The George Floyd Effect: How Protests and Public Scrutiny Changed Police Behavior

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The murder of George Floyd in May 2020 sparked a wave of Black Lives Matter protests in many cities throughout the United States. Protesters' demands ranged from constraints on police use of force to defunding and disbanding the police altogether. These have led some to worry about the possibility of a "Ferguson Effect," where police withdraw from policing, and in particular discretionary stops and searches, with deleterious consequences for crime. Drawing on data from four cities, we evaluate whether the 2020 BLM protests impacted police behavior, and whether changes in policing negatively impacted public safety. Regression discontinuity-in-time estimates suggest that although depolicing followed the BLM protests, in some respects the quality of policing improved, and public safety was not clearly impacted. Our findings have important implications for research on policing, social movements, and structural inequality in cities.

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George Floyd was murdered by police on May 25, 2020. Police officers handcuffed him, pinned him to the ground, and officer Derek Chauvin knelt on his neck for almost nine minutes, ending his life. A video of the incident quickly went viral and sparked what scholars have called, "the largest episode of social protest in both the catalogue of the BLM movement and the longer history of Black resistance against dehumanization and state violence in the U.S." (Reny and Newman 2021, 1499). By the first week of June 2020, protests had occurred in over 140 cities across the United States and extended to over 40 countries (Smith, Wu, and Murphy 2020). The 2020 BLM protests were tonally radical, pushing the language of abolition into the mainstream and redefining the discourse around policing. While calls to *defund the police* proved politically incendiary and the demands of activists varied, a desire to end police brutality, hold police accountable for misconduct, and decenter policing as the primary face of the state operative in race-class subjugated communities propelled the rise of BLM and fueled the protests to unprecedented scale (Soss and Weaver 2017).

Anecdotal accounts across various media outlets suggest that the protests led to a decline in policing (whether because officers were defunded, demoralized, or counter-protesting) and, in turn, a rise in crime (Arango 2021; Pagones 2020). But this is speculation. Little empirical evidence exists connecting depolicing and protests to crime, inclusive of the protests that occurred in 2020.

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Studies that do observe heightened crime following the protests pertain to single cities and the studies struggle to establish a causal relationship between the protests themselves, changing police behavior, and downstream public safety outcomes (Ratcliffe and Taylor 2023; Nix et al. 2024). The impact of the protests on both policing practices and public safety in cities across the country remains an open empirical question with implications for our understanding of policing, protests, and bureaucratic responsiveness.

Existing literature suggests two reasons why police activity may decline following protests. Police may respond to the demands of protesters by changing tactics in ways that reduce contact with citizens, improve the efficiency of their work, and, especially, reduce racially unequal outcomes. Thus, depolicing may be a response to protester demands. Researchers elsewhere demonstrate that protests do have the ability to hold public officials accountable (Gillion 2012; Gause 2022). In contrast, police may change their tactics in ways springing from demoralization, burnout, or retaliation against external critiques. Scholars refer to this kind of behavior as dissent shirking (Chanin and Sheats 2018). While a handful of studies tackle whether depolicing occurs, very few characterize withdrawal, raising questions around the reasons behind this behavior and its consequences for civilians (Nix, Wolfe, and Campbell 2018).

To address these questions, we evaluate several years of high resolution, incident-level data in four contexts: Seattle, WA; Austin, TX; Philadelphia, PA; and Los Angeles, CA. These cities are unique insofar as they offer data that are sufficiently rich to enable an evaluation of not only depolicing, but also the character of police withdrawal. That we are able to replicate these analyses across four contexts—where most other work drawing on similar data is limited to a single city—render our findings relatively broad.

Our first task is to evaluate whether depolicing occurred following the BLM protests. Using a regression discontinuity-in-time approach, we find a discontinuous and persistent drop in officer contact with civilians. This finding is durable and holds across all contexts. Leveraging 911 calls in two cities, we find that the change in stops is not driven by citizen requests for assistance. Second, we establish competing hypotheses about the pro- or antisocial nature of this decline. If depolicing is pro-social, we expect to see improvements in the quality of activity that does occur: more contraband per stop, more arrests per stop, and diminished racial disparities. On the other hand, if declines in activity are antisocial, we expect no change in overall quality, or even perhaps the opposite (Nix, Wolfe, and Campbell 2018). On balance, we observe an improvement in arrest rates in all contexts; diminished Black/white stop disparities in all but one context, but no meaningful difference in Latino/white stop disparities; and no consistent improvement in hit rates. We conclude that the character of depolicing is mixed, and context dependent. With respect to crime, the critical test is violent crime, which is understood to be less sensitive to policing tactics relative to against property/society crimes. We do not find consistent evidence that increased violent crime accompanied depolicing following the protests.

We make several contributions: First, we offer systematic and robust evidence that protests can compel widespread and durable changes in police behavior. Second, we are unable to consistently link depolicing with either pro- or anti-social policing behaviors. This suggests that the nature of depolicing can be either structured or unstructured. For example, consistent pro-social changes in Seattle suggest that a structured effort by leadership may have occurred, promoting coherent reforms. Fully investigating the circumstances that lead to structured (versus unstructured) declines in police service provision is an area for future research. Finally, we show that depolicing does not have clear consequences for violent crime.

In what follows, we begin by providing background on the protests, before providing an overview of the existing literature on the extent to which public officials respond to protests. We then review evidence around the conditions under which depolicing is likely to occur, and how such declines in service may impact crime. We develop three hypotheses, concerning depolicing overall, the quality of depolicing, and the consequences of depolicing for public safety. We then describe our case selection, data and analytic strategy, and review the results.

Background

George Floyd's death at the hands of police officers in May of 2020 followed from the suspicion that he had purchased a pack of cigarettes using counterfeit currency (Altman 2020). The policing strategies which lead to his death, including the excessive deployment of state force against Black civilians and in response to a minor infraction, were characteristic of the Minneapolis police department (Altman 2020; DOJ 2023). The specific chokehold in question had been deployed and rendered citizens unconscious nearly fifty times since 2015 and in more than half the cases the individuals were Black (Altman 2020). In Minneapolis, reports both by the Department of Justice and investigative journalists demonstrate that officers were very rarely held accountable (DOJ 2023).

Floyd's death, moreover, followed a series of fatal encounters with officers that made Minneapolis "a locus of racial-justice activism" in the handful of preceding years (Altman 2020). Notably, the film of the death of Philando Castille who was shot by police while sitting in his car went viral in 2016 on the crest of the first wave of BLM protests (Furber and Perez-Pena 2016). That wave, itself, was sparked by the killing of Michael Brown in Ferguson, Missouri, by officer Darren Wilson (DOJ 2015). As in the

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case of Minneapolis, an official investigation in Ferguson revealed practices that were racially discriminatory and extractive by design (DOJ 2015).

While permutations of such practices vary by department, they derive from a philosophy of crime management broadly embraced by departments across the country (Soss and Weaver 2017; Meares 2015; Meares 2014). This philosophy is rooted in the belief that police can deter crime through proactively and punitively intervening in low-level offenses before they escalate to more serious crimes and through the regulation of public disorder (Meares 2014; Herbert and Beckett 2017; Baumgartner, Epp, and Shoub 2018). Practically, this has meant more police on the streets; more officer-initiated interactions with civilians; more attention by law enforcement to communities understood to be high crime; more severe punishment for low-level infractions; and the functional criminalization of disadvantaged people and places, especially communities of color (Soss and Weaver 2017; Baumgartner, Epp, and Shoub 2018; Epp, Maynard-Moody, and Haider-Markel 2014). The expansion of police power overall, and prescriptive police presence in disadvantaged communities specifically, that follow from this philosophy "stigmatize and repress, ultimately turning government into an invasive, surveillant authority … in the forms of supervision, interference and predation", producing the political subordination of race-class subjugated (RCS) communities (Soss and Weaver 2017, 567).

For members of RCS communities, interactions with the police become routine, and the threat that such interactions might turn fatal is a risk of their second-class status (Edwards, Lee, and Esposito 2019). In other words, George Floyd's death sparked an uprising of unprecedented scale not because it was so egregious but because the denigration of Black life by law enforcement in American cities is so routine, and is facilitated by an approach to crime control that relies on a large number of unnecessary interactions with civilians overall. The demands made by protesters for reform, for policing to deliver more safety for citizens, and less violence perpetuated against them is in direct response to the dominant paradigm of crime prevention in which law enforcement operates. For this reason, declines in discretionary stops made by police following the protests have the potential to dramatically change the experiences over-policed communities have with law enforcement, and by extension the state (Lerman and Weaver 2014a; Lerman and Weaver 2014b). Further, an evaluation of the impact of the protests on officer behavior cannot be disassociated from a longer struggle for racial justice, which finds its current expression in resistance to contemporary policing and criminal justice practices (Lebron 2023). Thus, the remainder of this article is dedicated to answering questions regarding how police responded when the protesters demanded justice for Black lives, and understanding the consequences.

Can Anti-Police Protests Prompt Depolicing?

A review of the existing literature suggests that the 2020 BLM protests created an environment ripe for depolicing to occur. Little research has explicitly evaluated whether anti-police protests themselves are successful in extracting behavioral changes (and higher quality outcomes) from law enforcement, instead focusing on peripheral questions, like the impact of anti-police protests on officers' morale (Deuchar, Fallik, and Crichlow 2019; Mercado 2019; Nix, Wolfe, and Campbell 2018; Oliver 2017), and downstream impacts on crime (Tiwari 2016; MacDonald 2019; Lohman 2021; Capellan, Lautenschlager, and Silva 2020; Ratcliffe and Taylor 2023). Moreover, in the absence of a clear top-down directive from leadership, the underlying mechanisms that might lead to depolicing are disparate and varied. Even so, given what we know about the conditions under which individual officers are likely to change their behavior, depolicing is likely to have occurred following the onset of the protests.

A robust body of evidence suggests that public officials have incentives to respond to the demands of protesters, whether because of the electoral connection (in the case of elected officials) or from a reputational standpoint (in the case of unelected officials). The bulk of research demonstrating the responsiveness of officials to protester demands derives from a study of the struggle for civil rights and Black liberation (Wasow 2020; Enos, Kaufman, and Sands 2019; Reny and Newman 2021; Gillion 2012). While there is a clear link between past movements for racial justice and the current movement for police reform, whether anti-police protests yield a response from law enforcement is unknown.

The evidence that depolicing in particular occurs systematically in response to external pressure is mixed. The mechanisms by which depolicing might occur are varied. Interviews with officers themselves indicate that they believe depolicing happens, and that officers engage in this behavior for many individualized reasons (Nix, Wolfe, and Campbell 2018; Oliver 2017; Gau, Paoline, and Paul 2022; Foster, Rossler, and Scheer 2023). Scholars call withdrawal from duty that might occur in response to anti-police protests *dissent shirking*, where officers change their behavior because they feel that they have been unfairly maligned by the public (Chanin and Sheats 2018; Eckhouse 2022). Dissent shirking, however, carries with it the implication of retaliation, where officers withdraw from duty because they disagree with critiques of their activities. Officers may also alter their behavior because they do not want to draw attention to themselves or risk becoming the focus of a civil inquiry. This kind of behavior is better characterized as avoidant than dissident (Nix, Wolfe, and Campbell 2018). Officers may likewise police less because they are overwhelmed by the demands of the job, and public criticism may exacerbate feelings of

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burnout (Oliver 2017). Indeed, given that the strategies and practices officers are trained to deploy are themselves racialized in their preemptive construction, individual officers may find themselves in an impossible position, insofar as they must both carry out such practices but do so in ways that appear less racially unequal. Scholars leverage strain theory to organize officers responses to an increasingly stressful work environment resulting from external criticism (Nix, Wolfe, and Campbell 2018). From this perspective, depolicing is a coping mechanism that officers leverage to reduce stress by avoiding putting themselves in situations where they might use force, that invite evaluation, or to alleviate psychological distress arising from sustained criticism (Agnew 1992; Paoline 2004; Paoline 2003; Mac Donald 2017).

Because officer withdrawals from discretionary service provision may occur in an unstructured, highly individual way, depolicing may not always be observable in the aggregate. For example, surveys of law enforcement both before and after the 2014 Ferguson uprising suggest that withdrawals from service provision are limited in scope and duration (Marier and Fridell 2020; Cheng and Long 2022). Likewise, Chanin and Sheats (2018) find no change in police behavior in response to policy reforms imposed by the Department of Justice when misconduct violations are exposed, nor does Koslicki (2022) observe changes to use of force practices by the Minneapolis police department after the death of George Floyd. Yet evaluations of agencies in Missouri post-Ferguson find that misdemeanor arrests declined across the state the year following the protests (Shjarback et al. 2017; Powell 2022), and scholars observe that in at least one city, both the onset of the COVID-19 pandemic and the protests that followed shortly thereafter were associated with declining police activity (Nix et al. 2024).

Whether anti-police protests themselves can compel durable change in officer behavior is thus an open question. Qualitative evidence suggests that how individual officers respond to anti-police protests varies widely, but that declines in discretionary activity are most likely to follow from instances of extraordinary work-place strain. The volatile nature of the protests in many cities, ongoing criticism of law enforcement, and efforts by local officials to reform policing practices that followed suggest that the context of the 2020 BLM protests created a highly strained environment for individual officers—exactly the circumstances that might give way to depolicing. For these reasons, and building on suggestive findings in one city (Nix et al. 2024), we develop the following hypothesis:

Hypothesis 1: There will be a discontinuous decline in discretionary policing activities following the 2020 BLM protests.

Can Depolicing Be Characterized as Pro- or Anti-Social?

Whether depolicing (should we observe it) can be viewed as a net social good or loss likely depends on the underlying mechanisms by which it occurs. In the absence clear decisions about whether and how to provide services we cannot stake out a clear set of expectations around the outcomes that follow from declining police activity. In this section, we nevertheless make an effort to articulate how we might characterize the quality of those outcomes, given what we know about how discretionary policing works and the outcomes over-policed communities might like to experience, irrespective of officer motive. Depolicing might be characterized as pro-social if policing outcomes become more efficient (for example, higher hit rates when stops do occur as in Mummolo [2018]), racial disparities in stops decline, or marginalized communities experience better service provision (Nix, Wolfe, and Campbell 2018; Shjarback et al. 2017; Rosenfeld and Wallman 2019).

The character depolicing is likely to take following a protest is unclear and is context specific. On one hand, extant literature suggests that protests can function to hold public officials accountable by exerting political pressure. Mummolo (2018) finds that directives from agency leadership to document more fully the reason for conducting a Terry stop in New York City yielded an immediate increase in high-quality stops that produce evidence of criminal activity.[1] Scholars elsewhere find that the increased use of body-worn cameras (for example) following heightened outside scrutiny produce fewer instances of use-of-force and civilian complaints against officers (Ba and Rivera 2019; Campbell 2024). These findings suggest that structured directives aiming to improve service provision can indeed yield prosocial policing outcomes. Mayors and city councils often have a fair amount of control over local law enforcement activities, particularly via budgets. The city councils in all four cities included in this analysis—in keeping with most other major U.S. cities—passed resolutions to address use-of-force by law enforcement in the days following the onset of the protests. It may be the case that any decline in discretionary police activity we observe following the protests reflects accountability to protester demands vis-à-vis public officials. In this instance we would expect the quality of policing to improve overall.

However, declining police stops that follow the protests could produce an improvement in the quality of policing overall because of the nature of tactics over which law enforcement have discretion. Research demonstrates that preemptive practices and the underlying assumptions that inform them are applied in racially disproportionate ways and are not efficient at recovering contraband, but instead are designed to maintain social order (Meares 2014; Soss and Weaver 2017; Epp, Maynard-Moody, and Haider-

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Markel 2014; Baumgartner, Epp, and Shoub 2018; Meares 2015; Epp and Erhardt 2021). As noted earlier, it is this discretionary framework and its emphasis on preemption that facilitates excessive and risky interactions between civilians and law enforcement. In turn, it is the oppressive framework of policing that members of RCS communities protest. Fewer discretionary police stops may therefore lead to an improvement in quality of policing simply because officers shift to relying on practices requiring a higher threshold of suspicion and actualized evidence of criminal activity (e.g., officers may shift to relying more heavily on probable cause rather than consent to initiate contact with citizens). Turning away from preemptive practices towards reactive ones is likely to yield declining racial inequality and higher-quality stops and arrests overall (Meares 2014; Boehme 2023; Epp and Erhard, 2021). Both of these possibilities—accountability and shifts in the kind of stops officers engage in—lead to the following expectation around the quality of policing we may observe, post-BLM:

Hypothesis 2a: There will be a discontinuous improvement in the quality of policing overall following the 2020 BLM protests.

However, the accuracy of this hypothesis is highly contingent on local political context, so the null hypothesis—that there will be no change in the quality of policing overall—is also plausible. In the event that officers are simply policing less without changing the manner in which they police, we might expect to see no change in measures of quality. Even as there is some evidence that the city councils in all four cities included in the analysis attempted to address use-of-force practices following the protests, there is not much evidence that these efforts were more than symbolic (Walsh, Goodin-Smith, and Seidman 2021; Kamb and Beekman 2021). Thus, we may observe declines in service that we would characterize as anti-social, yielding no real improvement in terms of contraband hit rates and the like. This generates the following alternative hypothesis:

Hypothesis 2b: There will not be evidence of improvement in the quality of policing following the 2020 BLM protests.

In communities where over-policing is a concern, declines in police stops are likely welcome. Such declines might still be thought of as anti-social because they do not yield improved public safety outcomes.

Does Depolicing Lead to Increased Crime?

Much of the existing literature on depolicing examines the impact of anti-police protests on crime, where the fear is that protests compel police to withdraw, and the belief is that proactive policing from which they withdraw is vital to deterring (especially violent) crime (Capellan, Lautenschlager, and Silva 2020). This has been dubbed *The Ferguson Effect*, since this line of thinking gained traction in the wake of the 2014 Ferguson uprising. The fear that declining police activity will yield heightened violent crime is not wholly unfounded, since some research suggests that police presence overall, and strategic police presence through hot-spot policing specifically, can lead to meaningful reductions in crime (Braga et al. 2019; Dau et al. 2023; Piza and Chillar 2021; Weisburd et al. 2016).

However, researchers have struggled to clearly link both anti-police protests and depolicing to meaningful changes in violent crime rates (Tiwari 2016; MacDonald 2019; Lohman 2021; Capellan, Lautenschlager, and Silva 2020; Rosenfeld and Wallman 2019). A handful of studies link protests following the death of civilians at the hands of police officers to subsequent rises in violent crime, but do not empirically demonstrate that declining police activity explains that link (Ba and Rivera 2019; Shi 2009; Ratcliffe and Taylor 2023; Nix et al. 2024). Only two studies concern the 2020 protests specifically, and are unable to adequately disentangle the impact of the protests themselves from already-rising crime rates following the onset of the COVID-19 pandemic (Nix, Wolfe, and Campbell 2018; Ratcliffe and Taylor 2023; Piza and Connealy 2022). Moreover, these studies observe heterogeneity within city contexts, and note that one or two neighborhoods account for the rising crime (Nix, Wolfe, and Campbell 2018; Ratcliffe and Taylor 2023; Piza and Connealy 2022). Given the at best tenuous relationship between anti-police protests and crime, and the absence of a link between depolicing and crime, we develop the following, final hypothesis:

Hypothesis 3: There will not be a discontinuous change in violent crime following the 2020 BLM protests.

Data and Design

Case Selection

To select cities for inclusion in our analysis, we surveyed the open data websites of the top twenty most populous cities in the United States and collected all available incident-level data related to policing or crime.[2] We then identified the cities that had the following data available: 1) incident-level records of police activity, such as stops or officer-initiated 911 calls; 2) incident-level records including metrics of policing quality, such as recovery of contraband, and crucially, the race of civilian stopped; 3) incident-level records of crime that we could aggregate to the daily level (where previous work has relied on monthly counts of crime provided by the UCR), and 4) incident-level records up to at least one year prior to the onset of the protests.[3] We identified four cities that met

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Table 1
Data availability across top 20 most populated U.S. cities

City	State	Population Size	Crime Data (Geo)	Call Data (Geo)	Stop Data (Geo)	Stop Race Data (Geo)	Mayor Party	Evidence of BLM Protest
New York City	NY	8804190	×	✓	✓	✓	Democrat	✓
Los Angeles	CA	3898747	✓(address)	✓ (×)	✓ (×)	✓ (×)	Democrat	✓
Chicago	IL	2746388	✓	×	×	×	Democrat	✓
Houston	TX	2304580	×	×	×	×	Democrat	✓
Phoenix	AZ	1608139	✓	✓	×	×	Democrat	✓
Philadelphia	PA	1608139	✓(address)	× (×)	✓(address)	✓(address)	Democrat	✓
San Antonio	TX	1434625	×	×	×	×	Independent (Progressive)	✓
San Diego	CA	1386932	×	✓	✓		Republican	✓
Dallas	TX	1304379	✓	×	×	×	Democrat	✓
San Jose	CA	1013240	×	✓	×	×	Democrat	✓
Austin	TX	961855	✓(address)	× (×)	✓(address)	× (×)	Democrat	✓
Jacksonville	FL	949611	×	×	×	×	Democrat	✓
Fort Worth	TX	918915	✓	×	×	×	Democrat	✓
Columbus	OH	905748	×	×	×	×	Democrat	✓
Indianapolis	IN	897041	✓	×	×	×	Democrat	✓
Charlotte	NC	874579	✓	✓	✓		Democrat	✓
San Francisco	CA	873965	✓	✓	×	×	Democrat	✓
Seattle	WA	737015	✓(address)	✓ (beat)	✓ (beat)	✓ (beat)	Democrat	✓
Nashville	TN	715884	✓	✓	×	×	Democrat	✓
Denver	CO	715522	✓	×	✓	×	Non-Partisan (Democrat)	✓
D.C.	-	712816	✓	×	✓	✓	Democrat	✓

Note: Shaded rows denote cities included in study. Population data from U.S. Census (2020). Parentheses (geo) denote the existence of geocodeable data. Catalogue of available data conducted May 2023.

these criteria: Seattle, WA; Philadelphia, PA; Los Angeles, CA; and Austin, TX (table 1).[4]

No other city of which we are aware provides data detailed enough to evaluate our hypotheses. For example, Denver, CO, did not provide information on the race of civilian stopped; Dallas and Phoenix do not provide any information that would allow us to evaluate the quality of policing; New York City does not provide crime data;[5] Washington D.C.'s stop data does not extend a year prior to the 2020 BLM protest; and data from Charlotte, NC, is aggregated at the monthly level, precluding a daily regression discontinuity-in-time design that helps mitigate omitted variable bias. The four cities included in our analysis provide some regional coverage, as well as variation in the intensity of the protests and the responses of local city officials. Although there is no variation by city partisanship (all 4 cities were governed by a Democratic mayor), only three of twenty cities have Republican mayors.[6] Therefore, the cities included in our analyses are characteristic of the vast majority of major American cities. Additionally, all of the twenty largest cities in the United States had BLM protests. Since there is no cross-sectional variation in exposure to protests, we assess within-city variation in our outcomes of interest before and after the BLM protests to effectively understand the consequences of the protests, looking for patterns across cities.

The protests in each of our four cities were characterized by clashes between the police and protesters, which likely created a strained work environment for officers.[7] Although there is no major city that did not have a protest during the summer of 2020, there is variation in the intensity of the protests, which may in turn impact the likelihood and character of depolicing we might observe. Seattle perhaps represents the most volatile protest environment under study. The protests were contextualized by a long, conflicted history between community activists and law enforcement, which came to a boiling point in May of 2020. The city adopted a contract with the Seattle Police Officers Guild that pushed the department out of compliance with a previous consent decree, and moved to end outside monitoring imposed by that same decree (ACLU 2021). The protests lasted long into the summer, were characterized by police violence towards citizens, and famously, officers abandoned the East Precinct (ACLU 2021). The protests in Los Angeles were similarly intense, leading Governor Gavin Newsom to declare a state of emergency, deploying the National Guard (Reyes-Valarde

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et al. 2020; Petrie 2020). These two cases are perhaps where we would most expect to see depolicing occur and persist. In contrast, while protests in Austin and Philadelphia were also contentious, they died out by the end of the first week of June (Fernandez and Mccullough 2020; Gammage 2020). We expect to observe at least a short-term reduction in discretionary policing practices, but which may be unlikely to persist over the long term. In sum, although there are no major cities in the United States where protests did not occur, the cities included represent variation in protest intensity, which may impact outcomes of interest.

There is also variation across cities in how public officials responded to the protesters, which provides suggestive context for the character that depolicing might take. In Austin and Los Angeles, city officials responded quickly and resolutely in support of the protesters' demands. In Austin, less than three weeks after the protests erupted, the City Council approved cutting law enforcement's budget by one- third and passed a suite of policies designed to increase transparency and accountability (Venkataramanan 2020; Fernandez and Mccullough 2020). In Los Angeles, the City Council moved to cut the LAPD's budget by $150 million dollars, reallocating a sizable portion to non-police responses, to non-violent emergencies, and to poverty relief (Munoz 2021). In contrast, while Seattle's Mayor was at first supportive of the protesters, the city council was divided, and in the wake of the protests has continuously voted to increase funding for the police department (ACLU 2021). Given these differences in Los Angeles and Austin relative to Seattle, depolicing in the first two cases is likely to appear pro-social and in the latter case to appear anti-social. The response of public officials in Philadelphia was more mixed. The city council put forward proposals for an oversight commission and new restraints on the kind of force tactics available to officers (McCrystal 2020), but overall, the city did not appear particularly interested in pressuring the department to undertake radical change (Walsh, Goodin-Smith, and Seidman 2021).[8] While questions about the quality that depolicing is likely to take are difficult to study, context around protest intensity and the responses of public officials across cities both highlights the important variation across cases, and helps us interpret whatever findings we may have.

Data

To assess if the 2020 BLM protests reduced discretionary policing (Hypothesis 1), we draw on the following data in each city: traffic stops in Austin (January 2019–December 2020);[9] pedestrian stops (July 2018–February 2023) and traffic stops (July 2018–February 2023)[10] in Los Angeles; pedestrian (January 2018–December 2022) and traffic stops (January 2018–December 2022) in Philadelphia;[11] and Terry stops (March 2015–February 2022) in Seattle. We aggregate these data to a day-level time series characterizing the daily number of *stops.* If Hypothesis 1 is correct, we would expect *stops* to decrease post-protest.

For Hypothesis 2, we evaluate whether the 2020 BLM protests changed policing quality. We assess if the 2020 BLM protests increased policing efficiency and reduced the rate of fruitless police-citizen contact. In each city, we use the stop data to construct a daily time series of two efficiency measures: *hit rates* and *arrest rates. Hit rates* are the proportion of daily stops that result in the recovery of contraband. In Philadelphia, contraband is "firearms," "other weapons," "narcotics," or "other contraband."[12] In Austin, contraband is "narcotics," "illegal weapons," "money," "alcohol," or "other contraband."[13] In Los Angeles, contraband is "firearms," "ammunition," "weapons other than a firearm," "drugs/narcotics," "alcohol", "money," "drug paraphernalia," "cell phones," "electronic devices," "other contraband or evidence," and "suspected stolen property."[14] In Seattle, *hit rates* are measured differently in that they are the proportion of daily stops that resulted in an arrest, citation, offense report, or referral for prosecution as opposed to a field contact without action taken, implying no identification of criminal wrongdoing (i.e., a fruitless stop).[15]

Hit rates are measured differently across cities, which may make between-city comparisons concerning the effect of the BLM protests on hit rates difficult. However, we believe our outcome measures are appropriate for three reasons. First, we use the hit rate definition each city police department uses in their data or self-evaluation reports to mitigate researcher degrees of freedom that may bias statistical conclusions and to take for granted the priorities each department holds concerning what counts as a "hit." Thus, to the extent our hit rate measures account for each department's definition of what a "hit" means, our outcomes are harmonized across cities. Second, auxiliary analyses using stop data from Austin and Los Angeles show different types of hit rates (e.g., weapon recovery versus drug recovery) are correlated (appendix section K), suggesting our statistical conclusions would not change if we used harmonized hit rate measures across cities because different hit rate types are interdependent.[16] Third, in the Results section, we reference alternative analyses using more harmonized hit rate measures across cities which do not change our statistical conclusions.

Arrest rates are the proportion of stops resulting in an arrest,[17] suggesting the identified offense during a stop was arrest-worthy. Importantly, our *arrest rate* measure is distinct from other research normalizing the count of arrests over population size, which is another way of operationalizing policing intensity (Gelman, Fagan, and Kiss 2007). Our measure of arrest rates ostensibly captures prosocial policing behavior because if stops are more likely to lead to an arrest after the onset of the BLM protests, it suggests that police are no longer initializing superfluous

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or excessive civilian contact, but rather, based on contact conditional on the identification of arrest-worthy behavior.

Our final measure of quality is change in racially disparate stop patterns. To assess this, we evaluate if the 2020 BLM protests reduced the stop *rate ratio* between Black and white citizens across all four cities that we analyze and the stop *rate ratio* between Latino and white citizens across Austin, Los Angeles, and Philadelphia.[18] The *rate ratio* is the Black and Latino stop rate ([*BlackStops/BlackPopulation*] x 10,000; [*LatinoStops/LatinoPopulation*] x 10,000) divided by the white stop rate ([*WhiteStops/WhitePopulation*] x 10,000).[19]

If Hypothesis 2a is supported, then the 2020 BLM protests will have a positive effect on *hit rates* and *arrest rates*, and a negative effect on the *rate ratio.* However, we might expect different effects between the Black/white and Latino/white stop *rate ratios.* The BLM protests may decrease Latino/white stop *rate ratios* just as much as Black/white stop *rate ratios* since Latinos are also disparately and inefficiently policed (Pierson et al. 2020). However, the BLM protests may only decrease the Black/white stop *rate ratio* and not the Latino/white stop *rate ratio* since excessive policing of Latino (and especially non-Black Latino) communities is relatively peripheral to the messaging associated with the BLM protests. Conversely, if Hypothesis 2b is supported, then the 2020 BLM protests will have no effect on *hit rates*, *arrest rates*, or the *rate ratio.*[20]

To test Hypothesis 3, we use incident-level crime data obtained from each city's data portal. We rely on Federal National Incident Based Reporting System (NIBRS) rules for classifying crimes, separating them into three categories: *society* (e.g., drug possession, prostitution), *property* (e.g., burglary, car theft), and *violent* or *against persons* (e.g., robbery, assault). The temporal domain for the Austin,[21] Los Angeles,[22] Philadelphia,[23] and Seattle crime datasets[24] are January 2003–February 2022, January 2010–February 2023, January 2006–December 2022, and January 2008–February 2022 respectively. We are particularly interested in *violent crime* because identification of violent crimes is less sensitive to police effort, and more reflective of civilian reporting (Rosenfeld and Wallman 2019). Therefore, if police reduce activity post-protests, identification of violent crimes should be less endogenous to police response. In Seattle, for example, 94% of violent crimes are assault offenses; 5% are (non-consensual) sex offenses. The rest are consensual sex offenses, homicide offenses, and human trafficking. To evaluate the effect of the 2020 BLM protests on *violent crime*, we generate a daily time series of the count of violent crimes. If Hypothesis 3 is supported, the 2020 BLM protests should have no effect on *violent crimes*, although we may observe declines in the other two categories.

The independent variable for each of the daily time series is a binary indicator equal to 1 after the start of the 2020 *BLM protests* in each city. The start date for the BLM protests for Austin, Los Angeles, Philadelphia, and Seattle is May 29, 2020;[25] May 28, 2020;[26] May 30, 2020;[27] and May 29, 2020, respectively.[28] We validate these starting points by assessing the discontinuous effect of these start dates on two within-city measures of BLM protest intensity that we generate using Crowd Counting Consortium (CCC) data:[29] 1) daily BLM protests and 2) the daily number of BLM protest participants.[30] Indeed, there is nearly zero BLM protest activity in the respective cities prior to the BLM protest onset start dates that we choose. However, on these start dates, there is a discontinuous increase in BLM protest intensity, implying that our start dates (and regression discontinuity running variable cut-points) are justified (figure R87).

Our data are ideal to test our hypotheses. Consistent with prior research (Shjarback et al. 2017; Powell 2022), an alternative approach might use county-level data from the FBI Uniform Crime Report, and assess the differential effect of exposure to BLM protests on various crime and policing outcomes for agencies within specific counties. There are a few reasons to prefer our approach. First, not all police agencies report their crime and policing data to the FBI, and if they do, they do not necessarily report data for each month of a given year (30% of agencies do not report a full year's worth of data).[31] Our approach uses incident-level data that is directly reported from the agency instead of aggregated through an external organization (e.g., the FBI), reducing the risk of missing data. Second, our use of incident-level, daily data, allows us to assess the immediate, discontinuous effect of the BLM protests, reducing the risk that long-term time-varying factors or events (e.g., COVID policies) will bias our coefficient estimates.

Estimation Strategy

We use a regression discontinuity-in-time (RDiT) design to assess the discontinuous effect of the BLM protests. The core identifying assumption is that no other events are driving police behavior outside the *BLM protests* (i.e., the *continuity assumption*). Given that we use daily-level data and an estimation strategy that allows us to assess the effect of the *BLM protests* at the point at which they begin, it is unlikely that other factors are jointly driving the onset of the protests and shifts in police tactics. Although the RDiT design only allows us to assess immediate effects at the moment the *BLM protest* occurs, we believe this is the optimal research design since immediate effects are less likely to be perturbed by long-term unobserved time-varying factors that may influence policing and crime. Indeed, we validate the continuity assumption by demonstrating well-established covariates prognostic of crime, policing, and protest behavior[32] are largely balanced across the four cities we analyze before and after the BLM protest (figure J76).[33]

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Importantly, given that the *BLM protests* occur during the COVID-19 pandemic, our daily-level data in tandem with the RDiT design circumvents the possibility that governmental and public COVID-19 responses (e.g., restrictions) explain our results. Stay-at-home orders were initially implemented on March 17, 19, 23, and 24 of 2020 for Philadelphia, Los Angeles, Austin, and Seattle respectively, roughly two months before the *BLM protest* onset. Since the RDiT evaluates the immediate, discontinuous effect of the BLM protest at the daily-level, our *BLM protest* coefficients are likely not perturbed by concomitant COVID-19 responses. Although the COVID-19 pandemic was underway during the *BLM protests*, its influence should be constant given the nature of the design. We confirm this through temporal placebo tests (referenced appropriately throughout this article).

One potential shortcoming of our design is that the 2020 *BLM protests* characterize a *bundled treatment.* Mass and police behavior shifted across a variety of dimensions immediately after the onset of the *BLM protests* (e.g., some people participate in protests, some people stay home, the police counter-mobilize). We do not view this as a weakness primarily for theoretical reasons. Protests are never inherently clean or isolated treatments. By design, the mass public and police will immediately respond simultaneously to protests in a variety of different ways. Moreover, tactical policing shifts in response to protests are fundamentally interrelated and do not occur in a vacuum (Epp and Erhardt 2021). Therefore, evaluating the effect of a protest always requires acknowledging the existence of concomitant responses, especially at the moment the protest begins.

Nevertheless, we address the bundled treatment problem by assessing longer-term effects of the *BLM protest* that are less likely to be affected by the immediate influence of mass mobilization and counter-mobilization on the part of the police. To this end, we interpret RDiT coefficients after removing outcome data 1–100 days immediately after the *BLM protest.* Although this analysis may be subject to bias from temporal trends and should be understood as descriptive, we believe it is necessary to understand the durability of some of the effects we observe. If this exercise does not provide evidence of durable effects, it suggests our original RDiT coefficients may be statistical noise since there was not a sufficiently durable discontinuous shift in our outcomes to effectively estimate a post-*BLM protest* discontinuous effect in the first place. Moreover, the absence of durable effect patterns may suggest our initial RDiT coefficients are highly idiosyncratic to the immediate consequences of the *BLM protest.*

For brevity, we interpret and present standardized RDiT coefficients using a uniform kernel, first-order polynomial (degree = 1), and mean-squared optimal bandwidth acquired with the rdrobust package in R (Calonico, Cattaneo, and Titiunik 2015). We reference alternative specifications in the appendix as we describe the results when appropriate. Given that we analyze the effect of the *BLM protests* on policing activity and crime across four independent cities, we also estimate and present a Hartung-Knapp random effects meta-analytic estimate averaging the *BLM protest* coefficients across the four cities[34] with respect to each outcome of interest.[35] Refer to appendix section L for a theoretical and empirical justification as to why we include the meta-analysis.

Results

Hypothesis 1: Depolicing

We find support for Hypothesis 1. Figure 1 describes the volume of discretionary policing activity before and after the protests. There is a clear, large, and immediate decrease across all measures of discretionary policing in every city under study.

Figure 2 displays RDiT coefficients characterizing these relationships. Across all cities and outcomes, there is a substantially large and statistically significant decrease in policing ($p < 0.001$ for all coefficients). The RDiT *BLM protest* coefficient ranges from -0.2 to -2.8 standard deviations. These coefficients are equivalent to 100% (-345), 47% (-243), 90% (1251), 10% (-17), 26% (-211), and 83% (-9) of the pre-*BLM protest* stop mean for Austin traffic stops (347), LA pedestrian stops (516), LA traffic stops (1379), Philadelphia pedestrian stops (176), Philadelphia traffic stops (818), and Seattle Terry stops (11) respectively.

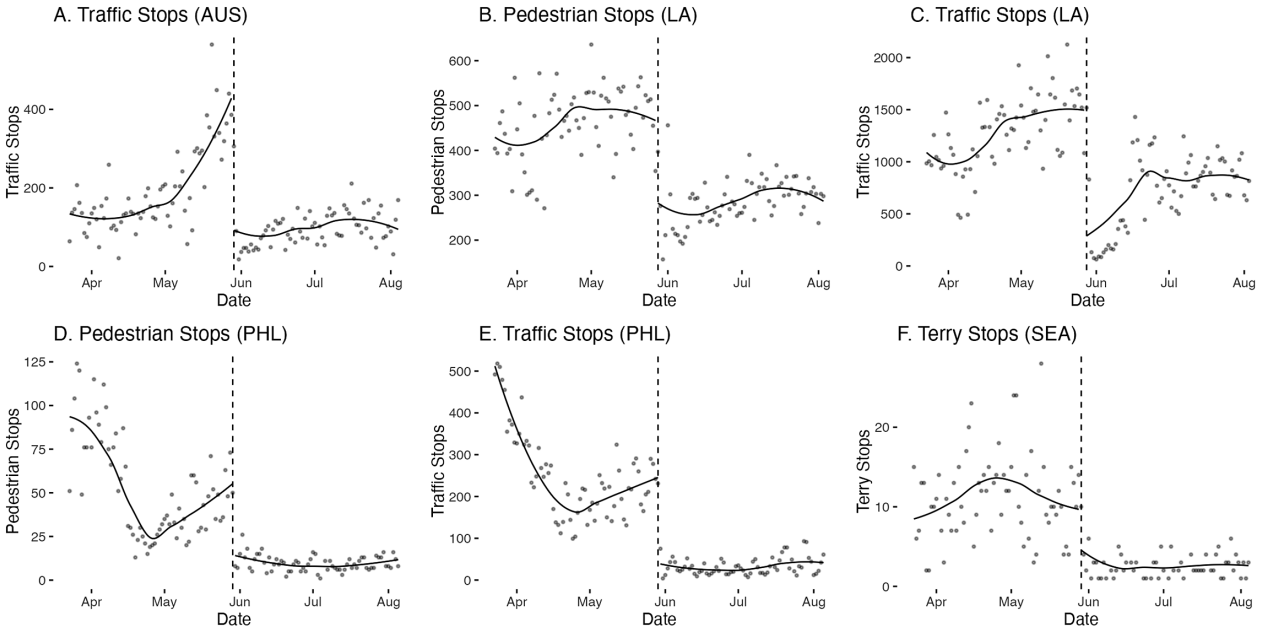
The study-adjusted random effects meta-analytic coefficient is -1.5 standard deviations.[36] These effects are not simply short-term effects intrinsic to the onset of the *BLM protests.* We also re-estimate RDiT coefficients omitting 1–100 days immediately after the protest to evaluate whether the discontinuous decrease in policing activity persists several days after its initial onset.

Observed decreases in police activity uniformly persist at least 100 days after the first BLM protests (appendix figures G41–G46). These estimates are also robust across kernel and polynomial specifications (appendix figures E7–E10), alternative bandwidths (appendix figures F11–F16), and balance covariate adjustment (figure J77). Finally, we conducted a temporal placebo test to assess whether changes in policing following the *BLM protests* were distinguishable from changes in policing behavior that may have occurred in all pre-*protest* days 30 days before the protest and 30 days after the beginning of the temporal domain of the data. Evidence of depolicing is robust to this test (appendix figures H71–H74), therefore, other events, such as the onset of COVID-19 and the respective lockdowns in each city, are not driving our findings.[37]

Finally, we evaluated whether the decline in police activity occurred in ways that were similar across different

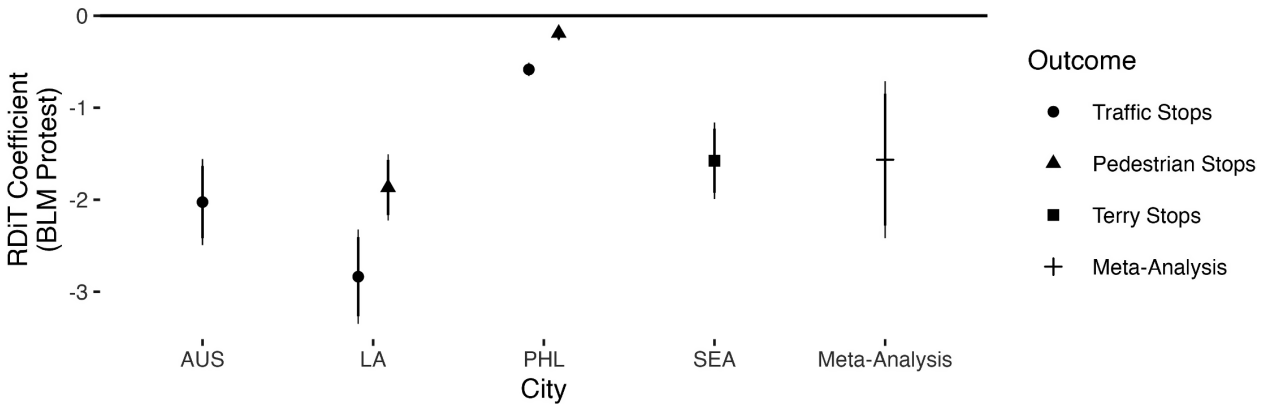
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Figure 1
Policing activity 2 months before and after BLM protests



Each plot characterizes the amount (y-axis) of daily (x-axis) policing activity for Austin (panel A), Los Angeles (panels B–D), Philadelphia (panels E–F), and Seattle (panels G–-H). Dashed vertical line denotes the onset of the 2020 BLM protests. Facet title denotes the specific outcome.

Figure 2
Standardized RDiT coefficients characterizing effect of BLM protests (y-axis) on policing activity across cities (x-axis)



Shape denotes outcome type across the cities. All estimates are from RD specifications with a uniform kernel and polynomial degree equal to 1. Study-adjusted random effects meta-analytic coefficient on display. 95% CIs displayed derived from robust SEs. Associated regression estimates can be found in appendix table B2.

neighborhoods. We may observe declining police activity in poorer or nonwhite neighborhoods, or we may observe shifting service provision from white and wealthy neighborhoods to nonwhite and poor neighborhoods. While we do not have geographic indicators associated with stops in all city contexts, we do have police beat where stops occurred in Seattle, and addresses for Terry stops in Austin, and vehicle stops in Philadelphia. In Seattle, we evaluated changes in Terry stops and officer-initiated 911 calls among police beats with the highest/lowest concentrations of nonwhites, and among those beats where income fell above/below the city's median. We

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found no differences in depolicing by race, class, and geography in Seattle. The analyses are displayed in table C6 in the appendix. Likewise, we evaluated changes in Terry stops in Austin, and vehicle stops in Philadelphia among block groups with the highest/lowest concentration of nonwhites generally, the Black population specifically, and income in the highest and lowest terciles. We found no differences in these cases either. These analyses are shown in table C7 and figure C2 for Austin and table C9 and figure C5 for Philadelphia.

Is Depolicing due to Reduced Civilian Demand?

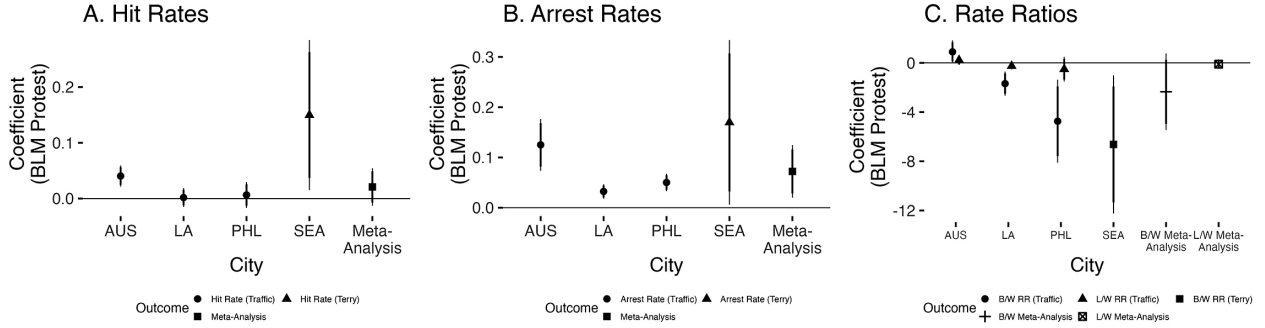
An alternative explanation for the finding that the *BLM protests* decreased police activity is that civilians reduced demand for police services instead of the police restraining their activity. Reductions in civilian demand may be due to individuals staying home during the protest or a reticence to request police intervention brought on by the protests themselves (Ang et al. 2021). We explore and evaluate whether the negative effect of the *BLM protest* on police activity is a function of reduced civilian demand in appendix section N and demonstrate that our results are not entirely a function of shifts in civilian demand for police service.

Hypothesis 2: Pro- or Anti-Social Police Responses?

We find mixed evidence with respect to *quality of policing*. Recall that Hypothesis 2A anticipates an improvement in the quality of policing overall, while the null Hypothesis 2B anticipates no change (or a decline) in quality. We measure quality of policing in terms of change in hit rates, arrest rates, and Black/white and Latino/white stop rate ratios. Figure 3, panel A suggests the *BLM protests* discontinuously increased the hit rate in Austin and Seattle by 0.04 and 0.15 respectively ($p < 0.001$, $p < 0.05$), 178% and 43% of the pre-treatment mean (or 240% and 64% of the outcome standard deviation). However, the hit rate does not discontinuously increase post-*BLM protest* in Los Angeles or Philadelphia. Moreover, the positive coefficients for Austin and Seattle are not temporally sustained. Auxiliary analyses excluding days immediately post-*BLM protest* demonstrates improved hit rates last only 15 and 30 days for Austin and Seattle respectively post-*BLM protest* (figures G47 and G50). On balance, with respect to hit rates, we find support for the null hypothesis. Our interpretation of the results in figure 3, panel A, is consistent with the statistically insignificant random effects meta-analytic coefficient across the four cities (B = 0.021, SE = 0.017, $p = 0.31$).[38]

In contrast, panel B suggests the *BLM protests* discontinuously increased the arrest rate in every city. RDiT coefficients range from 0.03–0.15 ($p < 0.001$ for all cities except Seattle at $p < 0.05$), equivalent to 190%–420% of the pre-treatment mean across the cities (or, 72%–296% of the outcome standard deviation). The discontinuous improvement in arrest rates following the protests is robust to a variety of model kernel and polynomial specifications (figures E7–E10), and alternative bandwidths (figures F21–F24). Unlike the hit rate outcome, auxiliary analyses cutting days immediately post-*BLM protest* and re-estimating the RDiT coefficient suggests the improvement in arrest rates persists over time, even up to 100 days post-*BLM protest* (figures G51–G54). These findings are informative, because they suggest the discontinuous increase in arrest rates is not simply a feature of police arresting more people participating in a protest conditional on initiating police contact. It is worth noting that in Austin and Los Angeles, we observe a dramatic improvement in arrest rates directly following the protest, which then declines precipitously by 15 days after the onset of the

Figure 3
RDiT estimates characterizing effect (y-axis) of BLM protests on policing quality across cities (x-axis)



Panels A, B, and C characterize the discontinuous effect of the BLM protests on *hit rates*, *arrest rates*, and *rate ratios* between whites and Black people. Shape denotes outcome type. All estimates are from RD specifications with a uniform kernel and polynomial degree equal to 1. Random effects meta-analytic coefficient on display for hit rate, arrest rate, and rate ratio outcomes. 95% CIs displayed derived from robust SEs. Associated regression estimates can be found in appendix Table B4.

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protests, even as they remain statistically higher than prior to the protests over the longer term. The improvement in arrest rates across various specifications likewise passes the temporal placebo test (figures H71–H74). Moreover, the effect of the *BLM protest* on *arrest rates* across all cities is robust to the inclusion of control covariates (figure J78). Durable and reliable improvements in arrest rates provide the strongest evidence that declines in police stops produced prosocial outcomes, supporting *Hypothesis 2a*. Our interpretation of the results on figure 3, panel B is consistent with our meta-analytic estimate suggesting the *BLM protest* discontinuously increased the arrest rate on average across the four cities (B = 0.07, SE = 0.02, $p < 0.05$).

Panel C indicates that Black/white stop rate ratios discontinuously declined in Los Angeles, Philadelphia and Seattle post-*BLM protest*, with coefficients of -1.8 (Los Angeles, $p < 0.001$); -4.7 (Philadelphia, traffic, $p < 0.01$); and -7 (Seattle, Terry, $p < 0.05$); equivalent to 35% (190%), 128% (200%), and 98% (100%) of the pre-treatment mean (standard deviation) respectively. However, Black/white stop *rate ratios* discontinuously increased post-*BLM protest* in Austin by 0.9 ($p < 0.05$). Auxiliary analyses cutting 0–100 days immediately post-*BLM protest* suggests the decrease in the Black/white stop rate ratio lasts at least up to 50 days (figures G55–G58). These estimates are most reliable across various specifications and robust to temporal placebo tests in Seattle and Philadelphia (figures E9–E10, and H73–H74). Indeed, the negative post-*BLM protest* effect on the Black/white *rate ratio* in Seattle and Philadelphia are also robust to the inclusion of control covariates (figure J79). They are somewhat sensitive to model specification in Los Angeles (figure E8), where it appears that the improvement is shorter term, occurs closer to the discontinuity, and returns to pre-treatment levels (figure G56) sooner than in Seattle and Philadelphia. In contrast, in Austin, the Black/white traffic stop rate ratio increased, though the increase lasted only 10 days, suggesting the discontinuous post-*BLM protest* coefficient is characterizing an effect that is short-term and intrinsic to the context of the protest (figure G55).

Additionally, the post-BLM protest RDiT effect on the Latino/white stop *rate ratio* is statistically null across Austin and Philadelphia, while positive and significant in Los Angeles (B = 0.51, $p < 0.05$). The meta-analytic estimate is statistically null and substantively close to zero. Evidence around the quality of policing as measured by rate ratios is therefore mixed: declines in police stops coincided with an improvement in Black/white stop rate ratios in three out of four cities, and endured in two. In keeping with this interpretation, the meta-analytic estimate is substantively negative (-2.3) but statistically insignificant (SE = 1.58, $p = 0.23$). Moreover, to the extent there were prosocial consequences of the BLM protest on the stop rate ratio between whites and non-whites, they appear (at least initially) stronger for the Black/white ratio relative to the Latino/white ratio. This may be because Latinos are a group relatively peripheral to conversations surrounding the 2020 BLM protests.

Overall, we find the strongest evidence in support of Hypothesis 2a in Seattle and Philadelphia. Declining police stops did not produce durable improvements in hit rates in either city, but are associated with reliable improvements in both arrest rates and Black/white stop rate ratios that persist over time. In contrast, in Los Angeles and Austin, declining police stops were not accompanied by durable and reliable improvements in either hit rates or Black/white rate ratios, cannot be characterized as pro-social, and providing support for Hypothesis 2b.[39]

Is the Increase in Arrest Rates Prosocial?

Given the *arrest rate* is the number of daily arrests normalized over the number of daily stops, the increase in the *arrest rate* across cities post-*BLM protest* could also be a function of reduced thresholds in arrest initialization and subsequent increases in the count of arrests post-*BLM protest.* This concern is particularly relevant since police may increase arrest initialization in direct response to protest activity, which could be conceived as anti-social, as opposed to pro-social, police behavior. In appendix section O, we engage in a series of tests to demonstrate the increase in arrest rates is prosocial and not a function of reducing the threshold for arrest initialization.

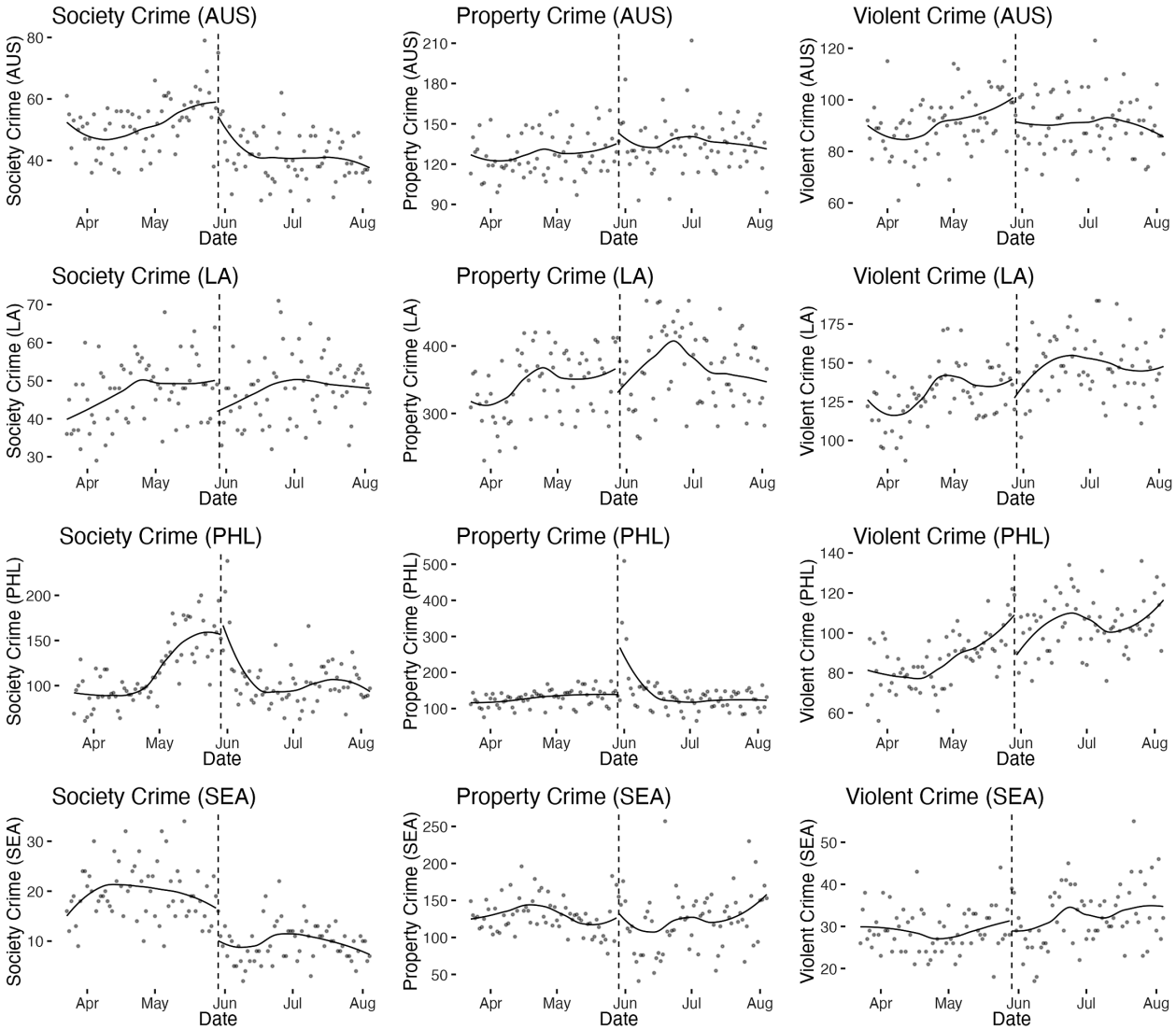
Hypothesis 3: Crime

Hypothesis 3 posits that there will be no change in violent crime following the protests. We also evaluate crimes against society and property, for comparison. The descriptive impact of the protests on crime is displayed in figure 4. In each city it appears that violent crime dipped directly following the protests, but then resumed an overall upward trend that predated the unrest. Figure 5 displays the standardized RDiT coefficients characterizing the discontinuous effect of the *BLM protest* on crime. Violent crime appears to increase in Philadelphia and LA by 0.5 ($p < 0.05$) and 0.9 ($p < 0.001$) respectively but does not change in Austin and Seattle. In Austin and Seattle, the null effect of the protests on violent crime appears to be robust across model specifications (figure E7 and figure E10) and bandwidth specifications (figure F29 and figure F38), and is not distinguishable from patterns of violent crime occurring during the same time period the previous year (figure H71 and figure H74).

In Philadelphia and Los Angeles, the increases in violent crime following the protests appear to be a function of trends that pre-dated the protests. In both cities, the effect of the protests are not significant when the polynomial degree is quadratic or cubic (figure E8 and figure E9), suggesting that there is no change in violent crime close to

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Figure 4
Crime 2 months before and after 2020 BLM protests



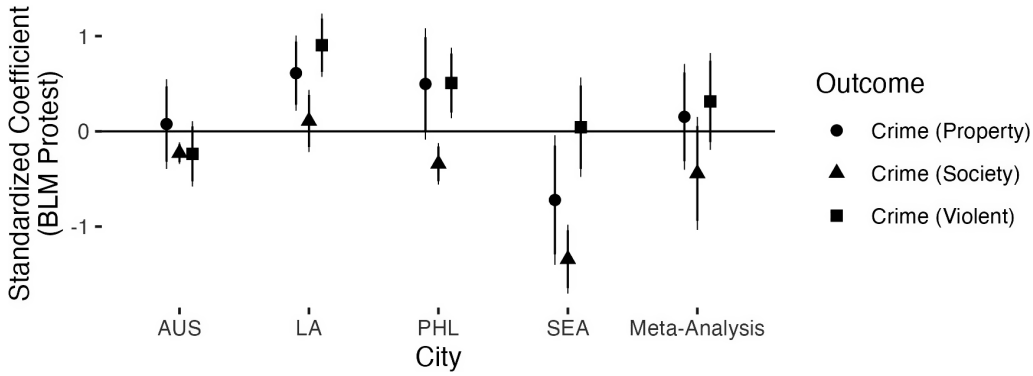
The x-axis is the date, the y-axis is the crime type. For each row, the crime types are *society*, *property*, and *violent* from left to right. From top to bottom, each row characterizes data from Austin, Los Angeles, Philadelphia, and Seattle respectively. Dashed vertical line denotes the onset of the *BLM protests*. Loess models fit on each side of the *BLM protest* onset. Associated regression estimates can be found in appendix table B5.

the discontinuity (confirmed by an examination of alternative bandwidth specifications, figure F35 and figure F32). In Philadelphia, changes in violent crime reflected in the linear estimate are not distinguishable from the temporal placebo test, suggesting that factors other than the protests account for the upward trend (figure H73). In Los Angeles, the difference between changes in violent crime that occurred around the protest and those that occurred the year prior approach statistical significance, but again do not hold across multiple polynomial degrees (figure H72). Moreover, the positive and statistically significant post-*BLM protest* coefficient does not hold after adjusting for balance covariates in Los Angeles and Philadelphia (figure J80). In fact, after balance covariate adjustment, the standardized meta-analytic *BLM protest* RDiT effect on violent crime is close to zero (-0.08). We therefore cannot conclude that the protests themselves (and co-occurring declines in police activity) are responsible for increasing violent crime. Thus, across all city contexts, we find support for HYPOTHESIS 3.

We also evaluate changes in crimes against society and property, which existing literature suggests may fluctuate, given that they are more sensitive to actions taken by police themselves. The protests do not prompt change in

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Figure 5
RDiT estimates characterizing standardized effect (y-axis) of BLM Protests on crime across cities (x-axis)



Shape denotes outcome type. All estimates are from RD specifications with a uniform kernel and polynomial degree equal to 1. Study-adjusted random effects meta-analytic coefficient on display. Ninety-five percent Cis displayed derived from robust SEs.

crimes against society. Figure 5 displays the standardized RDiT coefficients characterizing the discontinuous effect of the *BLM protest* on crime. In all cities but Los Angeles, the linear RDiT coefficients suggest that crimes against society decrease overall. However, only in Seattle are shifts in this category of crime robust to various specifications (figure E10), and are distinguishable from fluctuations that occurred during the same time period the previous year (figure H74). Across all contexts, changes to crimes against society are short-term (figures G61, G64, G67, and G70). On balance, we interpret the discontinuous effect of the *BLM protest* on crimes against society to be null.

Only in Philadelphia does it appear that the *BLM protest* led to a short-term rise in property crime, descriptively. Figure 5 suggests that this temporary increase is not distinguishable from zero. Otherwise, property crime does not appear to change in Austin, increases in Los Angeles by 0.6 ($p < 0.001$), does not statistically change in Philadelphia, and decreases in Seattle by -0.72 ($p < 0.05$) after the BLM protest. In no city context is any observed change to property crime reliable across model specifications (figures E7–E10) or persistent over time. We interpret the discontinuous effect of the *BLM protest* on property crime to be negligible.

Finally, we evaluate whether changes in crime vary across neighborhoods. The aggregate, city-level, measure may not be telling the whole story, if deleterious effects of depolicing on crime are concentrated in, for instance, poor or minority neighborhoods. Using the addresses of crimes committed in Austin, Los Angeles, and Philadelphia, and aggregating to the block group level, we were able to compare the effects between neighborhoods that are in the highest/lowest terciles of nonwhite and Black populations, and on income. We find no differences between these neighborhoods, allowing us to conclude that our effects are not driven by heterogeneous neighborhood-level effects. These results are displayed in table C7 and figure C1 for Austin, table C8 and figure C3 for Los Angeles, and table C9 and figure C4 for Philadelphia.

In sum, we do not find robust and reliable evidence that the protests prompted a rise in any category of crime, including violent crime (the critical test). Meta-analytic estimates of the post-*BLM protest* effect on crime corroborate our interpretation of the results. On average, the meta-analytic, discontinuous effect of the *BLM protest* on property, society, and violent crimes is statistically insignificant (B = -0.44, SE = 0.30, $p = 0.14$; B = 0.15, SE = 0.28, $p = 0.59$; B = 0.31, SE = 0.26, $p = 0.23$). Contrast this with estimates concerning HYPOTHESIS 1, which were highly robust, revealing, across all four contexts and multiple measures, a consistent and dramatic decline in police activity that is robust to a variety of robustness checks. We cannot be similarly confident in any of the findings around crime and are therefore unable to reject HYPOTHESIS 3, which posits that the *BLM protests* will not discontinuously impact violent crime.[40]

Are Our Results Driven by the Onset or Intensity of BLM Protests?

A potential issue with our analyses is that the effect of the BLM protests on police activity could primarily be a function of police responses to the intensity of BLM protest activity itself. For example, policing may decline or arrest rates may increase not just because of the *onset* of the BLM protest and concomitant public scrutiny, but also because of police working directly in ways related to the protests themselves (i.e., policing the protests, crowd control, traffic control). Thus, in the appendix (section Q), we conceptually distinguish between BLM protest *onset* and BLM protest *intensity*. We show that both the *short-* and *long*-term effect of BLM protest onset affects our

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outcomes in a manner consistent with our main results *net of adjusting for BLM protest intensity* (i.e., daily BLM protest count, daily BLM protest crowd size) across the four cities we analyze. In short, our results are not driven by the police directly working in ways related to the protests themselves, but rather, by the initial onset of the BLM protests.

Conclusion

We asked what was the impact of the 2020 BLM protests on policing and public safety? In the event that the protests prompted declines in service provision, what quality did that depolicing take? And finally, did the protests and concurrent declines in police activity impact crime? In order to address these questions, we evaluate police activity in four cities, drawing together an array of data unprecedented in detail and breadth, and leverage an RDiT approach to identify the direct impact of the protests on downstream outcomes. Across all four cities, we find strong evidence that the 2020 BLM protests led to depolicing, but little evidence that declines in service provision were accompanied by a rise in violent crime.

With respect to the quality of policing, results are mixed. We do not observe any sustained improvement in hit rates. At the same time, we do observe an improvement in arrest rates, suggesting that when officers do stop people they are more often doing so for reasons related to observed criminal activity. Both declining stops and improved arrest rates are likewise accompanied by declining disparities in stop rates between Black and white civilians in three out of four cities, and improvements in racial disparities persist in two. We find stronger support for Hypothesis 2a in Seattle and Philadelphia, leading us to characterize the quality of depolicing in these cities as mostly pro-social. We find stronger support for Hypothesis 2b in Los Angeles and Austin, leading us to characterize the quality of depolicing in these cities as mostly anti-social. In all four cities, however, there was some evidence along one or more dimension that the character of depolicing was pro-social. More generally, less contact between police and civilians that does not impact public safety is normatively pro-social.

We cannot disentangle the mechanisms by which declines in service provision occur, and by extension the character that depolicing takes. It may be that officers are genuinely improving the deployment of stops in response to demands made by the protesters. There is not much contextual evidence to support this idea. The response from elected officials across cities was mixed, with the exception of Los Angeles where the Mayor and City Council were unified in support of the protester's demands. It may simply be that shifting to relying more heavily on practices that require a higher threshold of suspicion itself produces more pro-social outcomes rather than relying more heavily on tactics that have a lower threshold. This would comport with research elsewhere evaluating the impact of reliance on consent searches on downstream outcomes, which finds that these kinds of strategies do not improve public safety outcomes (Boehme 2023; Epp and Erhardt 2021). It may also be the case that our metrics of quality are limited. Things like lawsuits, police-involved killings, complaints against officers, and budgetary shifts are other potential measures of policing quality that can shed light on the relationship between law enforcement and civilians in a given city. In sum, while there is evidence that depolicing yields some pro-social outcomes, contextual evidence and existing literature suggest that this is because of the intrinsic nature of the stops themselves, and not a reflection of accountability to protester demands—and future research should evaluate additional evidence of the relative prosociality of police behavior.

Our conclusions are threefold. First, even though we cannot determine that officers reduced discretionary stops out of an interest in meeting protester demands, we nevertheless conclude that public protest is a viable path for citizens fighting to achieve a decrease in police-citizen interactions. In this regard, protesters were remarkably effective, causing a dramatic decline in police activities. This is an important finding as there has been much scrutiny of high-contact and high-discretion modes of policing that drive racial disparities but produce very little in terms of contraband, arrests, or other readily apparent crimefighting benefits. That police made fewer stops across all four city contexts would likely be viewed as good news by the citizens calling for reforms in the summer of 2020.

Second, a chief contribution of our analysis concerns not only whether reduced contact occurred, but also how to characterize the nature of that reduced contact. We evaluated the quality of depolicing in terms of efficiency of stops, whether an arrest was made following a stop, and whether racial disparities improved. We therefore leverage new metrics of quality to develop a more nuanced understanding of withdrawal of service provision. Our findings suggest that this withdrawal can produce a net good, insofar as it is not associated with declining public safety. That said, identifying the city- or leadership-level factors that can promote systematic improvements in policing quality is an important area for future research.

Finally, our analysis offers reassurance to those worried about the public safety consequences of less policing. Violent crime, in particular, only appeared to increase in Los Angeles and Philadelphia, but these estimates do not stand up to rigorous analysis and appear to be attributable to temporal trends not intrinsic to the protests themselves. In Seattle and Austin, violent crime did not change as a consequence of declining police activity. This finding highlights that the kind of discretionary police activities that can easily change in the day-to-day are not the kind of

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activities that most effectively reduce violent crime, giving cause to rethink rote policing practices in American cities.

Supplementary material

To view supplementary material for this article, please visit http://doi.org/10.1017/S1537592725000052.

Data Replication

Data replication sets are available in Harvard Dataverse at: https://doi.org/10.7910/DVN/KKFQAH.

Notes

1 A Terry stop is a type of stop whereby officers detain an individual, whether on the street or in a vehicle, based on reasonable suspicion of criminal activity (Meares, Tyler, and Gardener 2015). Reasonable suspicion is a lower threshold of evidence than is required for an arrest, and relies on the consent of the individual stopped in order to proceed to an investigation of that individual's person or property. This type of stop get its name from the 1968 court case, *Terry v. Ohio* which established such stops as constitutionally permissible.

2 We consulted city employees involved in managing the city's data where appropriate (Roman et al. 2025).

3 This is to have sufficient data to conduct temporal placebo tests and assess if the 2020 BLM protests had an effect on our outcomes of interest larger than pre-treatment discontinuities

4 See table A1 for a full enumeration of police data available for the top twenty cities.

5 NYC does provide civilian complaint data that could serve as a proxy for crime. Given the unique nature of NYC's crime data and the NYPDs response to the BLM protests, we evaluate the effect of the BLM protest on both policing and crime in NYC in the appendix (section U). Ultimately, we show the BLM protests produced similar outcomes as our overall conclusions across the other four cities we analyze.

6 San Diego is headed by a Republican, and has all requisite data but crime. We evaluate San Diego as a robustness check. The full analysis is included in appendix section I, and is referenced where appropriate.

7 There is no way to speak about the protests that occurred in the cities under study without noting that violence occurred. In all four cities under study there is evidence that officers engaged in violence towards protesters. In all four cities there is evidence that citizens on the street at times committed property damage during the protests. Whether any property damage that occurred during the protests can be attributed to individuals who self-affiliated with the protests is unclear from available evidence. That should not be taken to mean that the BLM movement encouraged violence.

8 How public officials in a given city responded to the protests likely further varied by the partisanship of city leaders. We might anticipate that responses supportive of police diminished the workplace strain felt by officers, decreasing the likelihood of depolicing, and perhaps especially of decreasing antagonistic or anti-social depolicing. We evaluate San Diego to address this concern. Indeed, city officials in San Diego responded to the protests by voting almost unanimously to *increase* the police budget by $27 million (Flores 2020).

9 Source: https://data.austintexas.gov/browse?q=traffic+stops&sortBy=relevance&tags=racial+profiling

10 Source: https://data.lacity.org/Public-Safety/LAPD-RIPA-AB-953-STOP-Person-Detail-from-7-1-2018-/bwdf-y5fe/about_data

11 Source: https://opendataphilly.org/datasets/vehicle-pedestrian-investigations/

12 Source: https://www.phila.gov/media/20211109145453/executive-order-2021-06.pdf

13 Source: https://www.austintexas.gov/sites/default/files/files/Police/General_Orders.pdf

14 Source: https://policingequity.org/images/pdfs-doc/COPS-Guidebook22.pdf

15 Source: https://seattlepolicemonitor.org/sites/default/files/2022-05/Seattle_Police_Monitor_Comprehensive_Assessment.pdf

16 Unfortunately, we cannot disaggregate hit rate type in the Philadelphia stop data due to data limitations. Moreover, we cannot disaggregate hit rate type in Seattle because the hit outcomes are defined as mutually exclusive even though they may not be since they are operationalized on the basis of offense severity. For instance, an "arrest" may also include a "citation," but this is only recorded as an "arrest" in the data due to the higher severity level. Therefore, we focus on Austin and Los Angeles, whose stop data allow for hit rate type disaggregation, to assess how different hit rate types are correlated.

17 Arrests_{i}/Stops_{i}, where *Arrestsi* is the number of arrests in day *i*, and *Stopsi* is the number of stops in day *i*.

18 Seattle does not consistently record Latino ethnicity of stop subject. Generally, stops of Latinos are misclassified as "white" by police departments if there is no option to indicate Latino ethnicity (Laniyonu and Donahue 2023). Therefore, assuming Latinos are disparately policed relative to whites and are being classified as white (Pierson et al. 2020), we are likely underestimating the Black/white stop rate ratio in Seattle.

19 Racial group population estimates for each city are from the 2010 Census.

20 To evaluate policing quality, we focus our efforts on changes observed in vehicular stops (and omit

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pedestrian stops). We do this for parsimony, since each RDiT estimate presented requires a number of robustness checks, generating a lengthy and cumbersome appendix. An evaluation of pedestrian stops yields similar findings, and are available from the authors upon request.

21 https://data.austintexas.gov/Public-Safety/Crime-Reports/fdj4-gpfu

22 https://data.lacity.org/browse?q=crime&sortBy=relevance&tags=crime+data

23 https://data.phila.gov/visualizations/crime-incidents

24 https://data.seattle.gov/Public-Safety/SPD-Crime-Data-2008-Present/tazs-3rd5

25 https://www.kxan.com/news/local/austin/demonstrators-arrested-overnight-at-austin-po

26 https://www.cbs8.com/article/news/local/black-lives-matter-protesters-take-to-los-angeles-streets-freeway-over-death-of-george-floyd/509-56517320-da5f-48ee-848c-8953efaec162

27 https://www.inquirer.com/news/philadelphia/live/george-floyd-protest-philadelphia-minneapolis-police-20200530.html

28 https://www.capitolhillseattle.com/2020/05/seattle-defiant-walk-of-resistance-protest-planned-over-george-floyd-killing/

29 See: https://ash.harvard.edu/programs/crowd-counting-consortium/

30 We use the conservative CCC crowd size estimate.

31 See: https://ucrbook.com/county-level-ucr-data.html

32 These covariates are: 1) temperature (Heilmann, Kahn, and Tang 2021), precipitation, wind speed (Hart, Pedersen, and Skardhamar 2019), 311 calls (Wheeler 2018), and COVID cases at the daily level (Boman and Gallupe 2020; Rohlinger and Meyer 2024).

33 We primarily present RDiT estimates that do not adjust for balance covariates, but given some slight imbalance on these covariates across cities, we discuss when covariate adjustment may change the results where appropriate in the Results section.

34 We do not pool the data into a single dataset and estimate the discontinuous effect of the *BLM protest* on our outcomes of interest due to differences in the data-generating process and outcome measurement across cities (e.g., Terry stops versus traffic stops, or hit rate measurement differences). Although data-generating process differences may pose issues with the meta-analysis, the meta-analytic estimates can still teach us general patterns concerning the effect of the BLM protests.

35 The Hartung-Knapp random effects approach is advantageous since it adjusts estimates and standard errors in light of study effect heterogeneity, mitigating false positives (IntHout, Ioannidis, and Borm 2014).

36 "Study-adjusted" means if a city has more than one coefficient estimate due to having multiple outcomes in the data, the average of these coefficient estimates is taken within-city for the purposes of inclusion in the meta-analysis. This prevents "double counting" city estimates in the meta-analysis, artificially reducing standard errors.

37 As noted, we also evaluated depolicing in San Diego, CA, which features Republican leadership. Recall that the San Diego Mayor and City Council supported law enforcement, increasing their budget following the protests. Accordingly, while we do observe a decline in police activity directly following the protests, it returns to normal levels by the end of June. The full analysis is presented in section I of the appendix, and appendix figure I75.

38 Given differences in hit rate measurement across cities, we re-estimate the effect of the *BLM protest* on hit rates using more harmonized outcome measures. Since we can disaggregate hit rate type in Austin and Los Angeles, we generate a common hit rate between the two cities where "hits" are defined as the identification of alcohol, drugs, money, and weapons. These hit rate measures are also similar to the Philadelphia hit rate measure, which includes weapons, drugs, and "other" contraband, which may include money and alcohol. Our results and empirical conclusions do not significantly change using more harmonized hit rate measures (section M).

39 We also evaluated changes to the quality of policing in San Diego following the protests. It is unclear what to expect in terms of quality, conditional on partisanship of city leadership. We do observe findings similar to those observed in other cities: there is no impact of the protests on hit rates, but arrest rates and racial disparities do improve slightly. The full results are listed in section I, appendix figure I75.

40 We are not able to evaluate crime in San Diego, due to lack of appropriate data.

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