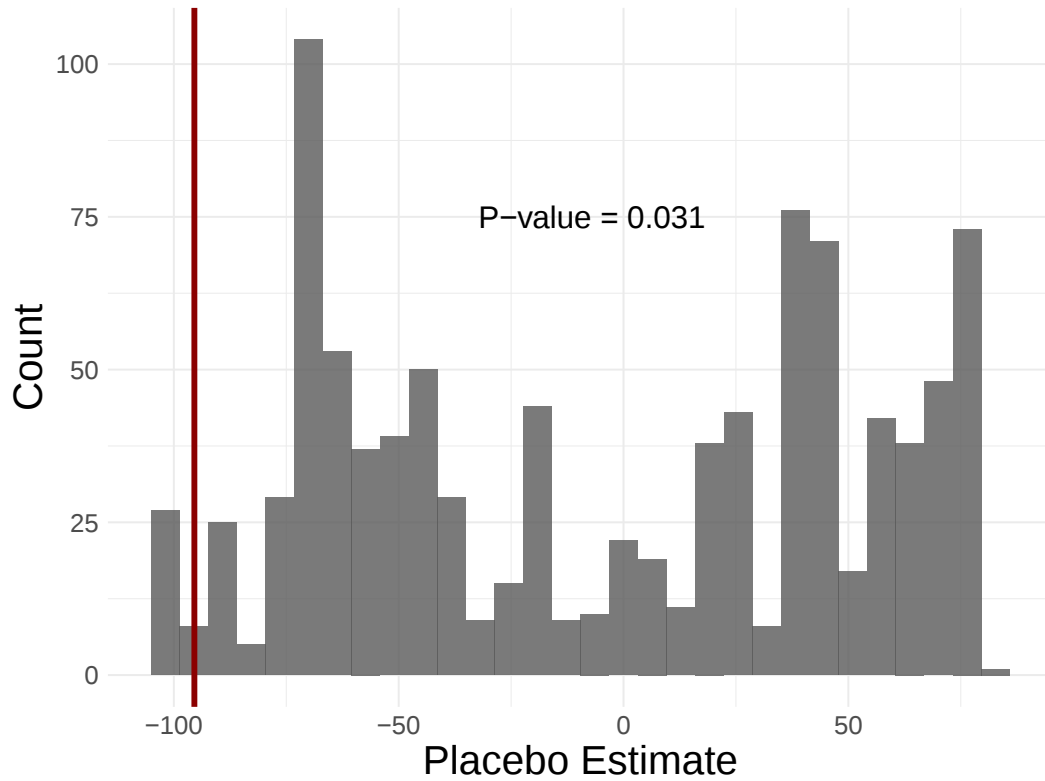


Figure A.II: Permutation inference

Notes: This figure plots the distribution of placebo estimates from randomly chosen treatment dates prior the murder of George Floyd. More precisely, we sample 1,000 randomly chosen treatment dates from the year long time period prior to May 23, 2020, such that we can build a comparable pre-post time window, and then estimate our baseline model from column (1) of table I using this sample. The vertical red line plots the actual estimate from the true treatment date. We find that only 3.1% of dates in the pre-period exhibit drops as large or larger than the drop that occurred in the aftermath of the George Floyd murder, which suggests that our results are unlikely to be a product of sampling variation.

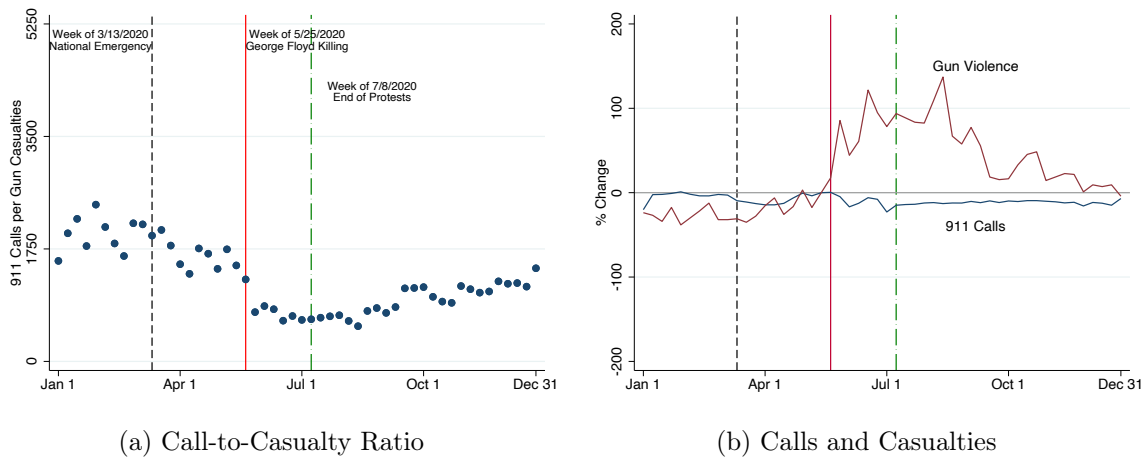


Figure A.III: Civilian Crime Reporting over Time (911 Calls to Gun Violence Casualties)

Notes: Panel A plots the ratio of 911 calls to gun violence casualties (i.e. deaths and injuries) over time. Data on gun violence casualties come from the Gun Violence Archive (gunviolencearchive.org). 911 calls and gun casualties are aggregated by week across acoustic gunshot detection coverage areas in all 13 sample cities. Panel B plots the percent change in 911 calls and gun violence casualties over time, relative to the value of each variable during the week prior to George Floyd's death. Dashed vertical line represents the week the COVID-19 National Emergency was declared. Red vertical line represents the week of George Floyd's death. Dashed green line marks the end of rioting and Black Lives Matter protests which we date using information from the Armed Conflict Location and Event Data (ACLED) project (see appendix figure A.XV)

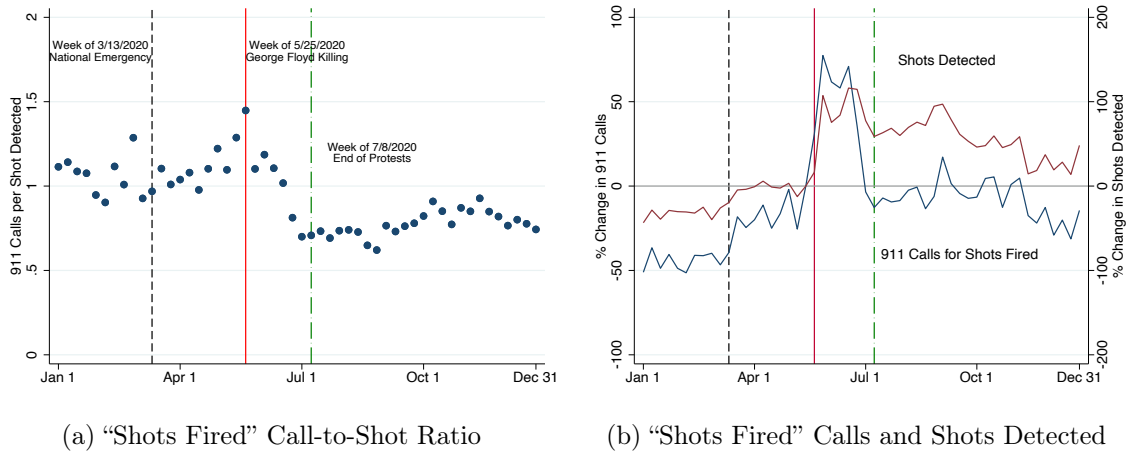


Figure A.IV: Civilian Crime Reporting over Time: "Shots Fired" Ratio

Notes: This figure is identical to figure III from the main text except that we replace the numerator with 911 calls that we could specifically identify as relating to gunshots using the associated description string. Panel A plots the "Shots Fired" 911 call-to-shot over time. Panel B plots the percent change in "Shots Fired" 911 calls and shots over time, relative to the value of each variable during the week prior to George Floyd's death. Calls and shots are aggregated by week across gunshot coverage areas in our thirteen sample cities. Dashed vertical line represents the week the COVID-19 National Emergency was declared. Red vertical line represents the week of George Floyd's death. Dashed green line marks the end of rioting and Black Lives Matter protests which we date using information from the Armed Conflict Location and Event Data (ACLED) project (see appendix figure A.XV).

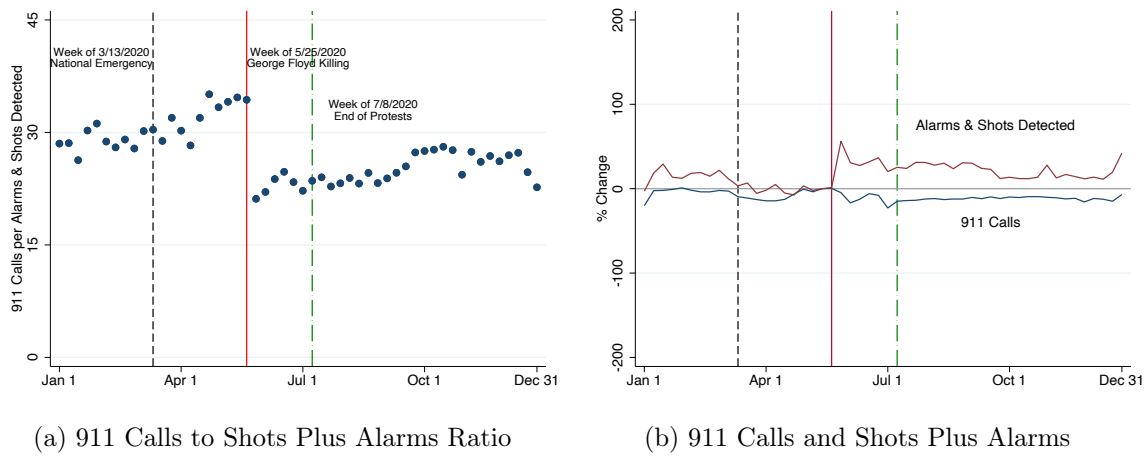


Figure A.V: Civilian Crime Reporting over Time (911 Calls to Shots Plus Alarms)

Notes: Panel A plots the ratio of 911 calls to the sum of acoustically detected gunshots and automated alarms over time. 911 calls and shots detected plus automated alarms are aggregated by week across acoustic gunshot detection coverage areas in all thirteen sample cities. Panel B plots the percent change in 911 calls and the sum of acoustically detected gunshots and automated alarms over time, relative to the value of each variable during the week prior to George Floyd's death. Dashed vertical line represents the week the COVID-19 National Emergency was declared. Red vertical line represents the week of George Floyd's death. Dashed green line marks the end of rioting and Black Lives Matter protests which we date using information from the Armed Conflict Location and Event Data (ACLED) project (see appendix figure A.XV).

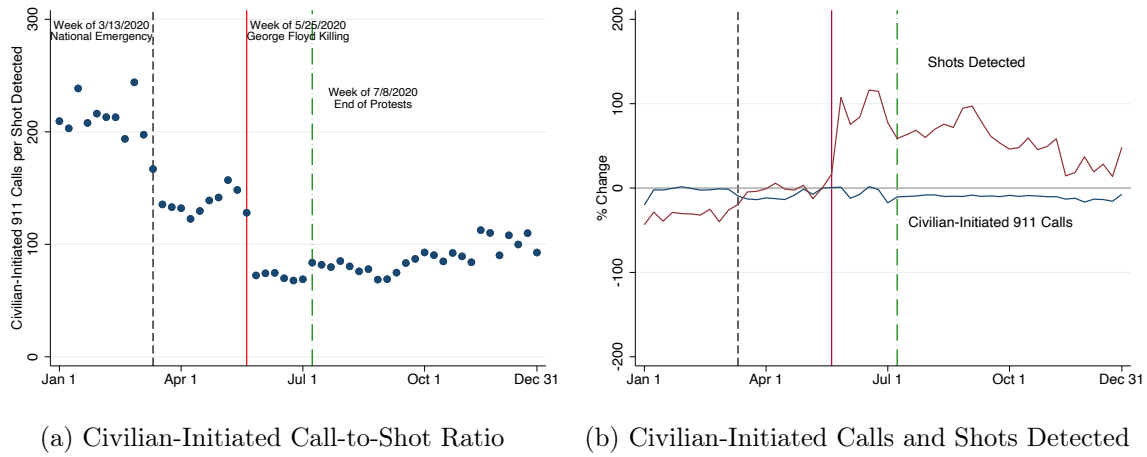


Figure A.VI: Civilian Crime Reporting over Time (Civilian-Initiated Calls to Shots Detected)

Notes: Panel A plots the ratio of civilian-initiated 911 calls to acoustically detected gunshots over time. To identify civilian-initiated 911 call volumes, we exclude calls with descriptions related to traffic stops and patrols, which may instead result from proactive policing encounters. Civilian-initiated 911 calls and shots detected are aggregated by week across acoustic gunshot detection coverage areas in all thirteen sample cities. Panel B plots the percent change in civilian-initiated 911 calls and acoustically detected gunshots over time, relative to the value of each variable during the week prior to George Floyd's death. Dashed vertical line represents the week the COVID-19 National Emergency was declared. Red vertical line represents the week of George Floyd's death. Dashed green line marks the end of rioting and Black Lives Matter protests which we date using information from the Armed Conflict Location and Event Data (ACLED) project (see appendix figure A.XV).

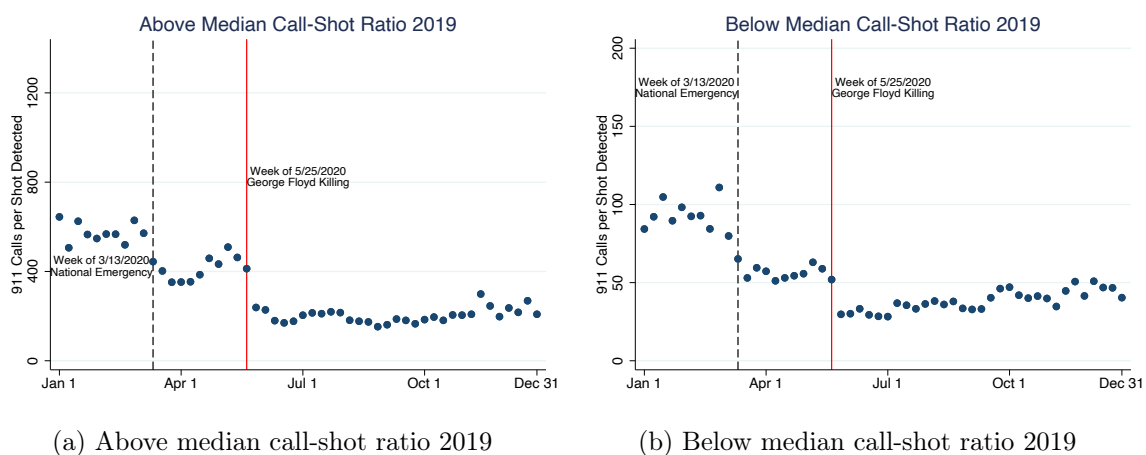
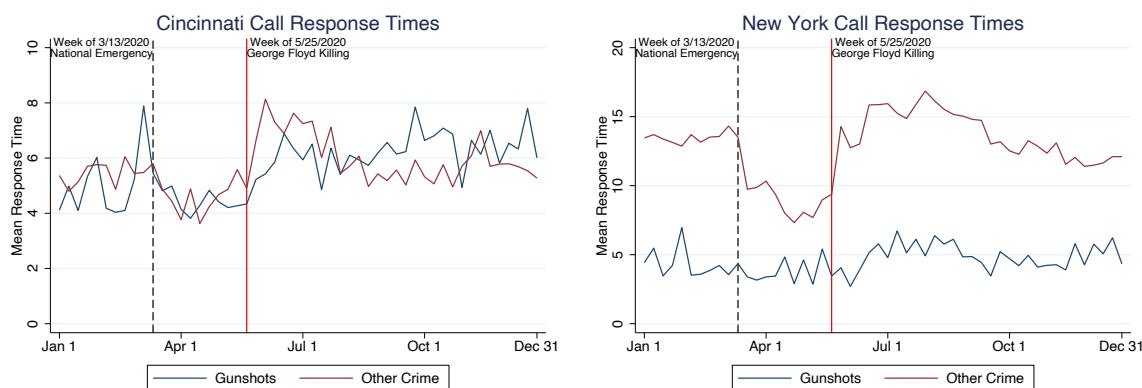


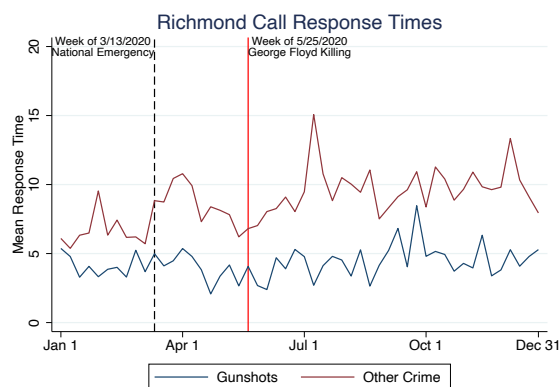
Figure A.VII: Call-to-shot Declines by Baseline Reporting

Notes: Both panels of this figure are identical to panel (c) of figure III from the main text except that we restrict to the sub-sample of census tracts which are above or below the median in the call-to-shot ratio at baseline.



(a) Cincinnati

(b) New York City



(c) Richmond

Figure A.VIII: Police Response Times

Notes: Figure plots the average police response at the weekly level during 2021 for Gunshots and all other crime for three cities in our data. Police response times are found in the 911 call logs for these three cities, but were not available in the public 911 call logs for the remaining cities in our data.

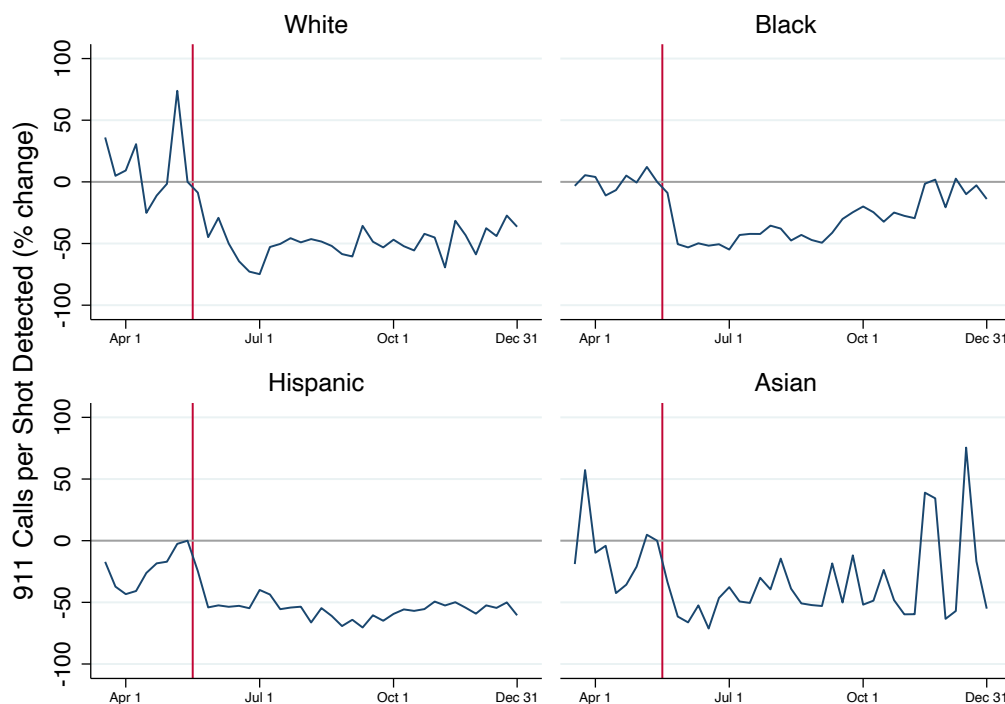


Figure A.IX: Civilian Crime Reporting by Neighborhood Race

Notes: Figure plots the percent change in the ratio of 911 calls to acoustically detected gunshots over time by neighborhood racial composition. For example, the top-left figure aggregates 911 calls and gunshots across all majority-white neighborhoods in the sample (i.e., Census tracts with >50% white residents in 2015-2019 ACS). For each neighborhood type, percent change is calculated relative to the call-to-shot ratio during the week prior to George Floyd's death. Due to noise from the small number of majority-Asian tracts in our sample, we censor percent changes in call to shot ratio for those neighborhoods at 100% in order to display all subfigures on the same scale. Sample is limited to weeks after the COVID-19 National Emergency was declared. Red vertical line represents the week of George Floyd's death.

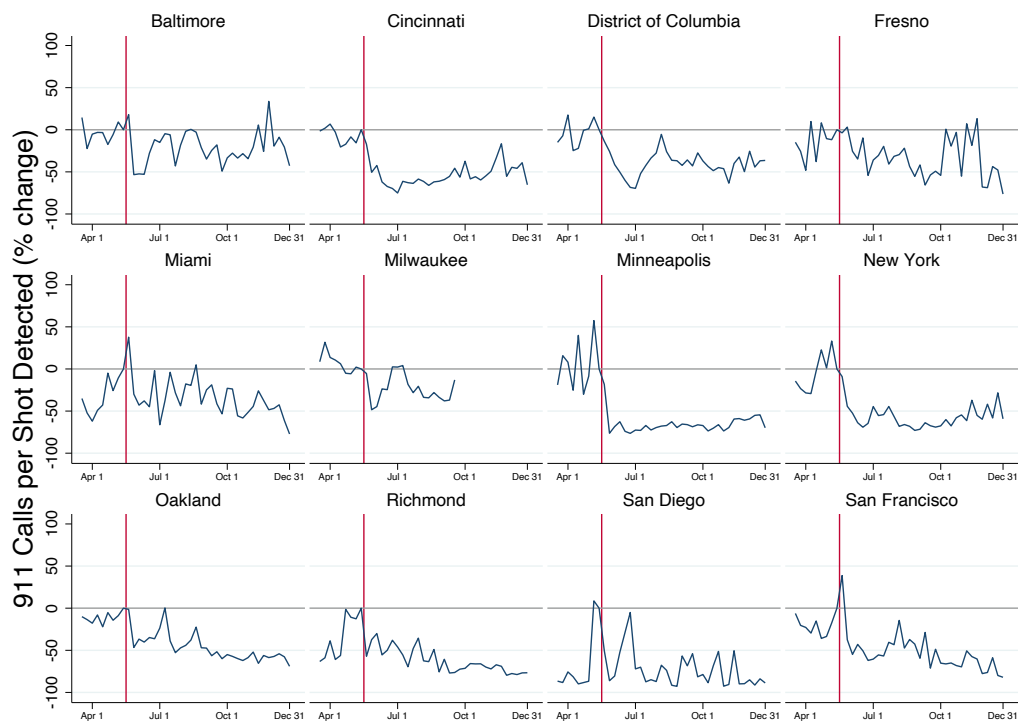


Figure A.X: Civilian Crime Reporting by City

Notes: Figure plots the percent change in the ratio of 911 calls to acoustically detected gunshots over time by city. For each city, percent change is calculated relative to the call-to-shot ratio during the week prior to George Floyd's death. Sample is limited to weeks after the COVID-19 National Emergency was declared. Red vertical line represents the week of George Floyd's death. We omit Glendale, AZ, from this figure so the panels can be arranged symmetrically; however, the pattern is similar in this city, and the figure is available upon request.

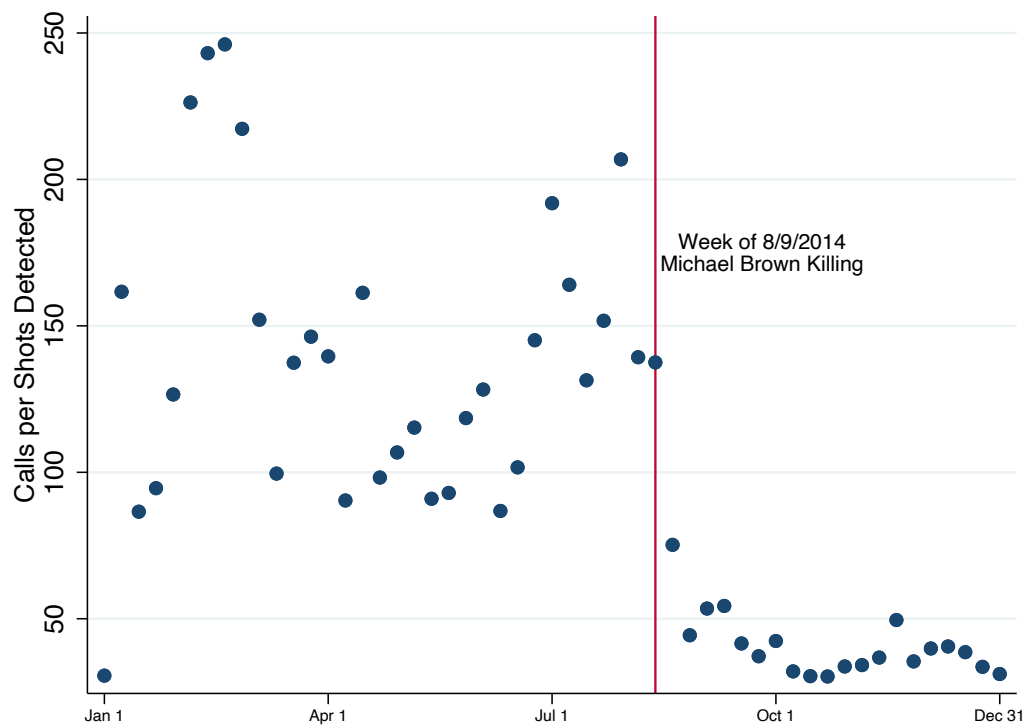


Figure A.XI: Civilian Reporting after the Murder of Michael Brown

Notes: This figure plots the ratio of 911 calls to acoustically detected gun shots for the year 2014. Calls and shots are aggregated by week across gunshot coverage areas in two cities: Milwaukee and Minneapolis. Red vertical line represents the week of Michael Brown's death.

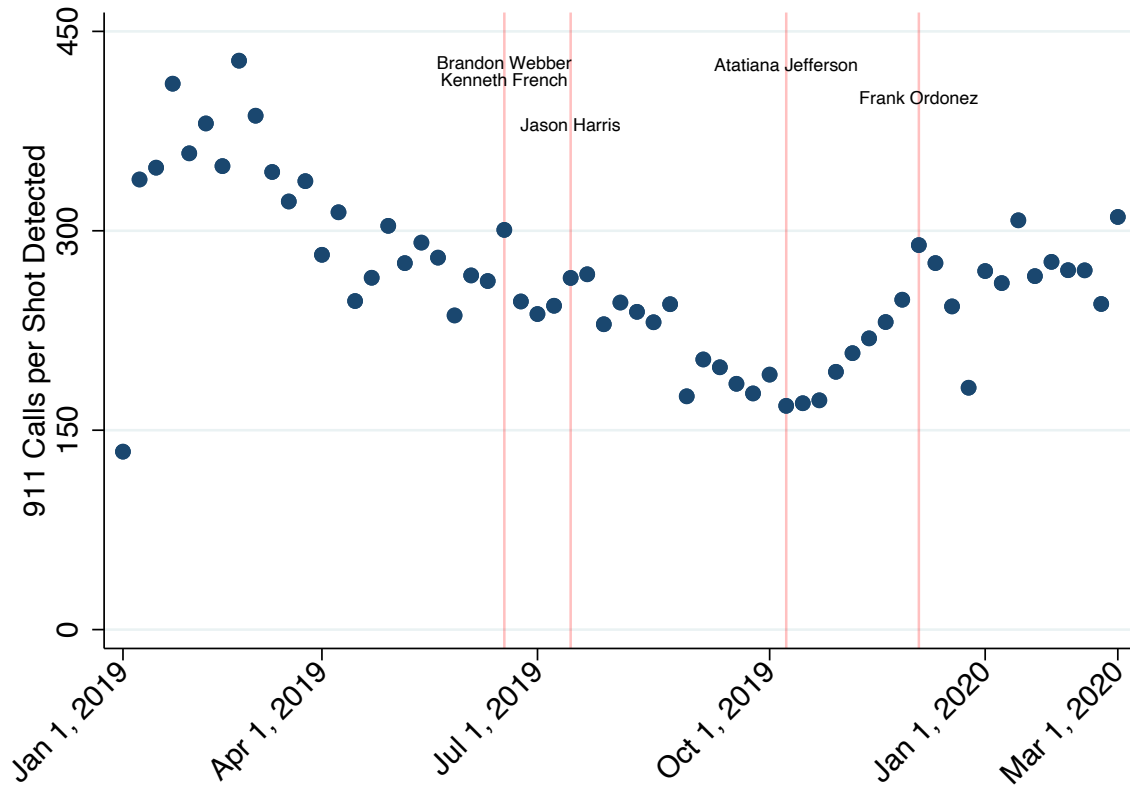
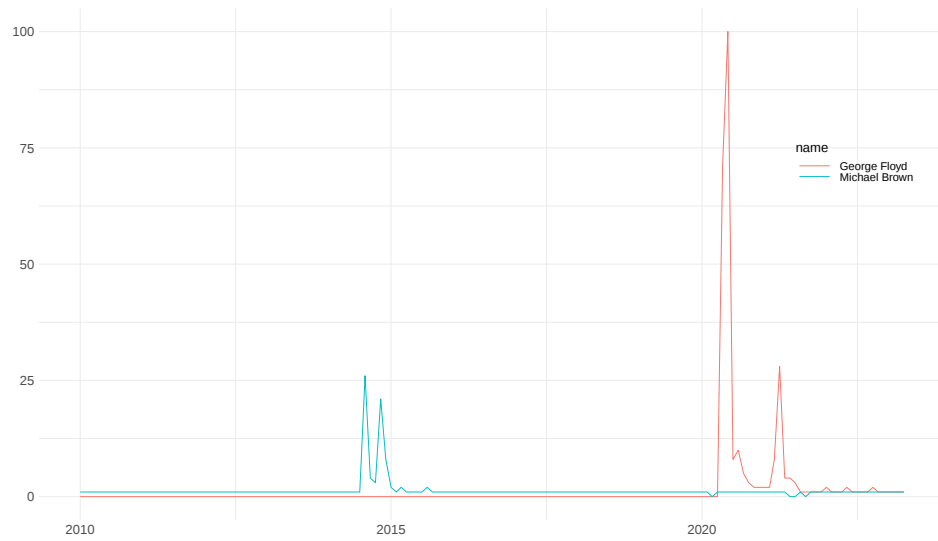
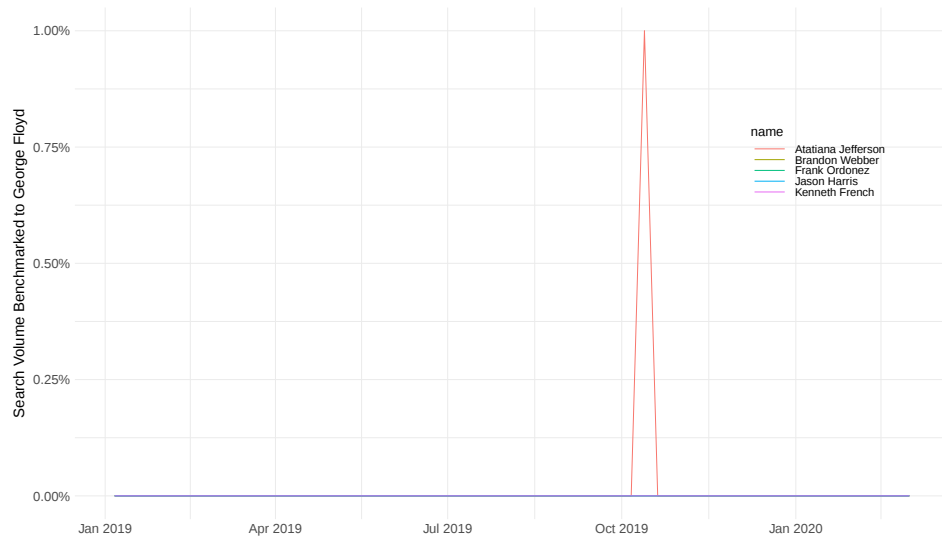


Figure A.XII: Other salient acts of police violence

Notes: Figure plots the ratio of 911 calls to acoustically detected shots over time for January 1, 2019, to March 1, 2020. 911 calls and shots detected are aggregated by week across acoustic gunshot detection coverage areas. Red vertical lines denote the five most salient police killings (as measured by Google Trends) that were contained in the Mapping Police Violence database and that occurred prior to George Floyd's murder.



(a) Michael Brown murder



(b) Most salient murders in 2019

Figure A.XIII: Other highly salient acts of police violence

Notes: Figure presents Google Trends data for other nationally salient police killings. Panel (a) displays search volume for the Michael Brown and George Floyd murders. Panel (b) displays the 5 most nationally salient, non-George Floyd related police killings in 2019 as found in the Mapping Police Violence data. In both panels, search volume is benchmarked to peak search volume for “George Floyd” in 2020. Note that in order to see any variation in panel (b), we had to re-scale the axis to run from 0-1%, whereas the top panel runs from 0-100%.

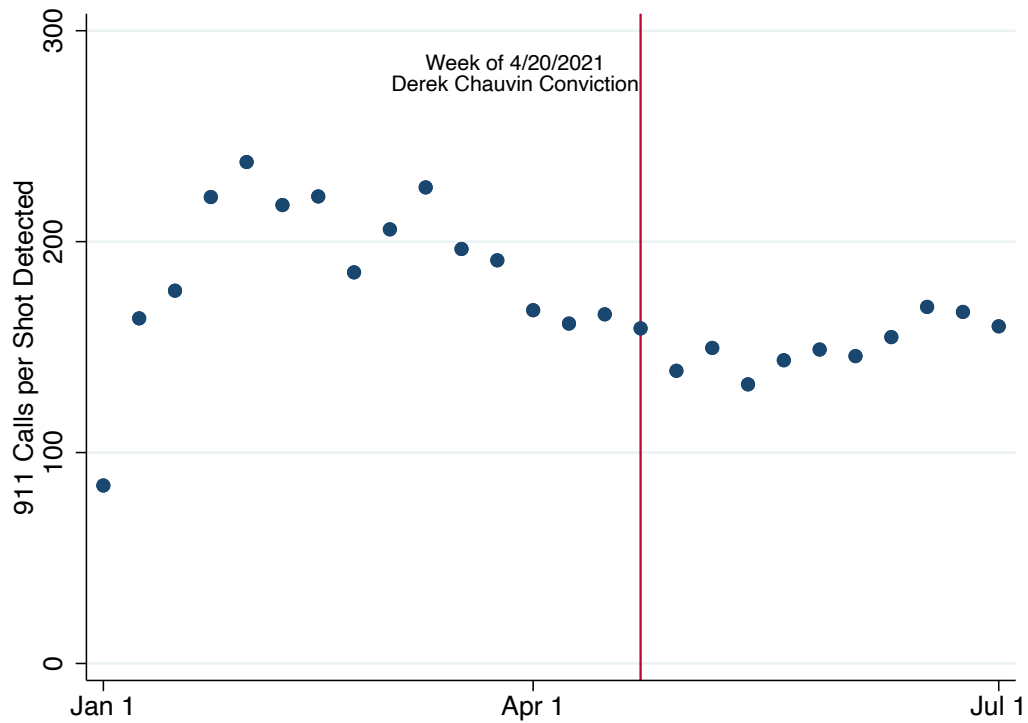
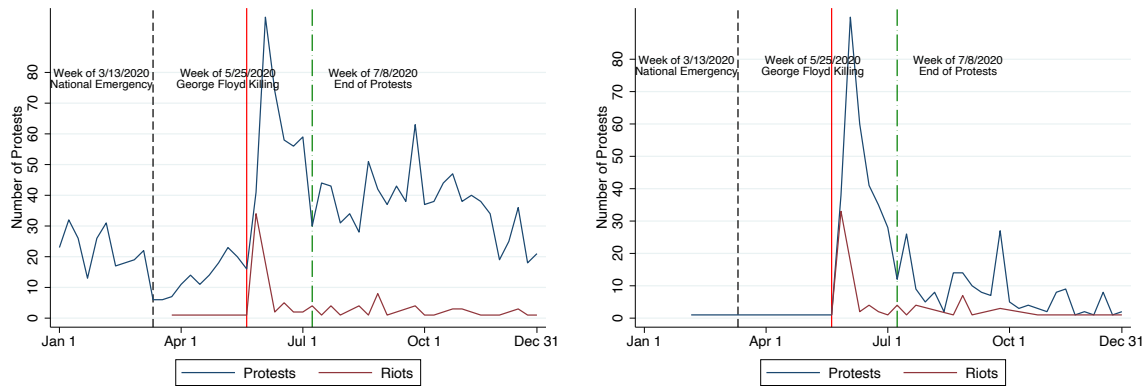


Figure A.XIV: Civilian Crime Reporting after Chauvin Conviction

Notes: Figure plots the ratio of 911 calls to acoustically detected gunshots over time for January 1 to July 1, 2021. 911 calls and shots detected are aggregated by week across acoustic gunshot detection coverage areas in all sample cities except Washington, DC (for which we only have acoustic gunshot detection data until December 31, 2020). Red vertical line represents the week of Derek Chauvin's conviction.



(a) All Riots/Protests

(b) Riots/Protests about George Floyd or BLM

Figure A.XV: Riots and Protests in the Aftermath of the Murder

Notes: Panel (a) plots the number of protests and riots that occurred in our sample cities over the same time period considered in figure III. Panel (b) is identical to panel (a) except restricted to riots and protests that were specifically in response to George Floyd's murder. Riots and protests are measured using data from the Armed Conflict Location and Event Data (ACLED) project. According to the ACLED website, these data are collected from select "local, national and international sources, including media, vetted social media accounts, government and NGO reports, and partner organizations," (ACLED, n.d.).