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

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July 26, 2023

Abstract

The murder of George Floyd in May 2020 sparked a wave of Black Lives Matter protests in many cities throughout the United States. Protesters demanded constraints against the police and policing. 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 racial and ethnic politics.

Keywords: depolicing; bureaucratic accountability; bureaucratic politics; Black Lives Matter

Introduction

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 the most significant wave of Black Lives Matter (BLM) protests to date. By November 2021, protests had occurred in over 140 cities across the US and extended to over 60 countries across all seven continents. The National Guard had been activated in over 20 states. In addition to their unprecedented scale, 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 reduce police brutality, reduce overall contact between citizens and police, and hold police accountable for misconduct animated the movement.

Anecdotal accounts across various media outlets suggest that the protests led to a decline in policing (whether because officers were defunded or demoralized) and in turn a rise in crime. But this is speculation. The impact of the protests on both policing practices and public safety remains an open empirical question with implications for our understanding of policing, protests, and bureaucratic responsiveness. We address this question by first looking for evidence of depolicing in the aftermath of the protests. Existing literature suggests two reasons why police activity may decline following protests. On one hand, police may respond to the demands of protesters by changing their policing tactics in ways that reduce contact with citizens, heighten the efficiency of their work, and, especially, reduce racially unequal outcomes. In other words, depolicing may be a reflection of accountability efforts. Researchers elsewhere demonstrate that protests do have the ability to hold public officials accountable (Gillion, 2012; Gause, 2022). On the other hand, police may respond to demands for reform by changing their tactics in ways springing from demoralization and burnout, or retaliation against citizens and municipal agents critical of the police. Scholars have referred to this kind of behavior as dissent shirking (Chanin and Sheats, 2018).

As such, we can characterize depolicing as pro-social when it meets demands for higher quality policing overall; or as anti-social when it manifests simply as a withdrawal from duty without commensurate gains in public safety or quality (Nix, Wolfe, and Campbell, 2018). While a handful of studies tackle whether depolicing occurs, very few characterize the nature of that withdrawal, raising questions around the reasons officers engage in this behavior and its various consequences for civilians (Nix, Wolfe, and Campbell, 2018). We therefore further ask: in the event that we observe depolicing, can withdrawal best be characterized as pro or anti-social?

Finally, we assess whether the protests, along with any reductions in policing that follow, produced unintended consequences in the form of increased crime. Crime is on the rise in cities across the country and pundits have vilified the 2020 BLM protests, and particularly the call to defund police, on this point. However, little empirical evidence exists connecting depolicing and protests to crime, and no evidence that we know of connects the protests of 2020 specifically to rising violence in cities. Yet, the tight linkage between the protests and rising crime asserted by various popular actors raises the need to investigate this question in the current political moment.

To answer these questions, we evaluate policing data in four contexts: Seattle, WA, Austin, TX, Philadelphia, PA and Los Angeles, CA. We surveyed the 20 largest cities in the United States by population size, and included all those contexts where sufficient incident level policing data with all relevant outcomes were available, either on the city's open data portal or via public records request. Ultimately, the cities included in the analysis represent wide geographic variation, as well as variation on intensity of protests. Across all cities, we evaluate changes in traffic and pedestrian stops (depending on context and data availability). Using a regression discontinuity-in-time approach, we find a discontinuous and persistent drop in officer contact with civilians. This finding holds across all metrics and persists over time in all four city contexts. Leveraging 911 calls in two cities, we find that the change in stops is not driven by citizen demands for officer intervention.

To measure nature of depolicing, we evaluate changes in three outcomes that speak to policing quality: contraband hit rates, arrest rates, and racial inequality in stops. Hit rates do not demonstrably and consistently improve; arrest rates do improve across all contexts; and black-white stop ratios decline, at least in the short term, in all but one context. Only in Seattle can we characterize the nature of depolicing as pro-social across all metrics employed. Across the other three cities, the nature of depolicing is mixed. With respect to crime, the critical test is violence, which is understood to be less sensitive to policing tactics themselves (relative to crimes against society, which are endogenous to attention). We do not find consistent evidence that an increase in violent crime accompanies depolicing following the protests. Any change in violent crime appears to be a function of trends that predated the protests.

In sum, we leverage several years of high resolution, incident level data that captures police activity in the days before and following the 2020 BLM protests to evaluate their impact on policing. We restrict our analysis to four cities because these cities offer data that are both rich and broad in nature, enabling an evaluation of both whether depolicing occurred and the character of police withdrawal. This allows us to make several original contributions: First, we offer systematic and robust evidence that protests can compel widespread and durable changes in police behavior. Second, while depolicing is real, the underlying motivations of law enforcement that drive this withdrawal appear idiosyncratic in that 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, across the board pro-social changes in Seattle point to a top-down effort by police or political leadership at coherent reforms. Third, we show that depolicing (at least to the levels seen in the four cities under investigation) does not have clear consequences for violent crime.

In what follows, we provide an overview of the existing literature on depolicing, particularly in the wake of anti-police protests. We then review research on the conditions under which public servants are likely to change their behavior in response to public opinion in order to develop hypotheses around how police officers might react to BLM protests. We then review our case selection, data and analytic strategy, and review the results.

Background

Depolicing as a response to Anti-Police Protests

Little research has explicitly evaluated if anti-police protests are successful in extracting higher quality outcomes from law enforcement. Instead, scholars have focused on the extent to which police are leveraged to manage and quell protests (e.g. Davenport, Soule, and Armstrong, 2011); 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).

A few researchers, however, have examined whether depolicing occurs, broadly speaking, and the evidence that it does occur systematically is mixed. Interviews with officers themselves indicate that they believe depolicing happens, and that individuals engage in this behavior for a variety of reasons (Nix, Wolfe, and Campbell, 2018; Oliver, 2017; Gau, Paoline III, and Paul, 2022; Foster, Rossler, and Scheer, 2023). Yet, surveys of law enforcement both before and after the 2014 Ferguson uprising suggest that even as this may occur on an individual level, this behavior is limited in scope and duration such that there is minimal evidence that it impacts policing in the aggregate (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. At the same time, 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). Even so, these studies rely on data aggregated to the year-agency level, and are therefore imprecise and vulnerable to omitted variable bias. Moreover, existing analyses provide no insight into how we should understand the character that depolicing took, which likely varies by agency.

More generally, scholarship has shown that public officials, both elected and non-elected, respond to protests organized around racial justice. Legislators motivated by reelection are sensitive to protests when they take place in the lawmaker's district (Gillion, 2012). Protests impact lawmaker decisions indirectly as well by durably shifting public opinion (Wasow, 2020; Enos, Kaufman, and Sands, 2019). It makes sense that elected officials are responsive to protests, and that mayors and members of city councils may likewise respond to protester demands by intervening in law enforcement activities. While research suggests that bureaucrats, who are not elected, are most responsive to protesters when there is a relatively high degree of political control over the agency, they may nevertheless be moved out of a desire to protect the legitimacy of their institution (Alon-Barkat and Gilad, 2016). This may be particularly relevant in the case of policing, which is facing a crisis of legitimacy that predated the events of 2020 (Bell, 2016; Meares, 2015).

There is some evidence that protests impact police behavior directly. Examining the consequences of the civil rights movement, Cunningham and Gillezeau (2018) find evidence of police backlash, where protests spurred a significant and persistent increase in officer involved deaths of non-white civilians. At the same time, they do not observe any meaningful change in police employment or overall crime. It may be that this response on the part of police was particularly pronounced in an era so instrumental to the dismantling of Jim Crow segregation. More recently, scholars observed a decline in fatal interactions between police and black civilians following the first wave of BLM protests in conjunction with the Ferguson uprising in 2014 (Skoy, 2021). Scholars have also observed heightened officer resignations following the 2020 BLM protests (Mourtgos, Adams, and Nix, 2021).

In sum, evidence exists suggesting that police do, at times, directly respond to anti-police protests by changing their behavior. With respect to depolicing, researchers have taken two approaches: they interview officers, and they evaluate stop and arrest rates following acute criticism from citizens and public officials, whether it takes the form of protests or federal intervention. When asked, officers confirm that depolicing occurs, although evidence that it does so systematically is mixed. Whether anti-police protests can compel a widespread and durable change in officer behavior therefore remains an open question.

The Character of Depolicing

In the event that officers do curtail their activities, reasons for doing so are varied. As noted above, officers may be concerned about the legitimacy of their institution and depolicing may reflect accountability and responsiveness to community demands. For example, 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. This directive occurred on the eve of a trial litigating the racialized patterns of stops in New York City, which itself was precipitated by citizen activism protesting the use of the practice. This chain of events suggests not only that the highly discretionary nature of policing practices means that an immediate change in officer behavior in response to protests is possible, but also provides evidence that they do sometimes respond to critique in ways that are pro-social.

At the same time, research around police and the extent to which they can be held accountable is dominated by questions of shirking and how it can be stopped (Eckhouse, 2021). Shirking is understood to be a common and persistent problem among departments, in large part because most routine activities police undertake happen in the field and are relatively unsupervised. With respect to depolicing, scholars have dubbed the 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 civilians and/or public officials (Chanin and Sheats, 2018). Yet, qualitative evidence suggests that officers' reasons for withholding services are varied. Dissent shirking carries with it the implication of retaliation, where officers withdraw from duty because they disagree with the politics of protesters calling their activities into question. Officers may nevertheless police less overall and reduce stops in minority neighborhoods because they do not want to draw attention to themselves or risk becoming the focus of a civil inquiry. This kind of behavior might be better characterized as avoidant than as dissident (Nix, Wolfe, and Campbell, 2018). Officers may likewise police less because they are overwhelmed or exhausted by the demands of the job, and feelings of burnout may be exacerbated by public criticism over policing practices (Oliver, 2017). Scholars have leveraged strain theory to loosely organize officers' responses to an increasingly stressful work environment that may result from external criticism (Nix, Wolfe, and Campbell, 2018). From this perspective, depolicing is a coping mechanism leveraged 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 III, 2004; Paoline III, 2003; Mac Donald, 2017).

We may observe depolicing occurring in the aggregate within a given city, and all of these factors may be at work since they vary at the individual level. In the absence of a clear, top-down directive from agency leadership (as in the case of Mummolo 2018) it is not possible to ascertain a singular motive for declines in discretionary policing using the kind of administrative data required to evaluate whether depolicing is occurring in the first place. However, even without assessing underlying motivations, we may be able to characterize the substantive nature of declining police activity as either pro- or anti-social. Pro-social depolicing would manifest as increasing efficiency (for example, higher hit rates when stops do occur), declining racial disparities in stops, or better service provision in marginalized communities (Nix, Wolfe, and Campbell, 2018; Shjarback et al., 2017; Rosenfeld and Wallman, 2019). In contrast, depolicing that we might characterize as anti-social would yield declines or no real improvement in the quality of policing on any of these metrics even as the quantity of policing lessens. In communities where over-policing is a concern, even this type of depolicing might be welcome, but it can still be thought of as anti-social because it is less likely to be driven by efforts to improve policing outcomes, and may lead to declines in public safety.

Very few studies examining depolicing take an additional step to characterize the nature of declining police activity that follow instances of public outcry over law enforcement practices. Shjarback et al. (2017), who do observe declining misdemeanor arrests in Missouri after the Ferguson uprising, find no improvement in hit rates. Rosenfeld and Wallman (2019), who are focused on crime among cities nationally also in the wake of Ferguson, observe neither a decline in arrest rates nor a differential drop in arrests by race. The dearth of research on the underlying character of police withdrawal prompt Nix, Wolfe, and Campbell (2018) to call researchers to, "uncover the underlying reasons for depolicing," (p. 47). While we cannot measure individual officers reasons for declining activity, an evaluation of the overall character depolicing takes is possible, and together with an account of the political context in which depolicing occurs may shed light on the factors that prompt pro- or anti-social outcomes.

Anti-police protests and 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, spurred by the murder of Michael Brown by Officer Darren Wilson. Findings are equivocal. Researchers have failed 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).[1] Some scholars have rejected the phenomenon solely as political rhetoric (Oliver, 2017). Others have simply found no real association between violent crime and depolicing (Rosenfeld, 2020; Neyroud, 2019). However, as noted above, very few studies attempt to characterize the nature of depolicing, and it may be that whether depolicing is pro- or anti-social likewise has consequences for crime. Pro-social depolicing, where police activities are higher quality overall, may lead to a decline in crime or they may have no impact on crime. In contrast, anti-social depolicing, where officers simply withhold service, may lead to increases in crime.

Expectations

How individual officers responded to the protests in any given city is unclear. In the wake of the protests, protester demands to defund the police, and efforts to reform use-of-force practices, departments across the country are facing officer shortages (Young, Sayers, and Sanchez, 2022; Conklin, 2022). The Seattle Police Department, one of the agencies included in this study, contended with record officer departures following the protests themselves. The Austin Police Department, another agency included in this analysis, is also contending with officer shortages, so much so that the mayor recently enlisted state troopers to patrol the city's streets (Fetcher, 2023).[2] The context of the protests in these and other cities heightened the level of strain felt by law enforcement. That strained context is reflected in the volatile nature of the protests in many cities, and ongoing criticism of law enforcement and efforts by local officials to reform policing practices that followed. These are exactly the circumstances that might give way to depolicing. For these reasons, we develop the following

[1]The one exception is Piza and Connealy (2022), who found that violent crime rose in and around the east precinct in Seattle, which police vacated during the course of the 2020 Black Lives Matter protests. However, total withdrawal of police service provision, including in response to civilian calls for service, is an exceptional and extreme form of depolicing that does not characterize draw-downs in discretionary service sometimes observed following public scrutiny.

[2]Reports of officer shortages should be understood in context. All sectors are experiencing a labor shortage – including other emergency response agencies in both Seattle and Austin, exacerbating public safety concerns (Markovich, 2022; Duret and Li, 2023).

hypothesis:

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

The character that such withdrawal is likely to take is unclear. On one hand, extant literature suggests that protests can function to hold city officials accountable by exerting political pressure. 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, then, that any decline in discretionary police activity we observe following the protests reflects accountability to protester demands vis-a-vis elected officials. In this instance we may expect the quality of policing to improve overall.

It may also be the case that declining police stops following from the protests are accompanied by an improvement in the quality of discretionary policing practices overall because of the nature of discretionary tactics. That is, the logic that underlies preemptive practices over which officers have discretion requires a high volume of civilian interactions in order to identify contraband, and relies on questionable assumptions to determine who is likely to commit crime and where it is likely to occur. Engaging less in these kinds of stops may therefore lead to an improvement in quality of policing overall simply because officers instead shift to relying on practices requiring a higher threshold of suspicion (e.g. officers may shift to relying more heavily on probable cause rather than consent to initiate contact with citizens). Even as we cannot disentangle the motivation for depolicing, both of these possibilities – accountability and shifts in the kind of stops officers engage – lead to the following hypothesis:

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

On the other hand, evidence that city councils enforced the reforms they passed early on in the protest period (including cuts to police budgets, in some cases) is slim (Walsh, Goodin-Smith, and Seidman, 2021; Kamb and Beekman, 2021). Instead, in many cities, reforms were regarded as largely symbolic. Moreover, there is anecdotal evidence from the cities included in this study of officer dissatisfaction with the anti-police sentiment expressed via the protests ("Over 200 Seattle police officers quit amid nation protests" 2021; Horcher, 2020; German, 2020). Interviews with officers indicate that it is this kind of dissatisfaction that leads to dissent shirking. In the instance that depolicing is driven by officer dissatisfaction and is not accompanied by any kind of external accountability, we may observe no change in the quality of policing overall. This generates the following alternative hypothesis:

Hypothesis 2b: There will not be a discontinuous change in the quality of policing overall following the 2020 BLM protests.

Finally, we are concerned with the impact of the protests on crime. Previous work has done very little to distinguish between pro- and anti-social depolicing, and it may be that anti-social declines in service provision are associated with increased crime. However, existing literature gives us no reason to expect that increased violent crime will follow either from depolicing or the protests. The kind of policing tactics over which police have a high degree of discretion, and whose deployment can shift in the day-to-day, are not the kind of tactics with a strong track record of abating violent crime. They are instead the kind of tactics designed for order maintenance, which has not been consistently linked to improved outcomes vis-a-vis violent crime. This leads to the following, final hypothesis:

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

Data and Design

Case selection

To select cities for inclusion in our analysis, we began by surveying the top 20 most populous cities in the United States according to the 2020 Census in an effort to obtain the following kind of data for each city: incident level records of police activity, such as stops and/or officer initiated 911 calls; records needed to cover the time period between May of 2019 (1 year prior to the onset of protests) and May of 2021 (1 year post the start of the protests); records needed to include metrics of quality of policing, such as recovery of contraband, and crucially, the race of civilian stopped; and we needed incident level records of crime that we could aggregate to the daily level (where previous work has relied on monthly level counts of crime provided by the UCR). We identified four cities: Seattle, WA, Philadelphia, PA, Los Angeles, CA, and Austin, TX. No other city of which we are aware provides data rich enough in detail to adequately evaluate the hypotheses identified above.[3] These four cities provide some regional coverage, as well as variation in the intensity of the protests and the responses of local city officials. For every city, we obtained incident-level data on police activity and crime via the city's open data portal. We consulted city employees involved in managing the city's data where appropriate.

The protests in Seattle were some of the most volatile in the country, and the response of political leaders was divided and mercurial. The volatility of the protests can be attributed, at least in part, to a long history of conflict between the police and the community. In 2011, the use-of-force practices engaged by the Seattle Police Department (SPD) were investigated by the DOJ at the behest of activists in the city, and the SPD was placed under federal oversight ("Timeline of Seattle Police Accountability" 2021). Efforts at meaningful reform were met with resistance, and after years of failed efforts, community leaders pushed for a

[3]For example we considered including Denver, CO, but they did not provide information on the race of civilian stopped. Similarly, Dallas, Phoenix and Chicago do not provide any information that would allow us to evaluate the overall quality of policing, and New York City does not provide information on crime outcomes.

new accountability law in 2017. During the same time period, the City Council adopted a new contract with the Seattle Police Officers Guild – a contract whose provisions pushed the department out of compliance with the consent decree. Despite this, the city moved to end outside monitoring imposed by the decree in May of 2020, just days before the murder of George Floyd in Minneapolis.

The subsequent protests, sparked by and in solidarity with the people of Minneapolis, drew thousands of citizens and they quickly became violent. Reports indicate that police were using pepper spray, "indiscriminately and vindictively, punching and kneeling on the necks of people who had been arrested, and using flashbang grenades," ("Timeline of Seattle Police Accountability" 2021). By June 1, the Office of Police Accountability reported receiving 12,000 complaints against officer misconduct during the weekend protests. The center of protest activity shifted to the Capital Hill neighborhood. Capitol Hill is the location of the East Precinct. Famously, police vacated the East Precinct, and protesters established the Capitol Hill Autonomous Zone (CHAZ). While the mayor at first appeared sympathetic to the protesters, the City Council was itself divided and has continuously voted to increase funding for the police department. The deeply volatile circumstances in Seattle certainly created a strained environment for officers, and there does not appear to be much responsiveness to protester demands from city officials. This is perhaps the most likely case where we would expect to observe depolicing, although, as noted above, whether changes to police behavior will yield pro- or anti-social outcomes is unclear.

Austin differs from Seattle in a number of ways that have implications for police behavior following the protests. The protests in Austin were deeply contentious, particularly given that they erupted one month after the death of Mike Ramos, an unarmed Black and Latinx man, at the hands of police on April 24, 2020 (Holley, 2020). Like in Seattle, the protests persisted over several days, and were at times violent. There was property damage that resulted from the protests, but not to the level observed in Seattle (Wilson and Bailey, 2020). Following the protests, the police department underwent an internal review to assess its performance, and concluded that the department was underprepared, in part because the protests themselves were, “unlike anything this city or department had seen.” For it’s part, the Austin City Council and Mayor Adler were immediately and unanimously in support of the activists (Venkataramanan, 2020; Fernandez and Mccullough, 2020). Less than three weeks after the protests erupted, the City Council approved cutting law enforcement’s budget by a third and passed a suite of policies designed to increase transparency and accountability of law enforcement. The protest environment did create strain, and law enforcement did face external criticism from civilians and city leaders. The conditions were right for depolicing, but whether it persists over time and the character it might take – especially in the presence of top-down directives from elected officials to alter behavior – is less clear. We might expect to observe an improvement in the quality of policing overall.

Philadelphia falls somewhere in between Austin and Seattle in terms of both the contentious nature of the protests and the relationship between the police department and the community. The initial burst of protest activity following the death of George Floyd were, like Seattle’s, characterized by high levels of violence. During the protests, police cars were burned and over 50 ATMs were blown up with dynamite (Palmer, 2020). Officers were caught on film doing things like pulling down a protester’s mask before deploying pepper spray and shooting them with pepper spray at point-blank range (Palmer, 2020). Even so, much of the protest activity died down by June 8 (Gammage, 2020). The Philadelphia City Council responded by putting 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). There is evidence that officers themselves were opposed to the demands made by activists associated with the Movement for Black Lives (German, 2020). The strain of the protests may have prompted a withdrawal of service provision, but given the middling response from city officials it may be that any depolicing observed does not persist over time.

Finally, we include Los Angeles in our analysis alongside Seattle, Austin, and Philadelphia. The protests in LA that followed the death of George Floyd were characterized by both protester and law enforcement violence (Reyes-Valarde et al., 2020). The violence that occurred early on prompted the mayor to issue a curfew, and Governor Newsom to declare a state of emergency, which allowed for the deployment of the national guard (Reyes-Valarde et al., 2020; Petrie, 2020). An investigation into the tactics employed by the LAPD after the fact found that law enforcement badly mishandled the protests, inappropriately using rubber bullets, flashbangs and beanbags, and in one instance, driving a vehicle into the crowd (Bogel-Burroughs, Eligon, and Write, 2021; Petrie, 2020). Activists in Los Angeles were already highly organized around issues related to police reform, pushing for the reallocation of police budgets, and doing so in response to a long history of conflict with the LAPD (Kingdale, 2020). In response to demands made by the activists and the ongoing protests, the LA 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 fighting poverty and homelessness (Munoz, 2021). Thus, while the protests in LA were volatile in ways similar to other cities included in the analysis, the response of city officials was decisively more supportive of protester demands than in other cities included in the analysis. Here again, we expect to observe depolicing. LA may be the most likely case where we would expect depolicing to yield pro-social outcomes, given the response of city officials.

Data

To assess if the 2020 BLM protests reduced discretionary policing (*Hypothesis 1*), we draw on the following incident level data in each city: traffic stops in Austin (January 2019-December 2020);[4] pedestrian stops (July 2018-February 2023) and traffic stops (July 2018-February 2023)[5] in Los Angeles; pedestrian (January 2018-December 2022) and traffic stops

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

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

(January 2018-December 2022) in Philadelphia;[6] 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 in each of the cities under study. First, 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* are the proportion of daily stops that result in the recovery of contraband.[7] In Seattle, *hit rates* are measured differently in that they are the proprotion 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). *Arrest rates* are the proportion of stops resulting in an arrest, suggesting the identified offense during a stop was arrest-worthy. We are also interested in whether any declines in discretionary policing are associated with changes 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. The *rate ratio* is the Black stop rate $((BlackStops/BlackPopulation) \times 10,000)$ divided by the white stop rate $((WhiteStops/WhitePopulation) \times 10,000)$.[8] If *Hypothesis 2a* is true, then the 2020 BLM protests will have a positive effect on *hit rates* and *arrest rates*, and a negative effect on the *rate ratio* indicating a smaller racial difference in the occurrence of stops between white and nonwhite people. Conversely, if *Hypothesis 2b* is true, then the 2020 BLM protests will have no effect on *hit rates*, *arrest rates*, or the *rate ratio*.[9]

[6] https://opendataphilly.org/datasets/vehicle-pedestrian-investigations/

[7] In Philadelphia, contraband is "firearms," "other weapons," "narcotics," or "other contraband" (Source: https://www.phila.gov/media/20211109145453/executive-order-2021-06.pdf). In Austin, contraband is "narcotics," "illegal weapons," and "gambling equipment" (Source: https://www.phila.gov/media/20211109145453/executive-order-2021-06.pdf). 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" (Source: https://policingequity.org/images/pdfs-doc/COPS-Guidebook22.pdf).

[8] Racial group population estimates for each city are from the 2010 Census.

[9] To evaluate quality of policing, we focus our efforts on changes observed in vehicular stops (and do not include an evaluation of pedestrian stops). We do this primarily for the sake of space, since each RDiT estimate presented requires a number of robustness checks, leading to a lengthy and cumbersome appendix.

To test *Hypothesis 3*, we use incident-level crime data obtained from each city's data portal. For each city, we rely on Federal National Incident Based Reporting System (NIBRS) rules for classifying crimes and and separate each respective city's crime data 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,[10] Los Angeles,[11] Philadelphia,[12] and Seattle crime datasets[13] are January 2003-February 2022, January 2010-February 2023, January 2006-December 2022, and January 2008-February 2022 respectively. We evaluate changes to society, property, and violent crimes after the BLM protest, but we are particularly interested in *violent crime*. This is because identification of violent crimes are less sensitive to police effort, and more reflective of civilian reporting (Rosenfeld and Wallman, 2019). Therefore, if police reduce activity post-protests, identification of against person crimes should be less endogenous to police response. In Seattle, for example, 94 percent of violent crimes are assault offenses.[14] Five percent 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. For comprehensiveness, we also evaluate the effect of the 2020 BLM protests on *against property* and *against society crimes*. If *Hypothesis 3* is correct, the 2020 BLM protests should have no effect on *violent crimes*, although we may observe declines in the other two categories, which are more sensitive to the deployment of discretionary policing tactics.

The independent variable for each of the daily time series is a binary indicator equal to one on or after the start of the 2020 *BLM protests* in each city. The start date for the

An evaluation of pedestrian stops yields similar findings to those derived from traffic stops, and are available from the authors upon request.

[10]`https://data.austintexas.gov/Public-Safety/Crime-Reports/fdj4-gpfu`

[11]`https://data.lacity.org/browse?q=crime&sortBy=relevance&tags=crime+data`

[12]`https://data.phila.gov/visualizations/crime-incidents`

[13]`https://data.seattle.gov/Public-Safety/SPD-Crime-Data-2008-Present/tazs-3rd5`

[14]The vast majority of assaults are not identified initially by police, but reported to police and subsequently identified (Friedman, 2020).

BLM protests for Austin, Los Angeles, Philadelphia and Seattle is May 29, 2020;[15] May 28, 2020;[16] May 30, 2020;[17] and May 29, 2020.[18]

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 other factors are jointly driving the onset of protest activity 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 secular trends that may influence policing and criminal activity. However, in order to assess the temporal durability of some of the post-*BLM protest* RDiT effects, we interpret RDiT coefficients that remove 1-100 days immediate after the *BLM protest.* Although this analysis may be subject to secular temporal trends, we believe it is an important exercise to understand the durability of some of the effects we characterize.

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.

[15] `https://www.kxan.com/news/local/austin/demonstrators-arrested-overnight-at-austin-police-headquarters/`

[16] `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`

[17] `https://www.inquirer.com/news/philadelphia/live/george-floyd-protest-philadelphia-minneapolis-police-20200530.html`

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

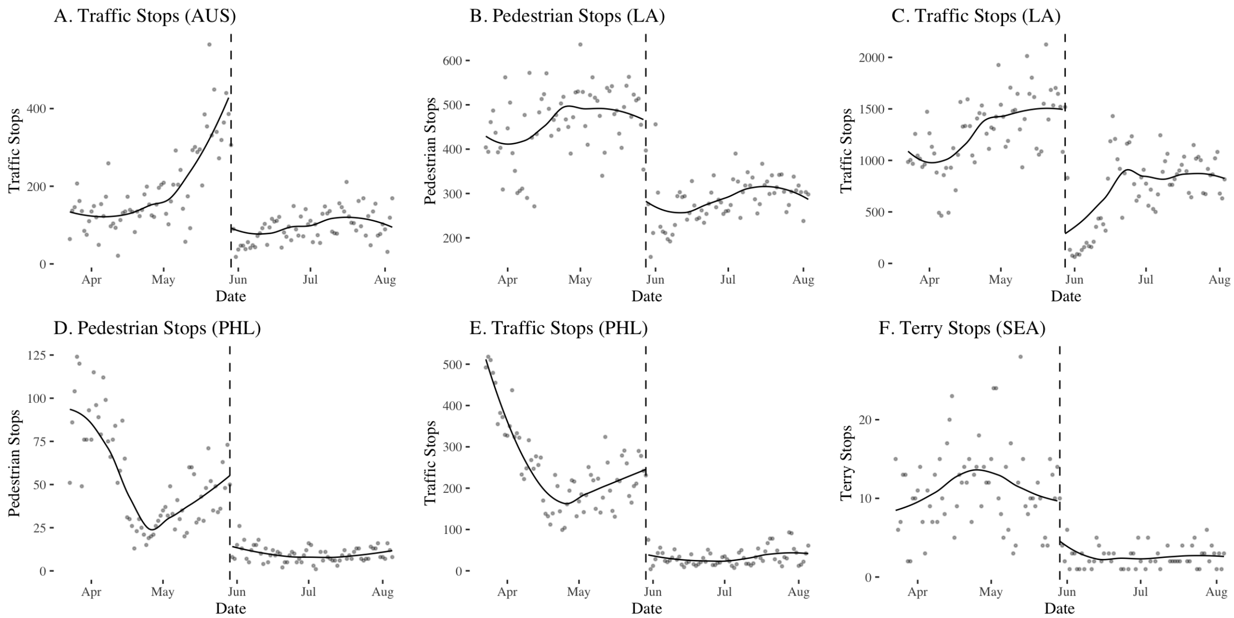

Figure 1: Policing Activity 2 Months Before and After BLM Protests. Each plot characterizes the amount (y-axis) of daily (x-axis) policing activity for Austin (Panel A), Los Angeles (Panel B-D), Philadelphia (Panels E-F), and Seattle (Panels G-H). Dashed vertical line denotes the onset of the 2020 BLM protests. Facet title denotes the specific outcome.

Results

Hypothesis 1: Depolicing

We find support for *Hypothesis 1.* Figure 1 provides a descriptive account of the volume of discretionary policing activity (pedestrian, traffic, and terry stops, depending on the data provided by a given city) before and after the protests. There appears to be 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.[19] Across all cities

[19]Did a decline in police activity occur in ways that were similar across different geographic contexts? We may observe declining police activity in poorer or heavily 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. In this city, then, to assess heterogeneity by geographies of race and class, we evaluated changes in terry stops and officer initiated 911 class among police beats with the highest and lowest concentrations of nonwhites, as well as among those beats in areas where income fell above and below the city's median. We found no differences in depolicing by race, class, and geography in Seattle. The analyses are displayed in Table B5 in the appendix.

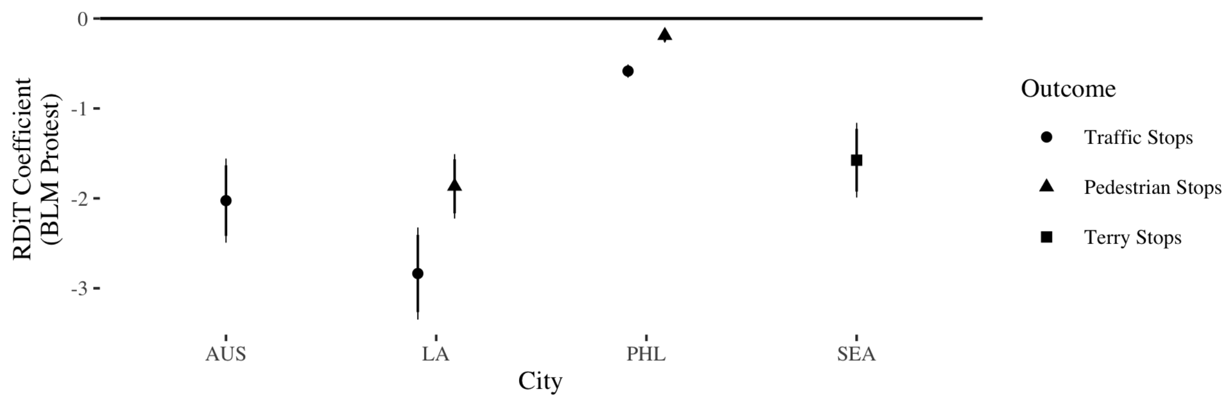

Figure 2: Standardized RDiT Coefficients Characterizing Effect of BLM Protests (y-axis) on Policing Activity Across Cities (x-axis). Shape denotes outcome type across the cities. All estimates are from RD specifications with a uniform kernel and polynomial degree equal to 1. 95% CIs displayed derived from robust SEs. Associated regression estimates can be found in Appendix Table A1.

and outcomes, there is a substantially large and statistically significant decrease in policing activity ($p < 0.001$ for all coefficients). The RDiT *BLM protest* coefficient is from -0.2 to -2.8 standard deviation across the cities and outcomes. These effects are not simply short-term effects intrinsic to the onset of the *BLM protests*. We conduct an auxiliary analysis and re-estimate RDiT coefficients omitting 1-100 days immediately after the *BLM protest* onset to evaluate if there is still a discontinuous decrease in policing activity several days after the onset of the *BLM protests* relative to just before the *BLM protest* onset. Across all cities, we find persistent decreases in policing activity at least 100 days after the first BLM protests (Appendix Figures F36-F41). These estimates are also robust across kernel and polynomial specifications (Appendix Figure D2 - Figure D5), in addition to alternative bandwidths (Appendix Figure E6 - Figure E11). Finally, we conducted a temporal placebo test to assess whether changes in policing following the 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 G66 - G69).

Is Depolicing Due To Reduced Civilian Demand?

An alternative explanation of our findings characterizing the effect of the *BLM protests* on depolicing is that our results are driven by reductions in civilian demand for police services instead of the police self-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 rule out this possibility by leveraging available 911 call data from two of the four cities we analyze: Seattle (2010-01-01 to 2021-01-01) and Los Angeles (2019-01-01 to 2021-01-01).[20] Emergency call data from these cities can be disaggregated between calls initiated by civilians and by police officers. Officer initiated 911 calls are often reports of encountered incidents.[21] Therefore, officer-initiated 911 calls serve as a measure of policing, while civilian-initiated calls serve as a measure of civilian demand for police services. Importantly, our goal is to rule out the possibility that declines in police stops are not wholly accounted for by reduced civilian demand. If we can show that decreases in officer-initiated 911 calls are more substantial and persistent than decreases in civilian-initiated calls, then we have suggestive evidence police self-restraint is operative in policing patterns net of civilian demand.

Figure 3 shows officer and civilian calls over time in both Seattle and Los Angeles. In Seattle, officer calls appear to discontinuously decrease post-*BLM protest*, and the decrease is persistent well into the rest of 2020 (Panel A). Conversely, civilian calls appear to discontinuously decrease post-*BLM protest*, but rebound to levels prior to the *BLM protest* by the end of 2020 (Panel B). Additionally, the decrease in officer calls appears much more substantial at the discontinuity than the decrease in civilian calls. The pattern is similar in Los Angeles. Officer calls discontinuously and persistently decrease while civilian calls also discontinuously decrease, but to a lesser extent then officer calls (Panels C-D). The descriptive statistics are corroborated by RDiT estimates characterizing the effect of the *BLM*

[20] Source: `https://data.seattle.gov/Public-Safety/Call-Data/33kz-ixgy` and `https://data.lacity.org/browse?q=calls%20for%20service&sortBy=relevance`

[21] Based on our correspondence with LA and Seattle Open Data.

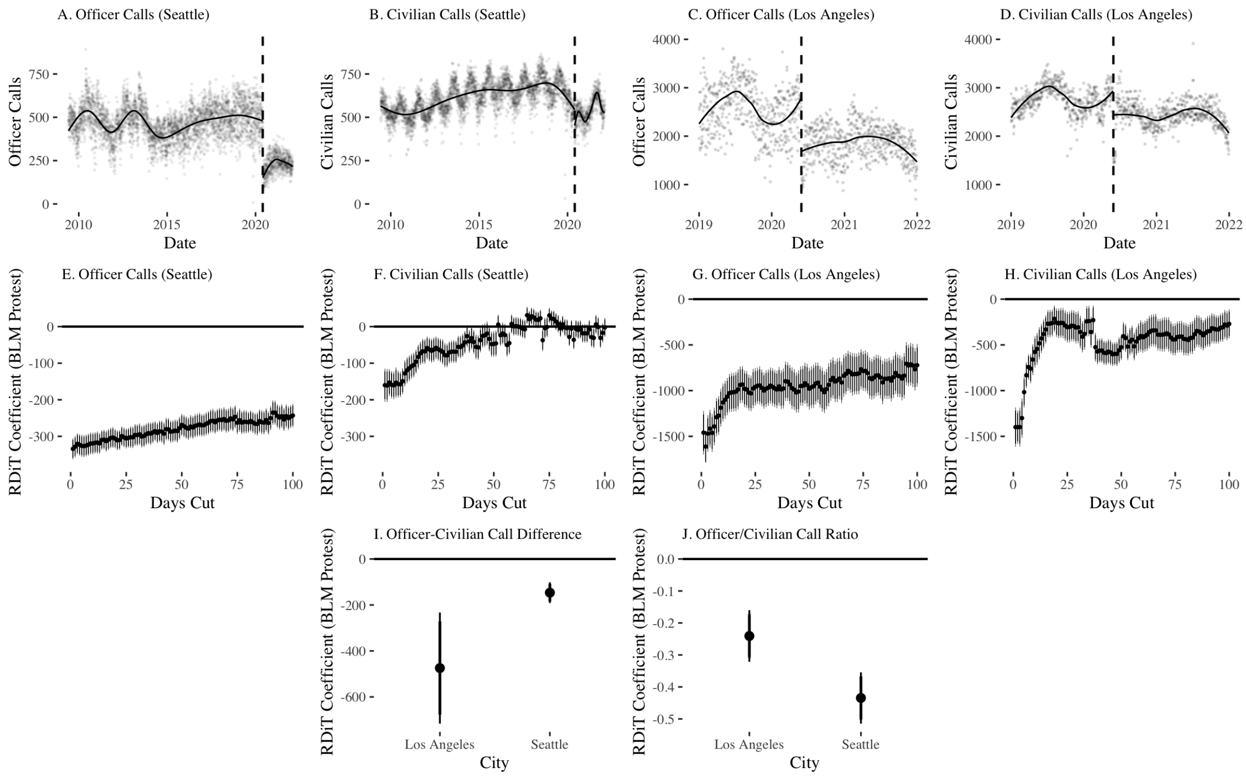

Figure 3: Assessing If Reductions in Policing Activity are Driven by Civilian Demand. Shape denotes outcome type across the cities. All estimates are from RD specifications with a uniform kernel and polynomial degree equal to 1. 95% CIs displayed derived from robust SEs. Regression estimates associated with panels I and J can be found in Appendix Table A2.

protest after cutting data from days immediately post-*BLM protest* on the right-hand side of the discontinuity. In Seattle and Los Angeles, officer calls discontinuously decrease in a persistent manner. But, civilian calls revert back to their pre-*BLM protest* average within 50 days in Seattle. Likewise, civilian calls rebound in Los Angeles roughly 20 days post-*BLM protest*. In both cases, the decrease in officer calls is statistically lower than the decrease in civilian calls (note the same y-axis scale on Panels E-H, limited overlap of confidence intervals). Finally, we conduct a formal test demonstrating the decrease in officer calls was more substantial than the reduction in civilian calls. We assess the discontinuous decrease in the *difference* and *ratio* between officer and civilian calls. If the decrease is negative, then observed reductions in police activity are likely driven by police themselves, not citizens. We find statistically significant and substantial reductions in the officer-civilian call difference

(β = -146, SE = 22, $p < 0.001$; β = -434, SE = 123, $p < 0.001$) and officer/civilian call *ratio* (β = -0.4, SE = 0.04, $p < 0.001$; β = -0.24, SE = 0.04, $p < 0.001$) in both cities. In sum, our findings are not likely driven by civilian demand.

Hypothesis 2a and 2b: 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, and *Hypothesis 2b* anticipates no change (or a decline) in the quality of policing overall. We measure quality of policing in terms of change in hit rates, arrest rates, and Black-White stop rate ratios. Figure 4, 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. However, the hit rate does not discontinuously increase post-*BLM protest* in Los Angeles or Philadelphia. Unlike our RDiT coefficients on policing activity, the positive coefficients for Austin and Seattle are short-term and not temporally sustained. Auxiliary analyses excluding days immediately post-*BLM protest* demonstrates the hit rate increase lasts only 15 and 30 days for Austin and Seattle respectively post-*BLM protest* (Figures F42, F45). On balance, with respect to hit rates, we find evidence for *Hypothesis 2b*, with improvements in hit rates in two cities being short term.

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 estimates except the Seattle coefficient at $p < 0.05$), equivalent to 190-420% of the pre-treatment mean across the cities. The discontinuous improvement in arrest rates following the protests is robust to a variety of model kernel and polynomial specifications (Figures D2 - D5), as well as alternative bandwidths (Figures E16 - E19). Unlike the hit rate outcome, auxiliary analyses cutting days immediately post-*BLM protest* and re-estimating the RDiT coefficient suggests there is a relative degree of temporal persistence in the discontinuous arrest rate increase across all four cities, even up to 100 days post-*BLM protest* (Figures F46-F49). These findings are

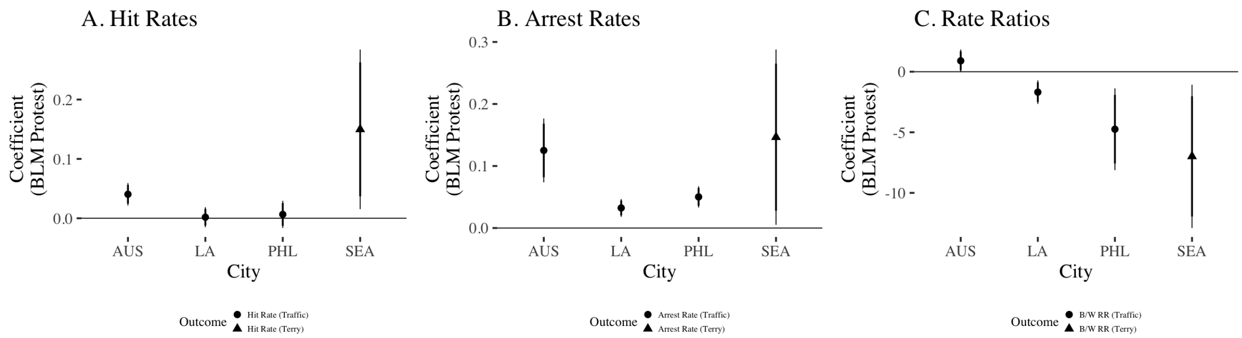

Figure 4: RDiT Estimates Characterizing Effect (y-axis) of BLM protests on Policing Quality Across Cities (x-axis). Panels A, B and C characterize the discontinuous effect of the BLM protests on *hit rates*, *arrest rates*, and *rate ratios* between whites and Latinos/Black people. Shape denotes outcome type. All estimates are from RD specifications with a uniform kernel and polynomial degree equal to 1. 95% CIs displayed derived from robust SEs. Associated regression estimates can be found in Appendix Table A3.

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 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, indicating that it is not simply a function of seasonality (Figures G66 - G69). Durable and reliable improvements in arrest rates provide the strongest evidence that declines in police stops produced prosocial outcomes, supporting *Hypothesis 2a*.

Panel C indicates 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%, 128%, and 98% of the pre-treatment mean 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 F51 - F53). These estimates are most reliable across various specifications in Seattle and Philadelphia (Figures D4 - D5). They are somewhat sensitive to model specification in Los Angeles (Figure D3), where it appears that the improvement is shorter term, occurs closer to the discontinuity, and returns to pre-treatment levels (Figure E22) 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 F50). Evidence around 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.

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 (across model specifications) improvements in both arrest rates and Black/white stop rate ratios that persist over time. In contrast, evidence suggests that 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, are not able to be characterized as pro-social, providing evidence for *Hypothesis 2b*.

Hypothesis 3: Crime

Recall that Hypothesis 3 posits that there will be no change in violent crime following the protests, even when accompanied by declining police stops. The descriptive impact of the protests on crime is displayed in Figure 5. In each city context it appears that violent crime dipped directly following the protests, but then resumed an overall upward trend that pre-dated the unrest. Figure 6 displays the standardized RDiT coefficients characterizing the discontinous 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 D2 and Figure D5) and bandwidth specifications (Figure E24 and Figure E33), and is not distinguishable from patterns of violent crime occurring during the same time period the previous year (Figure G66 and Figure G69).

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 themselves. In both cities, the effect of the protests are not significant when the polynomial degree is quadratic or cubic (Figure D3 and Figure D4), suggesting that there is no change in violent crime close to the discontinuity (confirmed by an examination of alternative bandwidth specifications, Figure E27 and Figure E30). In Philadelphia, moreover, changes in violent crime reflected in the linear estimate are not distinguishable from the temporal placebo test, suggesting that that factors other than the protests account for the upward trend (Figure G68). In Los Angeles, the difference between changes in violent crime that occurred around the protest and that which occurred the year prior approach statistical significance, but again do not hold across multiple polynomial degrees (Figure G67). We therefore have little confidence that the protests themselves (and co-occurring declines in police activity) are responsible for increasing violent crime. On balance, across all city contexts we find support for Hypothesis 3.

Existing literature suggests that crimes against property and society may fluctuate, given that these two categories are more sensitive to actions taken by police themselves. The protests do not appear to prompt change in crimes against society. Figure 6 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 D5), and distinguishable from fluctuations that occurred during the same time period the previous year (Figure G69). Across all contexts, changes to crimes against society are short term (Figures F54 - F65). 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 6 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 or persistent over time. On balance, we interpret the discontinuous effect of the *BLM protest* on property crime to be negligible.

In sum, we do not find robust and reliable evidence that the protests themselves prompted a rise in violent crime, and very little evidence that they impacted crimes against society or property. There is some variation across cities, but within cities most of the estimates of the impact of the protests on each type of crime themselves do not withstand various robustness checks. 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. Moreover the depolicing estimates withstand a variety of robustness checks and persist over time. 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.

Conclusion

We asked: What was the impact of the 2020 BLM protests on policing and public safety? In the event that the protests prompted a withdrawal from service provision, what quality does that depolicing take? And finally, did the protests and concomitant declines in police activity impact downstream crime? These were the questions preoccupying America in the summer of 2020. In order to address them, we evaluate police activity in four cities, drawing together

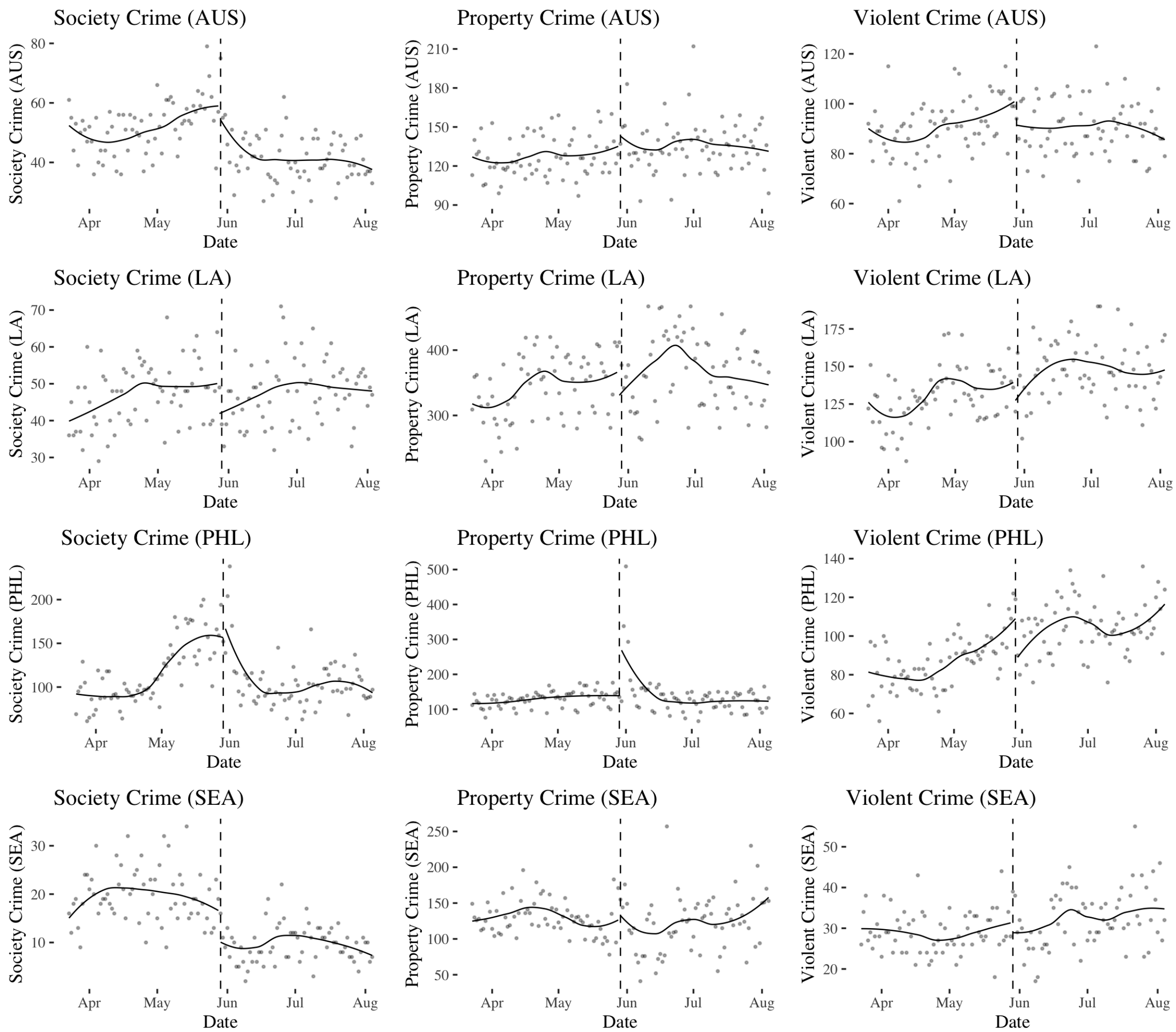

Figure 5: Crime 2 Months Before and After BLM protests. The x-axis is the date, the y-axis is the crime type. For each row, the crime types are *society*, *property*, and *violent* from left to right. From top to bottom, each row characterizes data from Austin, Los Angeles, Philadelphia, and Seattle 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 A4.

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 a withdrawal of service provision directly following the protests was accompanied by a rise in violent crime.

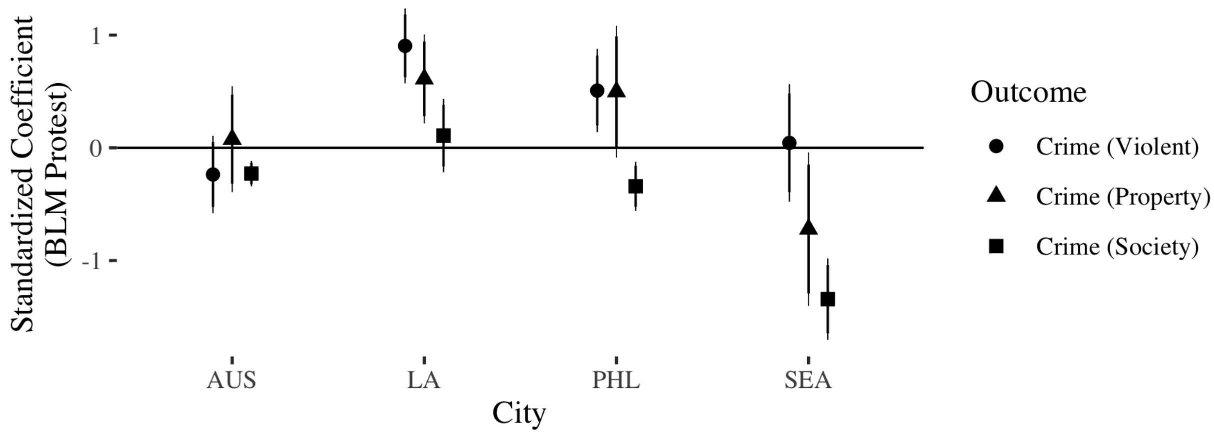

Figure 6: RDiT Estimates Characterizing Standardized Effect (y-axis) of *BLM Protests* on Crime Across Cities (x-axis). Shape denotes outcome type. All estimates are from RD specifications with a uniform kernel and polynomial degree equal to 1. 95% CIs displayed derived from robust SEs.

With respect to the quality of policing, results are mixed. We do not observe any sustained improvement in hit rates. But, at the same time, we do observe an improvement in rates of arrest that follow from stops, 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. And, 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 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). In sum, while there is evidence that depolicing yields some pro-social outcomes, contextual evidence and existing literature suggests this is because of the intrinsic nature of the stops themselves, and not a reflection of accountability to protester demands.

Our conclusions are three-fold. 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 crime-fighting 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.

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

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Supplemental Information

A RDiT Tables Associated with Figures Presented in Text

Table A1: RDiT Coefficients Characterizing the Effect of BLM Protests on Policing Activities.

City	Stops	Coeff	SE	P-Val	N-Val	Effective N	Bandwidth Est.
Austin	Traffic	-2.03	0.24	0.000	514	56.41	28.21
LA	Pedestrian	-1.87	0.18	0.000	697	164.25	82.13
LA	Traffic	-2.84	0.26	0.000	697	117.37	58.68
Philly	Pedestrian	-0.19	0.04	0.000	880	124.09	62.04
Philly	Traffic	-0.58	0.04	0.000	880	91.81	45.91
Seattle	Terry	-1.58	0.21	0.000	1885	170.99	85.49

All estimates are specified with a uniform kernel and polynomial degree equal to 1. Standard errors are robust.

Table A2: RDiT Coefficients Characterizing Changes in Officer Civilian Initiated Calls Following the BLM Protest.

City	Officer:Civilian Calls	Coeff	SE	P-Val	N-Val	Effective N	Bandwidth Est.
LA	Call Difference	-146.43	22.96	0.000	4014	96.15	48.07
Seattle	Call Difference	-0.43	0.04	0.000	4014	118.86	59.43
LA	Call Ratio	-474.43	123.30	0.000	513	170.24	85.12
Seattle	Call Ratio	-0.24	0.04	0.000	513	227.64	113.82

All estimates are specified with a uniform kernel and polynomial degree equal to 1. Standard errors are robust.

Table A3: RDiT Coefficients Characterizing the Effect of BLM Protests on Policing Quality.

City	Outcome	Coeff	SE	P-Val	N-Val	Effective N	Bandwidth Est.
Austin	Hit rate	0.04	0.01	0.00	514	82.98	41.49
LA	Hit rate	0.00	0.01	0.88	697	409.87	204.94
Philly	Hit rate	0.01	0.01	0.85	2341	239.23	119.62
Seattle	Hit rate	0.15	0.07	0.02	1885	337.01	168.50
Austin	Arrest rate	0.13	0.03	0.00	514	62.42	31.21
LA	Arrest rate	0.03	0.01	0.00	697	339.91	169.95
Philly	Arrest rate	0.01	0.01	0.85	2341	239.23	119.62
Seattle	Arrest rate	0.15	0.07	0.02	1885	337.01	168.50
Austin	B/W rate ratio	0.90	0.48	0.04	357	64.22	32.11
LA	B/W rate ratio	-1.69	0.50	0.00	697	236.81	118.40
Philly	B/W rate ratio	-4.74	1.72	0.00	2337	209.27	104.64
Seattle	B/W rate ratio	-6.99	3.02	0.01	339	183.84	91.92

All estimates are specified with a uniform kernel and polynomial degree equal to 1. Standard errors are robust.

Table A4: RDiT Coefficients Characterizing the Effect of BLM Protests on Crime.

City	Crime Type	Coeff	SE	P-Val	N-Val	Effective N	Bandwidth Est.
Austin	Violent	-0.24	0.18	0.07	6358	276.61	138.31
Austin	Property	0.08	0.24	0.96	6358	138.35	69.18
Austin	Society	-0.23	0.06	0.00	6358	277.56	138.78
LA	Violent	0.90	0.17	0.00	3800	383.81	191.91
LA	Property	0.61	0.20	0.00	3800	189.13	94.56
LA	Society	0.11	0.17	0.26	3800	351.43	175.71
Philly	Violent	0.51	0.19	0.03	5263	172.81	86.40
Philly	Property	0.50	0.30	0.05	5263	284.67	142.33
Philly	Society	-0.34	0.11	0.00	5263	178.23	89.11
Seattle	Violent	0.04	0.27	0.80	4532	150.25	75.13
Seattle	Property	-0.72	0.35	0.02	4532	179.89	89.94
Seattle	Society	-1.34	0.18	0.00	4532	274.78	137.39

All estimates are specified with a uniform kernel and polynomial degree equal to 1. Standard errors are robust.

B Depolicing does not vary by demographic composition of beat

Table B5: Regression discontinuity coefficient difference

	Coefficient difference	P-value	Bandwidth	Measure	DV
(1)	−0.878	.950	25	Income	Terry stops
(2)	−0.974	.946	50	Income	Terry stops
(3)	−1.209	.934	100	Income	Terry stops
(4)	1.460	.806	25	Nonwhite	Terry stops
(5)	1.069	.860	50	Nonwhite	Terry stops
(6)	1.166	.852	100	Nonwhite	Terry stops
(7)	−16.107	.985	25	Income	Calls
(8)	−19.631	.979	50	Income	Calls
(9)	−22.031	.976	100	Income	Calls
(10)	13.948	.976	25	Nonwhite	Calls
(11)	17.272	.964	50	Nonwhite	Calls
(12)	20.131	.947	100	Nonwhite	Calls

C Efficiency Is Not a Function of Identifying More Criminal Activity

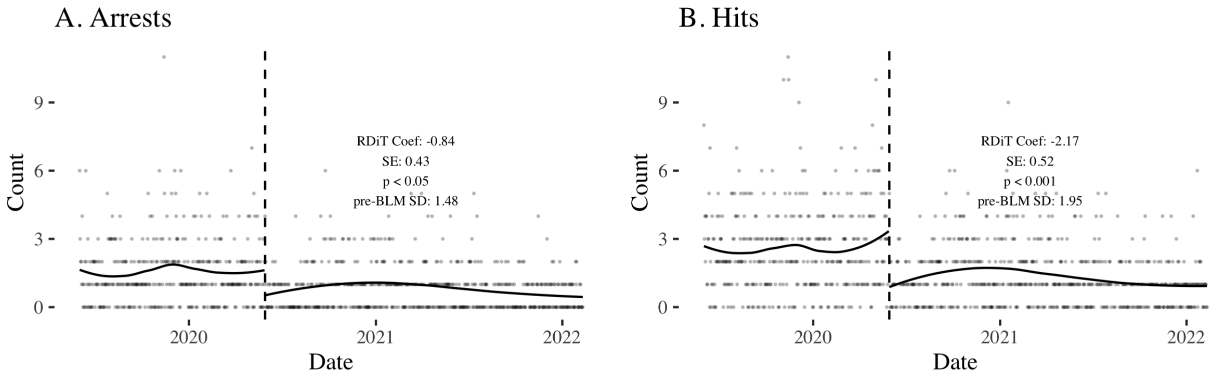

Figure C1: Terry Stop Arrest (Panel A) and Hit Counts (Panel B, y-axis) Over Time (x-axis). Loess lines fit on each side of the *BLM protest* discontinuity. Dashed vertical line denotes *BLM protest* onset. Annotations denote RDiT coefficients using a running variable to the 1st degree.

D Alternative RDiT Specifications

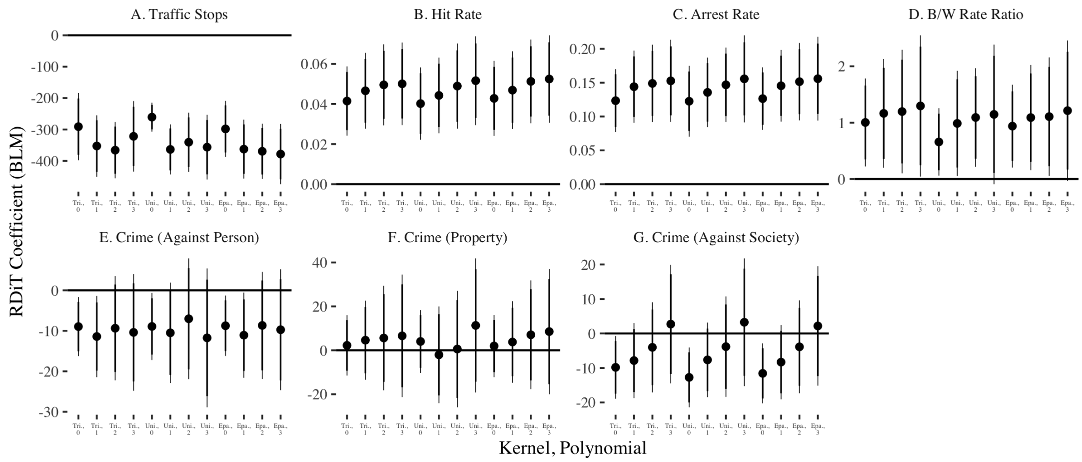

Figure D2: Alternative RDiT Specifications Across Outcomes (Austin) The x-axis characterizes the different kernel/polynomial specifications (0 = difference-in-means, 1 = linear polynomial, 2 = quadratic polynomial, 3 = cubic polynomial; Tri. = triangular kernel, Uni. = uniform kernel, Epa. = epanechnikov kernel). The y-axis characterizes the unstandardized RDiT coefficient for each of the respective outcomes (characterized by separate facets). 95% CIs displayed from robust SEs

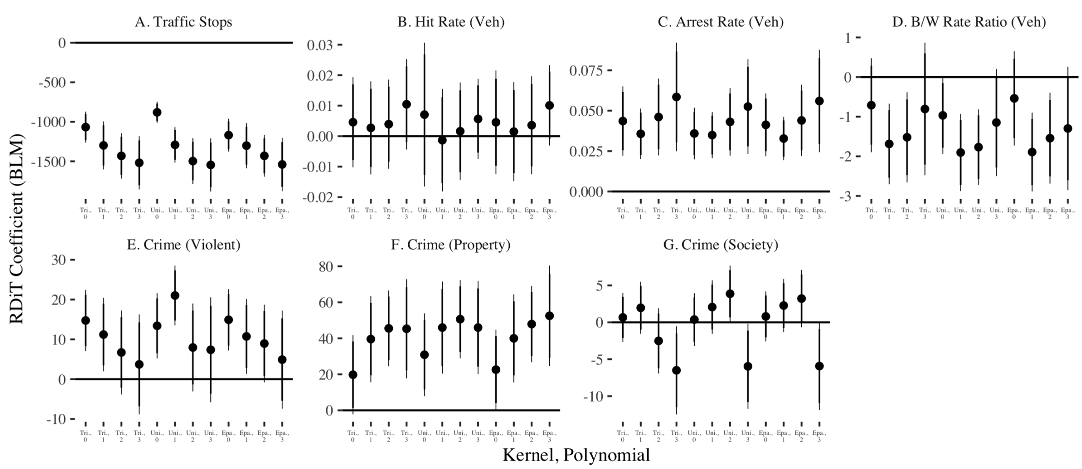

Figure D3: Alternative RDiT Specifications Across Outcomes (Los Angeles) The x-axis characterizes the different kernel/polynomial specifications (0 = difference-in-means, 1 = linear polynomial, 2 = quadratic polynomial, 3 = cubic polynomial; Tri. = triangular kernel, Uni. = uniform kernel, Epa. = epanechnikov kernel). The y-axis characterizes the unstandardized RDiT coefficient for each of the respective outcomes (characterized by separate facets). 95% CIs displayed from robust SEs

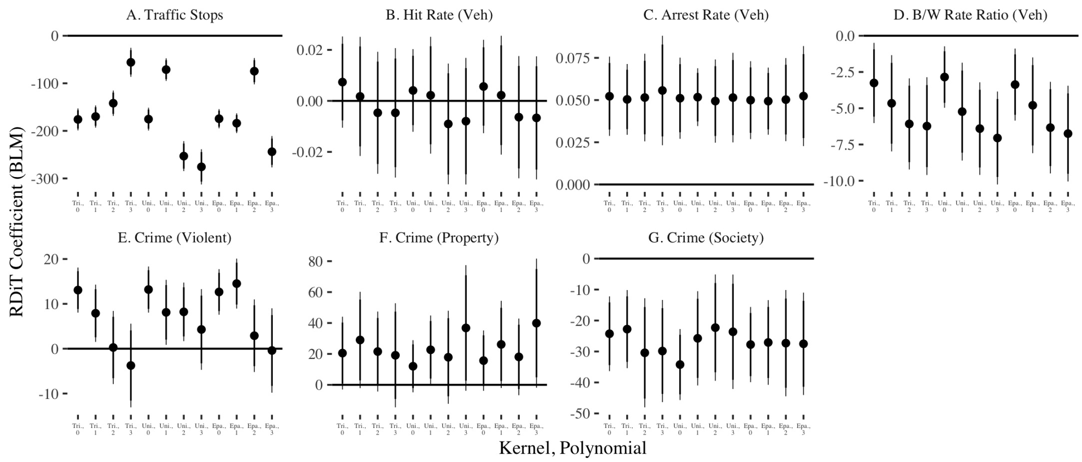

Figure D4: Alternative RDiT Specifications Across Outcomes (Philadelphia) The x-axis characterizes the different kernel/polynomial specifications (0 = difference-in-means, 1 = linear polynomial, 2 = quadratic polynomial, 3 = cubic polynomial; Tri. = triangular kernel, Uni. = uniform kernel, Epa. = epanechnikov kernel). The y-axis characterizes the unstandardized RDiT coefficient for each of the respective outcomes (characterized by separate facets). 95% CIs displayed from robust SEs

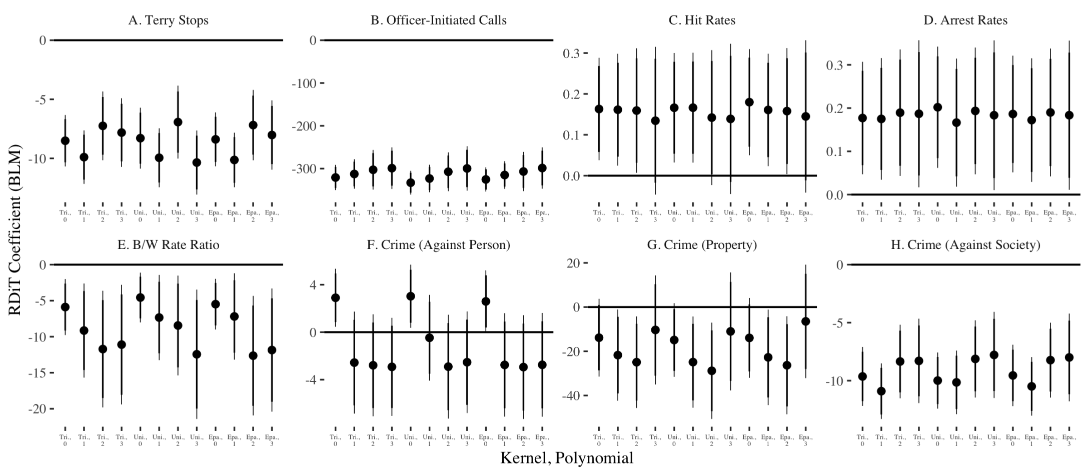

Figure D5: Alternative RDiT Specifications Across Outcomes (Seattle) The x-axis characterizes the different kernel/polynomial specifications (0 = difference-in-means, 1 = linear polynomial, 2 = quadratic polynomial, 3 = cubic polynomial; Tri. = triangular kernel, Uni. = uniform kernel, Epa. = epanechnikov kernel). The y-axis characterizes the unstandardized RDiT coefficient for each of the respective outcomes (characterized by separate facets). 95% CIs displayed from robust SEs

E Alternative Bandwidths

E.1 Policing Activity

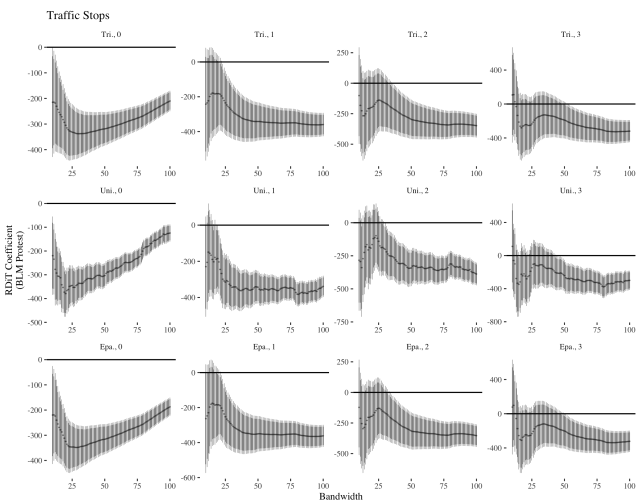

Figure E6: Alternative Bandwidths (10-100 Days, Austin, Traffic Stop Outcome) The x-axis is the number of days used in the data before and after the *BLM protest*. The y-axis is the unstandardized RDiT coefficient. Each facet characterizes a different kernel (Tri. = triangular, Uni. = uniform, Epa. = Epanechnikov) and polynomial (0, 1, 2, 3) specification. 95% CIs displayed from robust SEs.

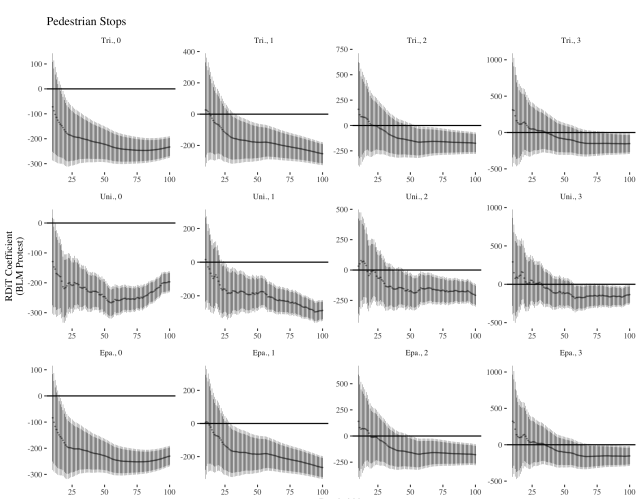

Figure E7: Alternative Bandwidths (10-100 Days, Los Angeles, Pedestrian Stops Outcome) The x-axis is the number of days used in the data before and after the *BLM protest*. The y-axis is the unstandardized RDiT coefficient. Each facet characterizes a different kernel (Tri. = triangular, Uni. = uniform, Epa. = Epanechnikov) and polynomial (0, 1, 2, 3) specification. 95% CIs displayed from robust SEs.

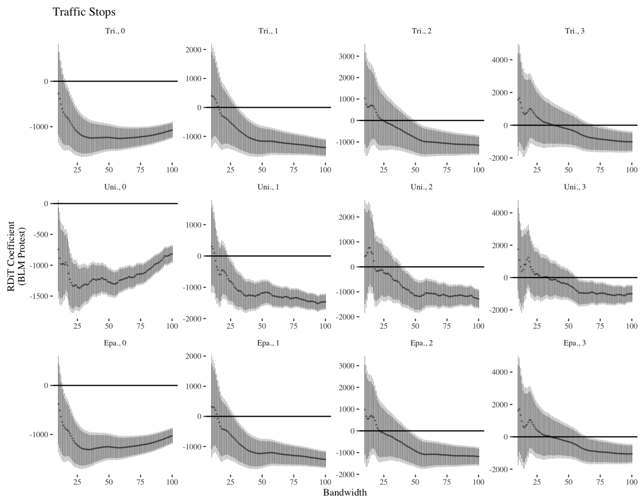

Figure E8: Alternative Bandwidths (10-100 Days, Los Angeles, Traffic Stops Outcome) The x-axis is the number of days used in the data before and after the *BLM protest*. The y-axis is the unstandardized RDiT coefficient. Each facet characterizes a different kernel (Tri. = triangular, Uni. = uniform, Epa. = Epanechnikov) and polynomial (0, 1, 2, 3) specification. 95% CIs displayed from robust SEs.

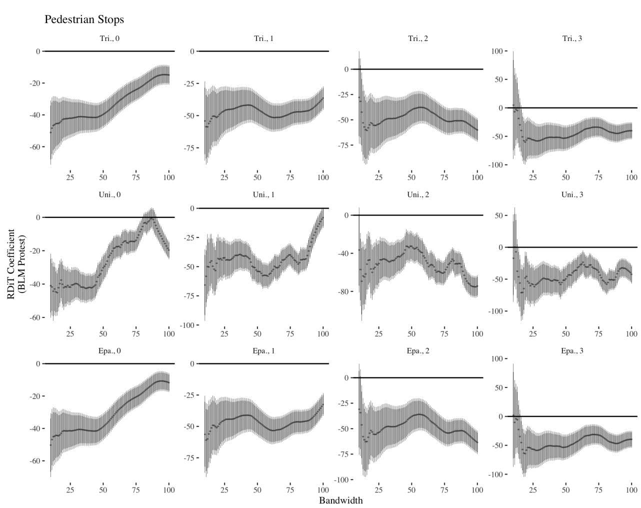

Figure E9: Alternative Bandwidths (10-100 Days, Philadelphia, Pedestrian Stops Outcome) The x-axis is the number of days used in the data before and after the *BLM protest*. The y-axis is the unstandardized RDiT coefficient. Each facet characterizes a different kernel (Tri. = triangular, Uni. = uniform, Epa. = Epanechnikov) and polynomial (0, 1, 2, 3) specification. 95% CIs displayed from robust SEs.

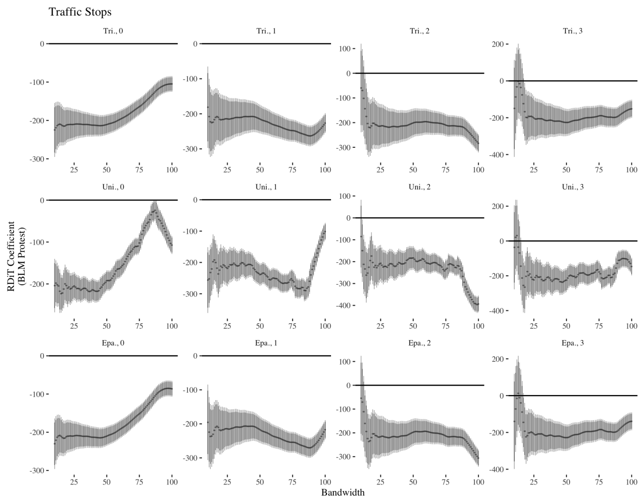

Figure E10: Alternative Bandwidths (10-100 Days, Philadelphia, Vehicle Stops Outcome) The x-axis is the number of days used in the data before and after the *BLM protest*. The y-axis is the unstandardized RDiT coefficient. Each facet characterizes a different kernel (Tri. = triangular, Uni. = uniform, Epa. = Epanechnikov) and polynomial (0, 1, 2, 3) specification. 95% CIs displayed from robust SEs.

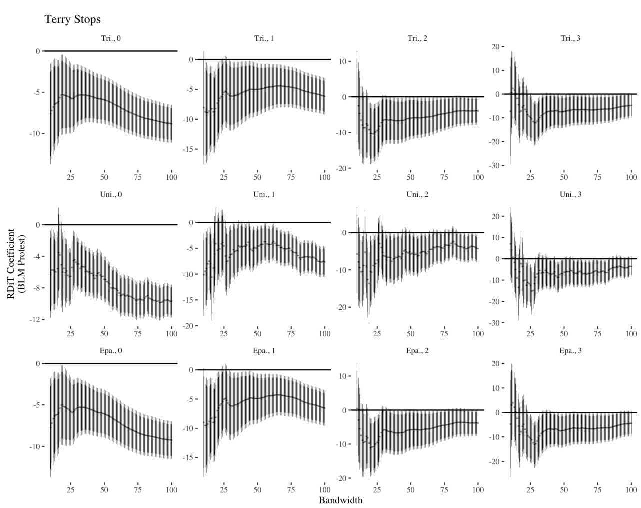

Figure E11: Alternative Bandwidths (10-100 Days, Seattle, Terry Stops Outcome) The x-axis is the number of days used in the data before and after the *BLM protest*. The y-axis is the unstandardized RDiT coefficient. Each facet characterizes a different kernel (Tri. = triangular, Uni. = uniform, Epa. = Epanechnikov) and polynomial (0, 1, 2, 3) specification. 95% CIs displayed from robust SEs.

E.2 Hit Rates

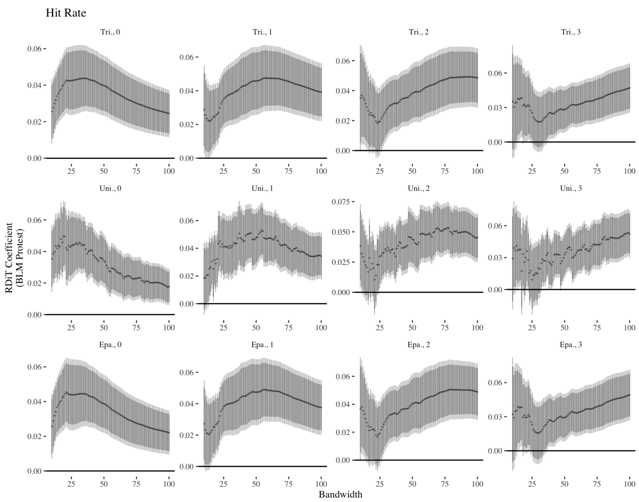

Figure E12: Alternative Bandwidths (10-100 Days, Austin, Hit Rates Outcome) The x-axis is the number of days used in the data before and after the *BLM protest*. The y-axis is the unstandardized RDiT coefficient. Each facet characterizes a different kernel (Tri. = triangular, Uni. = uniform, Epa. = Epanechnikov) and polynomial (0, 1, 2, 3) specification. 95% CIs displayed from robust SEs.

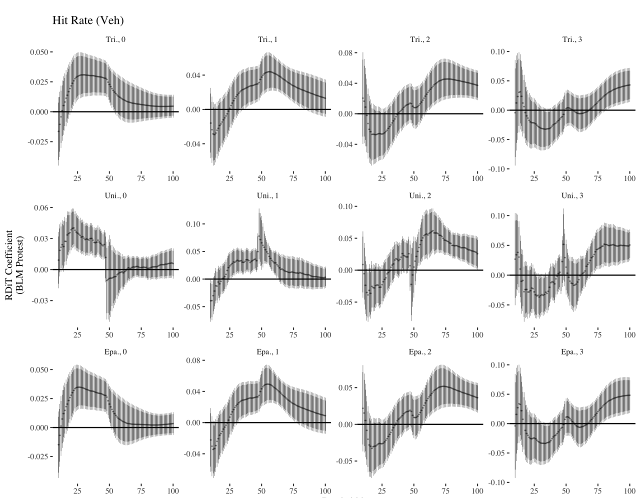

Figure E13: Alternative Bandwidths (10-100 Days, Los Angeles, Vehicle Hit Rates Outcome) The x-axis is the number of days used in the data before and after the *BLM protest*. The y-axis is the unstandardized RDiT coefficient. Each facet characterizes a different kernel (Tri. = triangular, Uni. = uniform, Epa. = Epanechnikov) and polynomial (0, 1, 2, 3) specification. 95% CIs displayed from robust SEs.

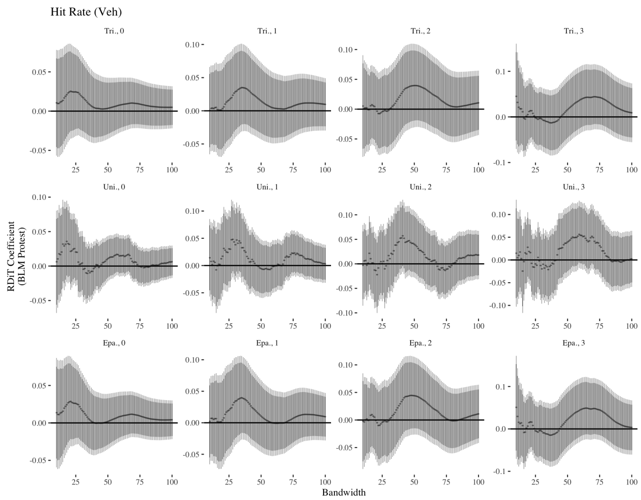

Figure E14: Alternative Bandwidths (10-100 Days, Philadelphia, Vehicle Hit Rates Outcome) The x-axis is the number of days used in the data before and after the *BLM protest*. The y-axis is the unstandardized RDiT coefficient. Each facet characterizes a different kernel (Tri. = triangular, Uni. = uniform, Epa. = Epanechnikov) and polynomial (0, 1, 2, 3) specification. 95% CIs displayed from robust SEs.

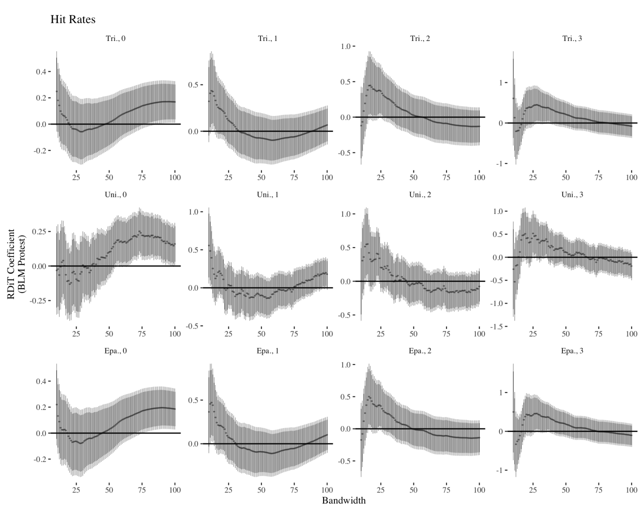

Figure E15: Alternative Bandwidths (10-100 Days, Seattle, Terry Stop Hit Rates Outcome) The x-axis is the number of days used in the data before and after the *BLM protest*. The y-axis is the unstandardized RDiT coefficient. Each facet characterizes a different kernel (Tri. = triangular, Uni. = uniform, Epa. = Epanechnikov) and polynomial (0, 1, 2, 3) specification. 95% CIs displayed from robust SEs.

E.3 Arrest Rates

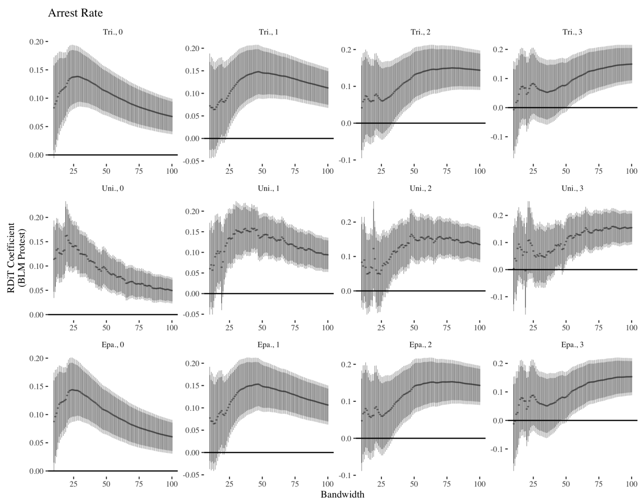

Figure E16: Alternative Bandwidths (10-100 Days, Austin, Vehicle Arrest Rates Outcome) The x-axis is the number of days used in the data before and after the *BLM protest*. The y-axis is the unstandardized RDiT coefficient. Each facet characterizes a different kernel (Tri. = triangular, Uni. = uniform, Epa. = Epanechnikov) and polynomial (0, 1, 2, 3) specification. 95% CIs displayed from robust SEs.

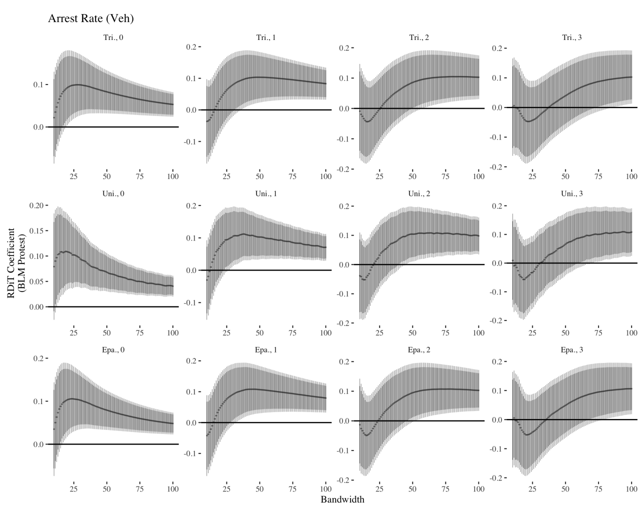

Figure E17: Alternative Bandwidths (10-100 Days, Los Angeles, Vehicle Arrest Rate Outcome) The x-axis is the number of days used in the data before and after the *BLM protest*. The y-axis is the unstandardized RDiT coefficient. Each facet characterizes a different kernel (Tri. = triangular, Uni. = uniform, Epa. = Epanechnikov) and polynomial (0, 1, 2, 3) specification. 95% CIs displayed from robust SEs.

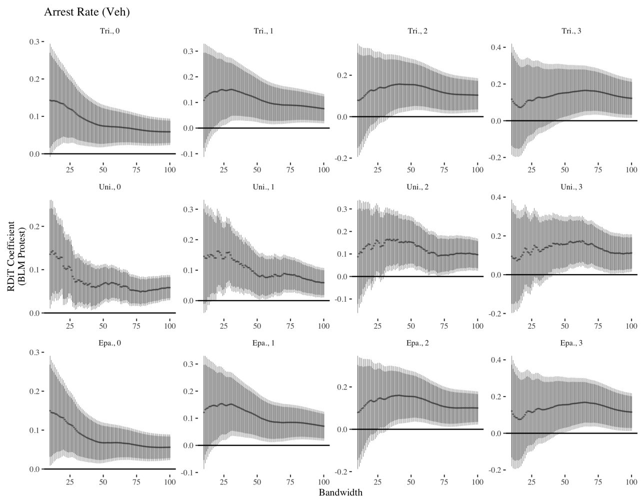

Figure E18: Alternative Bandwidths (10-100 Days, Philadelphia, Vehicle Arrest Hit Rates Outcome) The x-axis is the number of days used in the data before and after the *BLM protest*. The y-axis is the unstandardized RDiT coefficient. Each facet characterizes a different kernel (Tri. = triangular, Uni. = uniform, Epa. = Epanechnikov) and polynomial (0, 1, 2, 3) specification. 95% CIs displayed from robust SEs.

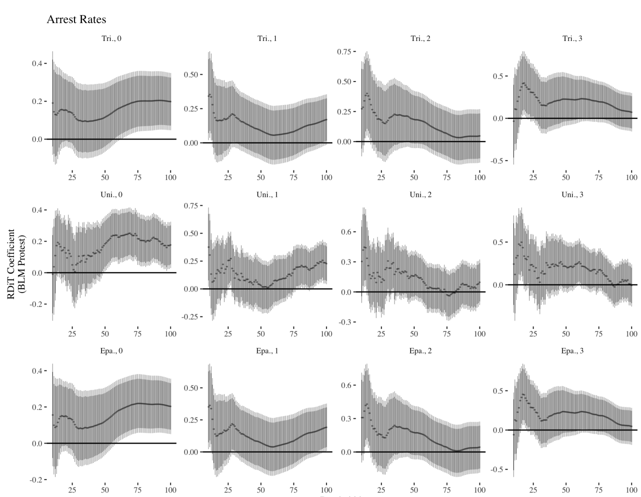

Figure E19: Alternative Bandwidths (10-100 Days, Seattle, Terry Stop Arrest Hit Rates Outcome) The x-axis is the number of days used in the data before and after the *BLM protest*. The y-axis is the unstandardized RDiT coefficient. Each facet characterizes a different kernel (Tri. = triangular, Uni. = uniform, Epa. = Epanechnikov) and polynomial (0, 1, 2, 3) specification. 95% CIs displayed from robust SEs.

E.4 Rate Ratios

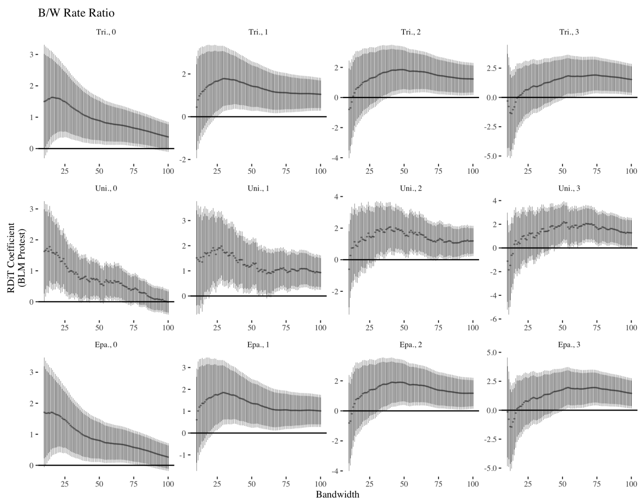

Figure E20: Alternative Bandwidths (10-100 Days, Austin, Black-White Vehicle Stop Rate Ratios Outcome) The x-axis is the number of days used in the data before and after the *BLM protest*. The y-axis is the unstandardized RDiT coefficient. Each facet characterizes a different kernel (Tri. = triangular, Uni. = uniform, Epa. = Epanechnikov) and polynomial (0, 1, 2, 3) specification. 95% CIs displayed from robust SEs.

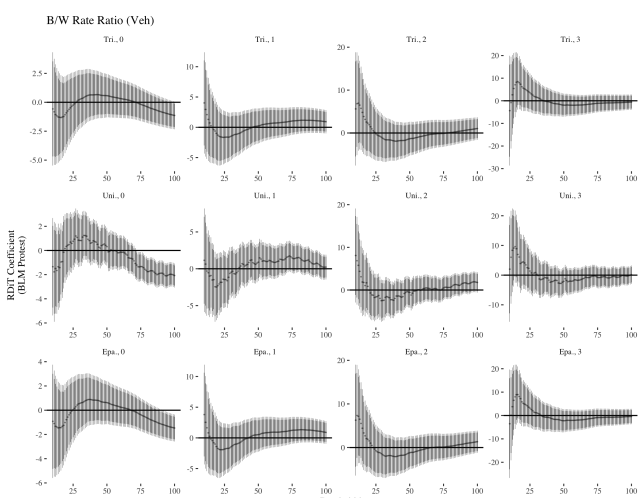

Figure E21: Alternative Bandwidths (10-100 Days, Los Angeles, Vehicle Stop Black-White Rate Ratio Outcome) The x-axis is the number of days used in the data before and after the *BLM protest*. The y-axis is the unstandardized RDiT coefficient. Each facet characterizes a different kernel (Tri. = triangular, Uni. = uniform, Epa. = Epanechnikov) and polynomial (0, 1, 2, 3) specification. 95% CIs displayed from robust SEs.

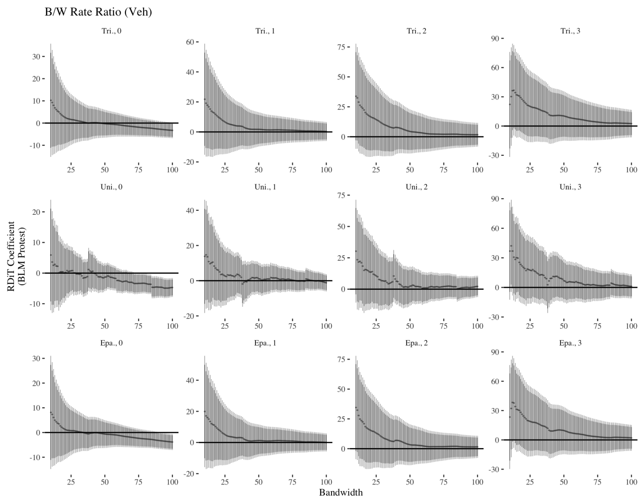

Figure E22: Alternative Bandwidths (10-100 Days, Philadelphia, Vehicle Stop Black-White Rate Ratio Outcome) The x-axis is the number of days used in the data before and after the *BLM protest*. The y-axis is the unstandardized RDiT coefficient. Each facet characterizes a different kernel (Tri. = triangular, Uni. = uniform, Epa. = Epanechnikov) and polynomial (0, 1, 2, 3) specification. 95% CIs displayed from robust SEs.

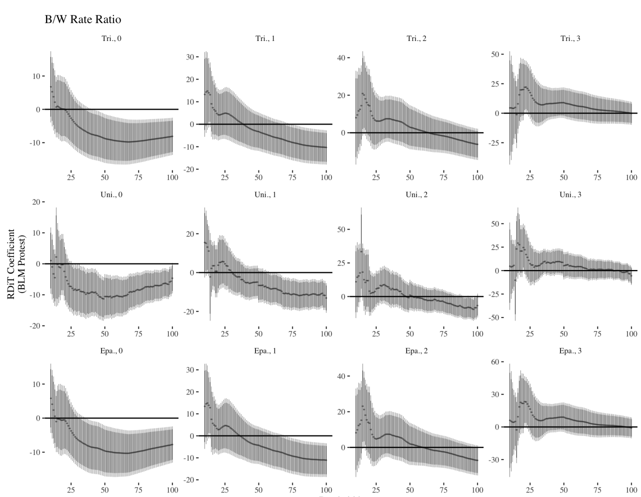

Figure E23: Alternative Bandwidths (10-100 Days, Seattle, Black-White Rate Ratio Outcome) The x-axis is the number of days used in the data before and after the *BLM protest*. The y-axis is the unstandardized RDiT coefficient. Each facet characterizes a different kernel (Tri. = triangular, Uni. = uniform, Epa. = Epanechnikov) and polynomial (0, 1, 2, 3) specification. 95% CIs displayed from robust SEs.

E.5 Crime

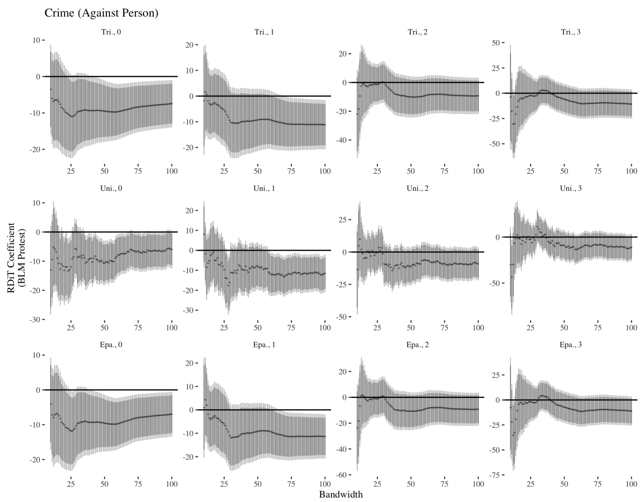

Figure E24: Alternative Bandwidths (10-100 Days, Austin, Crimes Against Person Outcome) The x-axis is the number of days used in the data before and after the *BLM protest*. The y-axis is the unstandardized RDiT coefficient. Each facet characterizes a different kernel (Tri. = triangular, Uni. = uniform, Epa. = Epanechnikov) and polynomial (0, 1, 2, 3) specification. 95% CIs displayed from robust SEs.

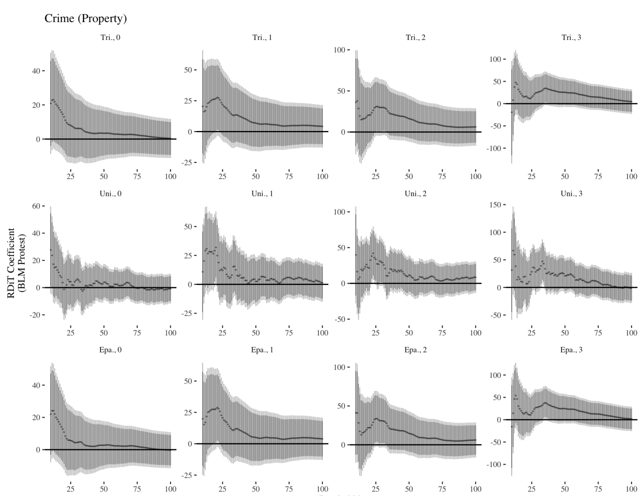

Figure E25: Alternative Bandwidths (10-100 Days, Austin, Crimes Against Property Outcome) The x-axis is the number of days used in the data before and after the *BLM protest*. The y-axis is the unstandardized RDiT coefficient. Each facet characterizes a different kernel (Tri. = triangular, Uni. = uniform, Epa. = Epanechnikov) and polynomial (0, 1, 2, 3) specification. 95% CIs displayed from robust SEs.

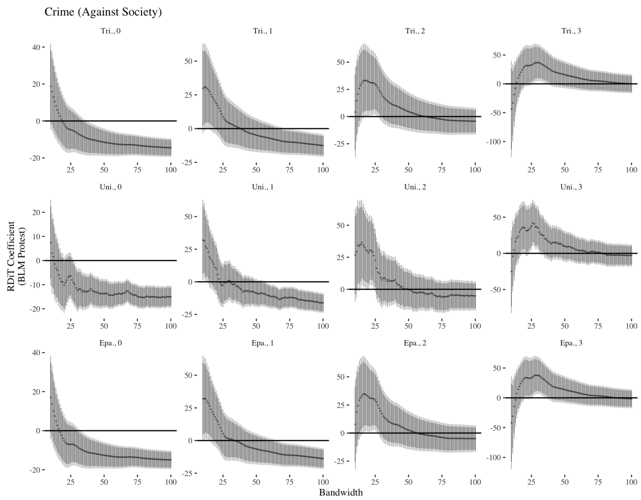

Figure E26: Alternative Bandwidths (10-100 Days, Austin, Crimes Against Society Outcome) The x-axis is the number of days used in the data before and after the *BLM protest*. The y-axis is the unstandardized RDiT coefficient. Each facet characterizes a different kernel (Tri. = triangular, Uni. = uniform, Epa. = Epanechnikov) and polynomial (0, 1, 2, 3) specification. 95% CIs displayed from robust SEs.

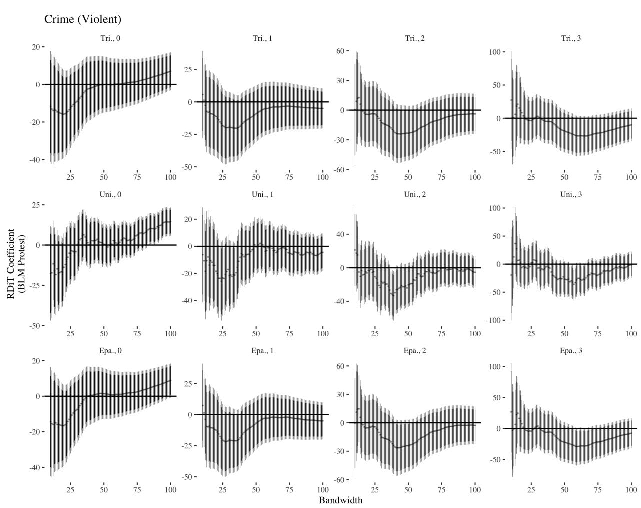

Figure E27: Alternative Bandwidths (10-100 Days, Los Angeles, Crimes Against Persons Outcome) The x-axis is the number of days used in the data before and after the *BLM protest*. The y-axis is the unstandardized RDiT coefficient. Each facet characterizes a different kernel (Tri. = triangular, Uni. = uniform, Epa. = Epanechnikov) and polynomial (0, 1, 2, 3) specification. 95% CIs displayed from robust SEs.

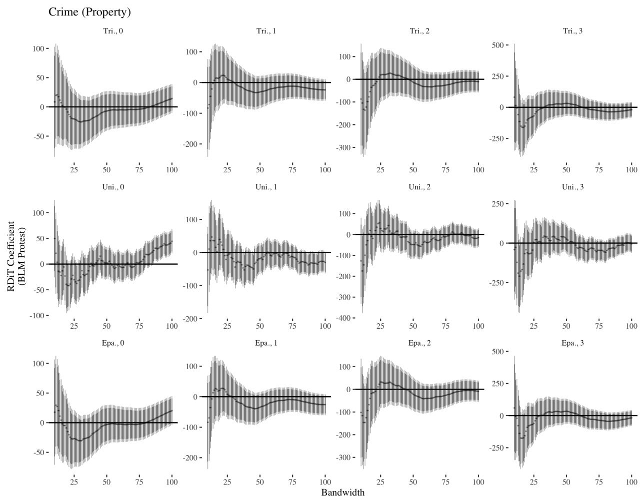

Figure E28: Alternative Bandwidths (10-100 Days, Los Angeles, Crimes Against Property Outcome) The x-axis is the number of days used in the data before and after the *BLM protest*. The y-axis is the unstandardized RDiT coefficient. Each facet characterizes a different kernel (Tri. = triangular, Uni. = uniform, Epa. = Epanechnikov) and polynomial (0, 1, 2, 3) specification. 95% CIs displayed from robust SEs.

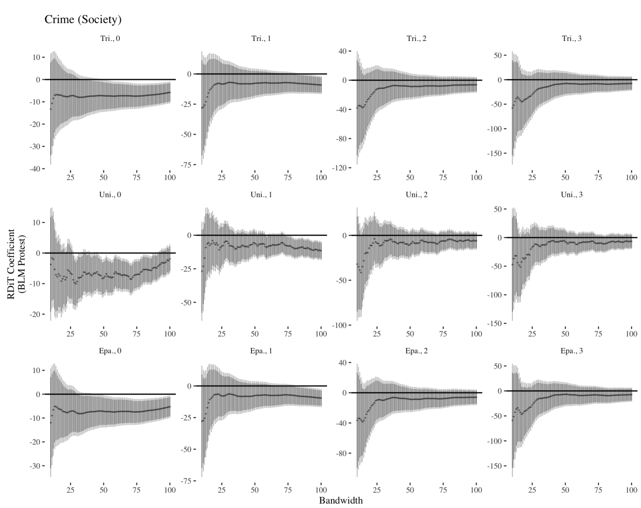

Figure E29: Alternative Bandwidths (10-100 Days, Los Angeles, Crimes Against Society Outcome) The x-axis is the number of days used in the data before and after the *BLM protest*. The y-axis is the unstandardized RDiT coefficient. Each facet characterizes a different kernel (Tri. = triangular, Uni. = uniform, Epa. = Epanechnikov) and polynomial (0, 1, 2, 3) specification. 95% CIs displayed from robust SEs.

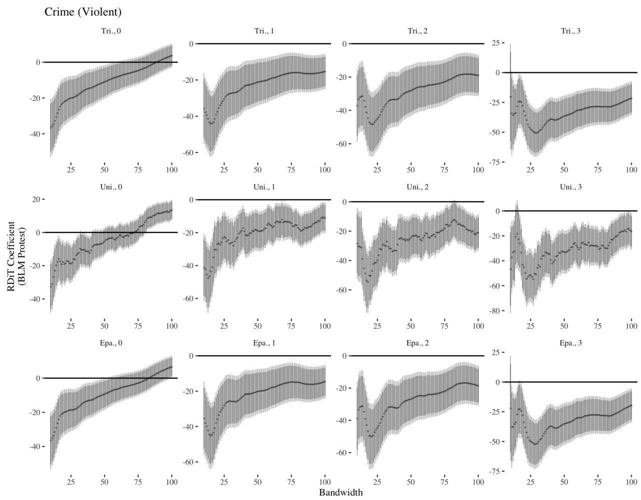

Figure E30: Alternative Bandwidths (10-100 Days, Philadelphia, Crimes Against Persons Outcome) The x-axis is the number of days used in the data before and after the *BLM protest*. The y-axis is the unstandardized RDiT coefficient. Each facet characterizes a different kernel (Tri. = triangular, Uni. = uniform, Epa. = Epanechnikov) and polynomial (0, 1, 2, 3) specification. 95% CIs displayed from robust SEs.

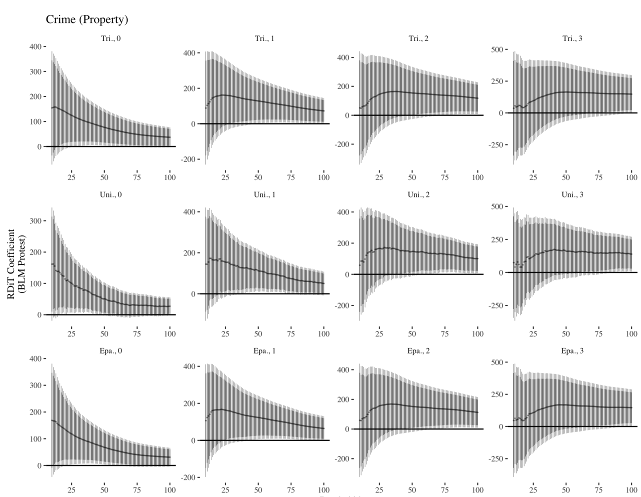

Figure E31: Alternative Bandwidths (10-100 Days, Philadelphia, Crimes Against Property Outcome) The x-axis is the number of days used in the data before and after the *BLM protest*. The y-axis is the unstandardized RDiT coefficient. Each facet characterizes a different kernel (Tri. = triangular, Uni. = uniform, Epa. = Epanechnikov) and polynomial (0, 1, 2, 3) specification. 95% CIs displayed from robust SEs.

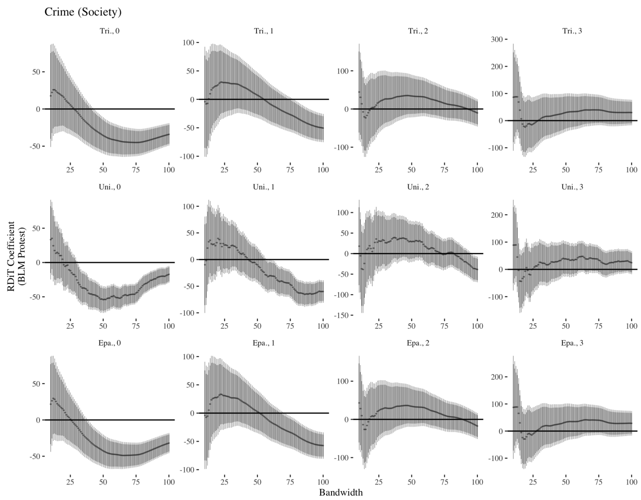

Figure E32: Alternative Bandwidths (10-100 Days, Philadelphia, Crimes Against Society Outcome) The x-axis is the number of days used in the data before and after the *BLM protest*. The y-axis is the unstandardized RDiT coefficient. Each facet characterizes a different kernel (Tri. = triangular, Uni. = uniform, Epa. = Epanechnikov) and polynomial (0, 1, 2, 3) specification. 95% CIs displayed from robust SEs.

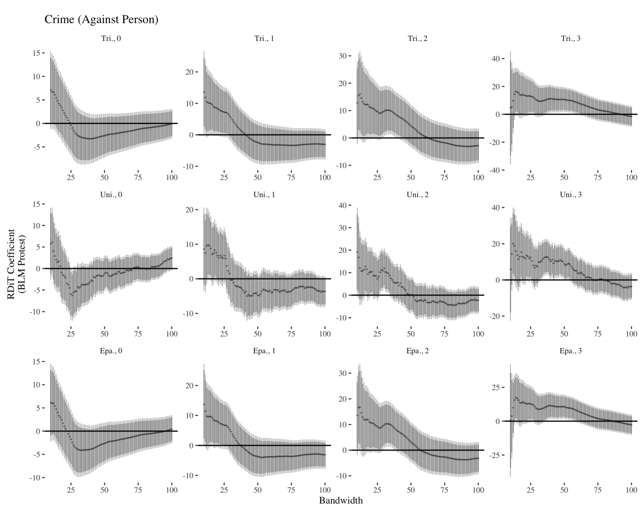

Figure E33: Alternative Bandwidths (10-100 Days, Seattle, Crimes Against Persons Outcome) The x-axis is the number of days used in the data before and after the *BLM protest*. The y-axis is the unstandardized RDiT coefficient. Each facet characterizes a different kernel (Tri. = triangular, Uni. = uniform, Epa. = Epanechnikov) and polynomial (0, 1, 2, 3) specification. 95% CIs displayed from robust SEs.

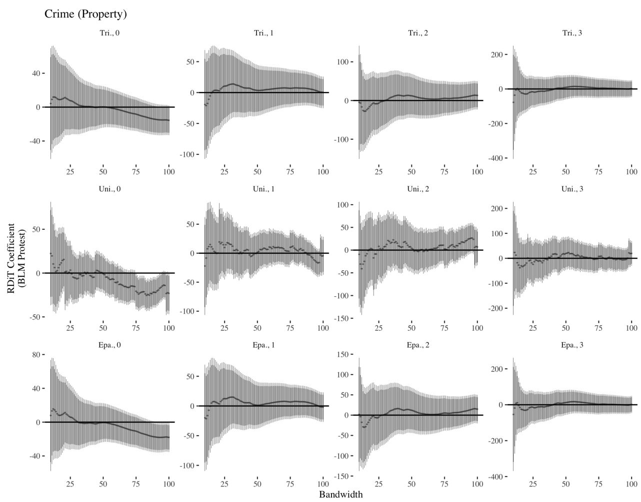

Figure E34: Alternative Bandwidths (10-100 Days, Seattle, Crimes Against Property Outcome) The x-axis is the number of days used in the data before and after the *BLM protest*. The y-axis is the unstandardized RDiT coefficient. Each facet characterizes a different kernel (Tri. = triangular, Uni. = uniform, Epa. = Epanechnikov) and polynomial (0, 1, 2, 3) specification. 95% CIs displayed from robust SEs.

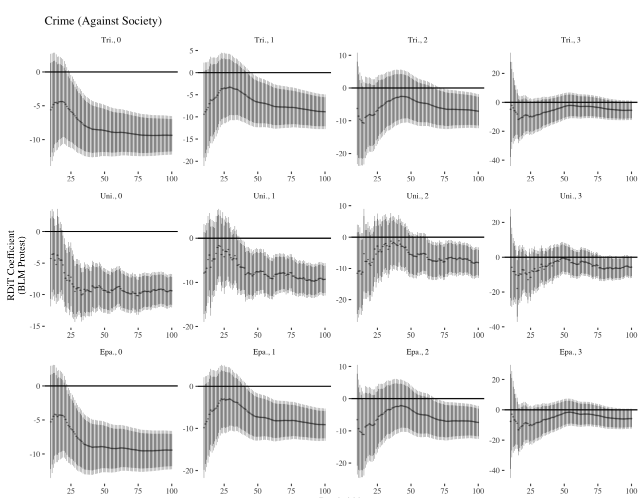

Figure E35: Alternative Bandwidths (10-100 Days, Seattle, Crimes Against Society Outcome) The x-axis is the number of days used in the data before and after the *BLM protest*. The y-axis is the unstandardized RDiT coefficient. Each facet characterizes a different kernel (Tri. = triangular, Uni. = uniform, Epa. = Epanechnikov) and polynomial (0, 1, 2, 3) specification. 95% CIs displayed from robust SEs.

F Long-Term Effects

F.1 Policing Activity

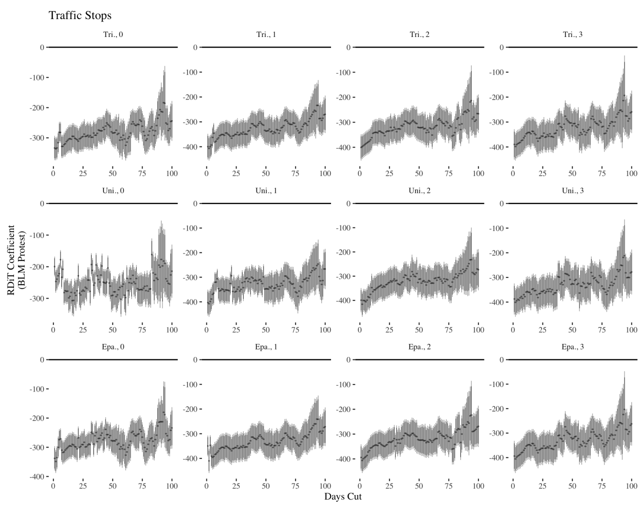

Figure F36: Assessing Persistence of Decreases in Policing Activity post-BLM Protest (Austin, Traffic Stop Outcome) X-axis is the number of days cut from the time series post-*BLM protest*. Y-axis is the unstandardized RDiT coefficient characterizing the discontinuous effect in the outcome between the time period immediately before the BLM protests and however many days after the onset of the BLM protests.

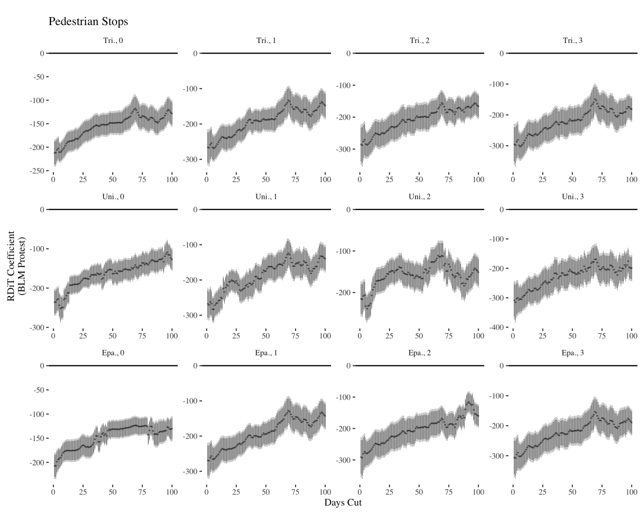

Figure F37: Assessing Persistence of Decreases in Policing Activity post-BLM Protest (Los Angeles, Pedestrian Stop Outcome) X-axis is the number of days cut from the time series post-*BLM protest*. Y-axis is the unstandardized RDiT coefficient characterizing the discontinuous effect in the outcome between the time period immediately before the BLM protests and however many days after the onset of the BLM protests.

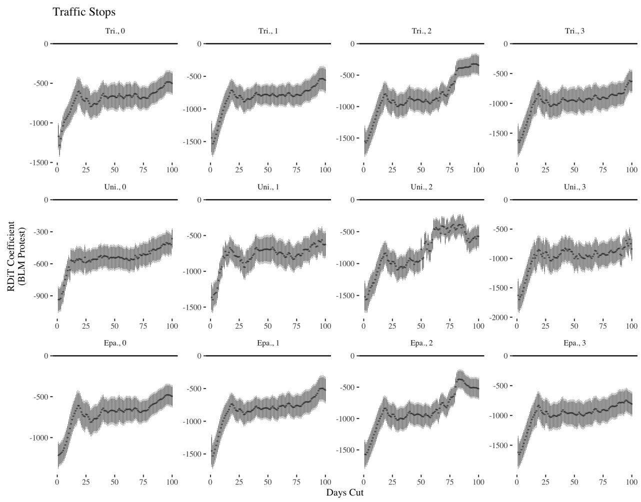

Figure F38: Assessing Persistence of Decreases in Policing Activity post-BLM Protest (Los Angeles, Traffic Stop Outcome) X-axis is the number of days cut from the time series post-*BLM protest*. Y-axis is the unstandardized RDiT coefficient characterizing the discontinuous effect in the outcome between the time period immediately before the BLM protests and however many days after the onset of the BLM protests.

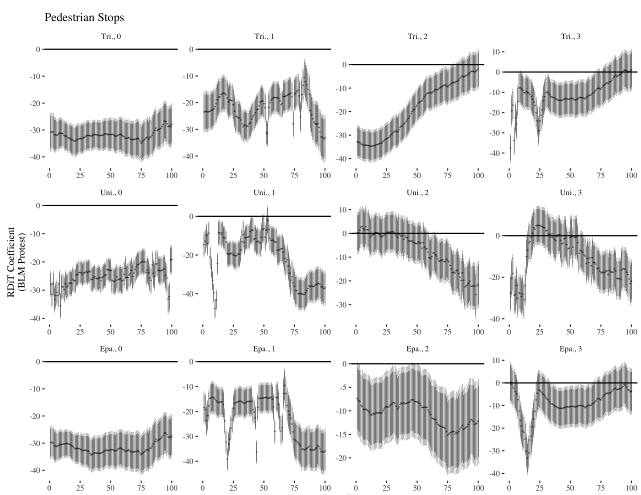

Figure F39: Assessing Persistence of Decreases in Policing Activity post-BLM Protest (Philadelphia, Pedestrian Stop Outcome) X-axis is the number of days cut from the time series post-*BLM protest*. Y-axis is the unstandardized RDiT coefficient characterizing the discontinuous effect in the outcome between the time period immediately before the BLM protests and however many days after the onset of the BLM protests.

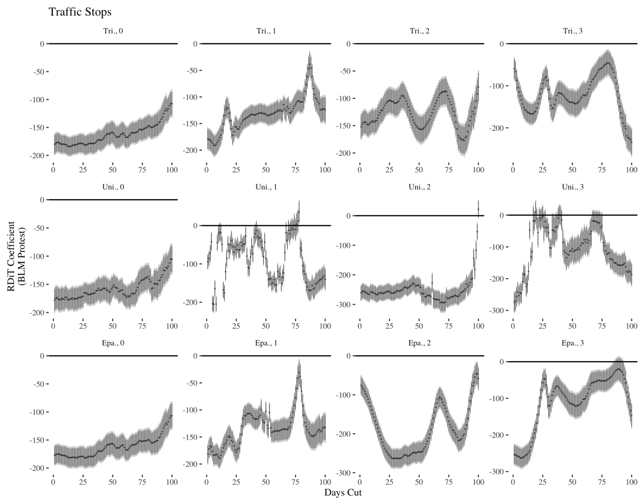

Figure F40: Assessing Persistence of Decreases in Policing Activity post-BLM Protest (Philadelphia, Traffic Stop Outcome) X-axis is the number of days cut from the time series post-*BLM protest*. Y-axis is the unstandardized RDiT coefficient characterizing the discontinuous effect in the outcome between the time period immediately before the BLM protests and however many days after the onset of the BLM protests.

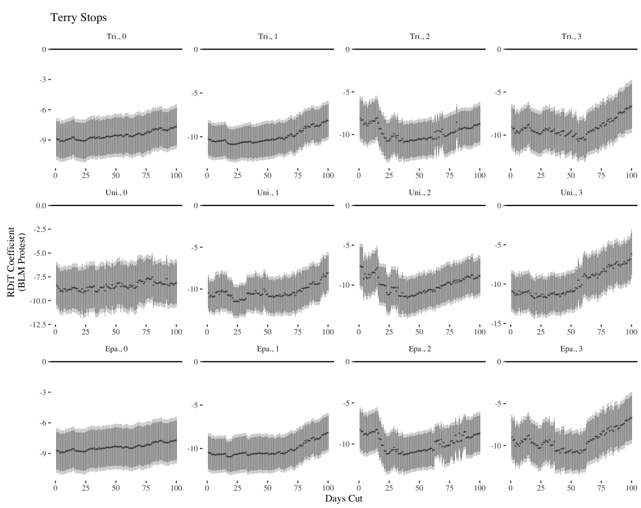

Figure F41: Assessing Persistence of Decreases in Policing Activity post-BLM Protest (Seattle, Terry Stop Outcome) X-axis is the number of days cut from the time series post-*BLM protest*. Y-axis is the unstandardized RDiT coefficient characterizing the discontinuous effect in the outcome between the time period immediately before the BLM protests and however many days after the onset of the BLM protests.

F.2 Hit Rates

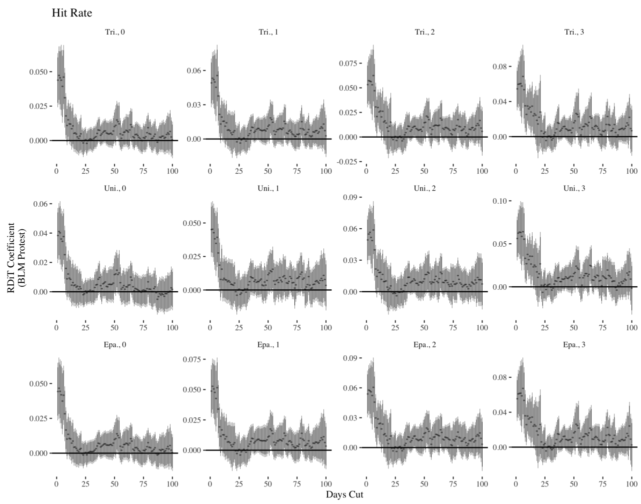

Figure F42: Assessing Persistence of RDiT Effects post-BLM Protest (Austin, Hit Rate Outcome) X-axis is the number of days cut from the time series post-*BLM protest*. Y-axis is the unstandardized RDiT coefficient characterizing the discontinuous effect in the outcome between the time period immediately before the BLM protests and however many days after the onset of the BLM protests.

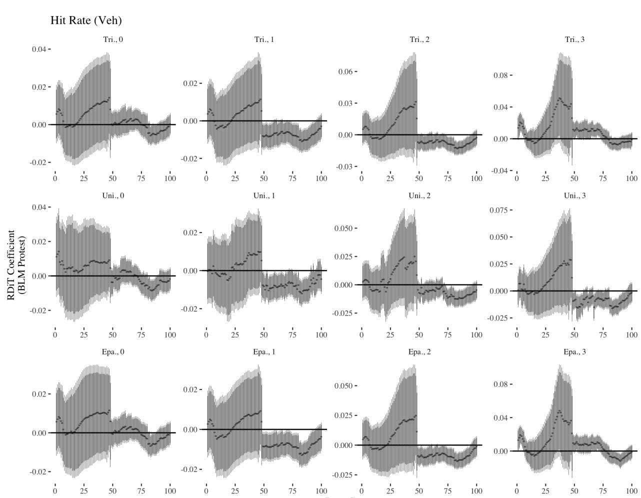

Figure F43: Assessing Persistence of RDiT Effects post-BLM Protest (Los Angeles, Traffic Hit Rate Outcome) X-axis is the number of days cut from the time series post-*BLM protest*. Y-axis is the unstandardized RDiT coefficient characterizing the discontinuous effect in the outcome between the time period immediately before the BLM protests and however many days after the onset of the BLM protests.

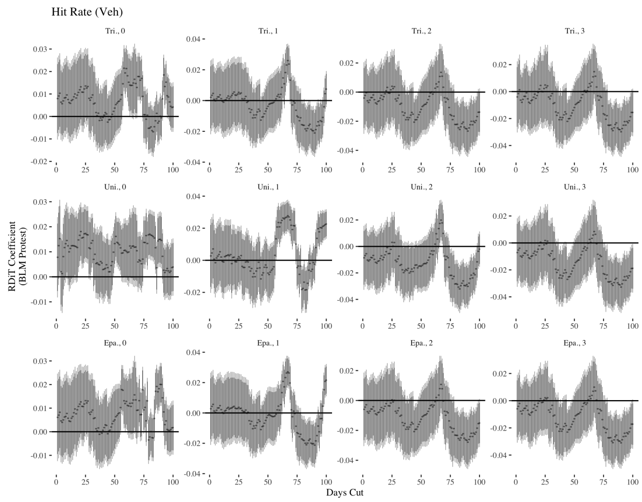

Figure F44: Assessing Persistence of RDiT Effects post-BLM Protest (Philadelphia, Traffic Hit Rate Outcome) X-axis is the number of days cut from the time series post-*BLM protest*. Y-axis is the unstandardized RDiT coefficient characterizing the discontinuous effect in the outcome between the time period immediately before the BLM protests and however many days after the onset of the BLM protests.

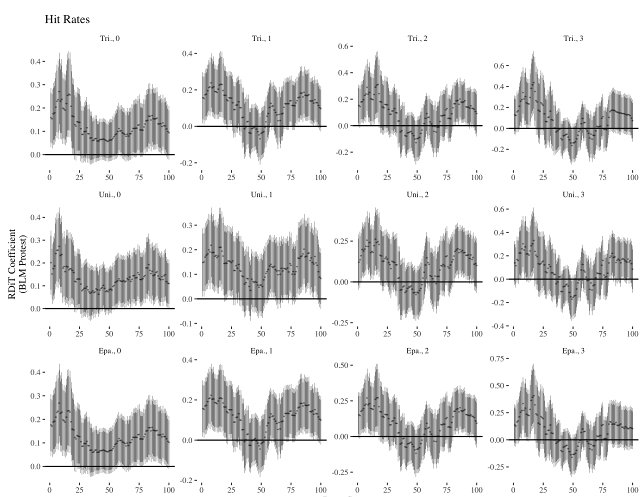

Figure F45: Assessing Persistence of RDiT Effects post-BLM Protest (Seattle, Terry Hit Rate Outcome) X-axis is the number of days cut from the time series post-*BLM protest*. Y-axis is the unstandardized RDiT coefficient characterizing the discontinuous effect in the outcome between the time period immediately before the BLM protests and however many days after the onset of the BLM protests.

F.3 Arrest Rates

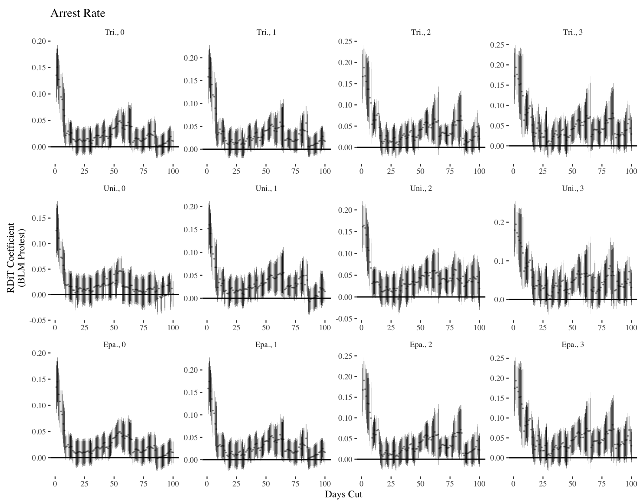

Figure F46: Assessing Persistence of RDiT Effects post-BLM Protest (Austin, Arrest Rate Outcome) X-axis is the number of days cut from the time series post-*BLM protest*. Y-axis is the unstandardized RDiT coefficient characterizing the discontinuous effect in the outcome between the time period immediately before the BLM protests and however many days after the onset of the BLM protests.

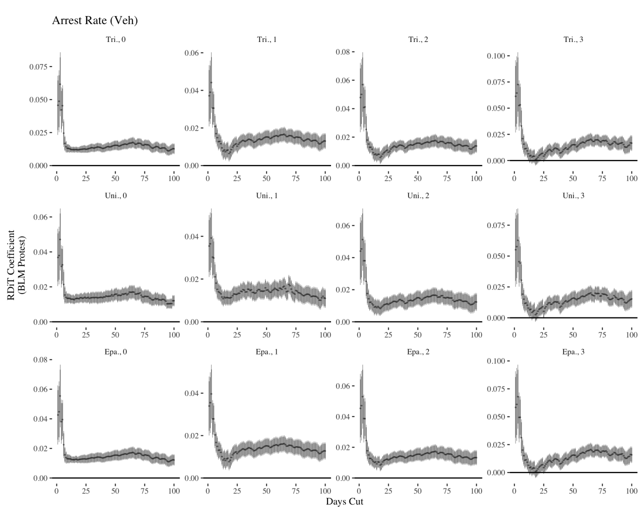

Figure F47: Assessing Persistence of RDiT Effects post-BLM Protest (Los Angeles, Traffic Arrest Rate Outcome) X-axis is the number of days cut from the time series post-*BLM protest*. Y-axis is the unstandardized RDiT coefficient characterizing the discontinuous effect in the outcome between the time period immediately before the BLM protests and however many days after the onset of the BLM protests.

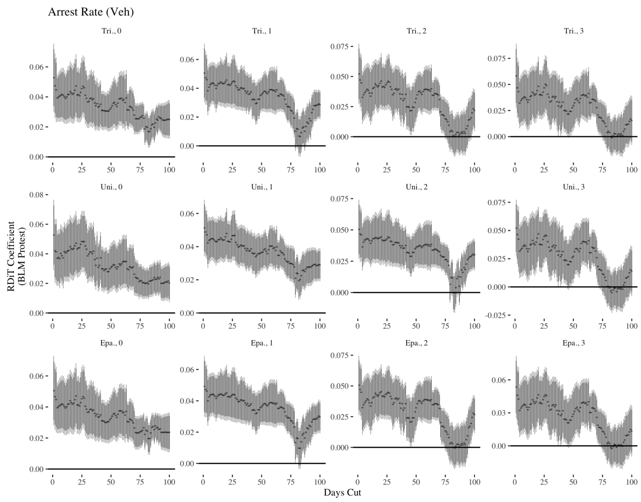

Figure F48: Assessing Persistence of RDiT Effects post-BLM Protest (Philadelphia, Traffic Arrest Rate Outcome) X-axis is the number of days cut from the time series post-*BLM protest*. Y-axis is the unstandardized RDiT coefficient characterizing the discontinuous effect in the outcome between the time period immediately before the BLM protests and however many days after the onset of the BLM protests.

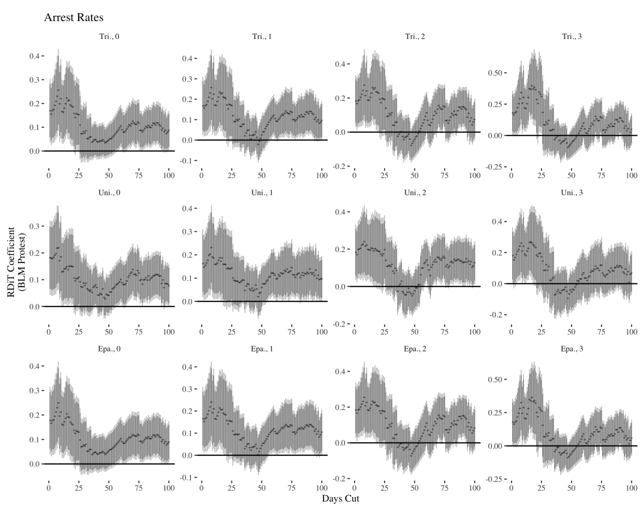

Figure F49: Assessing Persistence of RDiT Effects post-BLM Protest (Seattle, Terry Arrest Rate Outcome) X-axis is the number of days cut from the time series post-*BLM protest*. Y-axis is the unstandardized RDiT coefficient characterizing the discontinuous effect in the outcome between the time period immediately before the BLM protests and however many days after the onset of the BLM protests.

F.4 Rate Ratios

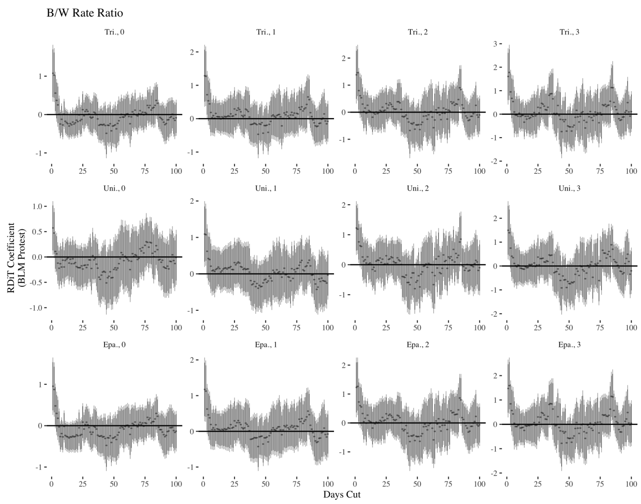

Figure F50: Assessing Persistence of RDiT Effects post-BLM Protest (Austin, Black/White Traffic Stop Rate Ratio Outcome) X-axis is the number of days cut from the time series post-*BLM protest*. Y-axis is the unstandardized RDiT coefficient characterizing the discontinuous effect in the outcome between the time period immediately before the BLM protests and however many days after the onset of the BLM protests.

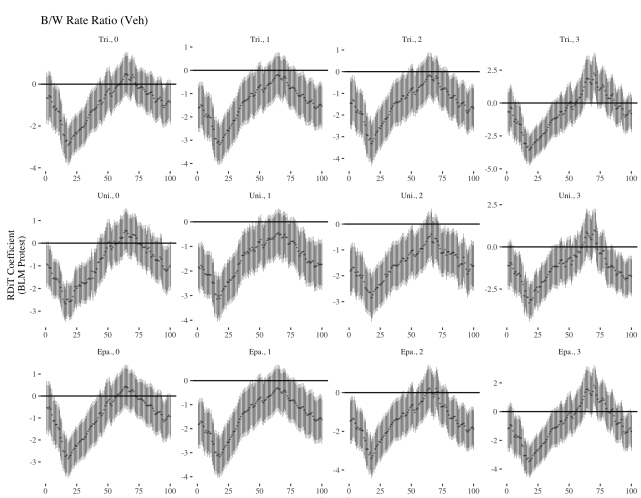

Figure F51: Assessing Persistence of RDiT Effects post-BLM Protest (Los Angeles, Black/White Vehicle Stop Rate Ratio Outcome) X-axis is the number of days cut from the time series post-*BLM protest*. Y-axis is the unstandardized RDiT coefficient characterizing the discontinuous effect in the outcome between the time period immediately before the BLM protests and however many days after the onset of the BLM protests.

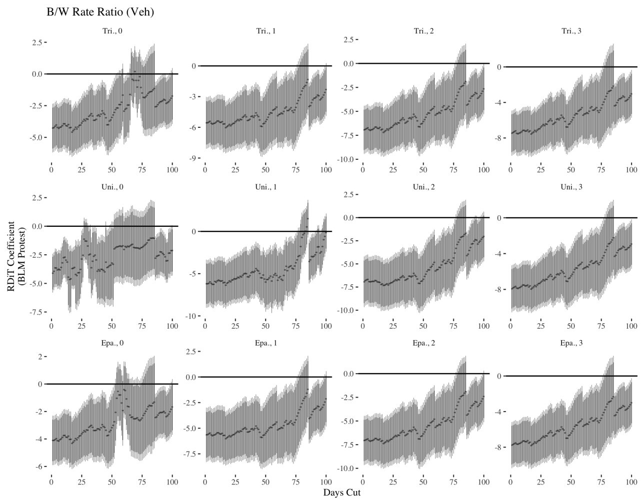

Figure F52: Assessing Persistence of RDiT Effects post-BLM Protest (Philadelphia, Black/White Traffic Stop Rate Ratio Outcome) X-axis is the number of days cut from the time series post-*BLM protest*. Y-axis is the unstandardized RDiT coefficient characterizing the discontinuous effect in the outcome between the time period immediately before the BLM protests and however many days after the onset of the BLM protests.

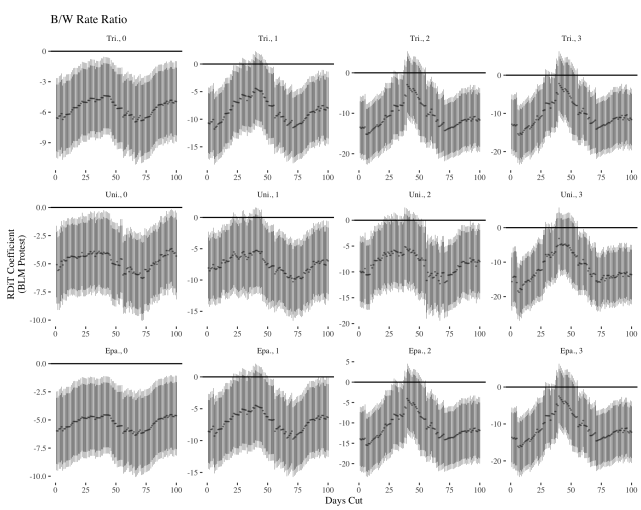

Figure F53: Assessing Persistence of RDiT Effects post-BLM Protest (Seattle, Black/White Terry Stop Rate Ratio Outcome) X-axis is the number of days cut from the time series post-*BLM protest*. Y-axis is the unstandardized RDiT coefficient characterizing the discontinuous effect in the outcome between the time period immediately before the BLM protests and however many days after the onset of the BLM protests.

F.5 Crime

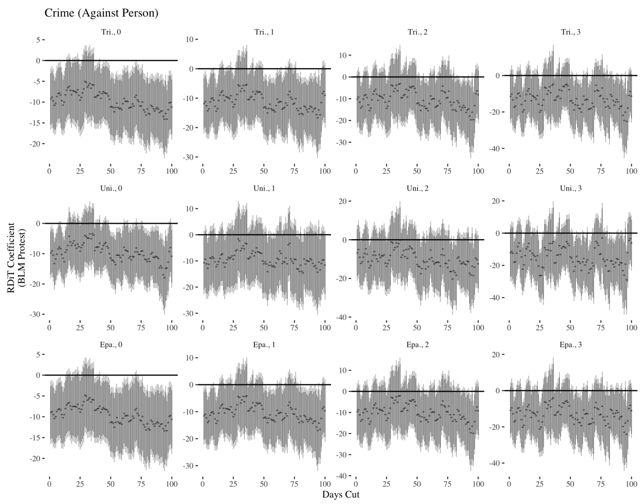

Figure F54: Assessing Persistence of RDiT Effects post-BLM Protest (Austin, Crimes Against Persons Outcome) X-axis is the number of days cut from the time series post-*BLM protest*. Y-axis is the unstandardized RDiT coefficient characterizing the discontinuous effect in the outcome between the time period immediately before the BLM protests and however many days after the onset of the BLM protests.

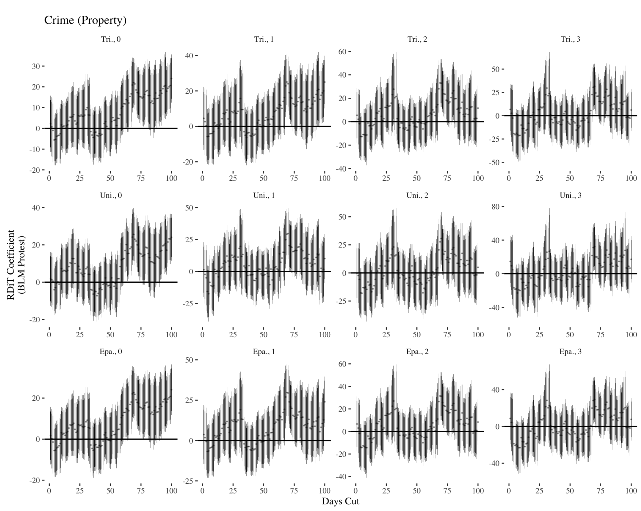

Figure F55: Assessing Persistence of RDiT Effects post-BLM Protest (Austin, Crimes Against Property Outcome) X-axis is the number of days cut from the time series post-*BLM protest*. Y-axis is the unstandardized RDiT coefficient characterizing the discontinuous effect in the outcome between the time period immediately before the BLM protests and however many days after the onset of the BLM protests.

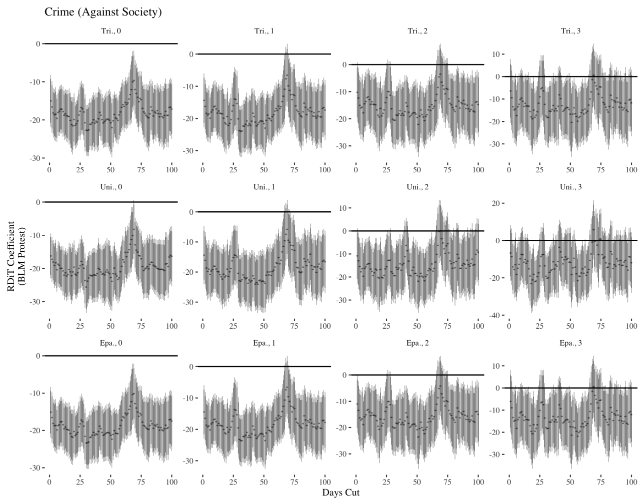

Figure F56: Assessing Persistence of RDiT Effects post-BLM Protest (Austin, Crimes Against Society Outcome) X-axis is the number of days cut from the time series post-*BLM protest*. Y-axis is the unstandardized RDiT coefficient characterizing the discontinuous effect in the outcome between the time period immediately before the BLM protests and however many days after the onset of the BLM protests.

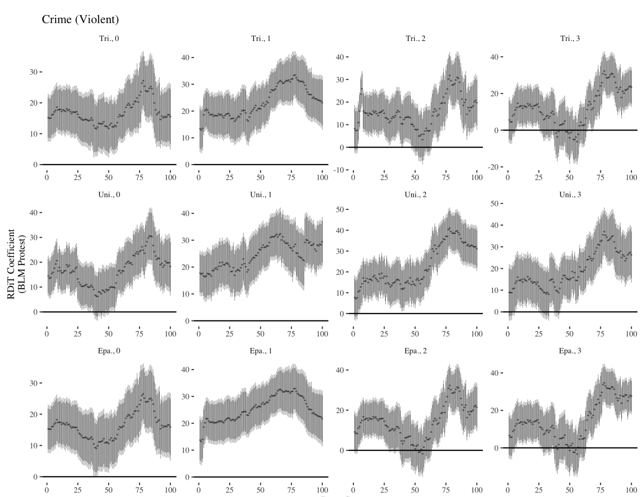

Figure F57: Assessing Persistence of RDiT Effects post-BLM Protest (Los Angeles, Crimes Against Person Outcome) X-axis is the number of days cut from the time series post-*BLM protest*. Y-axis is the unstandardized RDiT coefficient characterizing the discontinuous effect in the outcome between the time period immediately before the BLM protests and however many days after the onset of the BLM protests.

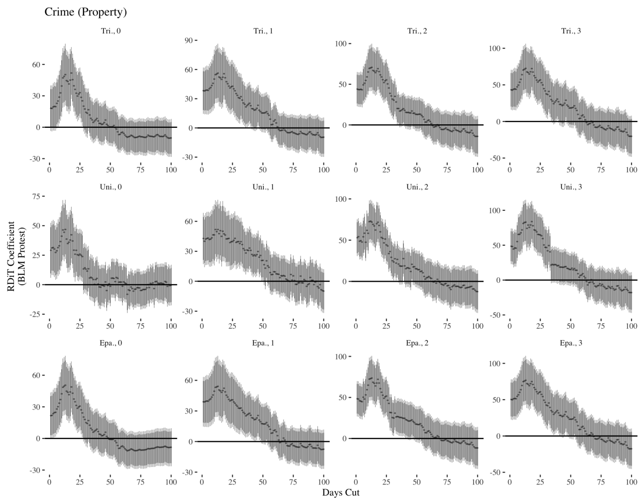

Figure F58: Assessing Persistence of RDiT Effects post-BLM Protest (Los Angeles, Crimes Against Property Outcome) X-axis is the number of days cut from the time series post-*BLM protest*. Y-axis is the unstandardized RDiT coefficient characterizing the discontinuous effect in the outcome between the time period immediately before the BLM protests and however many days after the onset of the BLM protests.

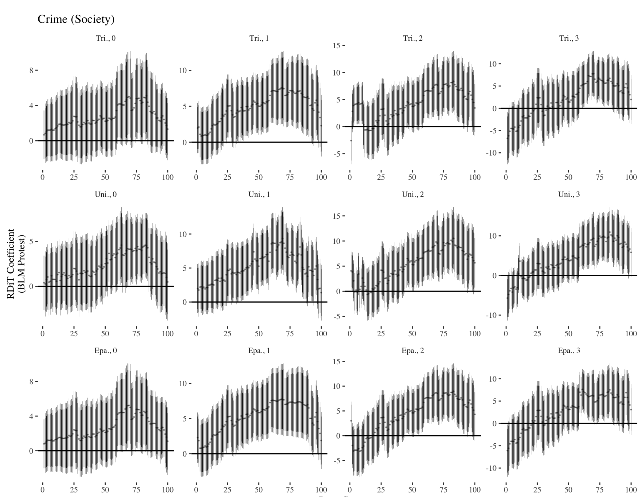

Figure F59: Assessing Persistence of RDiT Effects post-BLM Protest (Los Angeles, Crimes Against Society Outcome) X-axis is the number of days cut from the time series post-*BLM protest*. Y-axis is the unstandardized RDiT coefficient characterizing the discontinuous effect in the outcome between the time period immediately before the BLM protests and however many days after the onset of the BLM protests.

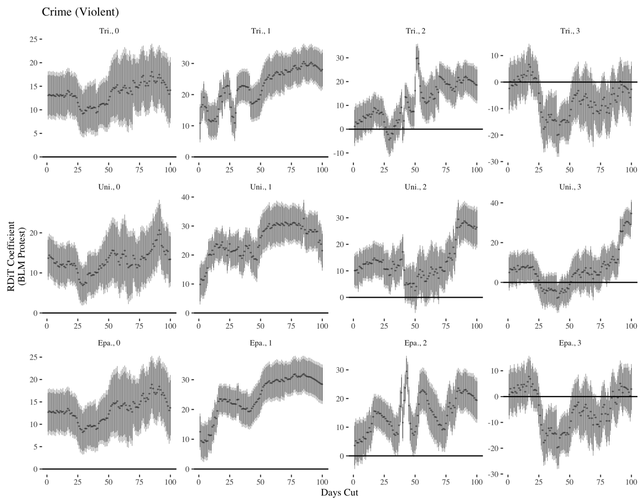

Figure F60: Assessing Persistence of RDiT Effects post-BLM Protest (Philadelphia, Crimes Against Persons Outcome) X-axis is the number of days cut from the time series post-*BLM protest*. Y-axis is the unstandardized RDiT coefficient characterizing the discontinuous effect in the outcome between the time period immediately before the BLM protests and however many days after the onset of the BLM protests.

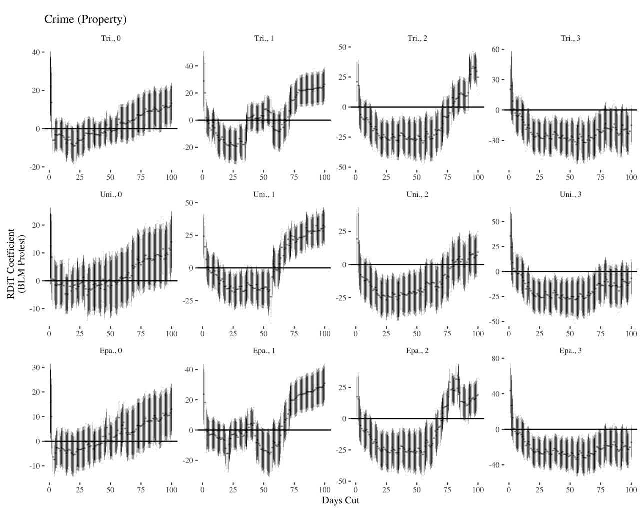

Figure F61: Assessing Persistence of RDiT Effects post-BLM Protest (Philadelphia, Crimes Against Property Outcome) X-axis is the number of days cut from the time series post-*BLM protest*. Y-axis is the unstandardized RDiT coefficient characterizing the discontinuous effect in the outcome between the time period immediately before the BLM protests and however many days after the onset of the BLM protests.

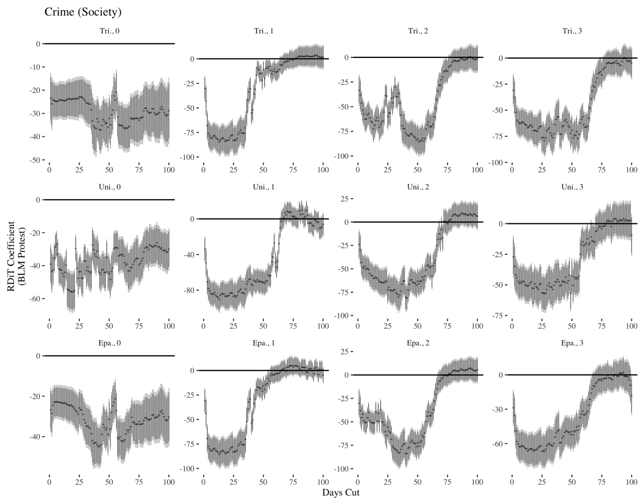

Figure F62: Assessing Persistence of RDiT Effects post-BLM Protest (Philadelphia, Crimes Against Society Outcome) X-axis is the number of days cut from the time series post-*BLM protest*. Y-axis is the unstandardized RDiT coefficient characterizing the discontinuous effect in the outcome between the time period immediately before the BLM protests and however many days after the onset of the BLM protests.

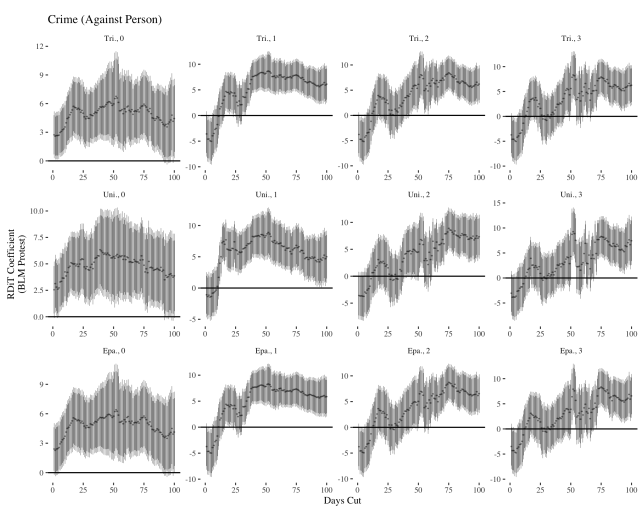

Figure F63: Assessing Persistence of RDiT Effects post-BLM Protest (Seattle, Crimes Against Persons Outcome) X-axis is the number of days cut from the time series post-*BLM protest*. Y-axis is the unstandardized RDiT coefficient characterizing the discontinuous effect in the outcome between the time period immediately before the BLM protests and however many days after the onset of the BLM protests.

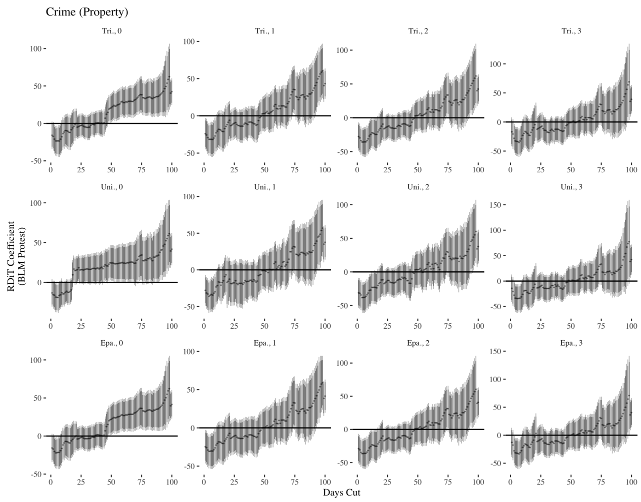

Figure F64: Assessing Persistence of RDiT Effects post-BLM Protest (Seattle, Crimes Against Property Outcome) X-axis is the number of days cut from the time series post-*BLM protest*. Y-axis is the unstandardized RDiT coefficient characterizing the discontinuous effect in the outcome between the time period immediately before the BLM protests and however many days after the onset of the BLM protests.

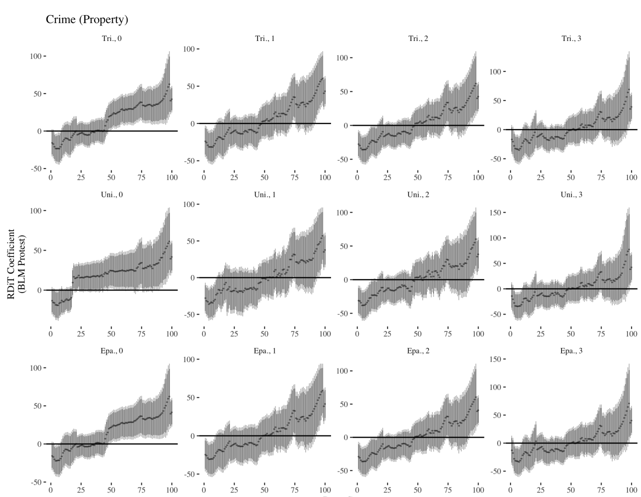

Figure F65: Assessing Persistence of RDiT Effects post-BLM Protest (Seattle, Crimes Against Society Outcome) X-axis is the number of days cut from the time series post-*BLM protest*. Y-axis is the unstandardized RDiT coefficient characterizing the discontinuous effect in the outcome between the time period immediately before the BLM protests and however many days after the onset of the BLM protests.

G Seasonal Placebo Test

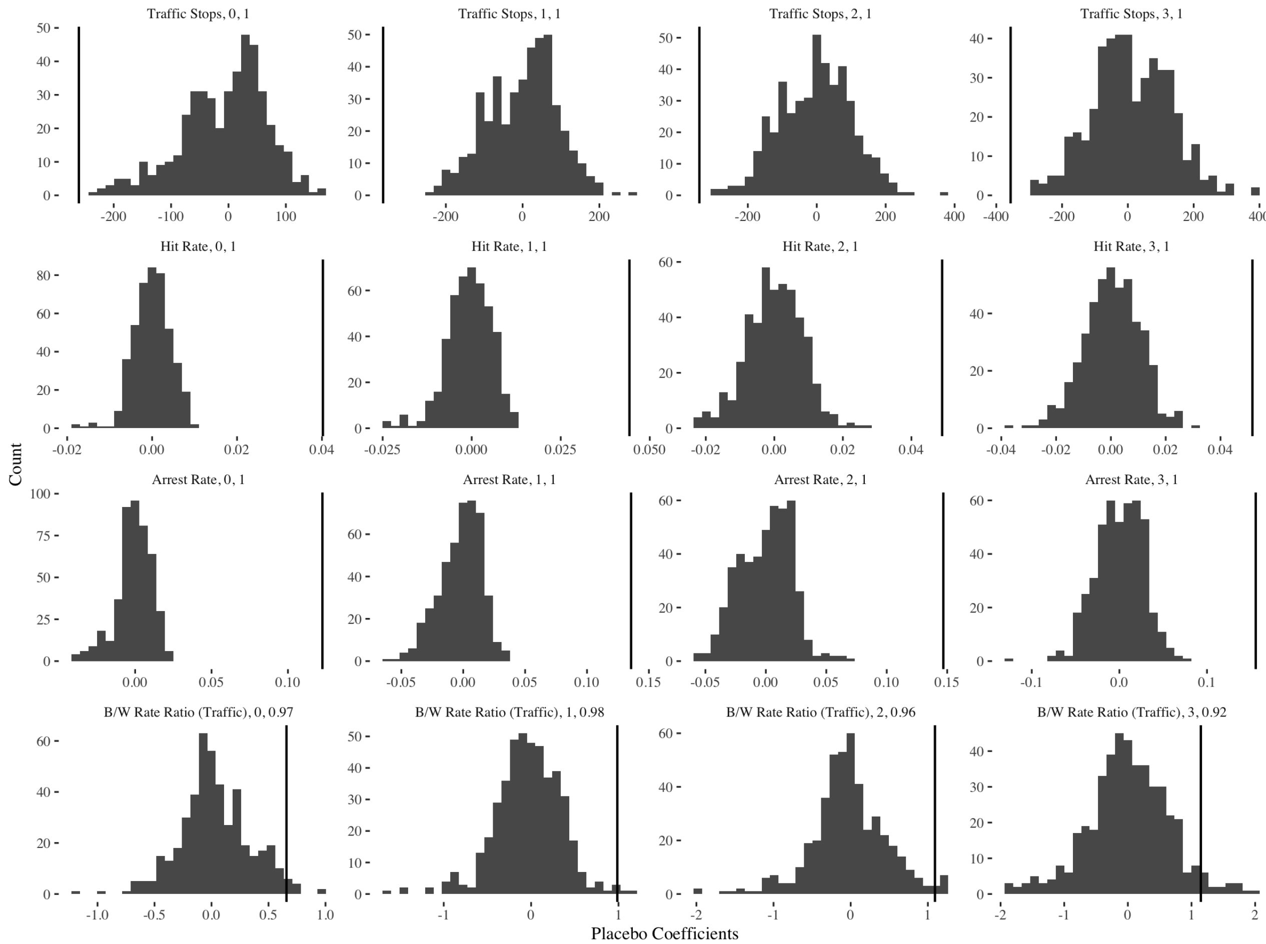

Figure G66: Temporal Placebo Tests Comparing True Post-BLM Discontinuous Effect to Pre-BLM Discontinuous Effects (Austin). The x-axis characterizes pre-BLM placebo coefficients. Solid vertical line denotes true post-BLM protest coefficient. Each facet denotes an outcome, polynomial degree, and the proportion of placebo coefficients (converted to absolute value) that the true coefficient (converted to absolute value) is larger than.

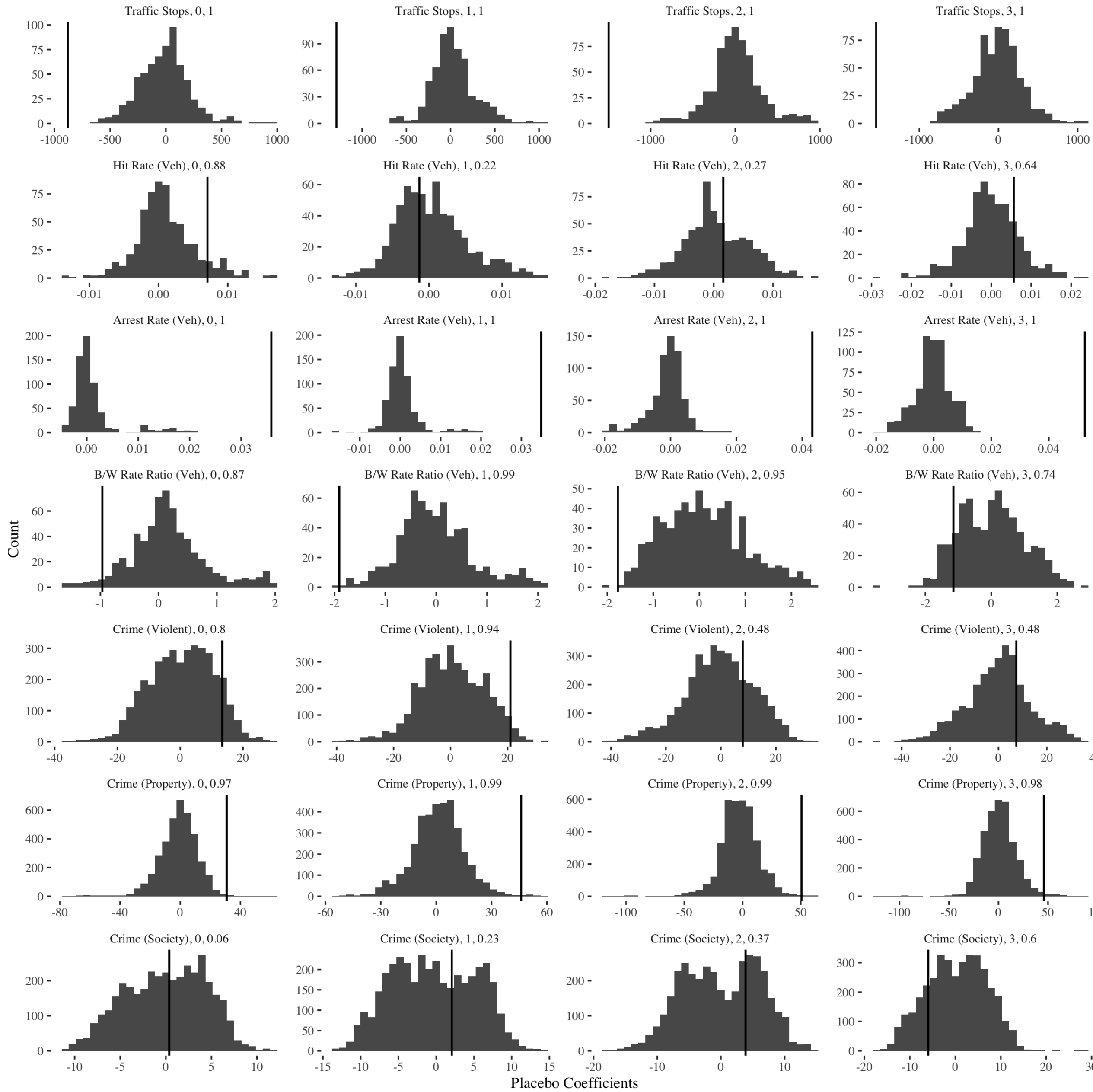

Figure G67: Temporal Placebo Tests Comparing True Post-BLM Discontinuous Effect to Pre-BLM Discontinuous Effects (Los Angeles). The x-axis characterizes pre-BLM placebo coefficients. Solid vertical line denotes true post-BLM protest coefficient. Each facet denotes an outcome, polynomial degree, and the proportion of placebo coefficients (converted to absolute value) that the true coefficient (converted to absolute value) is larger than.

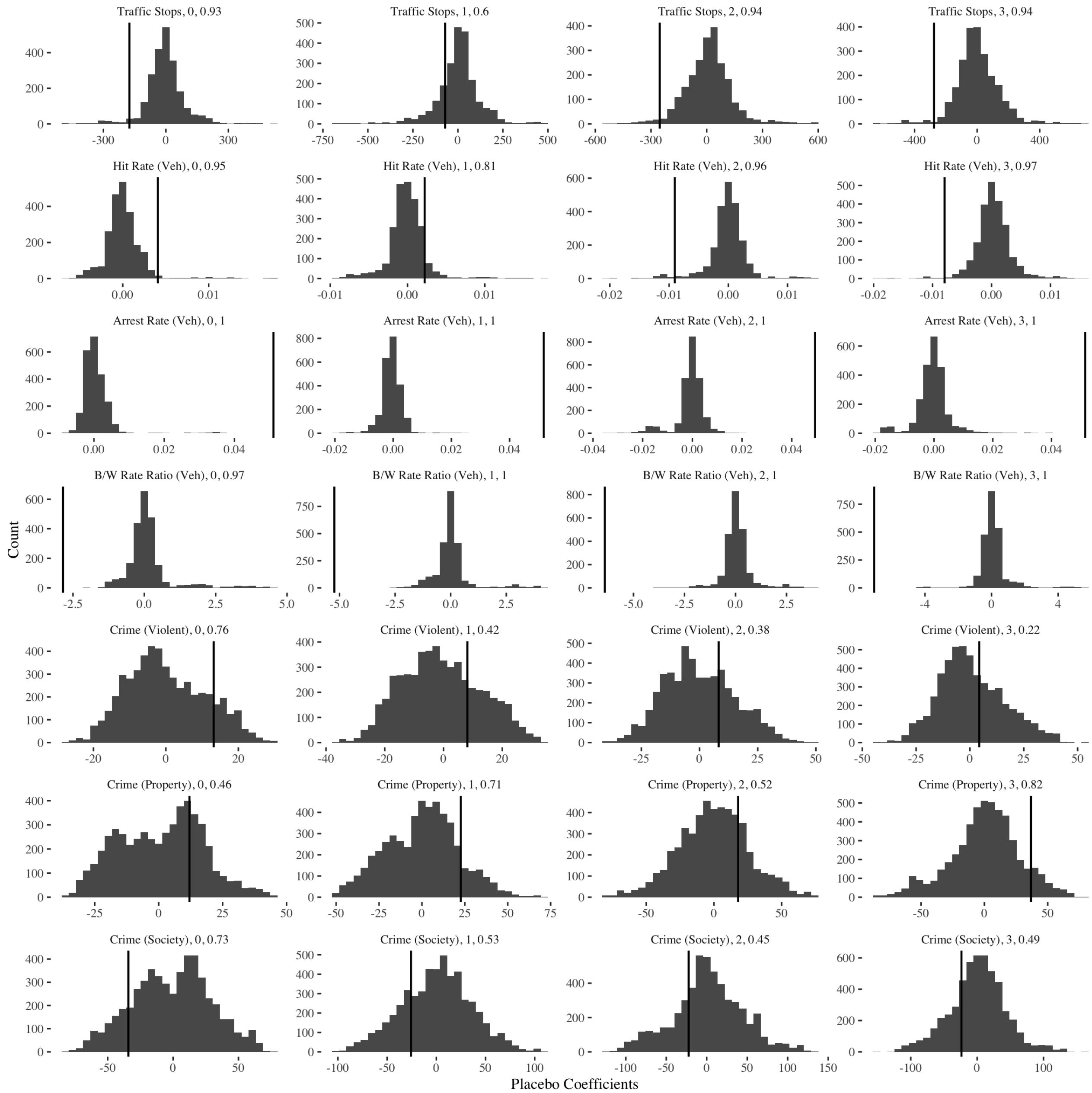

Figure G68: Temporal Placebo Tests Comparing True Post-BLM Discontinuous Effect to Pre-BLM Discontinuous Effects (Philadelphia). The x-axis characterizes pre-BLM placebo coefficients. Solid vertical line denotes true post-BLM protest coefficient. Each facet denotes an outcome, polynomial degree, and the proportion of placebo coefficients (converted to absolute value) that the true coefficient (converted to absolute value) is larger than.

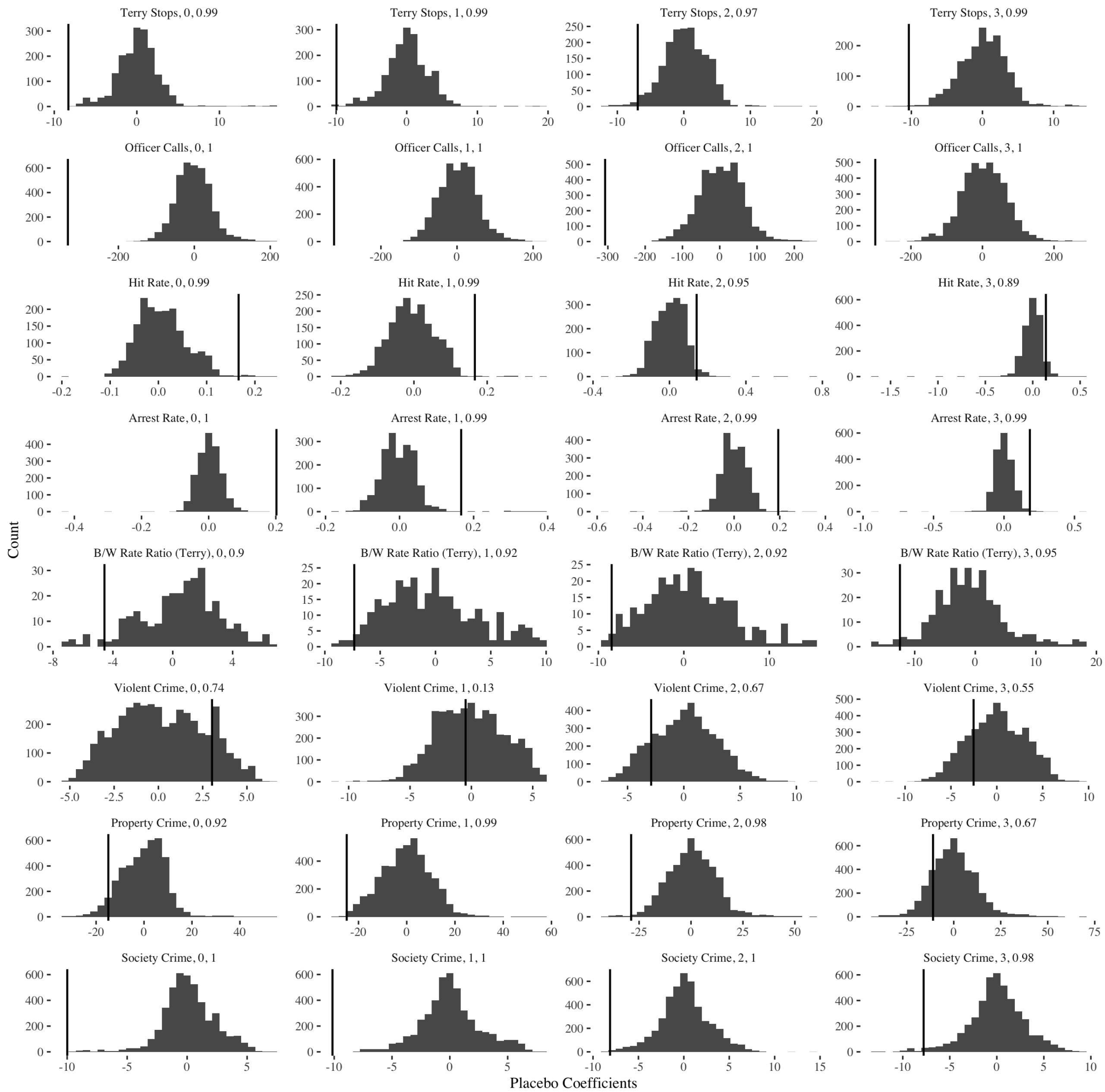

Figure G69: Temporal Placebo Tests Comparing True Post-BLM Discontinuous Effect to Pre-BLM Discontinuous Effects (Seattle). The x-axis characterizes pre-BLM placebo coefficients. Solid vertical line denotes true post-BLM protest coefficient. Each facet denotes an outcome, polynomial degree, and the proportion of placebo coefficients (converted to absolute value) that the true coefficient (converted to absolute value) is larger than.