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COMMUNITY ENGAGEMENT WITH LAW ENFORCEMENT AFTER HIGH-PROFILE ACTS OF POLICE VIOLENCE

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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. National survey data also indicate that victims of crime became less likely to report their victimization to law enforcement due to mistrust of police. 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.

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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. For example, recent studies examine 911-call volume after the 2005 police beating of Frank Jude in Milwaukee reach conflicting conclusions, with two claiming decreased call volume from Black neighborhoods (Desmond *et al.*, 2016, 2020) and the other finding no effect (Zoorob, 2020). Similarly, Brantingham *et al.* (2022) find that 911-call volume in Los Angeles declined after the murder of George Floyd, whereas in New York City they find that 911 call volume increased.[1]

[1] In related work, Lerman & Weaver (2014) find that 311 call volume declines after police use of force incidents in New York City, yet Cohen *et al.* (2019) find no impact of police use of force on 311 or 911 reporting in Los Angeles. Meanwhile, Jácome (2022) finds that prioritizing violent offenders for immigration enforcement increased crime reporting among Hispanic communities in Dallas.

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 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 many cities across the nation have installed 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; Mikdash & Zaiour, 2022).[2] We add to this emerging body of work exploring reporting issues with acoustically detected gunshot data by introducing the call-to-shot ratio as a measure of community trust and attitudes towards the police. 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

[2] Carr & 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. Mikdash & Zaiour (2022) examine how police killings affect detected gunshots and, separately, 911 calls for gunfire.

the aftermath of one of the most high-profile acts of police violence in American history. Examining data for thirteen major cities, we show a sharp drop in the ratio of 911 calls to acoustically detected gunshots immediately after the police murder of George Floyd in May 2020. While gunfire spiked following the killing and remained persistently high through the end of 2020, total calls-for-service volume declined. The net result is a greater than 50% reduction in crime reporting. This dramatic reduction in the call-to-shot ratio is consistent with contemporaneous findings by Mikdash & Zaiour (2022), who find local increases in gunfire but no change in 911 call volume following dozens of lower-profile Minneapolis police killings from 2009 to 2019.

Our main results 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. 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 and persistent decline. 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 other measures of local crime—such as gun violence casualties and automated alarms—and when subsetting to 911 calls related to gunfire. Finally, we demonstrate robustness to controls for seasonality in reporting, 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.

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

The murder of George Floyd was just one in 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 their effects 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 include a nationally representative sample of 6,250 weekly interviews from January to December 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.

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. When a bullet exits a gun, gases cause an acoustic blast (Maher, 2006). In turn, the technology relies on a dispersed set of permanently-mounted sensors located on buildings across a city to capture such sounds. Then, when a shot is fired, the sensors determine the origin location of the shot by multilateration—a process that uses the differing times of arrival of the signal to different monitors to locate the original source of the sound (Loeffler & Flaxman, 2018; Maher, 2006)—and send a notification to the local police department with the predicted location of the shot. It is a common tool in a recent movement towards technology-supported policing and has been implemented in approximately 100 cities nationwide (Electronic Frontier Foundation, n.d.).

Acoustic gunshot detection technology is costly, with one square mile of the technology costing cities $65-90,000 per year (Blackburn & Mares, 2019). Additionally, Blackburn & Mares (2019) argue that, because it increases the number of alerts officers respond to, it comes with the further cost of approximately $25,000 per square mile per year in policing time. For the most part, cities have been increasing their acoustic gunshot detection technology coverage over time, suggesting that an increasing number of urban residents live in neighborhoods with gunshot detection today.

Acoustic gunshot detection technology is not without controversy. Critics have raised important concerns about detection quality, fairness in deployment, and the use of the evidence it generates. While evaluations find relatively low rates of false negatives, with the technology able to detect over 80% 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), it is also prone to false positives, mis-classifying car backfires, helicopters, and fireworks as gunshots (Carr & Doleac, 2016). As acoustic sensors are overwhelmingly deployed in minority neighborhoods, civil rights organizations have raised concerns that the technology may serve as "a circular statistical justification for over-policing in communities of color" (Stanley, 2021). Lastly, critics have questioned whether the close relationships that exist between acoustic gunshot detection technology companies and law enforcement departments may compromise the integrity of the data and its admissibility as evidence in court cases.

Despite these concerns, 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 (Rivera & Ba, 2023), the fixed locations of acoustic sensors suggests that any measurement error in the gunshot detection data is unlikely to be correlated with the timing of George Floyd's murder. 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).[3]

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

[3]Data 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.1). The fact that we find similar patterns using either measure is thus reassuring.

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.

Given the inclusion of several major cities in our sample, the average population of sample cities exceeds that of the average large city in America (i.e. those with 100,000 or more residents). Sample cities have higher minority population shares than the national average—with roughly a quarter of residents identifying as Black, another quarter as non-white Hispanic, and one tenth as Asian. While median household income is similar to that of other large cities, violent and property crime rates are about 25% higher. Within our sample cities, gunshot detection technology is disproportionately deployed in poor and minority neighborhoods. Relative to sample city tracts overall, median income is about 15% lower in tracts with acoustic gunshot sensors, while Hispanic and Black population shares are 10% and 50% higher, respectively. Detailed summary statistics describing sample cities and covered tracts within those cities can be found in Appendix Table A.1.

Our final sample is restricted to 911 calls in sample cities initiated from neighborhoods with gunshot detection sensors.[4] 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. The resulting dataset 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.

[4]Specifically, 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.[5] 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 residents.[6]

[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.

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

[5]City 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.

[6]Similar 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 0.10 standard deviation reduction in local call-to-shot ratio (p-value $<$ 0.001) but little change in local call volume (β= -0.01, p-value = 0.240).

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 remain 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.

To quantify the magnitude of the drop empirically, we estimate the following time series model:

$$Y_t = \alpha + \beta D_t + \pi X_t + \epsilon_t \tag{1}$$

Where Y_t is the call-to-shot ratio across all sample cities on day t; D_t is a binary variable that takes a value of 1 after George Floyd's murder; and X_t is a vector of controls. Data are at the daily-level, and 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.

Our parameter of interest is β, which measures the magnitude of the drop conditional on trends captured by the included controls (X_t). Interpreting the drop in reporting measured by β as causal requires the following exogeneity assumption:

$$E(D_t \epsilon_t) = 0. \tag{2}$$

This condition requires that the timing of George Floyd's murder be uncorrelated with trends in the outcome not otherwise included in X_t. For inference, our model accounts for autocorrelation in the residuals 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 B for more detail.

The results of this analysis are contained in Panel A of table I.

[Table I about here.]

Column 1 presents results from our preferred model containing no controls. The remaining specifications contained in columns 2-6 of Panel A are meant to probe the robustness of this result to potential confounders. In particular, Column (2) adds a linear trend to the model to assess whether our results are confounded by pre-existing trends in the outcome. To account for potentially more complicated, non-linear trends in crime reporting that may have arisen during the pandemic as a result of changes in community mobility, Column (3) includes controls for the amount of time spent in residences based on Google tracking data. To account for seasonal patterns in crime reporting, Column (4) controls for 911 call volume from the same date in 2019. To account for seasonal patterns in violence, Column (5) controls for gunfire on the same date in 2019. Column (6) replaces the controls for reporting and gunfire with the call-to-shot ratio as measured on the same date in 2019, which accounts for seasonality while more closely mirroring the functional form of the dependent variable. In all cases, we find a large and significant decrease in civilian crime reporting of greater than 50% after the police murder of George Floyd. In fact, across specifications, the smallest decline implied by the upper bound of a 95% confidence interval is still in excess of 25%.

We note that the decline found here is an order of magnitude larger than the average effects identified by Mikdash & Zaiour (2022), who examine lower-profile Minneapolis police killings by using a difference-in-differences design to compare changes in incident blocks to those in surrounding areas. The authors find that police violence increases detected gunfire in incident blocks by 3-6%, while call volumes remain unchanged. By contrast, the 50% reduction in crime reporting we find is identified off of the time series break in reporting after the police killing of George Floyd.[7] What drives these differences in effect size? One potential explanation is the mediating role of salience. George Floyd's murder was the most widely-viewed act of police violence in recent history, and its impact on crime reporting may have been heightened by extensive media attention and high levels of national awareness. Another potential explanation is the possibility of spillover effects into control neighborhoods, which could lead to underestimation of treatment effects in a difference-in-differences design. In either case, we view the two studies as complementary: reinforcing the broad conclusion that police violence reduces civilian reporting, while pointing to salience and media coverage as natural areas for future work.

We also show in Appendix C that the decline in calls per shot are comparably sized across neighborhoods that are majority-white, majority-Black and majority-Hispanic; results that 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). We also find a pronounced drop in call rates immediately following the incident in nearly all individual cities in our analysis (see Appendix D).

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 from our preferred model are shown in Panel B of Table I, while corresponding

[7] Another important difference is that Mikdash & Zaiour (2022) focus on 911 calls related to gunfire, whereas our preferred measures examines all calls. However, as we demonstrate in the next subsection, even restricting to calls for "shots fired", we find declines of 22%.

figures showing the raw time trends are included in Appendix E.[8]

Fireworks, "shots 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, 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, we continue to find a significant decrease in civilian crime reporting when comparing "shots fired" calls to detected gunshots, as shown in Column (2).

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 serious 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

[8] More precisely, Figures E.1, E.2, E.3, and E.4 show visual representations of the samples/outcomes explored in columns (1) through (4) of Panel B of Table I, while Figure E.5 shows visual representations of the samples explored in columns (5) and (6) of Panel B.

benchmark for 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; Rivera & Ba, 2023).[9] 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 traffic and pedestrian stops. Focusing only on civilian-initiated 911 calls, we continue to find a significant decrease in crime reporting of over 50%. In Appendix Figure E.6, 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.

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.

[9] Relatedly, Cho *et al.* (2023) find police officer deaths lead to reductions in arrest activity but no change in crime.

4 Trust in Police as a Mechanism: Evidence from the National Crime Victimization Survey

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 erosion of 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. Respondents who experienced an incident are asked not only whether they reported the event to police but also the reason for their decision. Importantly, response codes for unreported incidents include reasons related to police mistrust: *"police wouldn't think it was important enough, wouldn't want to be bothered or get involved," "police would be inefficient, ineffective (they'd arrive late or not at all, wouldn't do a good job, etc.)"* and *"police would be biased, would harass/insult respondent, cause respondent trouble, etc."*

As each incident is identified to the month of victimization, these data allow us to explore changing perceptions of the police as a mediator for non-reporting after George Floyd's murder. Specifically, we estimate the following equation using all NCVS incidents from 2020:

$$Y_i = \beta PostFloyd_i + \pi X_i + \epsilon_i. \tag{3}$$

Here, Y_i is an indicator set to 1 if police mistrust was stated to be the most important reason incident i was not reported to law enforcement, X_i are a vector of controls related to the victim demographics, incident characteristics, and seasonality, and $PostFloyd$ is an indicator set to 1 for incidents that occurred after May 2020. The coefficient of interest, β, captures the average difference in non-reporting rates between observably similar incidents that occurred before and after George Floyd's murder. Standard errors are heteroskedasticity-robust.

[Table II about here.]

Regression estimates are shown in Panel A of Table II. Column 1 shows results from our base regression with no controls. We find a 1.6 percentage point increase in non-reporting after May 2020, representing a roughly 13% increase over the mean. Columns 2 and 3 reveal nearly identical point estimates when including controls for victim demographics (i.e., age, sex, and race) and incident characteristics (i.e., type of crime and whether the victim or household members were present at the incident), which suggests that changing selection into the NCVS sample is unlikely to explain our findings. Finally, to address concerns about seasonality patterns in incident reporting, Column 4 further controls for average non-reporting rates among NCVS incidents of the same crime type occurring during the same calendar month in 2019. Results are virtually unchanged.[10]

As further corroboration, Panel B of Table II displays results from a placebo test estimating changes in incident non-reporting due to other factors. Specifically, we examine the most commonly-stated reason for non-reporting: the belief that the incident was a private or minor concern.[11] Across specifications, point estimates are statistically insignificant and near zero.

Consistent with the observed decreases in police favorability ratings and 911 call-to-shot ratios across the nation, these results point to the role of police mistrust in mediating changes in civilian reporting behavior after the police murder of George Floyd. Like all surveys, the NCVS data may be subject to selective recall and inaccurate reporting. Nonetheless, they provide a unique window into the beliefs underlying crime-reporting decisions and indicate how institutional mistrust may dissuade civilians from reaching out to the police for help, even among those who experienced a crime.

[10] Appendix Figure F.1 provides visual corroboration through a scatterplot of monthly non-reporting rates throughout 2020. We find little evidence of pre-trends prior to George Floyd's murder and a large spike in the share of incidents unreported due to police mistrust immediately afterwards.

[11] Response codes correspond to *"private or personal matter or took care of it myself or informally; told offender's parents"* and *"minor or unsuccessful crime, small or no loss, recovered property."*

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 recent rise of cell phone cameras, social media platforms, and social justice movements. Given these changing circumstances, 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.

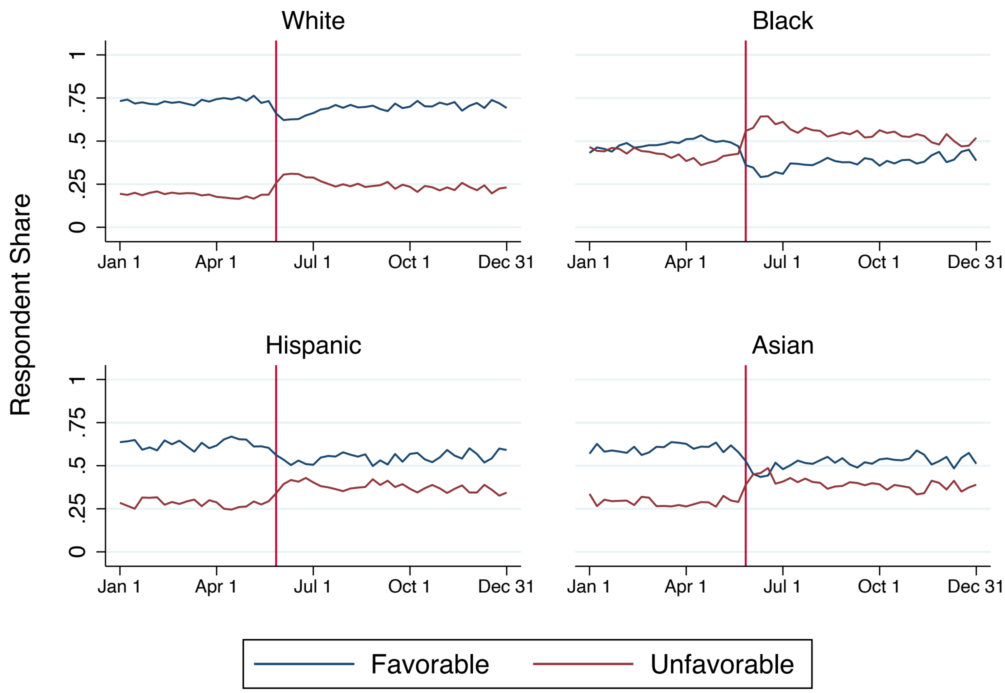
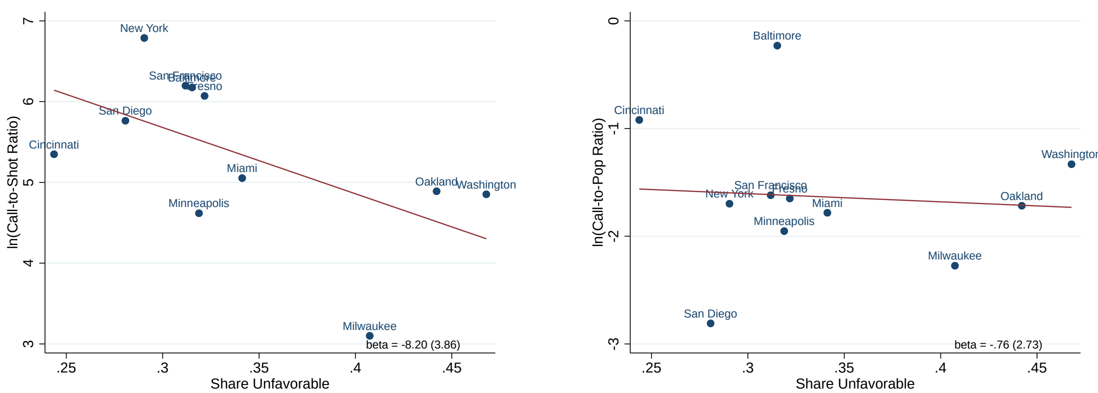


Figure I: Perceptions of Police over Time

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). Over the period of January 1, 2020, to December 31, 2020, Nationascape interviewed 213,935 white respondents, 38,085 Hispanic respondents, 38,379 Black respondents, and 27,375 Asian respondents. Each respondent was 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.

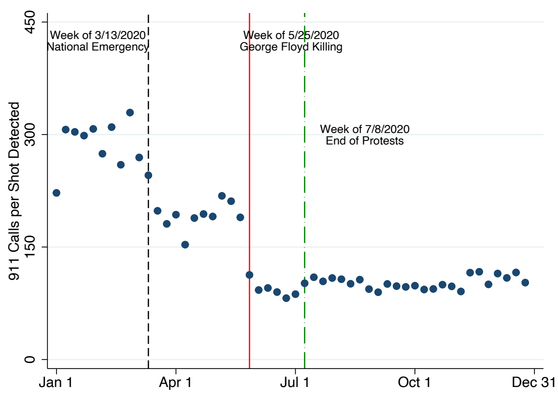


(a) Calls per Shot

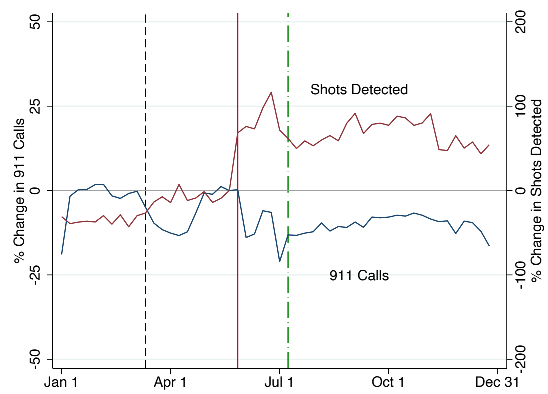
(b) Calls per Resident

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 Nationscape data across sample cities. As the Nationscape data is only geographically identified to the congressional district-level, we exclude Glendale, Arizona and Richmond, California, whose populations comprise only a small share of their respective congressional districts. Patterns are similar when including those cities. The slope of the best fit line is -8.20 with standard error of 3.86. 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 -0.76 with a standard error of 2.73.



(a) Call-to-Shot Ratio



(b) 911 Calls and Shots Detected

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 (Raleigh et al.*, 2023).*

Table I: Average Effects on Civilian Crime Reporting

	Panel A: Alternative Specifications					
	(1)	(2)	(3)	(4)	(5)	(6)
Post Floyd	-94.74	-102.4	-89.5	-90.06	-92.34	-92.99
	(8)	(19.06)	(22.72)	(19.96)	(19)	(20.01)
$\bar{Y}$	151.52	151.52	151.52	151.52	151.52	151.52
Outcome	Calls/Shots	Calls/Shots	Calls/Shots	Calls/Shots	Calls/Shots	Calls/Shots
Time Trend (Days)	No	Yes	Yes	Yes	Yes	Yes
Google Mobility	No	No	Yes	Yes	Yes	Yes
911 Calls from 2019	No	No	No	Yes	Yes	No
Shots from 2019	No	No	No	No	Yes	No
Calls/Shots from 2019	No	No	No	No	No	Yes
	Panel B: Alternative Samples and Outcomes					
	(1)	(2)	(3)	(4)	(5)	(6)
Post Floyd	-537.73	-0.23	-8.65	-67.38	-236.21	-26
	(55.31)	(0.07)	(0.92)	(5.3)	(23)	(2.08)
$\bar{Y}$	720.09	1.04	27.72	118.53	351.43	48.68
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 model 1. Data are at the daily-level, and 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. Panel A present results for models with different sets of controls. Column (1) contains our preferred model, which includes no controls. The remaining specifications contained in columns 2-6 of panel A are meant to probe the robustness of this result to potential confounds. Columns (2) adds a linear trend to the model. Column (3) includes controls for the amount of time spent in residences based on Google tracking data. Column (4) controls for 911 call volume from the same date in 2019. Column (5) controls for gunfire on the same date in 2019. Column (6) replaces the controls for reporting and gunfire withe the call-to-shot ratio as measured on the same date in 2019. Panel B shows results from our preferred model, but using 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. Appendix Figures E.1, E.2, E.3, E.4 and E.5 shows visual representations of the results in panel B. 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 B.1 for more detail.*

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

	(1)	(2)	(3)	(4)
Panel A: DV - Non-Report due to Police Mistrust				
Post-Floyd	0.016	0.017	0.015	0.015
	(0.008)	(0.008)	(0.008)	(0.008)
Mean	.128	.128	.128	.128
Panel B: DV - Non-Report due to Private Concern				
Post-Floyd	0.006	0.005	0.002	0.003
	(0.010)	(0.010)	(0.009)	(0.009)
Mean	.221	.221	.221	.221
Observations	8,287	8,287	8,287	8,287
Victim Demographics	No	Yes	Yes	Yes
Incident Characteristics	No	No	Yes	Yes
Monthly Non-Reporting in 2019	No	No	No	Yes

Notes: Table displays results from regressions of the form $Y_i = \beta PostFloyd_i + \pi X_i + \epsilon_i$ *where* $PostFloyd_i$ *takes a value of 1 for incidents that occured after George Floyd's murder (i.e., from June 2020 onwards) and* X_i *are a set of incident-level controls. Sample includes all NCVS crime victimizations from January 2020 to December 2020. 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, wouldn't want to be bothered or get involved", "police would be inefficient, ineffective (they'd arrive late or not at all, wouldn't do a good job, etc.)" 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 no controls. Column (2) includes controls for victim age, gender and race. Column (3) adds controls for the type of crime and the presence of the victim or household members at the incident. Column (4) further controls for average non-reporting rates among incidents of the same crime type during the same calendar month in 2019. Heteroskedasticity-robust standard errors are reported in parentheses.*

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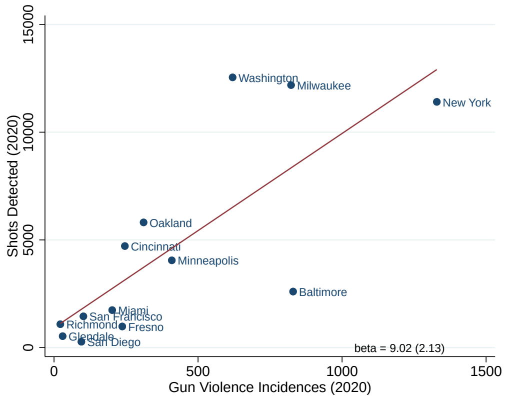
Appendix

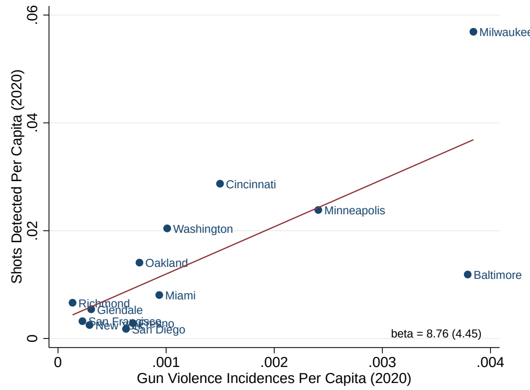
A Acoustically detected gunshot data and analysis sample of tracts and cities

Figure A.1 documents the correlation between gun violence incidents and shots detected. We find a strong correlation between the two measures, which suggests that acoustically detected gunshots data capture gunshot events.

Table A.1 compares the characteristics of our analysis sample of cities, which are those cities where we were able to obtain acoustically detected gunshots data during our period of analysis, to the national average for similarly sized cities (cities with 100,000 or more residents). We find that our sample cities are larger, have a lower share of white residents, have the same income, and have a higher rate of crime than large cities without the technology. These results are reported in Panel A. In Panel B of Table A.1, we compare our analysis tracts, which are those covered by acoustic gunshot detection technology, to all tracts within our sample set of cities. We use five-year estimates of tract-level characteristics from the American Community Survey from 2015-2019. Tracts in the analysis sample have a lower share of white residents and lower income than all tracts in the sample cities.



(a) Levels



(b) Per-Capita

Figure A.1: 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.

Table A.1: Sample Cities and Tracts Summary Statistics

Panel A: Sample Cities vs. All Cities With 100K+ Population		
	Sample Mean	National Mean
Total Population	1160301	304408
White Population Share	.349	.462
Hispanic Population Share	.272	.259
Black Population Share	.238	.161
Asian Population Share	.102	.08
Other Population Share	.039	.038
Median Household Income	68075	68023
Property Crime Rate (per 100k)	3919	2873
Violent Crime Rate (per 100k)	2079	1590
Panel B: Sample Tracts vs. All Tracts within Sample Cities		
	Sample Tracts	All Tracts
Total Population	3893	3952
White Population Share	.229	.344
Hispanic Population Share	.278	.249
Black Population Share	.377	.247
Asian Population Share	.083	.125
Other Population Share	.034	.04
Median Household Income	61009	71135

Note: Population, race, ethnicity, and income data come from the American Community Survey's (ACS) five-year estimates for 2015-2019 at the place level (Panel A) and tract level (Panel B). Crime rates in Panel A come from Uniform Crime Reporting (UCR) data for 2019, aggregated to the place level. We were able to obtain crime and demographics data for 303 out of 325 cities with a population over 100,000. The following list indicates the analysis sample cities: San Diego, CA; New York, NY; Baltimore, MD; Fresno, CA; Washington, DC; Milwaukee, WI; San Francisco, CA; Cincinnati, OH; Miami, FL; Minneapolis, MN; Oakland, CA; Richmond, CA; Glendale, AZ. To account for tracts that span city borders, results in Panel B are weighted by the share of each tract's 2010 population that resides within the analysis city's boundaries.

B Alternative inference

We provide alternative inference for the results in column (1) of Table I using permutation inference. 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 results are plotted in appendix Figure B.1 below. 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.

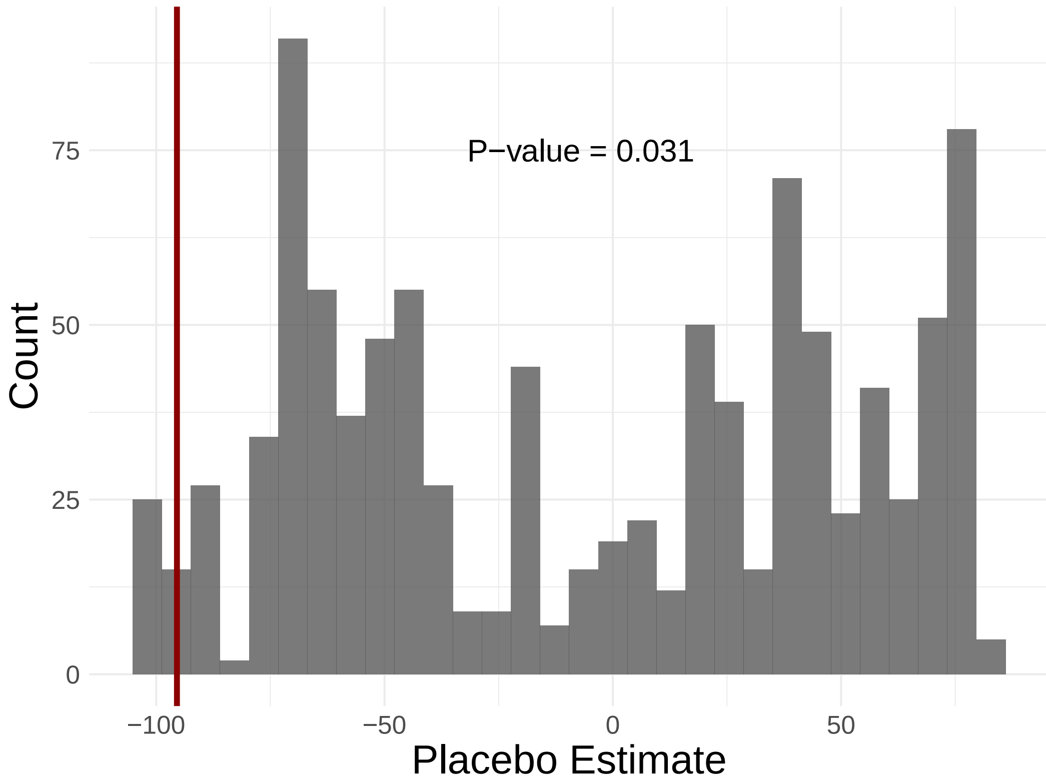


Figure B.1: Permutation inference

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.

C Neighborhood-level racial composition

In this appendix, we document 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 micro-data to Census tracts, which we merge with 2015-2019 American Community Survey data to obtain area demographics. We then 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.

Figure C.1 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.

Thus, 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.

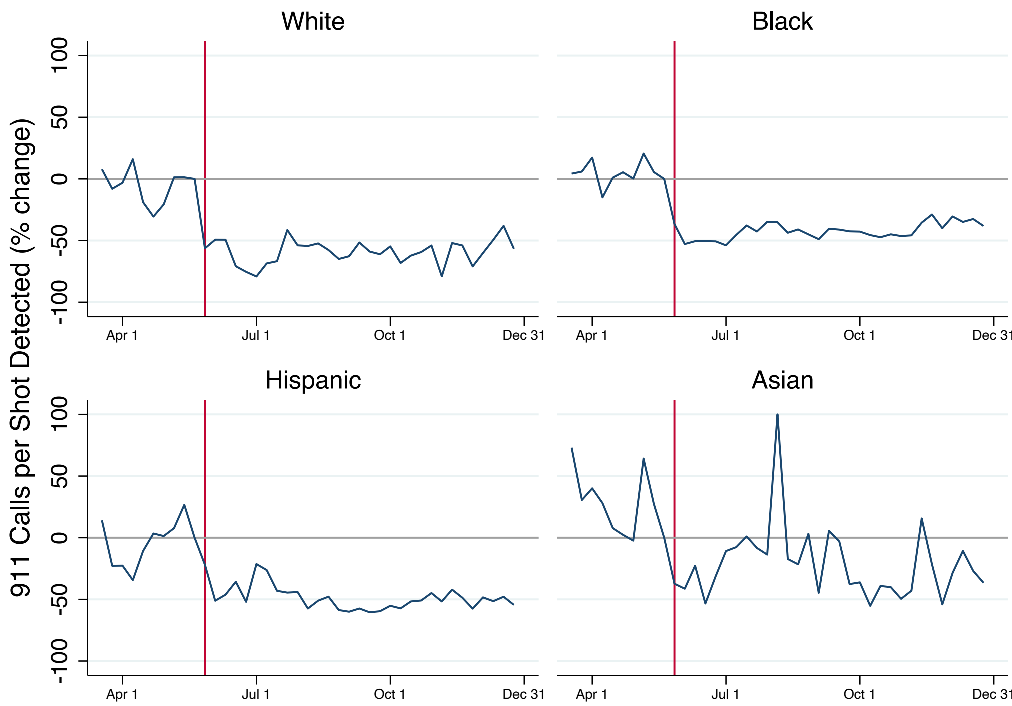


Figure C.1: Civilian Crime Reporting by Neighborhood Race

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.

D City-level variation

Figure D.1 disaggregates trends in call-to-shots ratios 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. Trends are least clear in Glendale, AZ, where the data are particularly noisy, and in San Diego, CA, which 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).

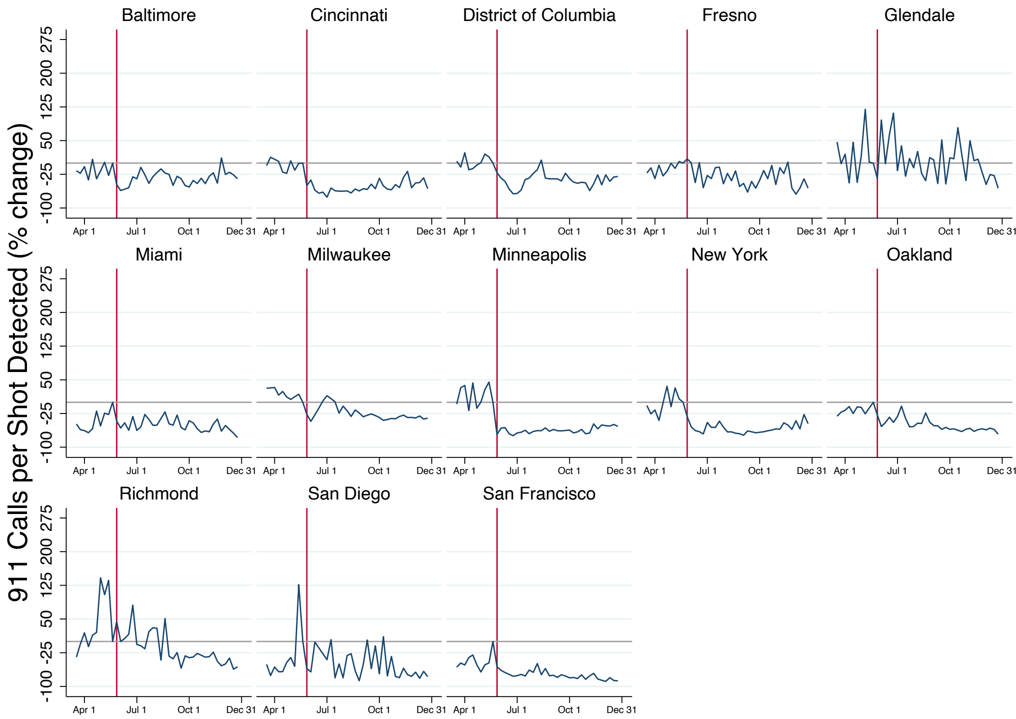
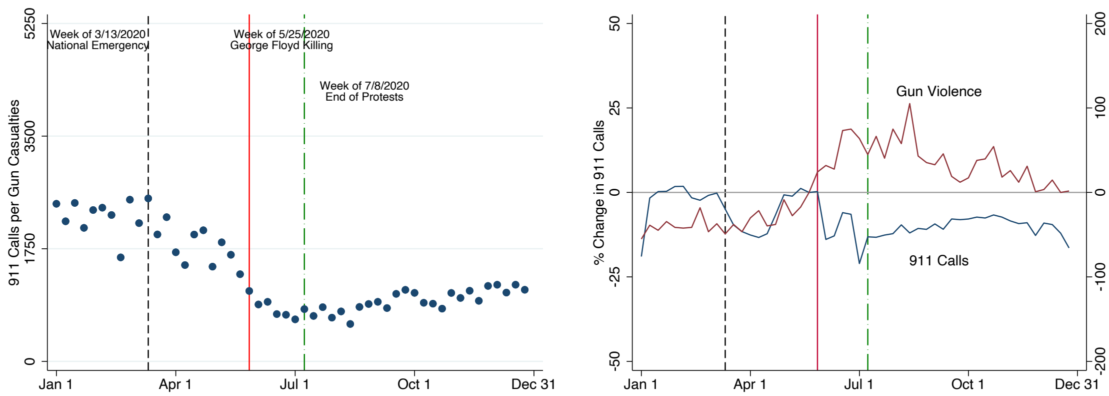


Figure D.1: Civilian Crime Reporting by City

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.

E Robustness

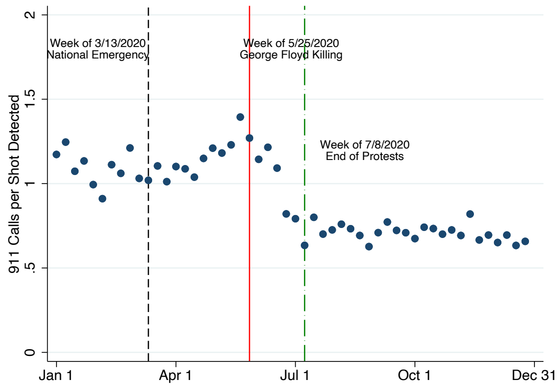


(a) Call-to-Casualty Ratio

(b) Calls and Casualties

Figure E.1: 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 (Raleigh et al.*, 2023).*

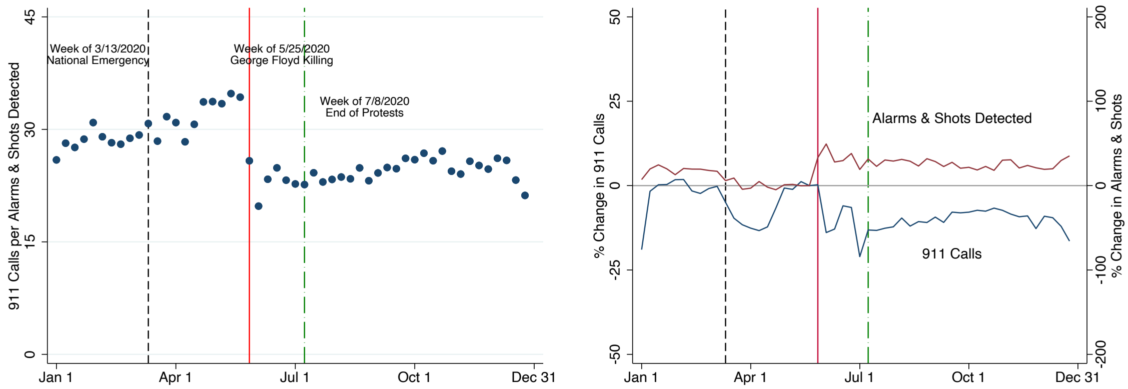


(a) "Shots Fired" Call-to-Shot Ratio

(b) "Shots Fired" Calls and Shots Detected

Figure E.2: 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 (Raleigh et al., *2023).*

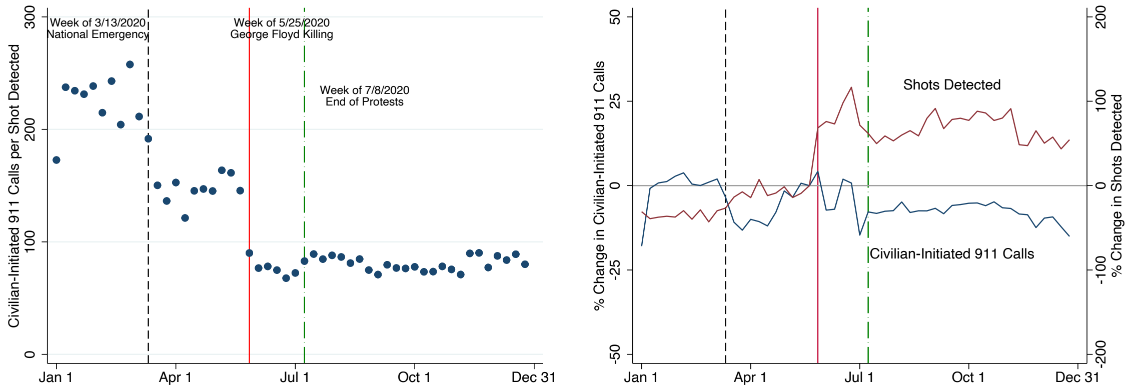


(a) 911 Calls to Shots Plus Alarms Ratio

(b) 911 Calls and Shots Plus Alarms

Figure E.3: Civilian Crime Reporting over Time (911 Calls to Shots Plus Alarms)

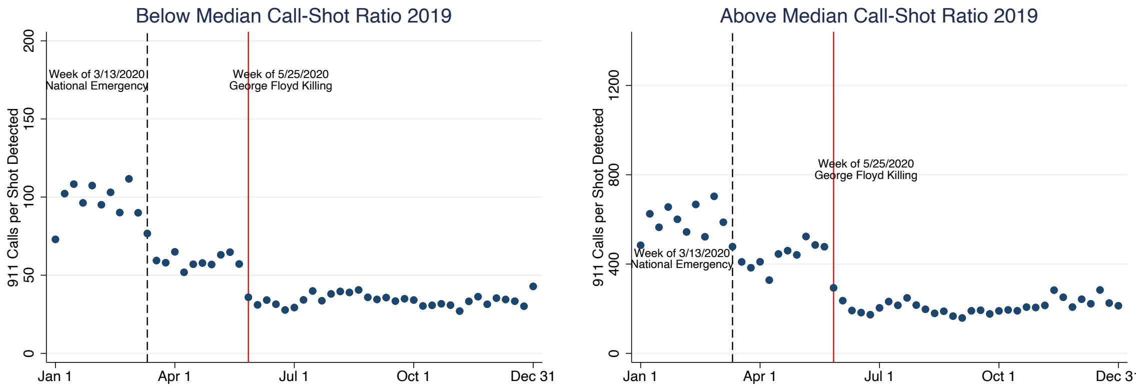
Notes: Panel A plots the ratio of 911 calls to the sum of acoustically detected gunshots and automated alarms over time. 911 calls and shots detected plus automated alarms are aggregated by week across acoustic gunshot detection coverage areas in all thirteen sample cities. Panel B plots the percent 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 (Raleigh et al.*, 2023).*



(a) Civilian-Initiated Call-to-Shot Ratio (b) Civilian-Initiated Calls and Shots Detected

Figure E.4: 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 (Raleigh et al., *2023).*

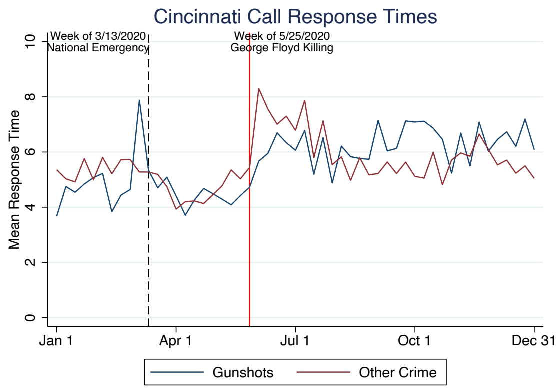


(a) Above median call-shot ratio 2019

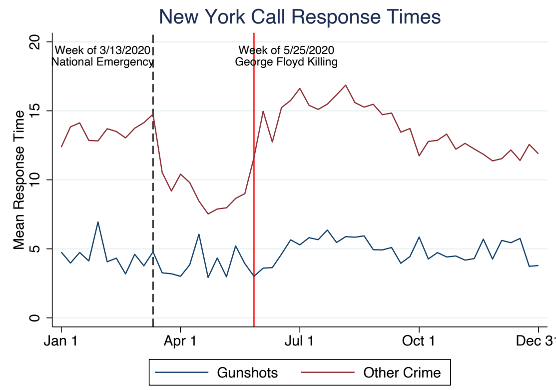
(b) Below median call-shot ratio 2019

Figure E.5: Call-to-shot Declines by Baseline Reporting

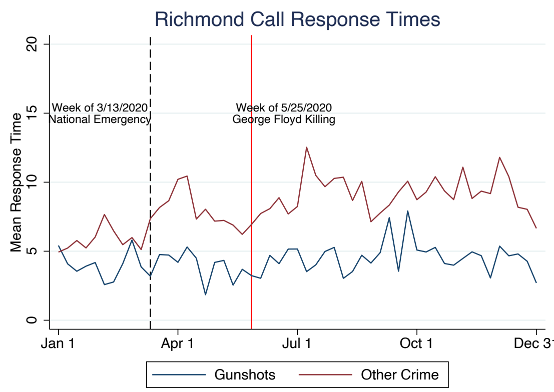
Notes: Both panels of this figure are identical to Panel (c) of Figure III from the main text except that we restrict to the sub-sample of census tracts which are above or below the median in the call-to-shot ratio at baseline.



(a) Cincinnati



(b) New York City



(c) Richmond

Figure E.6: Police Response Times

Notes: Figure plots the average police response at the weekly level during 2020 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.

F National Crime Victimization Survey Analysis

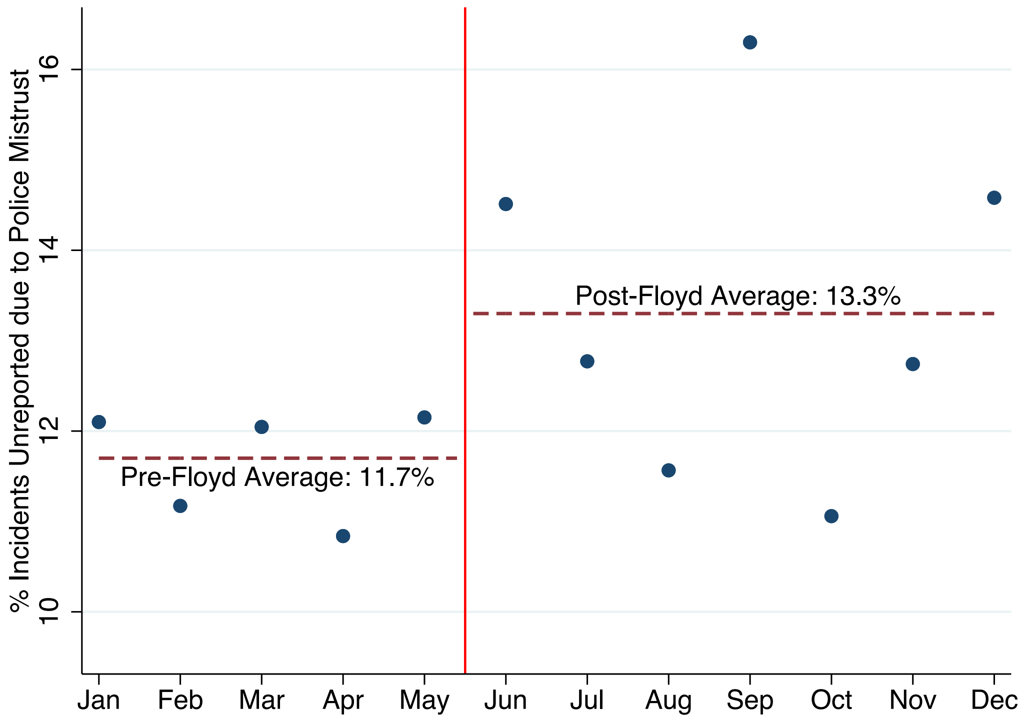


Figure F.1: Non-Reporting Due to Police Mistrust: National Crime Victimization Survey

Notes: Figure plots the monthly share of National Crime Victimization Survey incidents not reported to law enforcement due to mistrust of police. Police mistrust is identified by incidents for which the respondent stated the most important reason for not reporting to law enforcement was because "police wouldn't think it was important enough, wouldn't want to be bothered or get involved", "police would be inefficient, ineffective (they'd arrive late or not at all, wouldn't do a good job, etc.)" or "police would be biased, would harass/insult respondent, cause respondent trouble, etc." Dashed maroon lines represent the average non-reporting rate among incidents that occurred from January to May 2020 and among incidents that occurred from June to December 2020. Red vertical line represents the timing of George Floyd's death.