WHEN POLICE PULL BACK: NEIGHBORHOOD-LEVEL EFFECTS OF DE-POLICING ON VIOLENT AND PROPERTY CRIME

****Preprint version 3/3/2023****

Justin Nix
(Corresponding author; Email: jnix@unomaha.edu)
University of Nebraska Omaha

Jessica Huff
University of Nebraska Omaha

Scott E. Wolfe
Michigan State University

David C. Pyrooz
University of Colorado Boulder

Scott M. Mourtgos
University of Utah

KEYWORDS
COVID-19, crime, George Floyd, neighborhoods, policing, violence

Abstract
Many U.S. cities witnessed both de-policing and increased crime in 2020, yet it remains unclear whether the former contributed to the latter. Indeed, much of what is known about the effects of proactive policing on crime comes from studies that evaluate highly focused interventions atypical of day-to-day policing, use cities as the unit of analysis, or cannot rule out endogeneity. This study addresses each of these issues, thereby advancing the evidence base concerning the effects of policing on crime. Leveraging two exogenous shocks presented by the onset of COVID-19 and social unrest following the murder of George Floyd, we evaluated the effects of sudden and sustained reductions in high-discretion policing on crime at the neighborhood level in Denver. Multilevel models accounting for trends in prior police activity, neighborhood structure, seasonality, and population mobility revealed mixed results. On one hand, large-scale reductions in pedestrian stops and drug-related arrests were associated with significant increases in violent and property crimes, respectively. On the other hand, fewer vehicle stops and disorder arrests did not affect crime. These results were not universal across neighborhoods. We discuss the implications of these findings in light of debates concerning the appropriate role of policing in the 21st century.

1. INTRODUCTION

The COVID-19 pandemic, a contentious presidential election year, and civil unrest, unlike any seen since the civil rights movement, made 2020 one of the most stressful years in memory for most Americans. On top of this, the United States witnessed a near 30% spike in its homicide rate, marking the largest year-over-year increase on record. Other crimes such as aggravated assault and motor-vehicle theft also increased during this time. These spikes mostly occurred during the second half of the year and occurred in a wide range of mid- to large-sized U.S. cities (Rosenfeld & Lopez, 2021). This suggests that economic, political, and social forces broader than those at the local level likely prompted the crime increase. From a scientific standpoint, scholars across many disciplines will be studying the events of 2020 for years to come. The crime rate increase is worthy of serious empirical scrutiny not only because of its magnitude, but also because of the potential sources of the rise.

Currently, there are at least two popular explanations offered for why so many more murders and other crimes occurred in 2020 (Lopez, 2021). First, it is possible that the stress, anxiety, and uncertainty of the COVID-19 pandemic played a meaningful role in precipitating violence and property offending. Indeed, several studies have shown that the timing of the pandemic's start was associated with increases in domestic violence (Piquero et al., 2021; Richards et al., 2021), burglary (Campedelli et al., 2020), and violence (Kim & Phillips, 2021; Lang & Lang, 2021). Some have argued that COVID-19 led to an "arousal breakout" whereby people temporarily become more willing to engage in risky behaviors (Baumgaertner & Mitchell, 2021). Furthermore, the start of the pandemic may have been associated with crime increases as police officers withdrew from some of their duties. Agencies across the U.S. issued directives to engage in fewer civilian contacts (e.g., traffic stops) to help mitigate the spread of the virus (Lum et al., 2022a). To the extent that proactive policing is associated with crime reductions (National Academies of Sciences, 2018; Petersen et al., 2023), such a pullback may have emboldened would-be offenders, in part because they perceived lower chances of getting caught (Stickle & Felson, 2020).

The second explanation revolves around the murder of George Floyd by a former Minneapolis police officer. Floyd's death on May 25, 2020 was captured on cellphone video by a bystander and shows Derek Chauvin kneeling on his neck for 9 minutes and 29 seconds, during which time Floyd can be heard calling out for his mother—"Momma, Momma"—and saying "I can't breathe," a rallying cry of the Black Lives Matter movement since Eric Garner's death in

2014. The incident led to immediate public backlash and protests around the country and was widely condemned by public officials, civilians, and police officers alike. Calls for police reform had been occurring since the death of Michael Brown in Ferguson, MO in 2014 and a long string of controversial police-involved deaths of Black Americans in the intervening years. But Floyd's death and the public's reaction to it were different. Civil unrest and widespread protests continued throughout the United States for several weeks and prompted invigorated efforts to reform policing. Calls to "defund the police" became common in many American cities while some people even advocated for abolishing policing entirely (Kaba, 2020). Police departments around the country faced a legitimacy crisis in the aftermath of Floyd's death—more Americans were questioning police practices and voicing displeasure with outcomes received by Black Americans (Reny & Newman, 2021).

This likely had several consequences. For one, when people view the police as less legitimate, they are less likely to comply with the law or cooperate with law enforcement officers (Bolger & Walters, 2019; Tyler, 2006; Walters & Bolger, 2019). When people do not believe the police follow the laws they represent or adhere to principles of fairness, they are less likely to feel bound to the law (McLean & Wolfe, 2016). Thus, in the weeks following Floyd's murder, some people may have been emboldened to commit more crime because the moral bind to obey the law was weakened. Moreover, we would expect police officers themselves to respond to public criticism, especially in the face of such extreme attempts at police reform. Many police officers may have decided to pull back from their duties—particularly proactive styles of policing—in response to public criticism (Bottoms & Tankebe, 2012; Nix, Pickett, & Wolfe, 2020). In this view, if the community does not want policing, they will receive less of it.

Both the onset of the pandemic and public criticism of policing post-Floyd may have contributed to the 2020 crime spike, at least partially, because each may have led to a reduction in discretionary police activities.[1] In this way, 2020 offers researchers a unique case study for

[1] Some argue that the record number of gun sales that occurred in 2020 could have provided more opportunities for violent encounters. Gun sales increased by 65% in the U.S. during 2020 which may have helped augment the illegal gun market (Savidge & Cartaya, 2021). However, we are skeptical, because with ~400,000,000 guns in circulation there were ample opportunities to obtain a gun and use it in a violent crime prior to 2020 (Karp, 2018). Moreover, newly purchased firearms are rarely used in the commission of a felony (Hureau & Braga, 2018). The cause of the gun sales spike was likely fears of what the pandemic would bring, calls to defund the police (and people feeling they may need to take protection efforts into their own hands), and uncertainty about gun control law changes with the potential election of a new President. With a lack of a federal gun tracking mechanism, we may never know the extent to which increased gun sales in 2020 was or was not associated with increases in violent crime.

examining the effects of police pullbacks on crime rates. Examining whether policing impacts crime has been the focus of a wide range of research studies for several decades, the results of which have direct implications on how we envision the role of police in our society (Bittner, 1970; Meares & Tyler, 2020). To date, the effects of police pullbacks on crime are based mostly on city-level comparisons and we lack rigorous *neighborhood-level* examinations of de-policing (Cassella et al., 2022; Premkumar, 2021; White et al., 2022). This is a notable gap in our understanding of the effects of policing on crime because we know that policing is not administered equally across neighborhoods—police activity tends to be concentrated in areas with the highest crime (Wu & Lum, 2017)—and we know there is meaningful neighborhood-level variation in crime rates (Sadler et al., 2022; Sherman et al., 1989).

Accordingly, the present study will examine the effects of de-policing on crime rates in 2020 across neighborhoods in Denver, CO. We focus our attention on Denver because it witnessed large scale protests after Floyd’s murder (Phillips et al., 2020), the imposition of a state of emergency curtailing public activity in response to COVID-19, and the city provides access to high-quality neighborhood-level police activity and crime data on a weekly basis. We examine five questions: (1) Did property and violent crime rates deviate in 2020 from prior years? (2) Did discretionary police behavior deviate in 2020 from prior years? (3) Were changes in crime and policing associated with the pandemic and social unrest? (4) Did changes in policing account for changes in crime? (5) Were these associations universal across 78 neighborhoods in Denver?

2. DE-POLICING AND CRIME

One of the most pressing empirical issues in criminology is determining the extent to which the police reduce crime (Wilson & Boland, 1978; Zimring, 2012). Answering this question is key for theoretical and practical reasons. From a theoretical standpoint, there are several criminological frameworks that hypothesize a relationship between policing and crime—namely, deterrence theory (Chalfin & McCrary, 2017; Nagin, Solow, & Lum, 2015), broken windows theory (Kelling & Coles, 1996), and the systemic model of social control (Bursik & Grasmick, 1993; Shaw & McKay, 1942). On the practical side, determining the extent to which policing impacts crime is foundational to how we use police officers for social control efforts (Bittner, 1970). The research literature is clear that policing matters. But, that does not mean that the effects of policing are invariant, mattering in all contexts or across all units of analysis.

Moreover, what types of policing matter and how much they impact crime are still open empirical questions. Take for example the National Police Foundation's classic Kansas City Preventive Patrol experiment, which famously demonstrated that increasing the number of patrol officers in an area did not result in crime reductions (Kelling et al., 1974). There were two primary reasons for this finding. First, the public did not perceive a difference in patrol activity, so it stands to reason that potential offenders' perceptual deterrence was not impacted (see also, Kleck & Barnes, 2014). Second, as we have seen in several decades of research since the study, it is not simply the number of police that matters, but rather what police actually do in the community (Groff et al., 2015). A considerable amount of empirical evidence demonstrates that focused deterrence strategies (Braga et al., 2018), "hot spots" policing at micro places (Braga et al., 2019), and community-oriented policing that takes a problem-oriented approach (Gill et al., 2014; Hinkle et al., 2020) can save lives and reduce victimization in communities. Aggregate-level studies have even shown in recent years that cities with more police officers tend to experience larger reductions in crime over time (Chalfin et al., 2020). Importantly, however, the likely reason we observe such an effect is that cities with the resources to employ more officers also have the luxury of dedicating more of their officers' time to evidence-based policing strategies (Lum et al., 2011). Thus, quality policing may reduce crime.

The problem, however, is that most of our understanding of the police effect on crime is restricted to evaluations of targeted policing strategies. We know much less about what happens if we witness a sudden, sustained withdrawal of policing from communities. This is an important consideration because it speaks to the extent to which communities can tolerate reductions in police activity. Evidence-based policing can reduce crime, but we also know that such strategies can lead to meaningful racial and ethnic disparities given that crime is spatially concentrated in disadvantaged communities that are disproportionately occupied by minority residents (National Academies of Sciences, 2018; Wheeler, 2020). If we suddenly and significantly reduce police activity for a prolonged period, does it lead to more victimization in already marginalized communities?

2.1. Does de-policing lead to more crime?

The good news is that we have a growing evidence base on this issue. Several studies have examined whether police pullbacks, either from reducing the number of officers or from de-policing (i.e., same number of officers doing less), are associated with crime increases. A systematic review of 49 studies concerned with the effect of police presence on crime found that 30 reported significant crime suppression effects – especially on motor vehicle theft, crimes of violence, gun-related crimes, and property crimes (Dau et al., 2021). For example, Sarit Weisburd (2021) recently drew on Dallas patrol officers' responses to out-of-beat 911 calls to show that a 10% reduction in police patrol presence translated into a 7% increase in crime. The findings show that police pullbacks can have a meaningful impact on crime.

Most de-policing studies examine whether exogenous shocks—unexpected events that are external to policing itself, but nonetheless impact police job performance outcomes—are associated with police pullbacks. One way to examine the effects of de-policing is by examining what happens during a police strike. The limited evidence on the issue suggests that police strikes can lead to de-policing, but such inactivity has little effect on crime (Pfuhl, 1983). Other researchers have examined whether police pull back from their duties after losing salary arbitration. After such an event, officers have been shown to de-police in the form of issuing fewer tickets and arresting fewer people for minor offenses (Chandrasekher, 2016; Mas, 2006). In this context, less enforcement of minor offenses seems to lead to an increase in minor offending, but only small increases in larceny and assault (Chandrasekher, 2016). Recently, Piza and Chillar (2021) showed that violent crime spiked significantly in Newark after it furloughed 13% of its police force in 2009, whereas nearby and similarly situated Jersey City averted layoffs and experienced no such spike.

Scholars have also noted that police officers sometimes pull back from proactive enforcement activities in the aftermath of fellow officers being killed in the line of duty. For example, Cho, Goncalves, and Weisburst (2021) found that officer deaths were associated with a short-term reduction in arrests—their indicator of de-policing—but there was no subsequent impact on crime. Sullivan and O'Keeffe (2017) published a paper in *Nature Human Behavior* several years ago that received widespread media attention. They examined the organized de-policing by NYPD officers as a protest against several of their colleagues being feloniously killed. Interestingly, the researchers found that serious crimes like burglary, assault, and grand larceny *declined* as a result of the NYPD pullback. Thus, de-policing appeared to result in safer communities. However, Chalfin, Mitre-Becerril, and Williams (2021) recently demonstrated that, among other flaws, the Sullivan and O'Keeffe paper suffered from an ecological fallacy that called into question their results and conclusions. Upon reanalysis of the data, Chalfin et al. found that "…there was considerable variation in the intensity of the slowdown across NYC communities and that the communities which experienced a more pronounced reduction in police proactivity did not experience the largest reductions in major crime" (p. 1). They concluded that there is little evidence that the NYPD pullback had a meaningful effect on crime in either direction.

It is also common for researchers to explore whether police pull back in reaction to high-profile or viral incidents (Cheng & Long, 2022; Devi & Fryer, 2020; Premkumar, 2022). In Cincinnati, OH in 2001, there were riots in response to the police killing of an unarmed Black teenager. Shi (2009) found a significant reduction in arrest activity in the city after this widespread criticism of the police. What is more, this pullback coincided with spikes in serious felony crimes, particularly in communities with more Black residents. Research on the impact of viral incidents attracted the interest of scholars after the 2014 death of Michael Brown in Ferguson. In what has become known as the "Ferguson Effect," officers who felt more negatively impacted by public criticism of their profession reported on surveys being less willing to investigate suspicious behaviors (Morin et al., 2017) or partner with community members to solve problems (Wolfe & Nix, 2016), and that they believed their colleagues were less likely to engage in self-initiated stops (Gau, Paoline, & Paul, 2022). Pyrooz et al. (2016) were the first to examine whether the Ferguson Effect resulted in city-level crime rate changes. Their analysis of 81 large U.S. cities revealed no systematic change in crime trends post-Ferguson. However, robbery rates increased in the 12 months after Ferguson and cities with greater socioeconomic disadvantages, Black resident populations, and levels of violence were most likely to witness homicide increases. At the same time, however, Shjarback and colleagues (2017) examined traffic stop and crime trends in Missouri police agencies post-Ferguson. While Missouri law enforcement engaged in about 67,000 fewer stops from 2014 to 2015—a far greater reduction than in prior years and clear evidence of de-policing—there were no appreciable effects on violent or property crime. Morgan and Pally (2016) demonstrated a similar effect in Baltimore, where police proactivity, measured as arrests for a range of crimes, such as attempted murder, robbery, aggravated assault, deadly weapon, property destruction, driving violations, and disorderly conduct, declined after several high-profile incidents inside and outside of the city, but there was no significant impact on crime trends.

More recently, Piza and Connealy (2022) examined the impact of the Capitol Hill Occupational Protest (CHOP) in Seattle on crime. For 24 days in June of 2020, Seattle protestors established the CHOP, which operated as a semi-autonomous zone with no official police presence. Officers were ordered not to respond to any call within the CHOP unless it was a "mass casualty event." In the final 10 days of the CHOP, there were four shootings (two of which were fatal), prompting the mayor to order Seattle PD to disband the CHOP. Piza and Connealy showed that relative to a synthetic control area, crime was 133% higher in the CHOP.

Taken as a whole, the de-policing literature offers consistent evidence that exogenous shocks can result in short-term police pullbacks. The evidence concerning whether such de-policing leads to crime increases is more mixed (Cassella et al., 2022). Though importantly, the best evidence suggests that viral incidents are the most likely to result in de-policing that, subsequently, leads to spikes in violent crime (Devi & Fryer, 2020). The mixed evidence regarding the de-policing-crime connection may stem partially from the fact that most studies are based on city-level comparisons that likely mask variation at the neighborhood or street segment level (e.g., Cassella et al., 2022; Morgan & Pally, 2016). Indeed, several de-policing studies have noted that the impact on crime tends to be more pronounced in disadvantaged Black communities. If there is a connection between de-policing and crime it may be disproportionately felt in Black neighborhoods that already suffer from high levels of violence. Examining the de-policing-crime relationship at the neighborhood level will provide a clearer picture of the extent policing "matters."

2.2. De-policing, crime, and neighborhood variation

We know from decades of research that the places crime concentrates tend to be fairly stable over time (Schnell & McManus, 2022, Weisburd et al., 2004; but see, Sadler et al., 2021). The scientific evidence is so clear at this point that some refer to this phenomenon as the "law of crime concentration" (Weisburd, 2015). In any given city there will be neighborhoods with higher crime rates, and within those neighborhoods there will be street segments that account for the vast majority of crime in the area (Connealy, 2021; Connealy & Piza, 2019; Sherman et al., 1989). This is one of the reasons that police activity is not evenly distributed throughout a jurisdiction. It would be poor managerial practice to randomly distribute patrol cars across a city with the hope of impacting crime. Rather, a police agency may achieve greater return on investment if it uses data to intelligently guide its patrol resources into areas that experience the most criminal activity (Xu & Lum, 2017).

If police activity and crime are concentrated in particular neighborhoods, any de-policing-crime effect would likely not be experienced in the same way across a city. Rather, the effects of de-policing should be most pronounced in neighborhoods that are racially diverse, more socioeconomically disadvantaged, and have higher crime rates. Consequently, if a police pullback occurs and it is associated with crime increases, the bulk of this impact may be saddled on Black communities that have higher rates of crime to start. The year 2020 offers a unique opportunity to examine the extent to which this occurred.

2.3. Unpacking 2020

Two exogenous shocks occurred in close temporal proximity to one another in 2020—the start of the COVID-19 pandemic and the police legitimacy crisis sparked by the murder of George Floyd. We have theoretical reasons to believe both shocks could have led to de-policing and crime increases, albeit for different reasons. There are several arguments for why the pandemic's start could have increased various types of crimes mentioned earlier. Violence could have risen as people became more anxious or willing to engage in risky behaviors (Stickle & Felson, 2020). The pandemic also changed peoples' routine activities as many were forced to work from home more often. This likely decreased many criminal opportunities as suitable targets crossed paths with would-be offenders less regularly (Massenkoff & Chalfin, 2022). It is also possible that crime increased because motivated offenders perceived a lower probability of getting caught as police officers pulled back from their typical enforcement activities in response to the pandemic (Lum et al., 2022a). If this occurred, we would see police activity declining while crime went up. We can test whether pandemic-initiated de-policing is associated with crime increases by controlling for other factors that may explain a pandemic-crime relationship.

The next exogenous shock occurred about two months after the start of the pandemic. As discussed above, George Floyd's murder motivated intense scrutiny of the police across the United States and calls for drastic reform (Lum, 2021). We conceptualize Floyd's death, subsequent civil unrest, and sustained efforts to reimagine the role of policing in our society as a legitimacy crisis for American policing. Protests, riots, and serious attempts to drastically reform policing were signals that a non-trivial portion of the public was calling into question the legitimacy of police authority (Reny & Newman 2021). Use of this term does not imply we are claiming the police necessarily lost objective indicators of legitimacy, nor that all Americans question the legitimacy of the police. Indeed, support for the police remained high among some citizens during this time (McLean & Nix, 2021), and likely many residents of Denver as well. Rather, we use the term to highlight the reality of the situation faced by American policing in the summer of 2020, and that which continues today.

As research on police legitimacy clearly demonstrates, people are more likely to comply with the law voluntarily when they believe the police are a legitimate authority (Tyler, 2006). Treating people in a procedurally fair manner is the main mechanism by which the police can improve their legitimacy in the eyes of the public (Tyler, 2004). When people believe that the police do not adhere to principles of fairness—such as being perceived as disproportionately using excessive force against unarmed Black Americans—they are less willing to confer legitimacy onto the police. In turn, such people are less willing to obey the law that the police represent.

We contend that this legitimacy crisis was a strong enough exogenous shock to change police behavior and crime trends. Many police officers likely responded to increased public criticism by making a calculated decision to “lay low and avoid trouble” (Van Maanen, 1973: 408)—indeed, we have good evidence this took place post-Floyd in many areas around the United States (see e.g., White et al., 2022).

3. THE CURRENT STUDY

The purpose of this study is to determine the effects of de-policing on crime. We leverage the natural experiments presented by the pandemic and social unrest to determine if there were changes in policing and, if so, whether they were equally consequential for property and violent crime across neighborhoods in Denver. A neighborhood-level focus is beneficial as it stands in contrast to much of the research on de-policing, despite an extensive criminological literature on neighborhoods and crime (Hipp & Williams, 2020; Lanfear, Matsueda, and Beach, 2020). The temporal proximity of the public health emergency declarations associated with COVID-19 and the social unrest associated with the George Floyd protest movement requires more granular data to generate distinguishable effects than between-city comparative data can offer. Fortunately, we were able to gather such information in the city of Denver from public and non-public sources to examine how neighborhoods experienced crime and policing in 2020 relative to prior years.

4. METHODS

4.1. Data

The City and County of Denver is the capital of Colorado, with 716,000 residents at the 2020 Census. The unit of analysis in this study is the neighborhood-week, where we focus on crime and policing in Denver's 78 neighborhoods across the 52 weeks in 2020, where $N * T =$ 4,056, relative to prior years. We obtained data from several sources that differ based on within and between neighborhood variation.

Incident-level data on reported crimes, vehicular and pedestrian stops, and traffic accidents spanning a 5-year period from 2016 to 2020 were gathered from Denver's Open Data Catalog.[2] Arrest data for the same period were obtained through the filing of a Colorado Open Records Act request with the Denver Police Department. Each of these sources of data varied within and between neighborhoods. These data sources included address information for each incident, which was used to geocode and spatially join the individual incidents to Denver neighborhoods.[3]

Data derived from the American Community Survey's 2014-2018 five-year estimates provide information about neighborhood characteristics.[4] These data were provided by the City and County of Denver, which uses definitions of neighborhoods that differ from the U.S. Census Bureau's blocks and tracts. These measures vary between neighborhoods, but not within them.

Our final sources of data contain information that vary within neighborhoods, but not between them. As a proxy for resident mobility, we obtained restaurant reservation data compiled by OpenTable.[5] Such data have been used previously by economists studying the effects of COVID-19 stay-at-home orders on domestic violence calls for service (Leslie & Wilson, 2020). Precipitation and temperature data were gathered from the National Oceanic and Atmospheric Administration and Air Quality Index (AQI) data were gathered from the Environmental

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[2] See https://www.denvergov.org/opendata/dataset/city-and-county-of-denver-crime, https://www.denvergov.org/opendata/dataset/city-and-county-of-denver-police-pedestrian-stops-and-vehicle-stops, and https://www.denvergov.org/opendata/dataset/city-and-county-of-denver-traffic-accidents.
[3] Each dataset also had a small amount of missing information in the *incident date* and/or *neighborhood* fields. We used listwise deletion to remove cases missing on either field in the stop data (n=66,852 of 557,551 cases or 12% of the data), crime data (n=1 of 451,404 cases), arrests data (n=13,268 of 217,191 cases or 6% of the data), and accidents data (n=10,548 of 119,132 cases or 9% of the data).
[4] See https://www.denvergov.org/opendata/dataset/american-community-survey-nbrhd-2015-2019.
[5] See https://www.opentable.com/state-of-industry.

Protection Agency's website.[6,7] Prior research suggests these variables are associated with population mobility and crime (Bondy et al., 2020; Mares, 2013; Zhu et al., 2020)

4.2. Dependent variables

The dependent variables in this study were weekly counts of *violent* and *property crimes*, derived from incident-level data of reported crimes. Violent crimes include murder and nonnegligent manslaughter, robbery, and aggravated assault.[8] Property crimes include motor vehicle theft, residential and commercial burglary, larceny, and theft from motor vehicles. Table 1 displays descriptive statistics for all study variables.

TABLE 1. Descriptive statistics.

Variable	Mean	Std. Dev.	Min	Max
Outcomes				
Violent crime	1.040	1.476	0.000	14.000
Property crime	8.330	7.104	0.000	50.000
Mediators				
Pedestrian stops deviation	-2.709	6.220	-57.500	39.167
Vehicle stops deviation	-7.961	15.583	-104.267	105.433
Drug arrests deviation	-.830	1.845	-16.100	5.267
Disorder arrests deviation	-.145	.695	-5.300	9.433
Controls				
Average Temperature [a]	44.914	15.333	19.057	69.095
Average Precipitation [a]	.035	.041	0.000	.187
Average AQI [a]	5.527	2.306	2.607	11.786
Motor vehicle accidents	3.546	3.731	0.000	35.000
Open Table (% Change from 2019) [a]	-52.171	33.351	-100.000	8.000
Disadvantage [b]	.000	.962	-2.532	2.495
% Black [b]	7.946	9.112	.400	38.350
% Hispanic [b]	28.037	22.576	2.800	80.900
Immigration [b]	.000	.990	-1.021	3.041
Population [b]	8889.962	6409.847	818.000	37463.000

NOTE: Unless noted, unit of analysis is *neighborhood weeks* N = 4,056 (78 neighborhoods * 52 weeks).
[a] Varies within, but not between, neighborhoods.
[b] Varies between, but not within, neighborhoods.

[6] See https://www.ncdc.noaa.gov/cdo-web/datasets.
[7] See https://www.epa.gov/outdoor-air-quality-data/download-daily-data.
[8] We excluded rape from our violent crime outcome because it is less reliably reported than the other violent crimes (Langton et al., 2012).

4.3. Independent variables

4.3.1. Exogenous shocks

We measure two exogenous shocks that occurred in 2020. The first was the COVID-19 shock beginning on March 11, 2020, when Governor Jared Polis declared a state of emergency.[9] March 11th occurred during the eleventh week of 2020; thus, *COVID-19 Shock* is dummy variable where 0=weeks 1-10 and 1=weeks 11-52. The second shock occurred on May 25, 2020, when George Floyd was murdered in Minneapolis. The incident sparked a massive protest movement, made "Defund the Police" a household slogan (Crabtree, 2020), was the catalyst for more than 700 pieces of legislation across 36 states aimed at police reform, and brought back discourse about the "Ferguson Effect."[10] *Floyd Shock* is a dummy variable where 0=weeks 1-21 and 1=weeks 22-52. This coding scheme partitions 2020 into three periods: the pre-COVID-19/Floyd period, the COVID-19-only period, and the period of Floyd which incontrovertibly overlaps with the pandemic.

4.3.2. Police discretionary behaviors

Four independent variables capture high-discretion activities by Denver police officers: *pedestrian stops*, *vehicular stops*, *drug-related arrests*, and *public disorder-related arrests*. Pedestrian and vehicle stops refer to instances where the police stopped an individual or motor vehicle. Drug-related arrests include all offenses categorized as "drug-alcohol" (See Appendix Table S1). Public disorder-related arrests include minor offenses such as criminal mischief, curfew violations, loitering, and disturbing the peace (see Appendix Table S2).

To break the simultaneity of police and crime, which is often ignored in this line of study (e.g., Johnson & Roman, 2022), we establish the extent to which policing deviated from the status quo in Denver neighborhoods. We do this by generating 3-week rolling average deviations in policing from the same 3-week rolling average in (weighted) 2016-2019. First, we rely on multiple weeks (week t, t-1, and t-2) in the focal and comparison years to temper wider fluctuations in police activity. Second, to account for longer temporal trends in policing we upweight more and

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[9] Though the exact date that widespread changes to routine activities occurred because of COVID-19 is debatable, a study by Leslie and Wilson (2020) showed that widespread social distancing in the US began on March 9th, 2020.
[10] Nearly 100 of the legislative bills were enacted. See https://www.ncsl.org/research/civil-and-criminal-justice/law-enforcement-statutory-database.aspx.

downweight less recent years, such that 2016=0.4, 2017=0.8, 2018=1.2, and 2019=1.6, where the mean of the comparison years remains equal to 1. Third, by using a weighted rolling average of policing behavior, we establish a baseline of normal activity that is consistent with the rhythms of the year, such as festivals, holidays, schooling, and other events and states that bring meaning to neighborhoods. Finally, the weighted deviation approach to measurement establishes a counterfactual impervious to simultaneity, since the prior year's police activity cannot be determined by crime in the focal year. This measurement strategy is further bolstered by adjusting for dynamic and stable sources of potential confounding we include in our analytic models.

4.4. Control variables

We control for a variety of potentially confounding effects within and between neighborhoods. We include three measures that vary within but not between neighborhoods to account for climate-related influences that could alter public activity. First, *temperature* is based on aggregating daily averages (in Fahrenheit) in Denver to the weekly level. Second, *precipitation* is based on aggregating daily averages (in inches) in Denver to the weekly level. Finally, *AQI*, an overall measure of air quality that takes into consideration five major air pollutants (ground level-ozone, particle ozone, carbon monoxide sulfur dioxide, and nitrogen dioxide), is based on aggregating daily values in Denver to a mean weekly level.

We also include two measures that more directly tap aspects of public activity. First, *motor vehicle accidents* varied within and between neighborhoods, and were measured as the weekly count of motor vehicle accidents in 2020. Second, we used data from *OpenTable* as a proxy for public mobility, which varies within but not between neighborhoods. OpenTable data provided the weekly year-over-year percent change in restaurant reservations in Denver, beginning with the 7th week of 2020.[11]

We include a range of time-stable structural covariates commonly found in neighborhood-level research in criminology (e.g., Krivo et al., 2018): *disadvantage, percent Black, percent Hispanic, immigration,* and *population.* Since many neighborhood-level structural characteristics were highly correlated, consistent with macro-level criminological research (McCall, Land, and Parker, 2010), we constructed a *disadvantage* score derived from the weighted sum of standardized versions of percent poverty, percent less than a high school diploma, percent less than a bachelor's

[11] We imputed the first six weeks of OpenTable data with the mean of weeks 7-10 (M = -1.643).

degree, median home value (reverse coded), and median household income (reverse coded). *Percent Black* and *Percent Hispanic* represent the percentage of each neighborhood recorded as Black or African American and Hispanic (any race), respectively, as reported in the ACS 2014-18 5-year estimates. *Immigration* is an index that reflects the weighted sum of standardized scores of the percent of each neighborhood that speaks English less than very well, the percent who were foreign-born, and the percent who are non-citizens. Finally, *population* is the number of people residing in each neighborhood.

4.5. Analytic strategy

We analyze the relationship between policing and crime across the 4,056 neighborhood-weeks in Denver in 2020 (78 neighborhoods * 52 weeks). Our analysis proceeds in four stages. First, we begin descriptively by contrasting our two measures of reported crime and four measures of discretionary police behaviors in 2020 to prior years. Second, we formalize this descriptive account by estimating the effects of the two exogenous shocks on policing using mixed effects models, where weeks are nested within neighborhoods. Here, we determine if there were greater deviations in policing in 2020 relative to prior years.

Third, we extend the formal modeling to crime. Owing to the measurement and distribution of reported property and violent crime, we use mixed effects Poisson models to include random intercepts and slopes for the COVID period, Floyd period, and hypothesized mediators (i.e., discretionary police behaviors) nested within neighborhoods. Subsequently, the random effect coefficients for discretionary police behaviors are extracted to examine the variation across neighborhoods. Then, neighborhood-level random effects are added to the fixed effect coefficient for discretionary police behavior to estimate the final expected change in outcome variables associated with a one-unit increase in the discretionary police behavior variable for each neighborhood. Finally, interaction models are estimated to determine if the policing mediators were moderated by the COVID-19 and Floyd exogenous shocks, respectively.

All mixed effects models include mean-centered control variables for measures that vary between and/or within neighborhoods, and these associations with the outcomes can be found in the Supplemental Appendix. Diagnostics did not indicate any concerns related to multicollinearity (all variance inflation factors < 4). Analyses were conducted using Stata v17.0 and R 4.2.2.

5. RESULTS

5.1. Descriptive statistics on changes in crime and policing in 2020

To begin, we examine the extent to which crime and policing in Denver neighborhoods changed in 2020 relative to prior years. Figure 1 compares the weekly cumulative count of violent and property crimes in 2020 against the weekly cumulative count of violent and property crimes, on average, between 2016 and 2019 at the city level. Focusing first on violent crime (left panel), the figure illustrates there were 66 more violent crimes in the first 10 weeks of 2020 than there had typically been from 2016 to 2019 (652 vs. 586), an increase of 6.6 per week. A similar pattern was observed during the COVID period (weeks 11 – 21), when in 2020 there were 758 reported violent crimes versus 707 on average from 2016 to 2019 (a difference of +51), about 4.6 more per week. Then, from the week of George Floyd's murder through the remainder of 2020, Denver experienced 2,810 violent crimes, whereas during the same period on average from 2016 to 2019 there were 2,399 violent crimes, a difference of +411, translating to 13.3 more violent crimes weekly. Turning to property crime (right panel), the figure shows there were 4,965 property crimes through the first 10 weeks of 2020, compared to about 4,629 on average from 2016 to 2019 (an increase of 336). During the COVID period of 2020, there were 6,170 property crimes – about 812 more than the 2016-19 weighted average during the same eleven-week stretch. Finally, there were 22,651 property crimes throughout the remainder of 2020, compared to the 2016-19 weighted average of 16,596 (a difference of +6,055). All told, Denver experienced an additional 528 violent crimes and 7,203 property crimes in 2020 – increases of 14.3% and 27.1%, respectively.

Just like the rise in violent and property crime was concentrated temporally, it was also experienced unequally across Denver's 78 neighborhoods. Violent crime increased in 55 neighborhoods and decreased in 23. Central Park saw the largest increase (+53), while West Highland saw the largest decrease (-12). The median neighborhood experienced 4 additional violent crimes (see Appendix Figure S1). Meanwhile, property crime increased in 70 of Denver's neighborhoods and decreased in eight. Five Points saw the largest increase (+411), whereas Auraria saw the largest decrease (-101), while the median neighborhood encountered 70 additional property crimes (see Appendix Figure S2).

FIGURE 1. Weekly cumulative violent crime (left) and property crime (right) counts in Denver: 2020 vs. 2016-2019 weighted average.

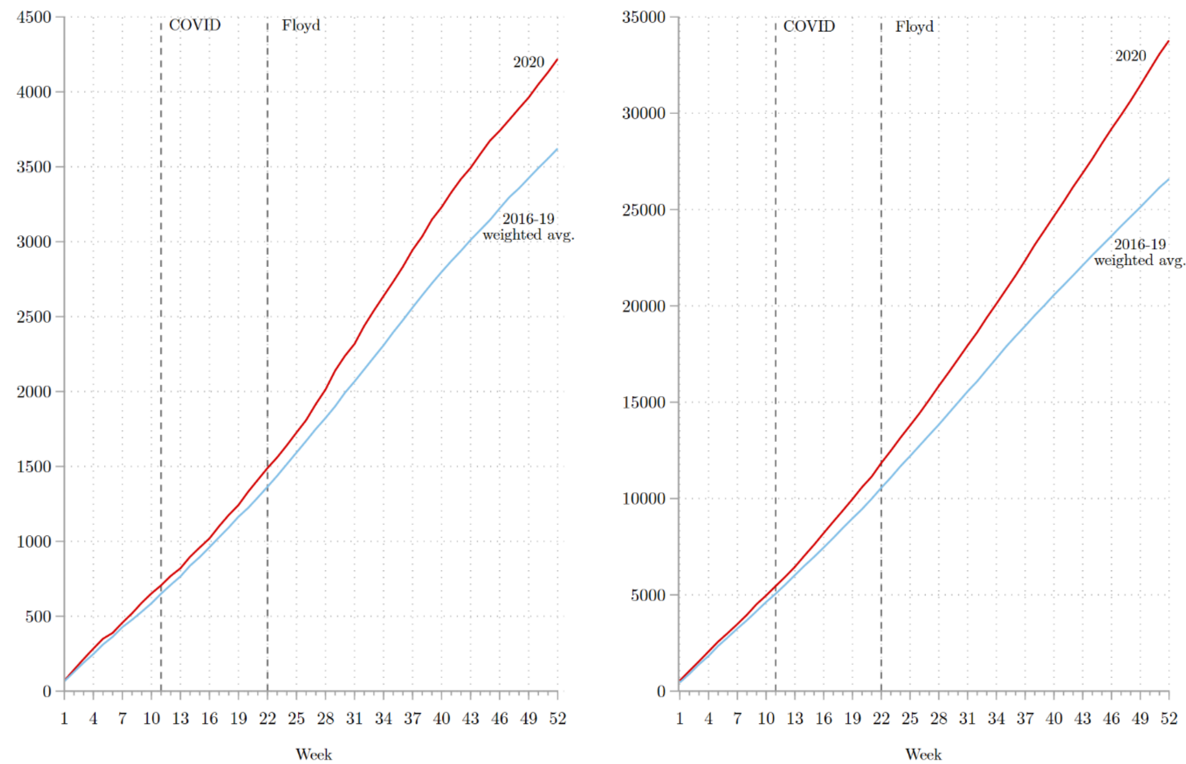
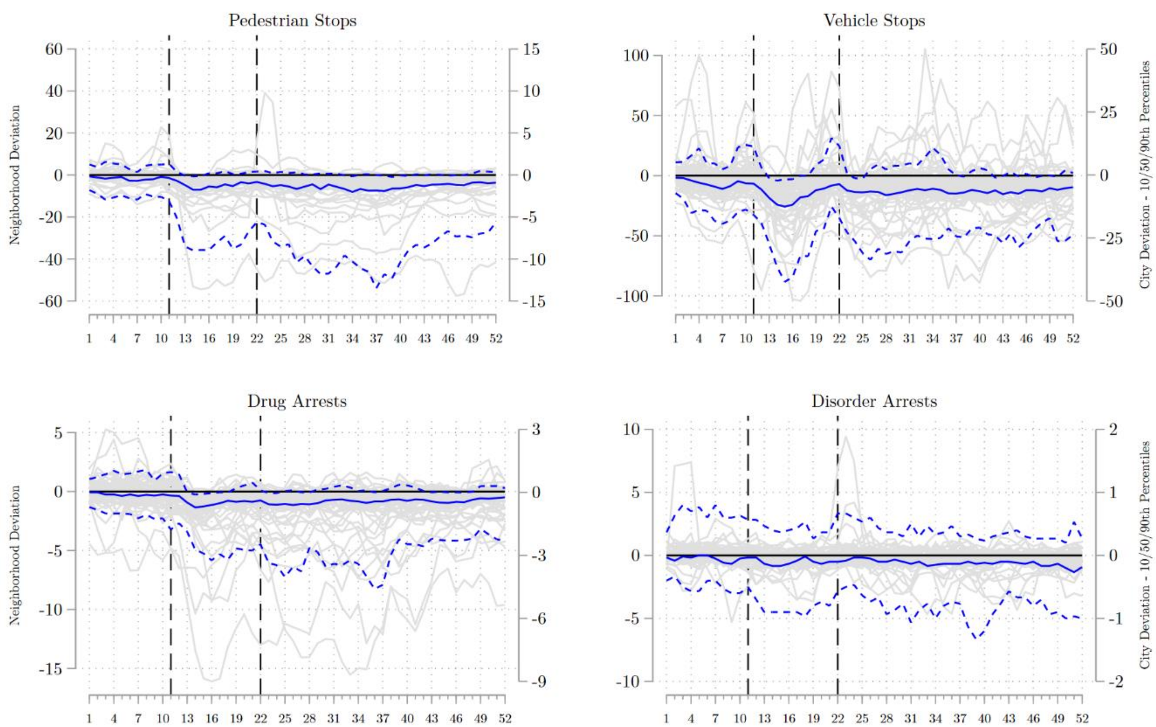


Figure 2 plots neighborhood-week deviations (from the 2016-2019 weighted average) in pedestrian stops, vehicle stops, drug-related arrests, and disorder arrests. Neighborhoods are depicted by 78 grey lines plotted on the primary *y*-axis; blue lines indicate the city-wide 10th, 50th, and 90th percentiles, respectively, plotted on the secondary *y*-axis. Dashed vertical lines represent the onset of the COVID-19 pandemic and the murder of George Floyd. A clear story emerges from Figure 2: Denver police officers engaged in significantly fewer discretionary activities in 2020. They made 11,150 pedestrian stops (down 50% from the weighted 4-year average of 22,289), 48,776 vehicle stops (down 40% from 81,357), 2,112 drug arrests (down 62% from 5,538), and 1,739 disorder arrests (down 26% from 2,343). The bulk of these reductions were in response to the COVID-19 and George Floyd shocks. In the first 10 weeks of 2020, police were making 10% fewer pedestrian stops, 14% fewer vehicle stops, 12% fewer drug arrests, and 2% more disorder arrests than they had been, on average, in the four years prior. Then, between the onset of COVID-19 and the murder of George Floyd, they made 52% fewer pedestrian stops, 47% fewer vehicle stops, 72% fewer drug arrests, and 27% fewer disorder arrests. From the week of Floyd's murder throughout the remainder of 2020, police made 61% fewer pedestrian stops, 48% fewer vehicle stops, 74% fewer drug arrests, and 34% fewer disorder arrests. Thus, by year's end, police had made 11,139 fewer pedestrian stops, 32,581 fewer vehicle stops, 3,416 fewer drug arrests, and 604 fewer disorder arrests.

Here again, reductions in police discretionary behaviors were not experienced equally across Denver neighborhoods. After the initial COVID-19 shock, the median neighborhood week was characterized by 1.3 fewer pedestrian stops (range -57.5 to 39.17), 6.7 fewer vehicle stops (range -104.3 to 105.4), 0.4 fewer drug arrests (range -16.1 to 2.7), and 0.13 fewer disorder arrests (range -4.7 to 9.4).

FIGURE 2. Weekly deviations (from the 2016-2019 weighted average) in police discretionary activities at the neighborhood level.



*Note: Week 11 = Start of COVID period; Week 22 = Start of FLOYD period

5.2. Mixed effects models predicting deviations in discretionary policing behavior

Table 2 displays the results of four mixed effects models that regressed police discretionary behaviors onto variables reflecting the start of the COVID and Floyd periods, respectively (along with all the control variables). The constant captures the neighborhood-week mean deviation in the outcomes pooled across the pre-COVID period, when all control variables are set to zero. Only drug and disorder arrests differed statistically in the pre-COVID period, and in opposite directions. Pedestrian and vehicle stops, while negative, were indistinguishable from zero. This period also serves as the reference category for the pooled-periods of exogenous shocks. During the COVID period, there was a much greater reduction in police activity than in the pre-COVID period, relative to the prior 4-year weighted average. Police made roughly 3.15 fewer pedestrian stops (constant=-.584 + COVID coefficient=-2.565), 9.05 fewer vehicle stops, 0.82 fewer drug arrests, and 0.26 fewer disorder arrests each week. These reductions persisted during the Floyd period, when again using the pre-COVID period as reference category, the police made roughly 3.24 fewer pedestrian stops, 10.13 fewer vehicle stops, 0.93 fewer drug arrests, and 0.47 fewer disorder arrests each week. These trends in policing activity are not explained by climate-related activity or population mobility but reflect shifts that are timed with two exogenous shocks that have come to define the experiences of citizens and police alike in 2020. The variance components—as reflected in standard deviations—also reveal there was significant variation across neighborhoods in the pre-COVID period, and there was dramatically more variation in the COVID and Floyd periods for pedestrian stops (+165% and +221%), vehicle stops (+31% and +53%), and drug arrests (+73% and +97%), but not disorder arrests. Put simply, even while there were wholesale reductions in policing across Denver’s neighborhoods, they were experiencing them very differently. The next stage of analysis aims to determine if those experiences corresponded with consequences for violent and property crime in neighborhoods.

TABLE 2. Mixed effects linear regression models of 4-year weighted average 3-week deviations of police activity in Denver neighborhoods in 2020 (N = 78, N * T = 4,056).

Components	Pedestrian stops	Vehicle stops	Drug arrests	Disorder arrests
Fixed	b (SE)	b (SE)	b (SE)	b (SE)
COVID period	-2.565***	-6.785***	-0.456*	-0.399***
	(0.723)	(1.782)	(0.187)	(0.081)
Floyd period	-2.656**	-7.152***	-0.615**	-0.329***
	(0.823)	(1.769)	(0.196)	(0.068)
Constant	-0.584	-2.262	-0.367***	0.135*
	(0.313)	(1.257)	(0.110)	(0.065)
Random	SD	SD	SD	SD
Neighborhoods	2.179	8.923	0.821	0.449
COVID period	5.782	11.700	1.423	0.427
Floyd period	7.009	13.688	1.62	0.445

***p<0.001; **p<0.01; *p<0.05; all models control for disadvantage, % Black, % Hispanic, immigration, total population, number of motor vehicle accidents, precipitation, temperature, air quality, and Open Table reservations; all control variables are mean-centered; full results available in Appendix Table S3.

5.3. Mixed effects models predicting property and violent crime

Tables 3 and 4 provide the results of mixed-effects Poisson models predicting violent and property crime. In both sets of results the pre-COVID period continues to serve as the reference category for the COVID and Floyd periods, with the expectation that the inclusion of the policing mediators should attenuate the relationship between these periods and crime. We also interact the mediators with the periods to determine if the effects of policing are generalizable across the year or confined to one or more of the specific periods.

We begin with violent crime, as reported in Table 3. The naïve coefficients (not reported in tabular form) for the period effects are 0.067 (p=0.232) and 0.326 (p<.001) for the COVID and Floyd periods, respectively, ignoring seasonality in climate, population mobility, and between-neighborhood compositional differences. Once controlling for these factors, as displayed in the left-most results column in Table 3, the coefficients reduce in magnitude, where neighborhood-week violence during COVID is indistinguishable statistically from the pre-COVID period, while there were still 0.226 more reported violent crimes per neighborhood week in the Floyd period. Recall, these effects are observed independent of weather and population mobility changes, so more violent crime during the Floyd period cannot be explained by the period overlapping with the summer months or changes in peoples' travel patterns associated with COVID.

TABLE 3. Mixed effects Poisson regression models of violent crime counts in Denver neighborhoods in 2020 (N = 78, N * T = 4,056).

		Direct effects models			
Components Fixed	Periods only b (SE)	Mediator: Pedestrian stops b (SE)	Mediator: Vehicle stops b (SE)	Mediator: Drug arrests b (SE)	Mediator: Disorder arrests b (SE)
COVID period	0.091	0.045	0.074	0.084	0.071
	(0.116)	(0.117)	(0.118)	(0.118)	(0.119)
Floyd period	0.226**	0.185*	0.213*	0.222*	0.213*
	(0.088)	(0.090)	(0.089)	(0.090)	(0.089)
Mediator	-	-0.022***	-0.002	-0.004	-0.011
	-	(0.003)	(0.001)	(0.011)	(0.020)
Constant	-0.534***	-0.529***	-0.536***	-0.529***	-0.524***
	(0.127)	(0.127)	(0.127)	(0.127)	(0.128)
Random	SD	SD	SD	SD	SD
Neighborhoods	0.899	0.889	0.895	0.894	0.903
COVID period	0.109	0.127	0.166	0.167	0.165
Floyd period	0.177	0.205	0.194	0.208	0.190
Mediator	-	0.012	0.002	0.015	0.020
		Interaction effects models			
Components Fixed	Periods only b (SE)	Mediator: Pedestrian stops b (SE)	Mediator: Vehicle stops b (SE)	Mediator: Drug arrests b (SE)	Mediator: Disorder arrests b (SE)
COVID period	0.091	0.023	0.044	0.084	0.057
	(0.116)	(0.119)	(0.121)	(0.118)	(0.119)
Floyd period	0.226**	0.158	0.202*	0.210*	0.199*
	(0.088)	(0.091)	(0.091)	(0.091)	(0.089)
Mediator	-	0.001	0.004	0.010	0.039
	-	(0.010)	(0.003)	(0.030)	(0.035)
COVID period	-	-0.011	-0.008*	0.006	0.00003
* Mediator	-	(0.012)	(0.003)	(0.030)	(0.058)
Floyd period *	-	-0.023*	-0.005	-0.022	-0.068
Mediator	-	(0.011)	(0.003)	(0.029)	(0.041)
Constant	-0.534***	-0.507***	-0.524***	-0.524***	-0.513***
	(0.127)	(0.127)	(0.127)	(0.128)	(0.128)
Random	SD	SD	SD	SD	SD
Neighborhoods	0.899	0.886	0.885	0.901	0.897
COVID period	0.109	0.183	0.191	0.151	0.142
Floyd period	0.177	0.222	0.194	0.214	0.169
Mediator	-	0.010	0.002	0.012	0.019

***p<0.001; **p<0.01; *p<0.05; all models control for disadvantage, % Black, % Hispanic, immigration, total population, number of motor vehicle accidents, precipitation, temperature, air quality, and Open Table reservations; all control variables are mean-centered; full results available in Appendix Tables S4 and S5.

The introduction of discretionary policing behavior mediators revealed a mixed story. On the one hand, pedestrian stops behaved in a manner consistent with theoretical expectations. Greater (increased) deviations in pedestrian stops from the prior 4-year weighted average were associated with 0.022 (p<.001) lower units of violent crimes per neighborhood-week, which translated to about a 2.2% reduction. The coefficient for the Floyd period reduced by about 19% in the pedestrian stops model. On the other hand, deviations in vehicle stops and both indicators of arrests exhibited statistically null relationships with violent crime. The evidence suggests that despite large-scale alterations in policing, the changes that were consequential to neighborhood violence pertained to pedestrian stops. In other words, neighborhoods that experienced reductions in pedestrian stops also experienced increases in violent crime.

The interaction models in the bottom panel of Table 3 assist in understanding that changes in discretionary police behavior were not general but temporally anchored to the three periods under investigation. For all four measures, deviations in police behavior were unrelated statistically to violent crime in the pre-COVID period. During the COVID period, however, only vehicle stops were associated with violence, where a one-unit negative deviation from the prior weighted-year corresponded with a 0.004 (b=-.008, p= 0.019), or 0.4 percent greater, expected count of violent crime across neighborhood-weeks (3.2% increase when fixed to 7.96, the mean neighborhood-week deviation). In contrast, pedestrian stops were associated with violence in the Floyd period, where a one-unit negative deviation in pedestrian stops corresponded with an increase of violent crime by 0.022 units (b=-0.023, p= 0.038), translating to a 6.1% increase in the expected count of violent crime across neighborhood-weeks when fixed to the mean deviation of 2.71.

Table 4 displays similar results, the main difference being that the ten mixed-effects Poisson models predict our second outcome, property crime. Here, the coefficients for the period effects on property crime increase in magnitude from 0.166 (p<.001) and 0.422 (p<.001) in the naïve models (not reported in tabular format) to 0.207 (p<.001) and 0.427 (p<.001) for the COVID and Floyd periods, respectively. The primary difference is that the COVID period was statistically significant. The results of the mediator analysis revealed that only drug arrest activity was associated with property crime—each one-unit negative deviation in drug arrests corresponded with .027 units of property crime, about a 2.7% increase. In contrast, there was no evidence to

TABLE 4. Mixed effects Poisson regression models of property crime counts in Denver neighborhoods in 2020 (N = 78, N * T = 4,056).

		Direct effects models			
Components	Periods only	Mediator: Pedestrian stops	Mediator: Vehicle stops	Mediator: Drug arrests	Mediator: Disorder arrests
Fixed	b (SE)	b (SE)	b (SE)	b (SE)	b (SE)
COVID period	0.207***	0.195***	0.207***	0.174***	0.199***
	(0.049)	(0.049)	(0.048)	(0.049)	(0.050)
Floyd period	0.427***	0.420***	0.426***	0.403***	0.423***
	(0.043)	(0.043)	(0.043)	(0.045)	(0.044)
Mediator	-	-0.001	-0.0004	-0.027**	-0.010
	-	(0.003)	(0.001)	(0.010)	(0.015)
Constant	1.591***	1.592***	1.590***	1.597***	1.592***
	(0.078)	(0.078)	(0.078)	(0.078)	(0.078)
Random	SD	SD	SD	SD	SD
Neighborhoods	0.647	0.644	0.645	0.641	0.640
COVID period	0.227	0.223	0.211	0.226	0.226
Floyd period	0.267	0.266	0.265	0.282	0.267
Mediator	-	0.006	0.002	0.046	0.066

		Interaction effects models			
Components	Periods only	Mediator: Pedestrian stops	Mediator: Vehicle stops	Mediator: Drug arrests	Mediator: Disorder arrests
Fixed	b (SE)	b (SE)	b (SE)	b (SE)	b (SE)
COVID period	0.207***	0.192***	0.203***	0.172***	0.200***
	(0.049)	(0.049)	(0.048)	(0.050)	(0.050)
Floyd period	0.427***	0.410***	0.427***	0.395***	0.424***
	(0.043)	(0.044)	(0.044)	(0.045)	(0.044)
Mediator	-	0.007	0.0002	-0.004	-0.018
	-	(0.006)	(0.002)	(0.017)	(0.023)
COVID period *	-	-0.007	-0.001	-0.023	0.004
Mediator	-	(0.006)	(0.002)	(0.016)	(0.029)
Floyd period *	-	-0.010	-0.001	-0.029	0.010
Mediator	-	(0.006)	(0.002)	(0.016)	(0.020)
Constant	1.591***	1.598***	1.591***	1.599***	1.591***
	(0.078)	(0.078)	(0.078)	(0.078)	(0.078)
Random	SD	SD	SD	SD	SD
Neighborhoods	0.647	0.643	0.645	0.647	0.641
COVID period	0.227	0.223	0.211	0.226	0.227
Floyd period	0.267	0.264	0.264	0.284	0.268
Mediator	-	0.005	0.002	0.046	0.065

***p<0.001; **p<0.01; *p<0.05; all models control for disadvantage, % Black, % Hispanic, immigration, total population, number of motor vehicle accidents, precipitation, temperature, air quality, and Open Table reservations; all control variables are mean-centered; full results available in Appendix Tables S6 and S7.

support deviations in pedestrian stops, vehicle stops, or disorder arrests as a source of variation in property crime across Denver neighborhoods.

The bottom panel of Table 4 contains the interaction results, which assess whether there were periods where policing effects on property crime were more or less acute. The results indicate that none of the measures of police activity varied significantly across either the COVID or Floyd periods.

Both Tables 3 and 4 also reveal that the effects of policing were not universal across neighborhoods. The variance components reported in the bottom of each panel revealed significant variation. The standard deviation for the policing coefficients ranged from as little as 0.002 (vehicle stops) to as much as 0.065 (disorder arrests). This begs the question: what are the characteristics of neighborhoods that were more impacted by de-policing in 2020? The final step of our analysis addresses this question.

5.4. Differences in policing effects across neighborhoods

Figure 3 displays the neighborhood random effects of pedestrian stops on violent crime, which ranged from -.047 to .004. Pedestrian stops had an inverse relationship with violent crime in all but two neighborhoods: Montebello and Five Points. Based on these coefficients, we split the neighborhoods into three groups – *stronger effects*, *moderate effects*, and *weaker effects* (denoted as red, blue, and grey dots, respectively, in Figure 3) – and ran a series of *t*-tests to determine if there were meaningful structural (e.g., poverty, median household income) or compositional (e.g., percent Black, immigration) differences among them. The results are shown in the top panel of Table 5. Counter to our theoretical reasoning, neighborhoods where the reduction in pedestrian stops had the strongest effect on violent crime had significantly *lower* levels of poverty, on average. They were also characterized by higher median home values and median household incomes, lower scores on our *disadvantage* index, and smaller concentrations of Hispanic residents.

Figure 4 plots the neighborhood random effects of drug arrests on property crime, which ranged from -.095 to .065. Drug arrests had an inverse relationship with property crime in 66 of the 78 neighborhoods. Again, we split the neighborhoods into three groups (stronger, moderate, and weaker effects of drug arrests on property crime) and ran *t*-tests to determine if there were significant structural or compositional differences among them. The results are shown in the

FIGURE 3. Neighborhood random effects of pedestrian stops on violent crime.

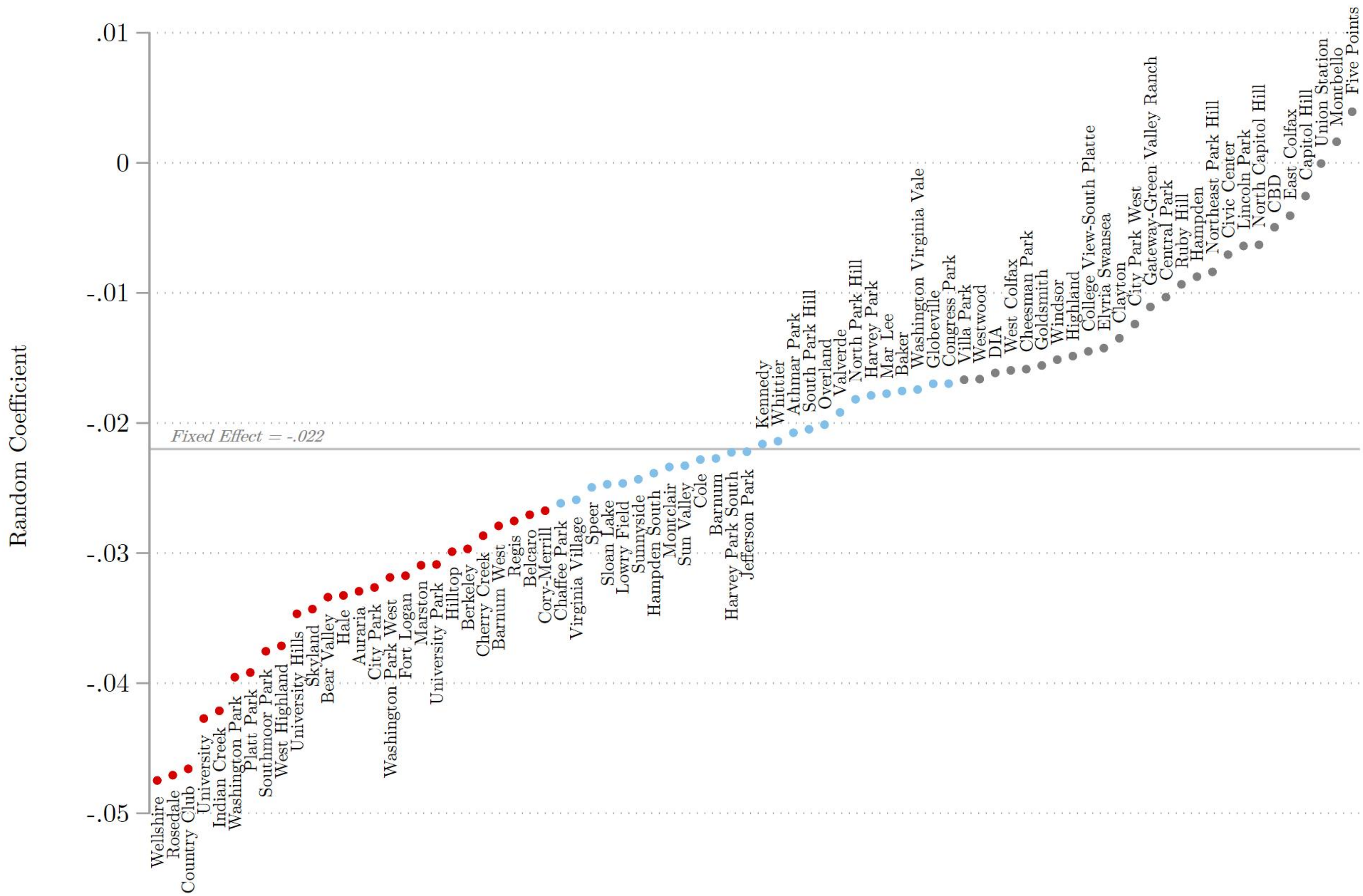


TABLE 5. Structural and compositional characteristics of neighborhoods where the significant effects of police mediators on crime in 2020 were stronger, moderate, and weaker.

	Effect of Pedestrian Stops on Violent Crime								
	Stronger (n=26) "S" (Range: -.047 to -.027)			Moderate (n=26) "M" (Range: -.026 to -.017)			Weaker (n=26) "W" (Range: -.017 to .004)		
Variable	Mean	SD	Differences[a]	Mean	SD	Differences[a]	Mean	SD	Differences[a]
Violent Crime	13.42	7.55	M**, W**	42.04	14.67	S**, W**	106.85	59.84	S**, M**
Property Crime	257.00	129.44	M**, W**	356.31	168.06	S**, W**	686.15	396.12	S**, M**
Population	9,021	9,069		8,959	4,488		8,690	4,561	
% Poverty	9.80	5.34	M**, W**	17.18	14.85	S**	15.38	7.99	S**
% ≥ HSD	87.65	19.77		83.94	13.26		86.04	12.80	
% ≥ Bachelors	31.16	12.85		26.84	12.54		28.79	10.48	
Med Home Val	450,605	191,811	M*	369,594	126,772	S*	397,333	166,642	
Med HH Income	81,049	30,436	M*	65,015	27,570	S*	70,256	27,438	
Disadvantage	-0.28	0.96	M*	0.23	0.97	S*	0.05	0.88	
%Black	6.63	8.20		8.05	9.92		9.16	8.96	
%Hispanic	22.51	18.53	M*	33.37	23.51	S*	28.23	23.96	
Immigration	-0.25	0.85		0.20	1.07		0.06	0.99	

	Effect of Drug Arrests on Property Crime								
	Stronger (n=26) "S" (Range: -.095 to -.040)			Moderate (n=26) "M" (Range: -.040 to -.014)			Weaker (n=26) "W" (Range: -.014 to .065)		
Variable	Mean	SD	Differences[a]	Mean	SD	Differences[a]	Mean	SD	Differences[a]
Violent Crime	37.00	39.03	W**	38.77	40.99	W**	86.54	61.36	S**, M**
Property Crime	307.04	234.34	W**	348.85	181.59	W**	643.58	386.03	S**, M**
Population	10,130	8,676		6,906	3,855	W**	9,634	5,634	M**
% Poverty	12.14	7.76		16.35	14.81		13.86	8.19	
% ≥ HSD	85.37	20.20		85.20	14.52		87.05	12.06	
% ≥ Bachelors	31.02	12.61		26.35	12.70		29.41	11.27	
Med Home Val	606,406	143,134		660,367	193,781		647,995	166,342	
Med HH Income	102,505	24,786		110,340	31,660		104,274	31,995	
Disadvantage	-.14	.86		.18	1.07		-.04	.98	
%Black	7.11	7.97		8.81	9.56		7.92	8.51	
%Hispanic	25.18	19.99		31.76	25.90		27.17	22.27	
Immigration	-.12	.88		.13	1.20		-.004	.91	

[a] Column denotes statistically significant group differences, based on independent sample t-tests, where S = stronger, M = moderate, and W = weaker. Ranges are for each group's *direct* + *random* effect.
** $p < .05$, * $p < .10$

bottom panel of Table 5. Here, the only significant difference was *total population*, where neighborhoods in the "moderate" group were significantly less populous than those in the "weaker" group.[12]

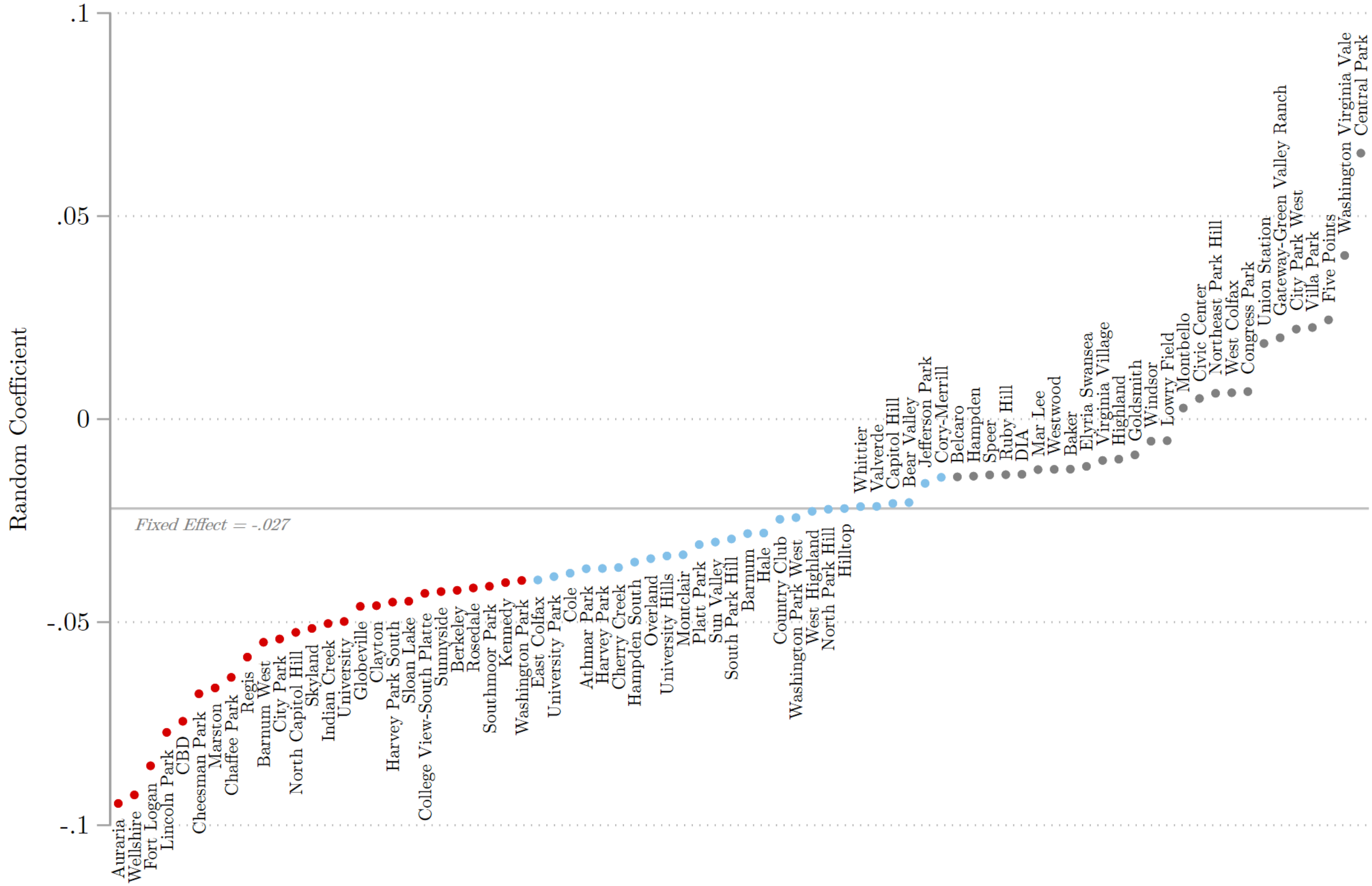
6. DISCUSSION

People will look back on 2020 as one of the most surreal years of their lives. Fundamental changes in our routine activities (Massenkoff & Chalfin, 2022) and how police respond to crime (Rosenfeld, Lopez & Abt, 2021) occurred throughout the year. Notably, police in many cities "pulled back" during COVID and following the Floyd protests (see e.g., Cassella et al., 2022; Lum et al., 2022a; White et al., 2021). Prior work tells us a sudden withdrawal of policing is often accompanied by an uptick in violence, but because COVID overlapped with much of the Floyd period, it is difficult to determine which was more consequential in terms of the surge in violence that occurred in many places, including Denver, throughout the latter half of 2020. Furthermore, the endogeneity problem (i.e., agencies deploy resources in response to fluctuations in crime) looms large but is frequently ignored in this line of research (Johnson & Roman, 2022). We addressed these problems at the neighborhood-level, where policing conceivably matters most. While 2020 was an atypical year in many respects, looking back, it offered two unique and independent exogenous shocks that could have contributed to police pulling back from proactivity but for very different reasons. In this way, 2020 presented an opportunity to disentangle the extent to which police pullbacks impact crime. This is an important public policy question as we have debates about the role of policing in our society and how we can (or if we should) reimagine policing (Lum et al., 2022b). Our study provides several takeaways that warrant further discussion.

The analysis revealed that policing has an impact on crime. Specifically, reductions in pedestrian stops during 2020 were associated with more violent crimes in Denver's neighborhoods. Reductions in drug arrests were associated with increases in property crimes. The results also

[12] One potential concern with our analysis is that our outcomes – reported violent and property crimes – are subject to changes in actual crime as well as citizens' propensity to report crimes. This might be especially problematic for our analysis of differences in policing effects across neighborhoods. For example, residents of wealthier neighborhoods that typically experience few crimes might be more fearful – and therefore more likely to report criminal victimizations – upon seeing news reports about rising crime in Denver than those in neighborhoods where crime is not atypical (Sampson & Jeglum-Bartusch, 1988). As a robustness check, we re-ran all analyses using *aggravated assault* and *motor vehicle theft* as outcomes, since these are more reliably reported and should thus be less sensitive to the exogenous shocks experienced in 2020. The results (available in Appendix Tables S8-S11) were consistent with our main models.

FIGURE 4. Neighborhood random effects of drug arrests on property crime.



suggest that increases in these types of police discretionary behavior would be associated with crime reductions. For example, the Denver data showed that one fewer pedestrian stop (compared to prior years during the same time) was associated with 2.1% more violent crimes during the Floyd period. While meaningful, we cannot speak to whether this effect is linear and the result should be interpreted with caution (e.g., 10 more pedestrian stops over prior years is not likely associated with a 23% reduction in violent crime). However, in Chicago from 2004 to 2018, Skogan (2023, p. 207) estimated that every 7,000 or so pedestrian stops was associated with one fewer murder and six fewer shootings city-wide.

Meanwhile, changes in traffic stop behavior and disorder-related arrests were unrelated to changes in violent or property crime trends in the main, direct effects models. These findings are consistent with the proactive policing literature and suggest that targeted or surgical use of police stops may provide more return on investment in the form of crime reductions (Braga et al., 2019; Chalfin et al., 2021; Hinkle et al., 2020). Pedestrian stops and drug arrest activity may be more likely to stem from problem-oriented and targeted enforcement of crime problems in Denver. Although we cannot determine if this is the case within our data, it is possible that such proactive policing strategies stand a better chance of impacting crime than random traffic stops and disorder arrests that are not part of a targeted enforcement strategy.

The results also advance our understanding of the impact of proactive policing by addressing the endogeneity problem that so often limits this type of research. The proactive policing mediators we used represent the deviation in policing behavior in a given week (during 2020) from what the average activity was from 2016-2019. As such, crime in a given week cannot be associated with what the police were doing on average during the four years prior to 2020. In this way, the results speak to the effect of proactive policing on crime after ruling out the possibility that changes in policing may be a function of the police responding to changes in crime trends.

Another looming issue within the policing literature is what happens when police “pull back” from proactive policing. We have little research on this question. What does exist is either focused on the city level, and thus subject to aggregation bias (Cassella et al., 2022; White et al., 2022), or constrained to changes in police staffing levels (Krahn & Kennedy, 1985), budget reductions (Piza & Chillar, 2021), or highly specific interventions like hot spots policing (Braga et al., 2019) and focused deterrence strategies (Braga et al., 2018). Our analysis is better able to speak to what happens when police pull back from their everyday proactive policing activities, which may have a more proximate effect on the neighborhoods officers police. We examined what happened to policing and crime after two separate exogenous shocks, COVID-19 and Floyd's murder. We anticipated that both would lead to police pullbacks and, in turn, would be associated with more crime, but for different reasons.

With COVID-19, the lockdowns that occurred in Denver and cities throughout the U.S. fundamentally changed police behavior. Police agencies, including the Denver Police Department, instructed their officers to only engage in absolutely necessary police actions (Lum et al., 2022a). As such, officers were instructed to pull back from proactive police activity. Indeed, we witnessed Denver police officers engage in 52% fewer pedestrian stops, 47% fewer vehicle stops, 72% fewer drug arrests, and 27% fewer disorder arrests per week during the COVID-19 period. Denver police pulled back in response to COVID, but as discussed above, only reductions in pedestrian stops and drug arrests were associated with increases in violent and property crime respectively. The observed reductions in pedestrian stops during the COVID period were associated with a 6.4% increase in violent crime, whereas the reductions in drug arrests were associated with a 1.7% increase in property crime. Thus, when the police are forced to de-police, we have evidence that impacts crime trends.

Conversely, we expected de-policing in the Floyd period as police responded to public criticism of their profession. After Floyd's murder and subsequent protests, we saw 61% fewer pedestrian stops, 48% fewer vehicle stops, 74% fewer drug arrests, and 34% fewer disorder arrests per week. This de-policing was associated with upticks in violent and property crime during the Floyd period. Specifically, observed reductions in pedestrian stops and drug arrests were associated with a 7.4% increase in violent crime and a 1.7% increase in property crime during the Floyd period, respectively. This suggests that police pullbacks that stem from public criticism occur and can impact crime trends. The magnitude of these crime increases were similar to those observed during the COVID period. In short, de-policing can lead to more crime.

Much of our discussion in the beginning of this paper focused on the uneven distribution of crime and policing activity in marginalized and disadvantaged communities, particularly neighborhoods of color. Because more crime and police activity occur in these locations, we anticipated that any de-policing effect on crime trends would be disproportionately felt in disadvantaged communities. We were wrong. Contrary to our expectations, reductions in pedestrian stops were more strongly associated with violent crime increases in more affluent neighborhoods. Our data are unable to speak to specifically why this occurred, but there are several possibilities that should be explored by future research. For starters, it is possible that crime trend changes in affluent neighborhoods are more noticeable than in disadvantaged communities with already high levels of violence and property crime. If this is true, what we may be observing is small crime numbers in affluent neighborhoods making it appear that changes impact such communities to a greater degree. Conversely, disadvantaged communities likely had much greater quantity of (likely proactive) policing activity prior to COVID and Floyd and, therefore, any pullback would be less noticeable. If so, it may not be the case that police pullbacks do not lead to crime increases, but rather that much larger reductions in police proactivity are required in disadvantaged communities before such a relationship is observable.

As we move forward, our results and the uncertainty about the neighborhood effects underscores the need for more research aimed at determining the amount of policing in various contexts that is "just right" (Sherman, 2022). We need to know how much and what types of policing activity are needed in what neighborhoods and for what types of problems. It is possible that pulling police out of certain communities, or at least reducing the amount of high-discretion enforcement, may be a good thing. Or, it is possible that results like ours simply reiterate a long line of prior work demonstrating that more intentional, focused, and evidence-based policing is needed. Disadvantaged communities likely need better policing, not simply less of it. In any event, we hope the neighborhood differences documented herein will provide ground for additional work on how crime variation impacts political preferences around crime control (Bateson, 2012).

Our findings should be interpreted with a few limitations in mind. First, Denver was just one of many jurisdictions nationwide that experienced the COVID and Floyd shocks to policing and criminal behavior. The generalizability of our findings is thus fair to question, and we encourage researchers to replicate our work in other jurisdictions. Second, our measure of *violent crime* was strictly a count of murders and nonnegligent manslaughters, robberies, and aggravated assaults – it did not include other crimes such as weapons offenses or other outcomes such as guns seized. Changes in police stops ostensibly could be associated with increased weapon carrying or trafficking, as well as a reduction in the rate at which officers take into custody people who are wanted for violent crimes. If so, our models likely underestimate the total effect of police stops on violence in the community. Third, our research questions were specifically about the effects of discretionary police behaviors on crime; it is possible that although vehicle stops were not associated with changes in crime, they enhance traffic safety (Fliss et al., 2020). Finally, we were unable to adjudicate precisely what was driving the pullback in police proactivity during the Floyd period. It might have been a function of short-staffing, due to the confluence of some officers being out sick with COVID and others being pulled off normal patrol assignments to work protest events. Alternatively, it may have been a product of "dissent shirking" (Chanin & Sheats, 2018) or officers opting to "lay low and avoid trouble" for a while (Van Maanen, 1973). Questions about *why* police pulled back need to be answered by future research. Such an understanding will prove useful if policymakers and practitioners want to find ways to intervene and prevent certain types of de-policing from leading to crime increases.

7. CONCLUSION

Does de-policing lead to more crime in communities? Our analysis of Denver's 2020 experience suggests the answer depends on what "policing" means and how much of it was occurring in the first place. In Denver, at least, pedestrian stops appear to have a suppression effect on violent crime and drug-related arrests appear to have a similar effect on property crime. However, Denver Police made ~32,000 fewer traffic stops in 2020 – a huge departure from the norm – and the reduction was unrelated to changes in crime. This suggests police can in fact reduce certain types of enforcement without disastrous consequences for violent and property crime, at least in the short term. Going forward, as we inevitably continue to debate the appropriate role of policing in society (Wickersham, 1931; Katzenbach, 1967; President's Task Force on 21st Century Policing, 2015), it is critical that we understand the effects of various types of policing on crime and communities (National Academies of Sciences, Engineering, and Medicine, 2018). We hope such discussions are guided by empirical evidence rather than emotion because lives are at stake.

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9. SUPPLEMENTAL APPENDIX

Contents

Table S1. Drug-related arrests.

UCR Code	Description
3501	Manufacture of a hallucinogenic drug
3503	Selling a hallucinogenic drug
3504	Possession of a hallucinogenic drug
3510	Selling heroin
3512	Possession of heroin
3520	Selling opium or an opium derivative
3522	Possession of opium or an opium derivative
3530	Selling cocaine
3532	Possession of cocaine
3540	Selling a synthetic narcotic drug
3542	Possession of a synthetic narcotic drug
3550	Possession of drug paraphernalia
3560	Selling marijuana
3562	Possession of marijuana
3563	Cultivation of marijuana
3570	Manufacture of methamphetamine
3571	Selling methamphetamine
3572	Possession of methamphetamine
3580	Manufacture of a barbiturate
3581	Selling a barbiturate
3582	Possession of a barbiturate
3599	Other dangerous drugs - PCS
3599	Manufacture or sell other dangerous drugs
4101	Manufacture of liquor
4102	Illegal sale of liquor
4104	Illegal possession of liquor
4105	Liquor law violation
4199	Liquor law violation - other
4200	Public intoxication

Table S2. Public disorder arrests.

UCR Code	Description
1316	Threatening to injure
1316	Harassment by stalking - domestic violence
1316	Threatening to injure
2901	Damaged business property
2902	Criminal mischief to private property
2903	Criminal mischief to public property
2999	Criminal mischief - other
2999	Criminal mischief to a motor vehicle
2999	Criminal mischief - graffiti
4001	Keeping a house of prostitution
4002	Procure for prostitution (trafficking, operating a bordello)
4002	Pimping for prostitution
4004	Engaging in prostitution
4004	Prostitution
4099	Aiding the act of prostitution
4099	Display for prostitution
5302	Inciting a riot
5303	Engaging in a riot
5309	Harassment
5309	Harassment - DV
5309	Obscene harassment
5309	Harassment - sexual in nature
5312	Disturbing the peace
5313	Curfew
5314	Loitering
5315	Vagrancy
5399	Public peace - other

Table S3. Mixed effects linear regression models of 4-year weighted average 3-week deviations of police activity in Denver neighborhoods in 2020.

Components	Pedestrian stops	Vehicle stops	Drug arrests	Disorder arrests
Fixed	b (SE)	b (SE)	b (SE)	b (SE)
COVID period	-2.565***	-6.785***	-0.456*	-0.399***
	(0.723)	(1.782)	(0.187)	(0.081)
Floyd period	-2.656**	-7.152***	-0.615**	-0.329***
	(0.823)	(1.769)	(0.196)	(0.068)
Motor vehicle accidents	0.007	0.030	0.013*	0.001
	(0.019)	(0.073)	(0.006)	(0.004)
Total population	0.00002	0.0001	0.00000	0.00000
	(0.00004)	(0.0001)	(0.00002)	(0.00001)
Disadvantage	0.190	-1.593	0.053	0.015
	(0.472)	(1.730)	(0.190)	(0.085)
% Black	-0.017	0.065	-0.004	0.002
	(0.031)	(0.112)	(0.012)	(0.005)
% Hispanic	-0.013	-0.175	-0.011	-0.003
	(0.025)	(0.090)	(0.010)	(0.004)
Immigration	-0.005	3.541*	0.022	-0.025
	(0.466)	(1.708)	(0.187)	(0.083)
Precipitation	-0.311	-0.169	-0.120	0.554*
	(1.091)	(4.244)	(0.338)	(0.231)
Temperature	-0.024***	0.081***	-0.006***	0.002
	(0.005)	(0.018)	(0.001)	(0.001)
AQI	0.001	0.480***	0.042***	-0.009
	(0.029)	(0.112)	(0.009)	(0.006)
Open Table	-0.008**	-0.006	0.001	-0.002**
	(0.003)	(0.011)	(0.001)	(0.001)
Constant	-0.584	-2.262	-0.367***	0.135*
	(0.313)	(1.257)	(0.110)	(0.065)
Random	SD	SD	SD	SD
Neighborhoods	2.179	8.923	0.821	0.449
COVID period	5.782	11.700	1.423	0.427
Floyd period	7.009	13.688	1.620	0.445

***p<0.001; **p<0.01; *p<0.05

Table S4. Mixed effects Poisson regression models of violent crime counts in Denver neighborhoods in 2020 (direct effects).

	Direct effects models				
Components	Periods only	Mediator: Pedestrian stops	Mediator: Vehicle stops	Mediator: Drug arrests	Mediator: Disorder arrests
Fixed	b (SE)	b (SE)	b (SE)	b (SE)	b (SE)
COVID period	0.091	0.045	0.074	0.084	0.071
	(0.116)	(0.117)	(0.118)	(0.118)	(0.119)
Floyd period	0.226**	0.185*	0.213*	0.222*	0.213*
	(0.088)	(0.090)	(0.089)	(0.090)	(0.089)
Mediator	-	-0.022***	-0.002	-0.004	-0.011
	-	(0.003)	(0.001)	(0.011)	(0.020)
Motor vehicle accidents	0.018**	0.017**	0.018**	0.018**	0.018**
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Total population	0.00001	0.00002	0.00001	0.00002	0.00001
	(0.00002)	(0.00002)	(0.00002)	(0.00002)	(0.00002)
Disadvantage	0.258	0.341	0.258	0.274	0.260
	(0.208)	(0.194)	(0.207)	(0.208)	(0.208)
% Black	-0.005	-0.014	-0.004	-0.006	-0.005
	(0.013)	(0.012)	(0.013)	(0.013)	(0.013)
% Hispanic	0.009	0.005	0.008	0.008	0.009
	(0.011)	(0.010)	(0.011)	(0.011)	(0.011)
Immigration	-0.259	-0.290	-0.239	-0.259	-0.262
	(0.205)	(0.180)	(0.204)	(0.205)	(0.205)
Precipitation	-0.849	-0.865	-0.847	-0.848	-0.834
	(0.445)	(0.445)	(0.445)	(0.445)	(0.446)
Temperature	0.006***	0.006**	0.006***	0.006***	0.006***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
AQI	-0.004	-0.003	-0.003	-0.004	-0.004
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Open Table	0.0003	0.0001	0.0003	0.0003	0.0002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Constant	-0.534***	-0.529***	-0.536***	-0.529***	-0.524***
	(0.127)	(0.127)	(0.127)	(0.127)	(0.128)
Random	SD	SD	SD	SD	SD
Neighborhoods	0.899	0.889	0.895	0.894	0.903
COVID period	0.109	0.127	0.166	0.167	0.165
Floyd period	0.177	0.205	0.194	0.208	0.190
Mediator	-	0.012	0.002	0.015	0.020

***p<0.001; **p<0.01; *p<0.05

Table S5. Mixed effects Poisson regression models of violent crime counts in Denver neighborhoods in 2020 (interaction effects).

	Interaction effects models				
Components Fixed	Periods only b (SE)	Mediator: Pedestrian stops b (SE)	Mediator: Vehicle stops b (SE)	Mediator: Drug arrests b (SE)	Mediator: Disorder arrests b (SE)
COVID period	0.091 (0.116)	0.023 (0.119)	0.044 (0.121)	0.084 (0.118)	0.057 (0.119)
Floyd period	0.226** (0.088)	0.158 (0.091)	0.202* (0.091)	0.210* (0.091)	0.199* (0.089)
Mediator	- -	0.001 (0.010)	0.004 (0.003)	0.010 (0.030)	0.039 (0.035)
COVID * Mediator	- -	-0.011 (0.012)	-0.008* (0.003)	0.006 (0.030)	0.00003 (0.058)
Floyd * Mediator	- -	-0.023* (0.011)	-0.005 (0.003)	-0.022 (0.029)	-0.068 (0.041)
Motor vehicle accidents	0.018** (0.006)	0.017** (0.006)	0.019*** (0.006)	0.017** (0.006)	0.018** (0.006)
Total population	0.00001 (0.00002)	0.00002 (0.00002)	0.00001 (0.00002)	0.00002 (0.00002)	0.00001 (0.00002)
Disadvantage	0.258 (0.208)	0.301 (0.193)	0.249 (0.206)	0.268 (0.207)	0.261 (0.208)
% Black	-0.005 (0.013)	-0.008 (0.012)	-0.005 (0.013)	-0.003 (0.013)	-0.005 (0.013)
% Hispanic	0.009 (0.011)	0.005 (0.010)	0.008 (0.011)	0.008 (0.011)	0.009 (0.011)
Immigration	-0.259 (0.205)	-0.234 (0.185)	-0.243 (0.203)	-0.255 (0.203)	-0.262 (0.205)
Precipitation	-0.849 (0.445)	-0.878* (0.446)	-0.749 (0.448)	-0.887* (0.445)	-0.849 (0.446)
Temperature	0.006*** (0.002)	0.006** (0.002)	0.006*** (0.002)	0.006*** (0.002)	0.006*** (0.002)
AQI	-0.004 (0.011)	-0.004 (0.011)	-0.002 (0.011)	-0.004 (0.011)	-0.005 (0.011)
Open Table	0.0003 (0.001)	-0.0002 (0.001)	0.0004 (0.001)	0.0002 (0.001)	0.00000 (0.001)
Constant	-0.534*** (0.127)	-0.507*** (0.127)	-0.524*** (0.127)	-0.524*** (0.128)	-0.513*** (0.128)
Random	SD	SD	SD	SD	SD
Neighborhoods	0.899	0.886	0.885	0.901	0.897
COVID period	0.109	0.183	0.191	0.151	0.142
Floyd period	0.177	0.222	0.194	0.214	0.169
Mediator	-	0.010	0.002	0.012	0.019

***p<0.001; **p<0.01; *p<0.05

Table S6. Mixed effects Poisson regression models of property crime counts in Denver neighborhoods in 2020 (direct effects).

	Direct effects models				
Components	Periods only	Mediator: Pedestrian stops	Mediator: Vehicle stops	Mediator: Drug arrests	Mediator: Disorder arrests
Fixed	b (SE)	b (SE)	b (SE)	b (SE)	b (SE)
COVID period	0.207***	0.195***	0.207***	0.174***	0.199***
	(0.049)	(0.049)	(0.048)	(0.049)	(0.050)
Floyd period	0.427***	0.420***	0.426***	0.403***	0.423***
	(0.043)	(0.043)	(0.043)	(0.045)	(0.044)
Mediator	-	-0.001	-0.0004	-0.027**	-0.010
	-	(0.003)	(0.001)	(0.010)	(0.015)
Motor vehicle accidents	0.004	0.003	0.004	0.004	0.004
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Total population	0.00003*	0.00003*	0.00003*	0.00003**	0.00003*
	(0.00001)	(0.00001)	(0.00001)	(0.00001)	(0.00001)
Disadvantage	0.144	0.147	0.145	0.129	0.174
	(0.139)	(0.139)	(0.139)	(0.137)	(0.137)
% Black	-0.019*	-0.018*	-0.019*	-0.018*	-0.020*
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
% Hispanic	-0.008	-0.007	-0.007	-0.007	-0.008
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Immigration	-0.051	-0.066	-0.062	-0.062	-0.071
	(0.137)	(0.137)	(0.137)	(0.134)	(0.135)
Precipitation	-0.700***	-0.716***	-0.699***	-0.718***	-0.709***
	(0.155)	(0.155)	(0.155)	(0.155)	(0.155)
Temperature	-0.0005	-0.001	-0.0005	-0.001	-0.0005
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
AQI	0.009*	0.010*	0.009*	0.009*	0.009*
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Open Table	-0.0003	-0.0004	-0.0003	-0.0005	-0.0004
	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Constant	1.591***	1.592***	1.590***	1.597***	1.592***
	(0.078)	(0.078)	(0.078)	(0.078)	(0.078)
Random	SD	SD	SD	SD	SD
Neighborhoods	0.647	0.644	0.645	0.641	0.640
COVID period	0.227	0.223	0.211	0.226	0.226
Floyd period	0.267	0.266	0.265	0.282	0.267
Mediator	-	0.006	0.002	0.046	0.066

***p<0.001; **p<0.01; *p<0.05

Table S7. Mixed effects Poisson regression models of property crime counts in Denver neighborhoods in 2020 (interaction effects).

	Interaction effects models				
Components	Periods only	Mediator: Pedestrian stops	Mediator: Vehicle stops	Mediator: Drug arrests	Mediator: Disorder arrests
Fixed	b (SE)	b (SE)	b (SE)	b (SE)	b (SE)
COVID period	0.207*** (0.049)	0.192*** (0.049)	0.203*** (0.048)	0.172*** (0.050)	0.200*** (0.050)
Floyd period	0.427*** (0.043)	0.410*** (0.044)	0.427*** (0.044)	0.395*** (0.045)	0.424*** (0.044)
Mediator		0.007 (0.006)	0.0002 (0.002)	-0.004 (0.017)	-0.018 (0.023)
COVID * Mediator		-0.007 (0.006)	-0.001 (0.002)	-0.023 (0.016)	0.004 (0.029)
Floyd * Mediator		-0.010 (0.006)	-0.001 (0.002)	-0.029 (0.016)	0.010 (0.020)
Motor vehicle accidents	0.004 (0.002)	0.004 (0.002)	0.004 (0.002)	0.003 (0.002)	0.004 (0.002)
Total population	0.00003* (0.00001)	0.00003* (0.00001)	0.00003* (0.00001)	0.00003* (0.00001)	0.00003* (0.00001)
Disadvantage	0.144 (0.139)	0.148 (0.139)	0.141 (0.139)	0.126 (0.136)	0.174 (0.137)
% Black	-0.019* (0.009)	-0.018* (0.009)	-0.019* (0.009)	-0.017* (0.009)	-0.020* (0.009)
% Hispanic	-0.008 (0.007)	-0.007 (0.007)	-0.007 (0.007)	-0.007 (0.007)	-0.008 (0.007)
Immigration	-0.051 (0.137)	-0.064 (0.137)	-0.060 (0.137)	-0.058 (0.133)	-0.071 (0.135)
Precipitation	-0.700*** (0.155)	-0.714*** (0.155)	-0.685*** (0.156)	-0.729*** (0.155)	-0.708*** (0.155)
Temperature	-0.0005 (0.001)	-0.001 (0.001)	-0.0004 (0.001)	-0.001 (0.001)	-0.0005 (0.001)
AQI	0.009* (0.004)	0.009* (0.004)	0.010* (0.004)	0.009* (0.004)	0.010* (0.004)
Open Table	-0.0003 (0.0004)	-0.0004 (0.0004)	-0.0003 (0.0004)	-0.0005 (0.0004)	-0.0004 (0.0004)
Constant	1.591*** (0.078)	1.598*** (0.078)	1.591*** (0.078)	1.599*** (0.078)	1.591*** (0.078)
Random	SD	SD	SD	SD	SD
Neighborhoods	0.64683	0.643	0.645	0.647	0.641
COVID period	0.22686	0.223	0.211	0.226	0.227
Floyd period	0.26728	0.264	0.264	0.284	0.268
Mediator	-	0.005	0.002	0.046	0.065

***p<0.001; **p<0.01; *p<0.05

Table S8. Mixed effects Poisson regression models of aggravated assault counts in Denver neighborhoods in 2020 (direct effects).

	Direct effects models				
Components Fixed	Periods only b (SE)	Mediator: Pedestrian stops b (SE)	Mediator: Vehicle stops b (SE)	Mediator: Drug arrests b (SE)	Mediator: Disorder arrests b (SE)
COVID period	0.178	0.153	0.153	0.153	0.149
	(0.140)	(0.139)	(0.141)	(0.141)	(0.143)
Floyd period	0.296**	0.273**	0.275*	0.275*	0.277*
	(0.107)	(0.106)	(0.109)	(0.108)	(0.109)
Mediator	-	-0.016***	-0.002*	-0.049**	-0.046*
	-	(0.003)	(0.001)	(0.018)	(0.023)
Motor vehicle accidents	0.018*	0.017*	0.017*	0.018**	0.018**
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Total population	0.00001	0.00002	0.00001	0.00002	0.00001
	(0.00002)	(0.00002)	(0.00002)	(0.00002)	(0.00002)
Disadvantage	0.265	0.342	0.245	0.345	0.260
	(0.220)	(0.211)	(0.219)	(0.215)	(0.218)
% Black	-0.004	-0.012	-0.004	-0.010	-0.003
	(0.014)	(0.013)	(0.014)	(0.013)	(0.014)
% Hispanic	0.012	0.011	0.011	0.008	0.012
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Immigration	-0.304	-0.380	-0.278	-0.327	-0.307
	(0.214)	(0.201)	(0.213)	(0.205)	(0.212)
Precipitation	-1.087*	-1.094*	-1.079*	-1.078*	-1.055*
	(0.532)	(0.532)	(0.532)	(0.532)	(0.532)
Temperature	0.005*	0.005*	0.005*	0.005*	0.005*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
AQI	-0.021	-0.021	-0.020	-0.022	-0.022
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Open Table	0.0001	-0.0001	0.00003	0.00002	-0.0001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Constant	-0.997***	-1.000***	-0.994***	-0.986***	-0.985***
	(0.149)	(0.148)	(0.149)	(0.149)	(0.150)
Random	SD	SD	SD	SD	SD
Neighborhoods	1.02995	1.02285	1.0234	1.0282	1.03125
COVID period	0.18827	0.0974	0.2022	0.20062	0.2191
Floyd period	0.25613	0.18766	0.27153	0.24255	0.26463
Mediator	-	0.00858	0.00146	0.06151	0.01818

***p<0.001; **p<0.01; *p<0.05

Table S9. Mixed effects Poisson regression models of aggravated assault counts in Denver neighborhoods in 2020 (interaction effects).

	Interaction effects models				
Components	Periods only	Mediator: Pedestrian stops	Mediator: Vehicle stops	Mediator: Drug arrests	Mediator: Disorder arrests
Fixed	b (SE)	b (SE)	b (SE)	b (SE)	b (SE)
COVID period	0.178 (0.140)	0.109 (0.142)	0.124 (0.144)	0.162 (0.142)	0.138 (0.143)
Floyd period	0.296** (0.107)	0.230* (0.111)	0.263* (0.112)	0.274* (0.109)	0.262* (0.109)
Mediator	- -	-0.001 (0.012)	0.004 (0.003)	-0.083* (0.037)	-0.015 (0.043)
COVID * Mediator	- -	-0.001 (0.014)	-0.007 (0.004)	0.064 (0.038)	0.030 (0.068)
Floyd * Mediator	- -	-0.015 (0.013)	-0.006 (0.004)	0.041 (0.037)	-0.048 (0.050)
Motor vehicle accidents	0.018* (0.007)	0.017* (0.007)	0.018** (0.007)	0.018** (0.007)	0.017* (0.007)
Total population	0.00001 (0.00002)	0.00002 (0.00002)	0.00001 (0.00002)	0.00002 (0.00002)	0.00001 (0.00002)
Disadvantage	0.265 (0.220)	0.316 (0.212)	0.244 (0.218)	0.336 (0.218)	0.268 (0.219)
% Black	-0.004 (0.014)	-0.004 (0.013)	-0.004 (0.014)	-0.006 (0.014)	-0.003 (0.014)
% Hispanic	0.012 (0.011)	0.009 (0.011)	0.011 (0.011)	0.008 (0.011)	0.012 (0.011)
Immigration	-0.304 (0.214)	-0.284 (0.205)	-0.265 (0.212)	-0.295 (0.209)	-0.305 (0.213)
Precipitation	-1.087* (0.532)	-1.137* (0.532)	-0.987 (0.536)	-1.104* (0.532)	-1.073* (0.533)
Temperature	0.005* (0.002)	0.004* (0.002)	0.005* (0.002)	0.005* (0.002)	0.005* (0.002)
AQI	-0.021 (0.013)	-0.022* (0.013)	-0.020 (0.013)	-0.022* (0.013)	-0.023* (0.013)
Open Table	0.0001 (0.001)	-0.0005 (0.001)	0.0002 (0.001)	-0.0001 (0.001)	-0.0003 (0.001)
Constant	-0.997*** (0.149)	-0.962*** (0.148)	-0.981*** (0.148)	-0.983*** (0.148)	-0.973*** (0.149)
Random	SD	SD	SD	SD	SD
Neighborhoods	1.02995	1.015	1.014	1.006	1.026
COVID period	0.18827	0.237	0.224	0.164	0.200
Floyd period	0.25613	0.296	0.278	0.230	0.255
Mediator	-	0.006	0.001	0.059	0.011

***p<0.001; **p<0.01; *p<0.05

Table S10. Mixed effects Poisson regression models of motor vehicle theft counts in Denver neighborhoods in 2020 (direct effects).

	Direct effects models				
Components	Periods only	Mediator: Pedestrian stops	Mediator: Vehicle stops	Mediator: Drug arrests	Mediator: Disorder arrests
Fixed	b (SE)	b (SE)	b (SE)	b (SE)	b (SE)
COVID period	0.640***	0.612***	0.634***	0.594***	0.626***
	(0.086)	(0.087)	(0.083)	(0.087)	(0.086)
Floyd period	0.966***	0.942***	0.962***	0.930***	0.955***
	(0.067)	(0.067)	(0.065)	(0.069)	(0.068)
Mediator	-	-0.012*	-0.0003	-0.060***	-0.054*
	-	(0.006)	(0.001)	(0.018)	(0.022)
Motor vehicle accidents	0.014***	0.015***	0.014***	0.016***	0.014***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Total population	0.00004***	0.00004***	0.00004***	0.00004***	0.00004***
	(0.00001)	(0.00001)	(0.00001)	(0.00001)	(0.00001)
Disadvantage	0.287*	0.301*	0.293*	0.274*	0.282*
	(0.138)	(0.136)	(0.137)	(0.134)	(0.133)
% Black	-0.019*	-0.020*	-0.018*	-0.018*	-0.019*
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
% Hispanic	-0.004	-0.005	-0.003	-0.004	-0.006
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Immigration	-0.149	-0.148	-0.165	-0.133	-0.093
	(0.135)	(0.133)	(0.134)	(0.128)	(0.129)
Precipitation	-1.126***	-1.159***	-1.126***	-1.153***	-1.138***
	(0.319)	(0.319)	(0.319)	(0.319)	(0.319)
Temperature	-0.007***	-0.007***	-0.007***	-0.007***	-0.007***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
AQI	-0.001	-0.001	-0.001	-0.002	-0.002
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Open Table	0.002*	0.001*	0.002*	0.002*	0.002*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Constant	-0.255***	-0.270***	-0.252***	-0.259**	-0.253**
	(0.081)	(0.082)	(0.080)	(0.081)	(0.081)
Random	SD	SD	SD	SD	SD
Neighborhoods	0.531	0.530	0.518	0.518	0.528
COVID period	0.219	0.213	0.127	0.225	0.216
Floyd period	0.263	0.244	0.218	0.281	0.270
Mediator	-	0.018	0.000	0.080	0.079

***p<0.001; **p<0.01; *p<0.05

Table S11. Mixed effects Poisson regression models of motor vehicle theft counts in Denver neighborhoods in 2020 (interaction effects).

	Interaction effects models				
Components	Periods only	Mediator: Pedestrian stops	Mediator: Vehicle stops	Mediator: Drug arrests	Mediator: Disorder arrests
Fixed	b (SE)	b (SE)	b (SE)	b (SE)	b (SE)
COVID period	0.640***	0.600***	0.620***	0.598***	0.627***
	(0.086)	(0.087)	(0.084)	(0.088)	(0.086)
Floyd period	0.966***	0.928***	0.962***	0.933***	0.959***
	(0.067)	(0.068)	(0.066)	(0.070)	(0.068)
Mediator	-	0.007	0.002	-0.073**	-0.087*
	-	(0.013)	(0.002)	(0.034)	(0.049)
COVID * Mediator	-	-0.019	-0.003	0.017	0.015
	-	(0.013)	(0.003)	(0.034)	(0.063)
Floyd * Mediator	-	-0.020	-0.002	0.014	0.040
	-	(0.013)	(0.002)	(0.032)	(0.051)
Motor vehicle accidents	0.014***	0.015***	0.014***	0.016***	0.014***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Total population	0.00004***	0.00004***	0.00004***	0.00004***	0.00004***
	(0.00001)	(0.00001)	(0.00001)	(0.00001)	(0.00001)
Disadvantage	0.287**	0.298**	0.287**	0.275**	0.281**
	(0.138)	(0.136)	(0.135)	(0.133)	(0.133)
% Black	-0.019**	-0.020**	-0.018**	-0.018**	-0.019**
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
% Hispanic	-0.004	-0.005	-0.002	-0.004	-0.007
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Immigration	-0.149	-0.144	-0.176	-0.133	-0.088
	(0.135)	(0.133)	(0.132)	(0.128)	(0.128)
Precipitation	-1.126***	-1.146***	-1.090***	-1.154***	-1.131***
	(0.319)	(0.319)	(0.320)	(0.320)	(0.319)
Temperature	-0.007***	-0.008***	-0.007***	-0.007***	-0.007***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
AQI	-0.001	-0.001	-0.001	-0.002	-0.002
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Open Table	0.002**	0.002**	0.002**	0.002**	0.002**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Constant	-0.255***	-0.259***	-0.249***	-0.262***	-0.256***
	(0.081)	(0.082)	(0.079)	(0.081)	(0.082)
Random	SD	SD	SD	SD	SD
Neighborhoods	0.53113	0.527	0.508	0.516	0.530
COVID period	0.21861	0.185	0.144	0.229	0.217
Floyd period	0.26268	0.236	0.211	0.282	0.274
Mediator	-	0.018	0.001	0.080	0.079

***p<0.001; **p<0.01; *p<0.05

Figure S1. Neighborhood-level violent crime deviations in 2020 (relative to 2016-19 weighted average).

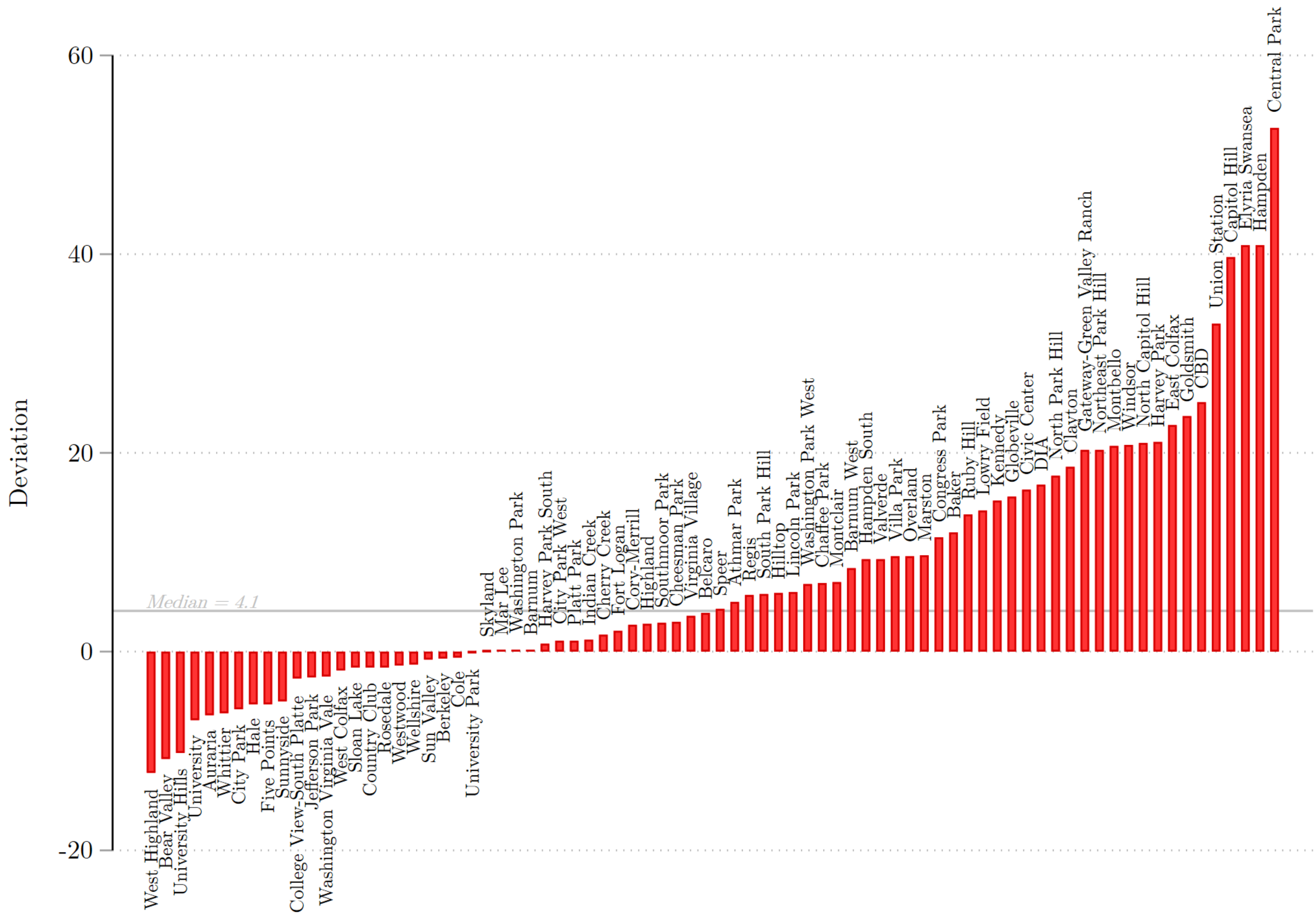

Figure S2. Neighborhood-level property crime deviations in 2020 (relative to 2016-19 weighted average).