The George Floyd Effect: How Protests and Public Scrutiny Change Police Behavior in Seattle

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Abstract

The murder of George Floyd in May 2020 sparked a wave of Black Lives Matter protests in many cities throughout the United States. Protests 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. Using data from Seattle, we evaluate whether the 2020 BLM protests impacted police behavior, and whether changes in policing negatively impacted public safety outcomes. Regression discontinuity-in-time estimates suggest that although depolicing followed the BLM protests, there was no improvement in policing quality, and crime was not impacted. Our findings have important implications for fields of research in policing, social movements, and racial and ethnic politics.

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

1 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 several minutes, ultimately causing his death. A video of the incident quickly went viral and sparked the most significant wave of Black Lives Matter 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 Black Lives Matter 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.

This paper asks: what was the impact of the 2020 Black Lives Matter Protests on police behavior? Anecdotal accounts across various media outlets suggest that the protests led to a decline in policing (whether because they were defunded or demoralized) and in turn a rise in crime. Yet, the impact of the protests on both policing practices and public safety outcomes remains an open empirical question. We begin by evaluating whether, following the protests, we observe a reduction in police activity.

Existing literature suggests two possible mechanisms for a decline in police activity following protests. On the 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. 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 (especially in the latter case) as dissent shirking (Chanin and Sheats, 2018). As such, depolicing may be characterized as pro-social when it meets demands for higher quality policing overall; or it may be characterized 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 (Nix, Wolfe, and Campbell, 2018). We therefore further ask: in the event that we observe depolicing, is withdrawal pro or anti-social in nature?

Finally, we assess whether demands for reductions in policing made by protesters, along with any reductions in policing that follow, produce unintended consequences in the form of increased crime. Crime is on the rise in cities across the country and pundits have vilified the 2020 Black Lives Matter 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. Some research, developed in the wake of the 2014 Black Lives Matter protests spurred by the murder of Michael Brown in Ferguson, Missouri, examines similar questions, finding no downstream consequences for crime overall. 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.

In order to assess these questions, we develop a case study of Seattle, Washington. Seattle was the site of some of the most volatile protests in the country. The weekend after George Floyd was murdered, thousands of protesters marched into downtown. While most protests were peaceful, at times the scene became raucously violent. Police cars were burned, rocks and bottles were thrown at officers, weapons were stolen from abandoned police vehicles, and downtown businesses were damaged and looted. Law enforcement responded in kind. Complaints were filed against officers for actions including the use of pepper spray, punching protesters, covering up badge numbers, using flashbangs, and causing harm to both persons and property. At the onset the protests lasted for eleven straight days. They escalated into repeated clashes between citizens and police in the city's Capitol Hill neighborhood, and culminated in the expulsion of police from the East Precinct and the establishment of the Capitol Hill Autonomous Zone (CHAZ, later rebranded as the Capitol Hill Organized Protest, or CHOP). At the same time, the city has been at the forefront of embracing racial justice reforms related to policing, though largely by compulsion, given previous investigations undertaken by the Department of Justice. Seattle makes a good case study for these reasons. The relationship between the police department and citizens has a recent history of being contentious which raises the possibility that law enforcement may draw down policing activities out of retaliation, but recent reforms aimed at increasing transparency and constitutional practices suggest the possibility of accountability as well. Finally, because the protests in Seattle were so antagonistic and the police response so severe, it is exactly the kind of context where we would expect to see crime rise as a consequence of dampened police presence.

In order to evaluate the impact of the 2020 Black Lives Matter protests on police behavior and crime, we draw on a wealth of incident-level policing data from 2016-2022. These data include information on the nature of an incident, how and why citizen contact was

initiated, and the outcome of the incident overall. We also leverage 911 call data, which offers rich information on the source and nature of police dispatches. We employ a regression discontinuity-in-time approach to evaluate the impact of the protests themselves on police behavior. Our first order expectation is that depolicing followed from the 2020 Black Lives Matter protests in Seattle. In order to assess this assumption, we evaluate changes in two key forms of police-citizen interaction over which police officers themselves have significant discretion, and which can be altered in response to top-down commands from leadership: terry stops and officer-initiated 911 calls. A discontinuous and persistent drop in terry stops and officer-initiated calls following the protests can be taken as evidence of depolicing, and this is exactly what we find.

We further characterize depolicing in Seattle as either pro-social or anti-social. Depolicing is pro-social when it leads to an improvement in the quality of policing overall, meeting the demands of the protesters. We measure *quality* in three ways: the efficiency of stops, where fewer stops translates into fewer unnecessary stops as reflected in higher rates of the identification of criminal activity (hit rates) and higher rates of arrest; racial disparities, where if the quality of stops goes up overall we expect to see a decline in stop disparities; and police response times to civilian-initiated requests for emergency assistance (911 calls), where if depolicing is a product of anti-social behavior related to shirking or demoralization we would expect response times to increase. On balance, evidence suggests that depolicing following the 2020 protests is best characterized as anti-social. We find the 2020 protests had no impact on arrest and hit rates. At the same time, they discontinuously increased 911 call response times, an effect which is moderate in duration, returning to pre-treatment levels after about 100 days. However, we do find the protests discontinuously reduced disparities in stops between Black and white citizens, but this appears to be a consequence of simply stopping people less. Finally, to adjudicate the question of crime, we evaluate changes in violent crime following the protests. We focus on violent crime because this is the category least sensitive to changes in policing tactics themselves. We find no change in violent crime following the protests. In some respects, protesters achieved their goal of reducing contact between citizens and police and this increased racial equality in who was stopped. Yet, perhaps unsurprisingly, making fewer stops did not improve the rate at which officers identified serious contraband or other criminal activity, and also had no backlash effect on violent crime.

We begin with an overview of the existing literature on depolicing, particularly in the wake of anti-police protests, in order to outline the conditions under which depolicing has occurred in the past. We then review research on the conditions under which non-elected public servants are likely to nevertheless change their behavior in response to public opinion,

in order to develop competing hypotheses around how police officers might react to BLM protests. We then outline Seattle as a case, before moving on to discussing our data and analytic strategy and reviewing the results. We conclude with a discussion of the policy implications and limitations of our findings, and outline directions for future research.

2 Theory and Literature

2.1 Depolicing as a response to Anti-Police Protests

Little research has explicitly evaluated the impact of anti-police protests on the behavior of officers, insofar as protests may function to extract 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, in the event that depolicing does occur, the downstream impact on crime (Tiwari, 2016; MacDonald, 2019; Lohman, 2021; Capellan, Lautenschlager, and Silva, 2020). However, few researchers have examined whether depolicing occurs, broadly speaking, and the evidence that it does occur systematically is mixed. In-depth 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). Yet, surveys of law enforcement both before and after the Ferguson protests 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). 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. At the same time, an evaluation of agencies in Missouri pre- and post-Ferguson find that misdemeanor arrests declined across the state in the year after the protests occurred (Shjarback et al., 2017). Evidence of depolicing in the wake of protests is therefore mixed.

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), and they are more responsive to the demands of marginalized protesters than to more well-resourced ones (Gause, 2022). 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 2020 protests (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). At the same time, scholars have 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. The study that most directly speaks to our query, which asks whether police changed their behavior in response to the 2020 BLM protests, examines depolicing in Missouri in the wake of the Ferguson uprising. While Shjarback et al. (2017) find evidence that depolicing did occur across the state, their analysis relies on data aggregated to the year/agency level, and looks for changes in the year that followed the year that the protests occurred. The authors therefore offer imprecise estimates of the impact of the protests themselves, and their analytic approach leaves open the possibility for omitted variable bias. Whether anti-police protests can compel a widespread and durable change in officer behavior therefore remains an open question.

2.2 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 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 (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. 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 (higher hit rates when stops do occur, improved responsiveness to citizen requests for assistance), 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 no real improvement in quality of policing, but is simply a declining provision of service.

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, insofar as it may be described as pro- or anti-social.

2.3 Anti-police protests and crime

Much of the existing literature 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 at the hands of police. Yet, researchers have failed to clearly link neither anti-police protests nor depolicing to meaningful changes in violent crime rates (Tiwari, 2016; MacDonald, 2019; Lohman, 2021; Capellan, Lautenschlager, and Silva, 2020; Rosenfeld and Wallman, 2019). While depolicing was initially cited as a main reason for increases in violent crime, scholars across disciplines have debunked this theory. 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-social 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. Given that the extant literature does not find a link between depolicing and crime, and the kinds of activities that police may withdraw from are not clearly linked to declining crime, we do not expect to find any relationship between protests, depolicing and heightened crime. It is nevertheless practically important that we ask the question.

2.4 Seattle case study

In order to evaluate the impact of the 2020 BLM protests on police behavior, we develop a case study of Seattle, Washington. Seattle makes a good case study for a number of reasons. Seattle has a historically contentious relationship between citizens and officers, so much so that the city was investigated by the Department of Justice for excessive use of force, and entered into an oversight agreement with the DOJ in 2012. In part because of that agreement and the deep, long activism around police accountability that precipitated the investigation and persists beyond it, Seattle has been on the forefront of embracing reforms to address issues related to racial justice, transparency and accountability. At the same time, the protests held in Seattle were some of the most volatile in the country, lasting throughout the summer and into the fall, and resulting in formal complaints filed by protesters about police tactics used during the protests that yielded sanctions by the courts. In the event that we observe depolicing, it is just as likely to be a reflection of dissatisfaction with demands for reforms on the part of officers as it is to be a reflection of the police responding to protesters' demands in good faith.

In December of 2010, 35 community organizations signed a letter to the U.S. Department of Justice requesting an investigation into the practices of the Seattle Police Department (SPD), citing several recent instances of disproportionate use of force against people of color, with anecdotal evidence that instances of use of force were racially biased and/or motivated ("Timeline of Seattle Police Accountability" 2021). The DOJ's investigation concluded that the SPD systematically engaged in practices that violated constitutional protections against unnecessary use of force, and they also reported that those patterns were racially biased. As such, the SPD entered into an agreement with the DOJ that placed them under a consent decree, requiring the SPD to revise its practices, to establish a civilian oversight board, and after reforms were made to demonstrate compliance with the consent decree for two years in order to end federal oversight ("Timeline of Seattle Police Accountability" 2021). Yet, even as the city took steps to initiate reforms, scandal inhibited meaningful reform. In 2014 the interim Chief of Police was found to have reversed findings of officer misconduct and discipline in the department ("Timeline of Seattle Police Accountability" 2021). The Office

of Police Accountability is led by law enforcement, and they have a track record of failing to investigate complaints filed against the SPD (Bick, 2022b; Bick, 2022c; Bick, 2022a). As such, nearly 50 community leaders came together to push for a new accountability law, which was finally passed in 2017 after numerous efforts to change internal policies failed to produce outcomes that put the SPD in compliance with the consent decree ("Timeline of Seattle Police Accountability" 2021). That year, the city also requested that a judge find them in compliance with the consent decree to initiate the two-year probationary period. Over the following year, the city undertook consideration of a new contract with the Seattle Police Officers Guild. Local groups opposed the contract due to the fact that it established oversight that was out of compliance with the consent decree, but the city adopted it anyway in 2018. In early 2019, the judge who initially found the city compliant with the consent decree ruled that the city was now out of compliance, requiring that the city both take steps to become compliant and that they restart the two-year probationary period. Public hearings were held in 2019, where citizens pressed for change in the new contract. In early May of 2020, the city council filed a motion to end outside monitoring of the progress of the reforms required to bring them in compliance with the consent decree, and did so without addressing the court's finding that they had fallen out of compliance.

The city took steps to end outside monitoring even as they were determined non-compliant by the court on the eve of the murder of George Floyd in Minneapolis, which would spark a summer of anti-police protests on an unprecedented scale. The second day of protests in the city drew thousands of citizens to the downtown core, and the protests 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 protests in Seattle continued throughout the following week, even as protests in other cities wound down, and they were volatile and expansive. The center of ongoing protests shifted away from the downtown core and into the adjacent neighborhood, Capitol Hill. Capitol Hill is historically LGBTQ-focused and remains the city's cultural center. It is also the location of the East Precinct. Famously, police vacated the East Precinct during the 2020 protests, and protesters established the Capitol Hill Autonomous Zone (CHAZ), joining Minneapolis in the minds of the pundits and public alike as a symbol of the strength of the protesters and a vision of a police-free future. But in Seattle the story is a little more complicated. According to an extensive story by KUOW, the local NPR station, who reviewed roughly 2,000 pages of public records, the police made the decision to vacate the East Precinct in order to de-escalate the protests and because the

building in which the East Precinct is housed is a century old wood-frame structure that would burn to the ground should the protesters light it on fire, as they did in Minneapolis (Allam, 2020). According to later reporting, the Mayor's office even briefly considered giving the building to the King County chapter of Black Lives Matter, although how and by whom the call was ultimately made remains outstanding (Beekman and Kamb, 2022).

The CHAZ, later re-branded as the Capitol Hill Organized Protest (CHOP), existed for about three weeks. During that time, it is reported that the SPD refused to respond to 911 calls from in and around the area, and while daytime activities resembled a festival atmosphere, night time brought instances of violence, including multiple homicides (Kamb and Beekman, 2021). Mayor Durkan therefore ordered the dismantling of the CHOP, which occurred with the arrest of over 30 protesters (Allam, 2020). Highlighting the thorny and conflicted nature of policing and police reform, the office of the Mayor and the city council are now facing a lawsuit from business owners and residents of the area, citing willful neglect that they claim lead to the violence (Kamb and Beekman, 2021; Bick, 2022a).

How individual officers responded to the volatile situation is unclear. What we do know is that some officers intentionally tried to increase the tension by engaging in chatter over the radio that suggested a contingent of the violent white nationalist Proud Boys had turned up at the protests (Chapman, 2022). We also know that several officers participated in the January 6 insurrection at the Capitol in Washington, DC, and that officers from Seattle represented the largest group from any one police department present on that day. This suggests that a significant number of officers hold views in direct opposition to those expressed by the 2020 BLM protesters (Haroun, 2022). Finally, we know that a significant number of officers have left the department because of low morale and in protest against calls to defund the police ("Over 200 Seattle police officers quit amid nation protests" 2021). Exit interviews offer insight into why individuals quit. One officer wrote that, "I refuse to work for this socialist City Council and their political agenda. It ultimately will destroy the fabric of this once fine city," and another said, "The people who run the department have to be politicians and work with people who hate us," (Horcher, 2020).[1]

While we cannot know the underlying motivations of all the officers involved, the deeply contentious nature of policing in Seattle, and the ever-changing response of the mayor, heightens the likelihood that we will observe depolicing following the protests. For some officers, at least, withdrawal of service provision is likely anti-social in nature. Yet, by some accounts the decision to evacuate the East Precinct itself was motivated by an attempt to

[1]Reports of officers quitting the SPD should be understood in context. All sectors are experiencing a labor shortage – including other emergency response agencies in Seattle, where the 911 call center (no longer under the direction of the SPD) is reported to be operating 26 percent below staffing capacity (Markovich, 2022).

restore trust between officers and protesters, and seems to have been a tactic to de-escalate and allow protesters within marching distance of the East Precinct without endangering officers. Moreover, the ongoing consent decree that installed formal mechanisms of accountability raises the possibility that any depolicing that may have occurred was pro-social in nature. Finally, while some individual officers may perceive that they are hated by local politicians, the city council has continually approved budget *increases* for the department in the subsequent two years (Casey, 2022).

The recent history of the police department is deeply fraught, and that is reflected in officer attitudes that, whatever the reason, reflect strain; it is reflected in the changing attitudes of city leadership; and it is reflected in the mass demonstrations by citizens in solidarity with the protesters in Minneapolis, and the volatility between police and citizens that followed. This is exactly the kind of circumstances that might give way to depolicing, although the character that such withdrawal is likely to take is unclear and likely difficult to empirically evaluate. Below, we restate our research questions and identify our specific expectations, before moving forward with a discussion of our data and analytic strategy.

2.5 Expectations

Recall that our first order question is as follows: What is the impact of the 2020 BLM protests on police behavior? It is unclear from the existing literature *when* depolicing should occur. Research around the so-called Ferguson Effect theorizes that crime may go up following anti-police protests because of depolicing, and some work does find evidence of depolicing following protests. Other work looks for depolicing in response to other means of interfering with law enforcement activities, such as DOJ investigations and court cases, but does not find strong evidence that depolicing occurs in these circumstances. Both circumstances were present in Seattle. There is anecdotal evidence that police officers were dissatisfied with the demands being made by activists during the 2020 BLM protests, which capped off a long period of contention between the police, activists, and city leadership. The conditions are ripe for the withdrawal of service provision in response to strain. For these reasons, we develop the following hypothesis:

Hypothesis 1: Following the 2020 BLM protests in Seattle, there will be a discontinuous decline in discretionary policing activities.

Our second order question asks: In the event that we observe a decline in police activities following the protests, is that decline better characterized as pro-social or anti-social? Very few studies move beyond an evaluation of depolicing and the downstream consequences for crime to characterize the nature of depolicing itself. There is anecdotal evidence that

many officers were deeply disaffected by city politics that preceded the protests, and further by the protests themselves, in which civilians damaged property and officers engaged in violence. We might therefore expect that depolicing was driven by officer dissatisfaction, was retaliatory, and declining service is likely to be best characterized as anti-social. At the same time, however, the consent decree imposed formal accountability mechanisms on the Seattle Police Department. Moreover, existing literature also suggests that indirect political pressure from city leaders can lead to changes in officer behavior and an improvement in the quality of policing overall. This leads to the following two competing hypotheses:

Hypothesis 2a: Following the 2020 BLM protests in Seattle, there will not be a discontinuous change in the quality of policing overall.

Here, we would observe depolicing without a commensurate change in the quality of policing, which would lead us to conclude that depolicing is anti-social, irrespective of the exact motivations held by officers for withdrawing service provision.

Hypothesis 2b: Following the 2020 BLM protests in Seattle, there will be a discontinuous improvement in the quality of policing overall.

Here, we would observe depolicing with a commensurate change in the quality of policing, which would lead us to conclude that depolicing is pro-social, where police are responding to protester's demands to improve service provision and reduce contact with civilians.

Finally, our third order question asks: In the event that we observe a decline in police activities following the protests, is that decline in activity accompanied by increasing violent crime? Previous studies have not tried to distinguish between pro-social and anti-social depolicing. It may be that depolicing identified as pro-social leads to better public safety outcomes. It may also be that depolicing identified as pro-social improves citizens' experiences without sacrificing public safety. Alternatively, it may be that when depolicing is acutely anti-social, crime increases. However, existing literature gives us no reason to expect that increased violent crime will follow from either depolicing or the protests. Moreover, 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 associated with fighting violent crime. They are instead the kind of tactics targeted to 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: Following the 2020 BLM protests in Seattle, there will not be a discontinuous change in violent crime overall.

3 Data and Design

3.1 Outcomes

To assess if the 2020 BLM protests reduced discretionary policing (*Hypothesis 1*), we use Seattle Police Department (SPD) incident-level data on terry stops and officer-initiated 911 calls between June 2019-February 2022 and January 2016-June 2021 respectively.[2] We aggregate these data to a day-level time series characterizing the daily number of terry stops (*stops*) and officer-initiated calls (*officer calls*). Given that *Hypothesis 1* relates specifically to discretionary policing activity we subset the terry stop data to officer-initiated stops (i.e. "onview" stops), as opposed to stops incidental to calls for service. According to our correspondence with Seattle Open Data, *officer calls* are an appropriate police effort measure since they capture officer identification of criminal activity and subsequent motivation to resolve the activity. If *Hypothesis 1* is correct, we would expect *stops* and *officer calls* to decrease post-protest.

For *Hypothesis 2*, we evaluate if the 2020 BLM protests changed policing quality. First, we assess if the 2020 BLM protests increased policing efficiency and reduced the rate of fruitless police-citizen contact. We use the terry stop data to construct a daily time series of two efficiency measures. *Hit rates* are the proportion of daily stops that resulted in an arrest, citation, offense report, or referral for prosecution as opposed to a field contact without action taken, implying no identification of criminal wrongdoing (i.e. a fruitless stop). We use this *hit rate* measure since it is the standard SPD uses to determine if terry stops are effective at identifying wrongdoing.[3] *Arrest rates* are the proportion of terry stops resulting in an arrest, suggesting the identified offense during a terry stop was arrest-worthy. Second, we assess the effect of the protests on civilian-initiated 911 call *response times*. *Response time* is the time, in minutes, for police officers to arrive at the scene of a civilian request for police service. Slower response times can be taken as evidence of anti-social policing, conditional on the number of police officers available for patrol.[4] Third, to assess if police are responding to calls to reduce racially disparate policing, we evaluate if the 2020 BLM protests reduced the terry 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

[2]Sources: `https://data.seattle.gov/Public-Safety/Terry-Stops/28ny-9ts8` and `https://data.seattle.gov/Public-Safety/Call-Data/33kz-ixgy`

[3]See: `http://spdblotter.seattle.gov/wp-content/uploads/sites/11/2019/12/Dkt-600-1-Exhibit-SPDs-Disparity-Review-Part-II.pdf`

[4]We limit an evaluation of response times to the months immediately following the protest. In May of 2021, the 911 call dispatch center switched to being managed by civilians, rather than police.

rate ($(WhiteStops/WhitePopulation) \times 10,000$).[5] If *Hypothesis 2a* is true, then the 2020 BLM protests will have no effect on *hit rates*, *arrest rates*, *response times*, or the *rate ratio*. Conversely, if *Hypothesis 2b* is true, then the 2020 BLM protests will have a positive effect on *hit rates* and *arrest rates*, a positive or null effect on *response times*, and a negative effect on the *rate ratio* indicating a smaller racial difference in the occurrence of stops.

To test *Hypothesis 3*, we use SPD crime incident-level data between January 2016-February 2022.[6] SPD relies on the Federal National Incident Based Reporting System (NIBRS) rules for classifying crimes. We primarily focus on violent, that is, *against person crime*. This is because *against person crimes* are less likely to be reported in the administrative record as a function of police effort (Rosenfeld and Wallman, 2019). Rather, they are more likely to be reported as a function of civilian reporting. Therefore, if police reduce activity post-protests, identification of *against person crimes* would not be endogenous to the police response. 94% of against person crimes are assault offenses. 5% are (non-consensual) sex offenses. The rest are consensual sex offenses, homicide offenses, and human trafficking. To evaluate the effect of the 2020 BLM protests on *against person crimes*, we generate a daily time series of the count of *against person crimes.* For comprehensiveness, we also evaluate the effect of the 2020 BLM protests on the two other NIBRS crime categories: *against property* and *against society crimes*.[7] We also evaluate the effect of the 2020 BLM protests on all crimes. If *Hypothesis 3* is correct, the 2020 BLM protests should at least have no effect on *against person crimes*.

3.2 Independent Variable

The independent variable, for each of the daily time series, is a binary indicator equal to 1 on or after the 2020 *BLM protests* in Seattle (May 29, 2020). We choose May 29 as the onset date since that is when the first *BLM protest* is held in Seattle after George Floyd's murder.[8]

[5]Racial group population estimates from the 2010 Census.

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

[7]The composition of *against property* offenses are 50% larceny/theft, 16% burglary, 13% property damage, 8% motor vehicle theft, 6% fraud offenses, 3% robbery, 1% stolen property, with the remaining offenses being counterfeiting, bad checks, arson, embezzlement, extortion, and bribery. The composition of *against society* offenses are 42% property trespass, 21% drug offenses, 16% DUIs, 9% weapon violations, 6% non-violent family offenses, 3% prostitution offenses, with the remaining being liquor law violations, curfew violations, pornography violations, animal cruelty, peeping tom, drunkenness, and gambling offenses.

[8]Source: `https://www.capitolhillseattle.com/2020/05/seattle-defiant-walk-of-resistance-protest-planne`

3.3 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.

We estimate the discontinuous effect of *BLM protest* on relevant outcomes using a variety of specifications where the running variable bandwidth is between 10-100 days, the running variable is set to the 0th, 1st, and 2nd polynomial degree (e.g. difference-in-means, linear, quadratic), and the kernel is set to triangular, uniform, and epanechnikov function.[9] For brevity, we present results using a subset of model specifications. We present tables using a uniform kernel, linear and quadratic running variable polynomials, and bandwidths of 25 and 50 days from the *BLM protest* cutoff. The figures present estimates using a uniform kernel and linear polynomial degree, for bandwidths spanning 10-100 days from the cutoff. Coefficient estimates from other specifications are in the appendix. We prioritize interpreting RDiT estimates from 25 and 50-day bandwidths for two reasons. First, RDiT estimates including data further from the cutoff may be biased since they're informed by data potentially influenced by secular trends affecting police behavior outside the protest. Second, these bandwidths exclude data prior to Washington State's COVID lockdown, which started on March 23, 2020, allowing us to acquire RDiT estimates less informed by changes in police behavior due to COVID policy shifts.

4 Results

4.1 Policing

We find support for *Hypothesis 1.* Table 1 displays RDiT coefficients characterizing the discontinuous effect of the 2020 *BLM protests* on discretionary policing activity. Depending on the specification, the *BLM protest* reduced terry stops by 9-21 stops per day ($p < 0.05$ for all specifications except where the bandwidth is 50 and running variable degree is quadratic). The reduction is equivalent to 190-286% of the pre-BLM outcome standard deviation.

Officer-initiated 911 calls also decreased discontinuously at the cutoff, by 122-213 calls per day ($p < 0.05$ for all specifications except where the bandwidth is 50 and running variable degree is quadratic). This reduction is equivalent to 94-541% of the pre-BLM outcome

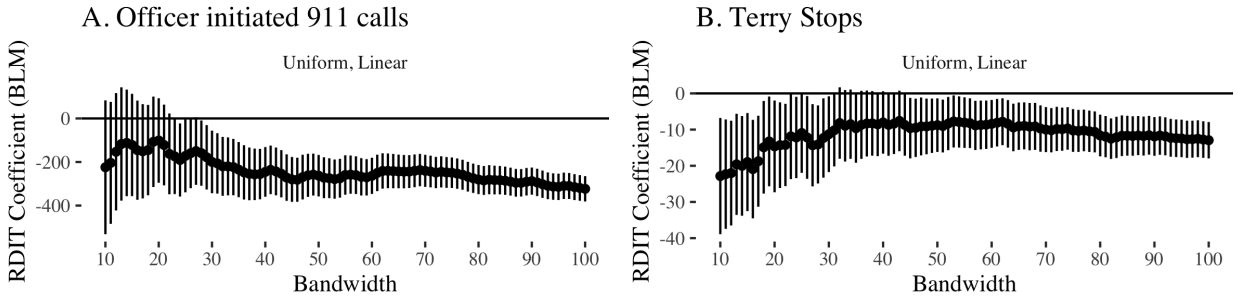
[9]Implemented with the `rdrobust` package in R, see Calonico, Cattaneo, and Titiunik (2015)

Table 1: Coefficients characterizing RDiT effect of BLM protests on discretionary policing

Outcome	Polynomial	Bandwidth	RDiT Coef. (BLM)	SE	p-value
Terry Stops	Linear	25	-10.94	5.62	0.05
Terry Stops	Linear	50	-8.77	3.77	0.02
Terry Stops	Quadratic	25	-20.57	7.20	0.00
Terry Stops	Quadratic	50	-10.27	5.40	0.06
Officer Calls	Linear	25	-172.04	85.32	0.04
Officer Calls	Linear	50	-262.83	48.59	0.00
Officer Calls	Quadratic	25	-122.14	130.07	0.35
Officer Calls	Quadratic	50	-213.68	76.89	0.01

Note: Robust SEs displayed. All estimates use uniform kernel.

Figure 1: Coefficients characterizing the RDiT effect of BLM protests on policing activity, varying bandwidths



Note: Uniform kernel, linear polynomial, bandwidths ranging from 10-100

standard deviation. Figure 1 corroborates the results for terry stops and officer calls from Table 1 across multiple bandwidths.

4.2 Quality

We find support for *Hypothesis 2a* but not *Hypothesis 2b*. There is no statistically significant discontinuous change in arrest rates, hit rates, or stop rate ratios between Black and white citizens. We do observe a decline in response times to civilian initiated 911 calls. The decline persists until September, and then returns to pre-treatment levels. Table 2 displays the RDiT coefficients for the effect of the 2020 *BLM protests* on policing quality, and these results are corroborated across several bandwidth specifications, displayed in Figure 2.

Table 2: Coefficients characterizing RDiT effect of BLM protests on policing quality

Outcome	Polynomial	Bandwidth	RDiT Coef. (BLM)	SE	p-value
Arrest Rate	Linear	25	0.26	0.13	0.05
Arrest Rate	Linear	50	0.03	0.12	0.79
Arrest Rate	Quadratic	25	0.10	0.18	0.59
Arrest Rate	Quadratic	50	0.16	0.12	0.20
Hit Rate	Linear	25	0.20	1.34	0.88
Hit Rate	Linear	50	-0.60	0.96	0.53
Hit Rate	Quadratic	25	0.83	1.85	0.65
Hit Rate	Quadratic	50	-0.36	1.33	0.79
B/W Rate Ratio	Linear	25	4.50	6.67	0.50
B/W Rate Ratio	Linear	50	-6.60	4.55	0.15
B/W Rate Ratio	Quadratic	25	5.71	6.92	0.41
B/W Rate Ratio	Quadratic	50	1.08	5.82	0.85
Response Time	Linear	25	7.56	8.94	0.40
Response Time	Linear	50	21.18	5.90	0.00
Response Time	Quadratic	25	-14.39	11.47	0.21
Response Time	Quadratic	50	9.81	8.38	0.24

Note: Robust SEs displayed. All estimates use uniform kernel.

4.3 Crime

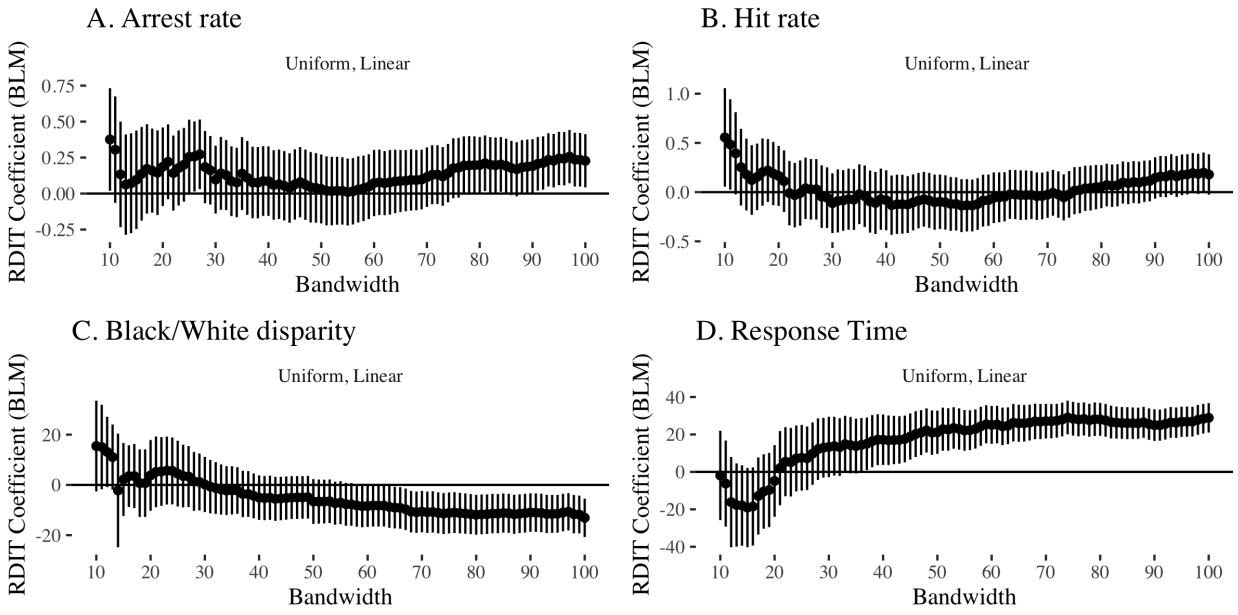
Finally, we find support for *Hypothesis 3*. Table 3 displays RDiT coefficients characterizing the discontinuous effect of the 2020 *BLM protests* on crime rates. The coefficients are all insignificant, with the exception of *against society*. Crimes in this category decreased, but this is unsurprising because these are also the kinds of crimes that are sensitive to police activity itself. For the most relevant measure for our purposes, *against person crimes*, there was no discontinuous change to the crime rate under any model specification.

4.4 Robustness Checks

A problem inherent to our design is that the RDiT only estimates the short-term, discontinuous effect of *BLM protest*, but not long-term patterns that may manifest in response to *BLM protests*. Related, the null *BLM protest* effects on discretionary policing and crime may be the result of a short-term bundled treatment. Police may be policing less and identifying less crime because the *BLM protests* may have 1) reduced the number of people engaging in criminal activity or 2) distracted police from identifying crime and policing the city more broadly.

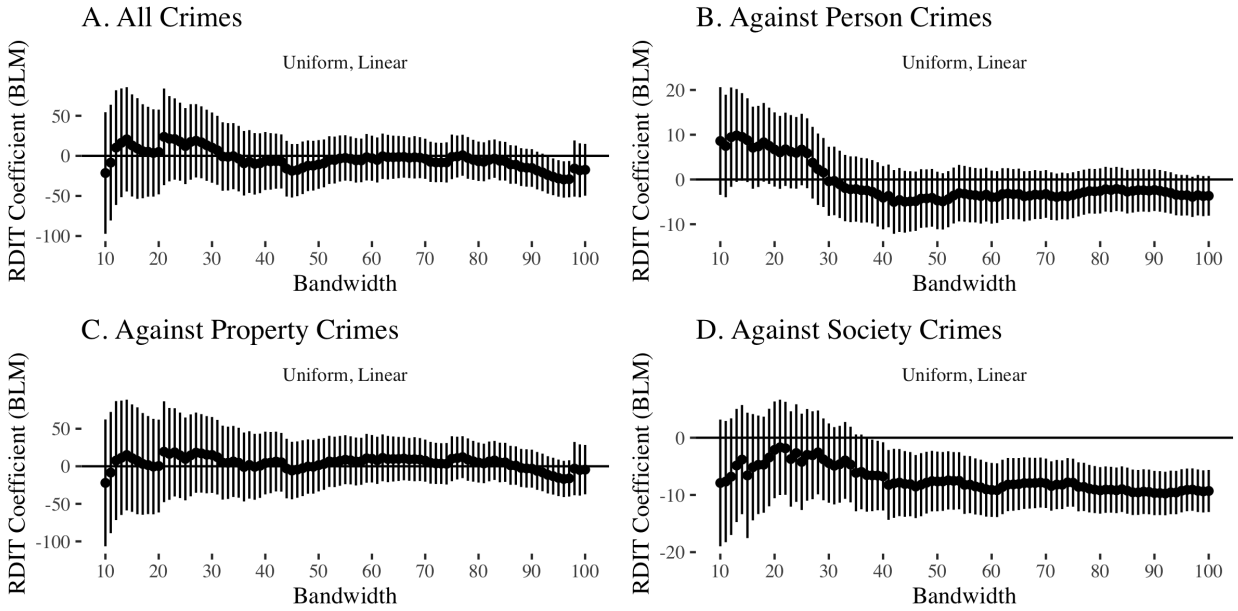
To alleviate these two separate concerns, we re-estimate our findings in three different ways. First, we re-estimate RDiT coefficients with a 50-day bandwidth censoring 1-317 days

Figure 2: Coefficients characterizing the RDiT effect of BLM protests on policing quality, varying bandwidths



Note: Uniform kernel, linear polynomial, bandwidths ranging from 10-100

Figure 3: Coefficients characterizing the RDiT effect of BLM protests on crime, varying bandwidths



Note: Uniform kernel, linear polynomial, bandwidths ranging from 10-100

Table 3: Coefficients characterizing RDiT effect of BLM protests on crime

Outcome	Polynomial	Bandwidth	RDiT Coef. (BLM)	SE	p-value
All Crimes	Linear	25	12.16	24.19	0.62
All Crimes	Linear	50	-10.73	15.50	0.49
All Crimes	Quadratic	25	12.71	34.41	0.71
All Crimes	Quadratic	50	-1.35	23.39	0.95
Property Crimes	Linear	25	9.81	28.14	0.73
Property Crimes	Linear	50	1.53	17.86	0.93
Property Crimes	Quadratic	25	8.28	38.40	0.83
Property Crimes	Quadratic	50	3.93	26.95	0.88
Against Society Crimes	Linear	25	-4.21	4.29	0.33
Against Society Crimes	Linear	50	-7.64	2.66	0.00
Against Society Crimes	Quadratic	25	-2.86	5.30	0.59
Against Society Crimes	Quadratic	50	-5.55	3.71	0.13
Against Person Crimes	Linear	25	6.68	4.08	0.10
Against Person Crimes	Linear	50	-4.69	3.28	0.15
Against Person Crimes	Quadratic	25	7.30	4.95	0.14
Against Person Crimes	Quadratic	50	0.23	4.06	0.95

Note: Robust SEs displayed. All estimates use uniform kernel.

from the post-*BLM protest* side of the temporal cutoff. For example, if we censor 20 days post-*BLM protest* from the data (May 29, 2020 to June 17, 2020), then we are estimating the discontinuous effect of the BLM protest using data before May 29, 2020 and after June 17, 2020.[10] In effect, this re-estimation strategy allows us to derive long-term RDiT *BLM protest* coefficients. If, for instance, terry stops returned to their pre-treatment daily average, censored RDiT coefficients would trend toward 0. Second, we use a difference-in-differences (DiD) approach comparing the difference in the weekly average of the relevant outcomes after the week of the *BLM protest* relative to before the week of the *BLM protest* in 2020. Then, we take the difference derived from 2020 and compare it to prior years (e.g. 2010-2019 for *calls* and *response times*; 2016-2019 for *stops*, *rate ratios*, *arrest rates*, and *hit rates*; 2008-2019 for *against person crimes*). The DiD approach allows us to assess an assumed constant effect of the *BLM protest* on respective outcomes that may not present itself in the RDiT approach. Third, we generate a monthly time series of relevant outcomes and assess if the post-*BLM protest* period (after June 2020) is statistically distinct in terms of outcome levels after adjusting for year fixed effects, a monthly trend, and an indicator for the onset of the Washington State shutdown (after March 2020). All of these designs pose trade-offs. However, we want to iterate that we prefer the RDiT approach since it reduces the possibility *BLM protest* coefficient estimates are not biased by secular unobserved time trends other

[10]We censor up to 317 days since there are still 50 days until the end of the SPD data (April 2021) for estimating a 50-day bandwidth *BLM protest* RDiT coefficient.

than the protest itself (e.g. byproducts of an unfolding pandemic, the 2020 election).

We find evidence the *BLM protests* durably reduced discretionary policing in the long-run. RDiT estimates censoring 1-317 days post-*BLM protest* are consistently negative and significant for both the *terry stop* and *officer call* outcomes (Figures D3, D4). Both DiD and monthly time series estimates also suggest the *BLM protests* durably reduced *stops* and *officer calls* (Tables E1, E3, E2, E4). These findings suggest, net of the mechanical effects of the protests themselves (e.g. fewer people outside, fewer cops conducting routine policing), the *BLM protests* motivated a long-term reduction in discretionary policing throughout Seattle. Moreover, *against person crime* does not increase in the long-term. Censored RDiT estimates are consistently near 0 and statistically insignificant (Figure D9). DiD and monthly time series estimates also suggest the *BLM protests* had no long-term effect on *against person crimes* (Tables E11, E12).

Evidence on the long-term consequences of the *BLM protests* on policing quality is mixed. Censored RDiT estimates suggest the increase in response times was durable at least 100 days after the onset of the *BLM protests* (September 6, 2020), well after the large protests in June and July. However, after 100 days, the RDiT coefficients trend toward 0 (Figure D5. DD estimates suggest the BLM protest did not change response times (Table E5), but monthly time series estimates show monthly response time means increased by 18 minutes (Table E6). Censored RDiT estimates demonstrate the *BLM protests* did not durably change *hit* or *arrest rates* (Figures D6, D7). However, DiD and monthly time series analyses suggest the *BLM protests* durably increased *hit rates* (Figures E7, E8).[11] The increase in hit rates identified by the DiD and monthly time series approach appears short-term, since the censored RDiT specifications suggest the *BLM protest* increased the hit rate in some specifications, but the positive coefficient becomes statistically 0 after censoring roughly 30 post-treatment days. However, consistent with the main results, censored RDiT, DiD, and monthly time series estimates show the *BLM protests* did not durably change the Black/white *rate ratio* (Figure D8, Tables E9, E10). In summary, consistent with the main results, the *BLM protests* did not durably change policing quality, with the exception of increases in response times, which were durably increased for at least 100 days after the *BLM protest.*

An alternative explanation to our findings is that depolicing is a function of reduced civilian demand for police services. RDiT estimates suggest the *BLM protests* not only discontinuously reduced discretionary policing, but also civilian-initiated 911 calls. However, we contend our results are not entirely driven by reductions in civilian demand for police services. We generate two outcome measures and assess the effect of the *BLM protests* on

[11] We do not implement the DiD or monthly time series approach with the *arrest rate* outcome since SPD did not collect arrest information prior to May 2019.

them. One measure is the proportion of calls that are officer calls (out of officer and civilian calls). The other is the ratio of officer calls to civilian calls. If officer calls are dropping at a level net of the drop in civilian calls after the BLM protest, we should expect the proportion of officer calls and ratio of officer calls to civilian calls to decrease. We find that this is the case across multiple RDiT specifications (Figures F16, F17).

Conclusion

The 2020 BLM protests in Seattle did cause depolicing, both as measured by terry stops and officer-initiated 911 calls. Fewer terry stops and officer-initiated 911 calls were made after the onset of the 2020 BLM protests. This is consistent with a story of depolicing as a response to protests centered specifically around dissatisfaction with the behavior of the police.

Our results characterize this depolicing as primarily anti-social. In the wake of the 2020 protests neither arrest nor hit rates increased significantly, indicating that the decrease in policing activities was not targeted to cutting out unnecessary police-citizen contact. Response times to citizen-initiated 911 calls discontinuously increased at the onset of the BLM protests, however, these effects are relatively short lived and response times quickly return to pre-protest levels, suggesting that this may be an effect of police overstretch while the protests were ongoing. Crucially, the 2020 BLM protests are not connected to any change in racial disparities in police stops of Black and white citizens. Altogether, these results are consistent with a story of anti-social depolicing, indicating that the Seattle Police Department changed their practices in response to protests, but not in a way that was responsive to the demands of protesters.

Finally, this depolicing had no effect on levels of violent against person crime, which is consistent with the existing literature. This is the expected outcome, as violent crime is the type of crime that is the least responsive to the kind of tactics that police have a high degree of discretion over and can change in the short term. There is also no change to crime against property or crime rates overall, though there were discontinuous decreases in crimes against society. However, these are likely endogenous to policing patterns, as they are the most sensitive to "discovery" by police (e.g. drug busts). These findings, especially considering violent crime, run counter to the popular narrative that emerged in the wake of the 2020 BLM protests, claiming that depolicing had not only occurred on a large scale but had caused a spike in violent crime across the country. We find that this did not happen in Seattle.

Our conclusions are three-fold. First, we conclude that public protest is a viable path

for citizens fighting to achieve a decrease in police-citizen interactions. 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 terry stops would likely be viewed as good news by the citizens calling for reforms in Seattle in the summer of 2020, insofar as depolicing reduced contact with civilians. Second, while pundits and some academics fret over the public safety consequences of less policing, our analysis suggests they need not worry. In Seattle, increasing crime did not accompany declining police activity. Finally, 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 fight crime. Indeed, the rote strategies officers are trained to engage under the framework of preemption are manipulable in terms of quantity but unresponsive in terms of quality, highlighting the need to rethink police in American cities.

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Supplemental Information

A Efficiency Is Not a Function of Identifying More Criminal Activity

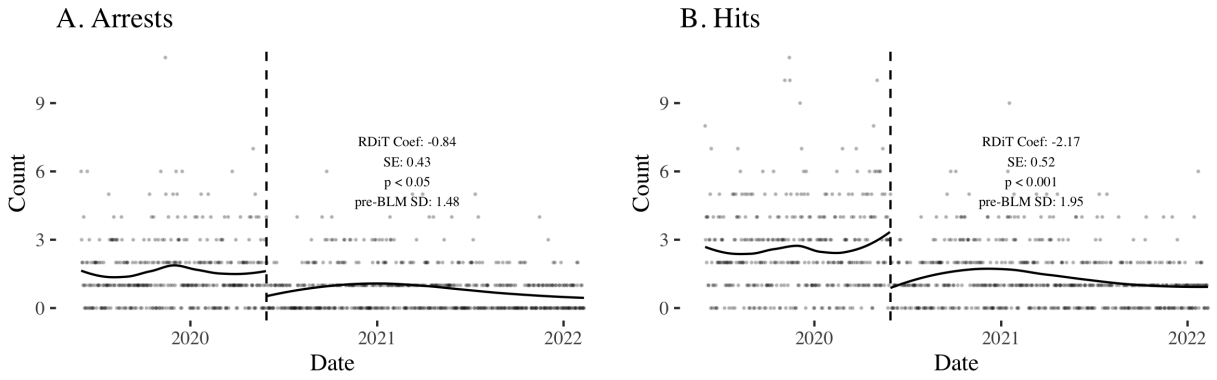
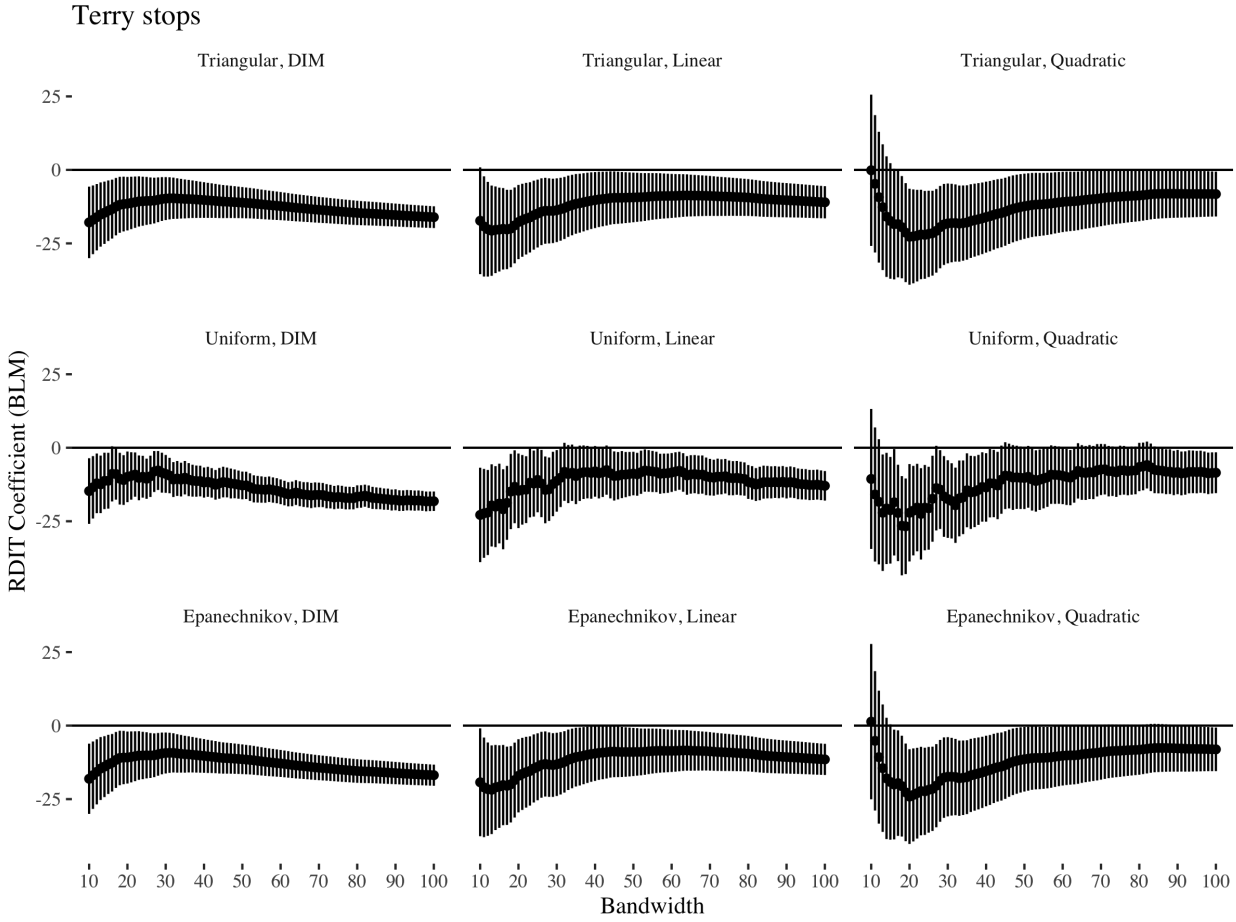
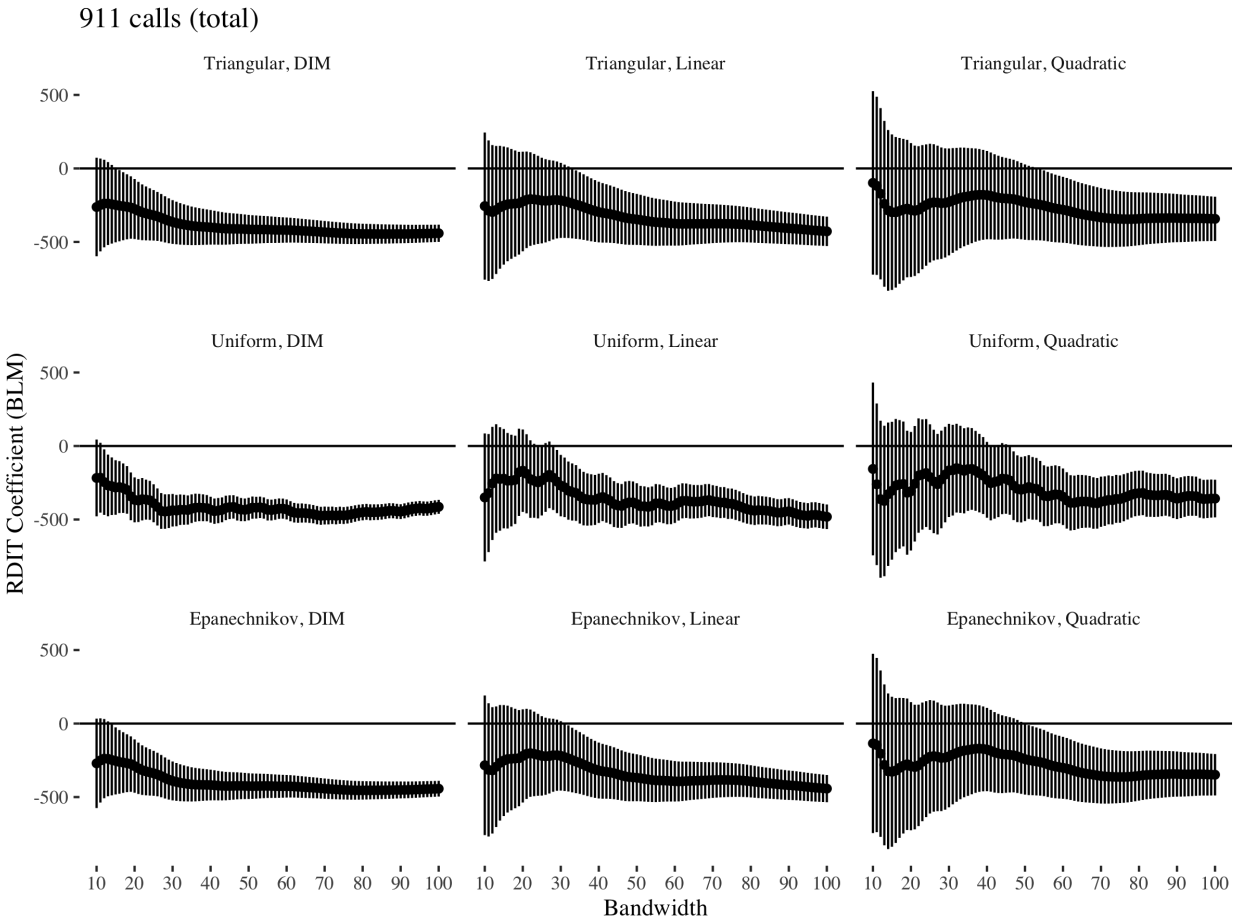


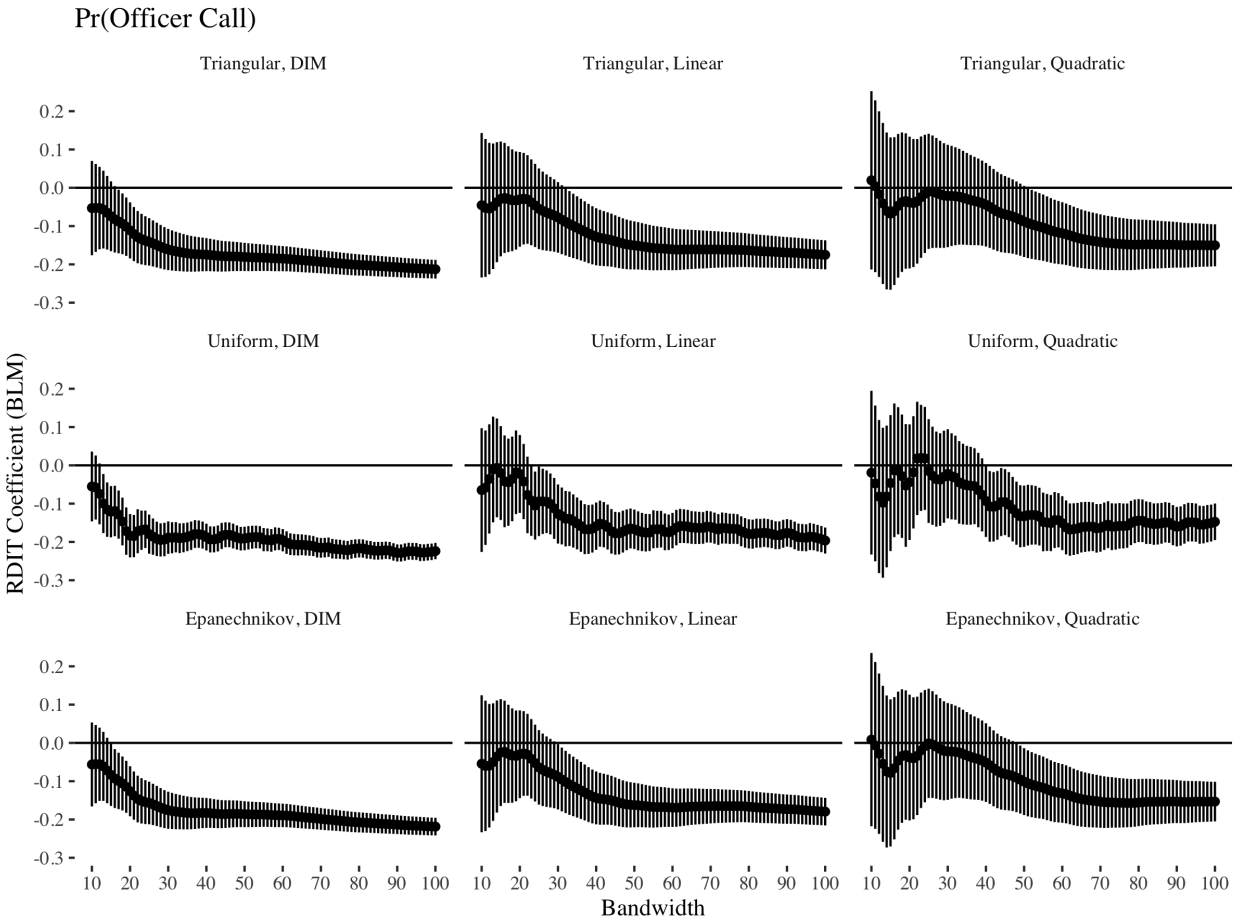
Figure A1: Terry Stop Arrest (Panel A) and Hit Counts (Panel B, y-axis) Over Time (x-axis). Loess lines fit on each side of the *BLM protest* discontinuity. Dashed vertical line denotes *BLM protest* onset. Annotations denote RDiT coefficients using a running variable to the 1st degree.

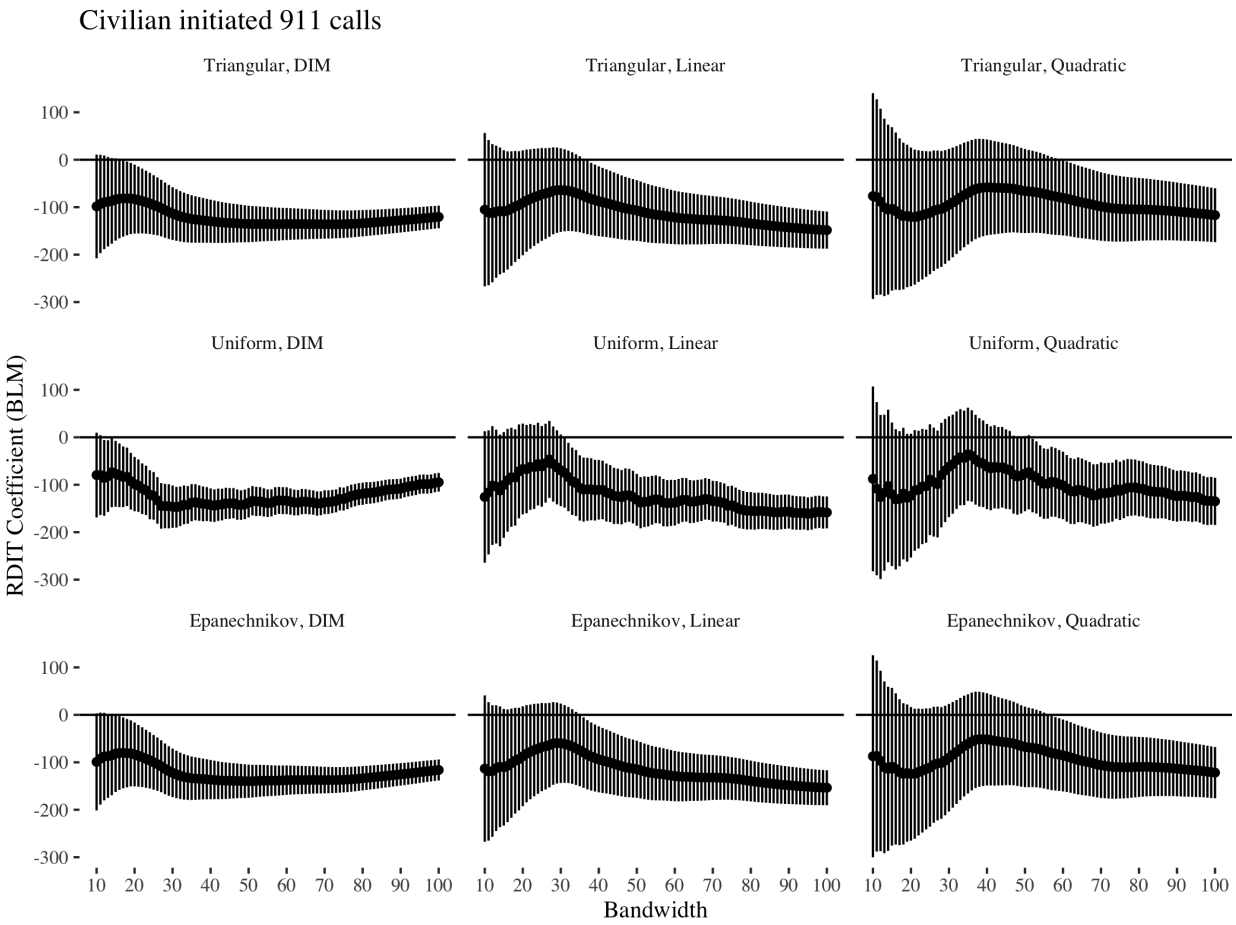
B Alternative Specifications

B.1 Policing

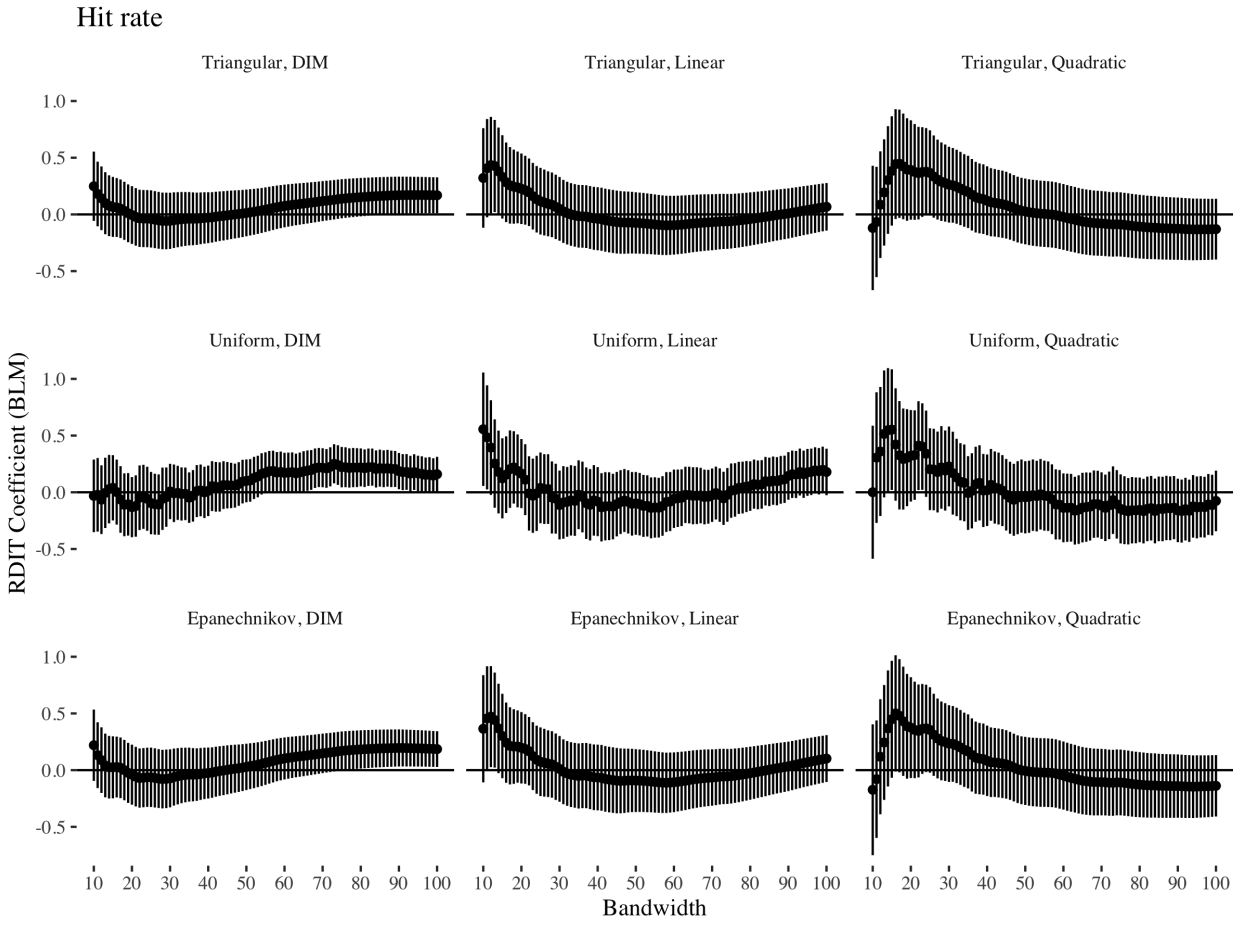


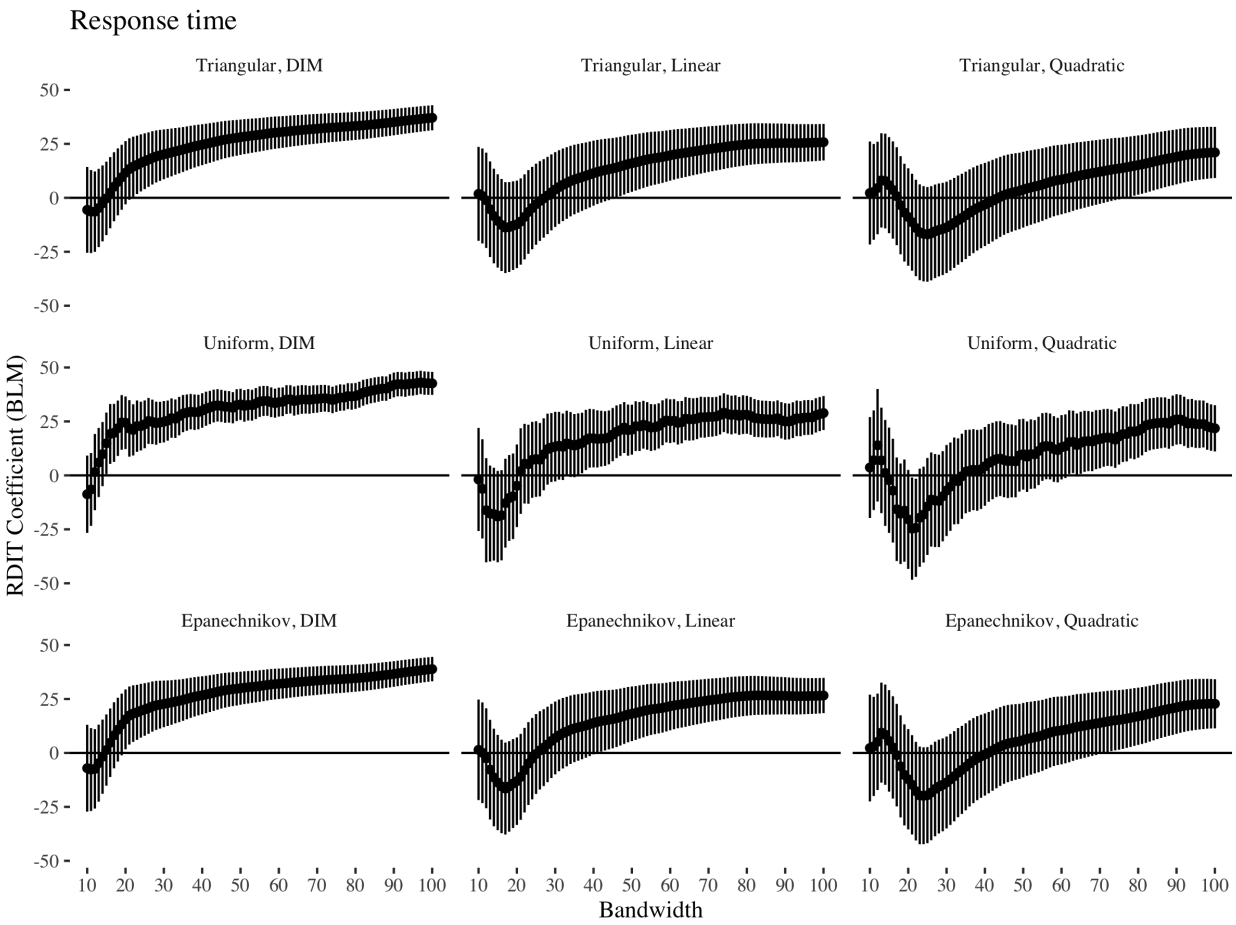

911 calls (total)
Triangular, DIM
Triangular, Linear
Triangular, Quadratic
Uniform, DIM
Uniform, Linear
Uniform, Quadratic
Epanechnikov, DIM
Epanechnikov, Linear
Epanechnikov, Quadratic
RDIT Coefficient (BLM)
500
0
-500
10 20 30 40 50 60 70 80 90 100
Bandwidth


Pr(Officer Call)
Triangular, DIM
Triangular, Linear
Triangular, Quadratic
Uniform, DIM
Uniform, Linear
Uniform, Quadratic
Epanechnikov, DIM
Epanechnikov, Linear
Epanechnikov, Quadratic
RDIT Coefficient (BLM)
0.2
0.1
0.0
-0.1
-0.2
-0.3
10 20 30 40 50 60 70 80 90 100
Bandwidth

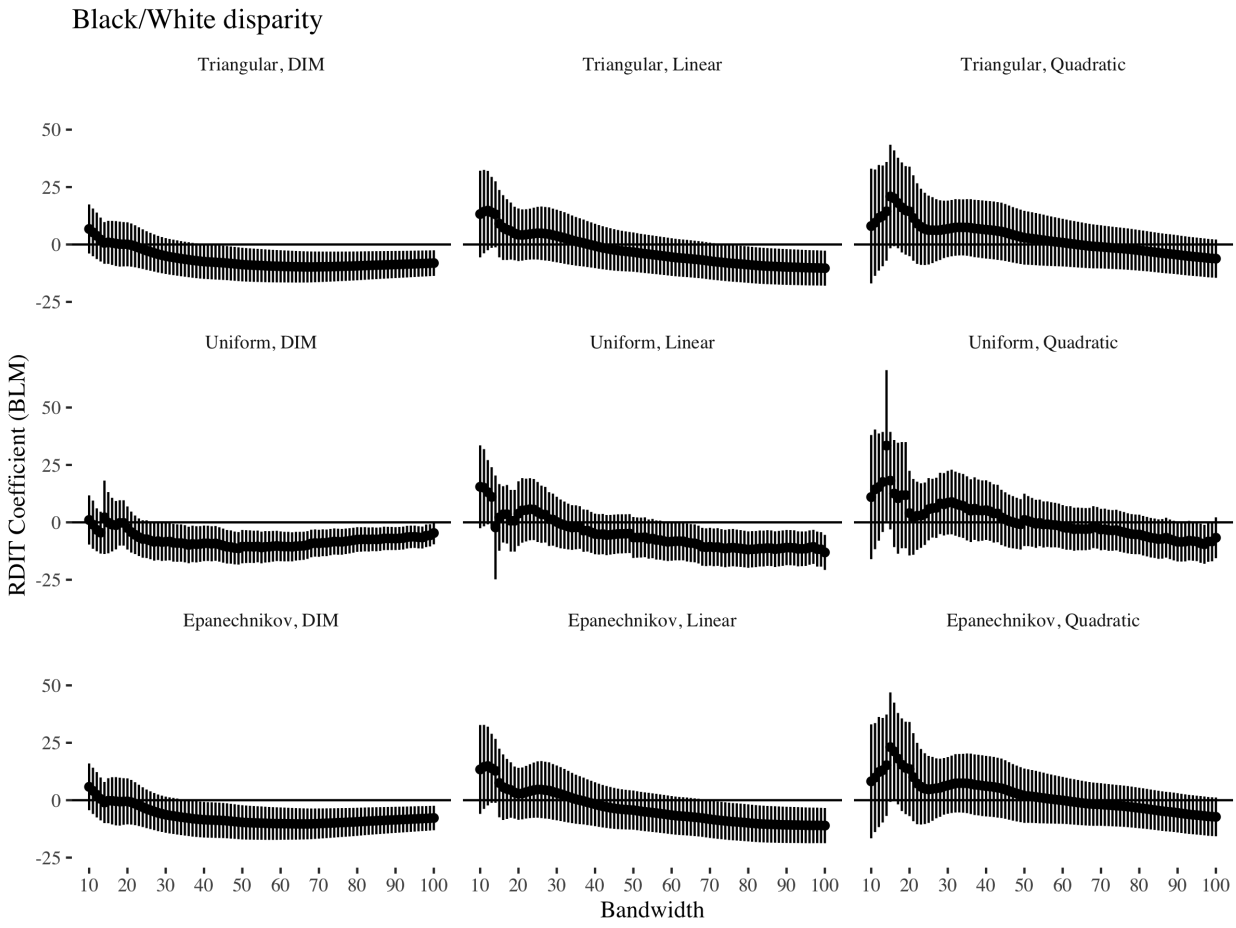

Civilian initiated 911 calls
Triangular, DIM
Triangular, Linear
Triangular, Quadratic
Uniform, DIM
Uniform, Linear
Uniform, Quadratic
Epanechnikov, DIM
Epanechnikov, Linear
Epanechnikov, Quadratic
RDIT Coefficient (BLM)
100
0
-100
-200
-300
10 20 30 40 50 60 70 80 90 100
Bandwidth

B.2 Quality

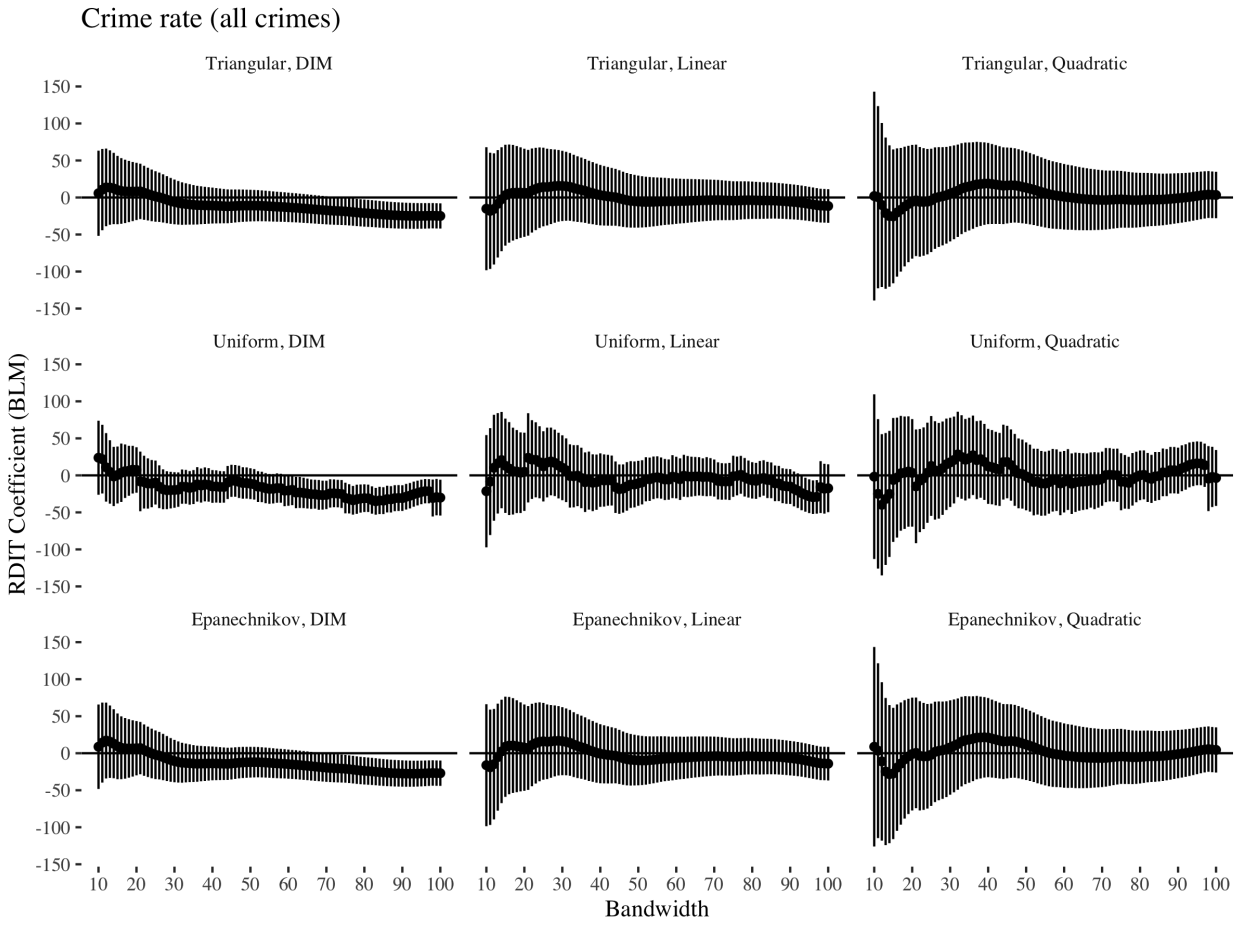

Hit rate
Triangular, DIM
Triangular, Linear
Triangular, Quadratic
Uniform, DIM
Uniform, Linear
Uniform, Quadratic
Epanechnikov, DIM
Epanechnikov, Linear
Epanechnikov, Quadratic
RDIT Coefficient (BLM)
1.0
0.5
0.0
-0.5
10 20 30 40 50 60 70 80 90 100
Bandwidth

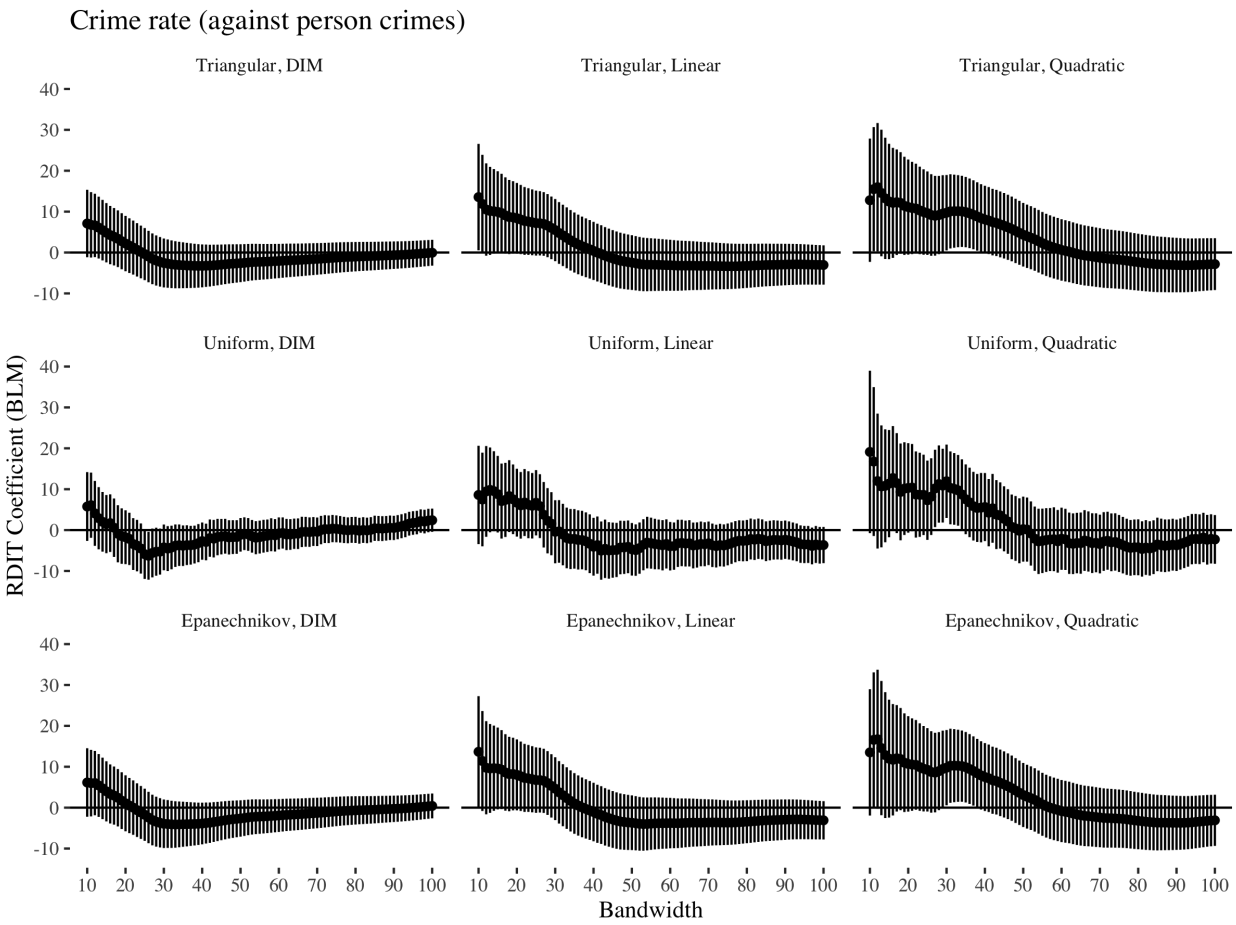

Response time
Triangular, DIM
Triangular, Linear
Triangular, Quadratic
Uniform, DIM
Uniform, Linear
Uniform, Quadratic
Epanechnikov, DIM
Epanechnikov, Linear
Epanechnikov, Quadratic
RDIT Coefficient (BLM)
50
25
0
-25
-50
10 20 30 40 50 60 70 80 90 100
Bandwidth

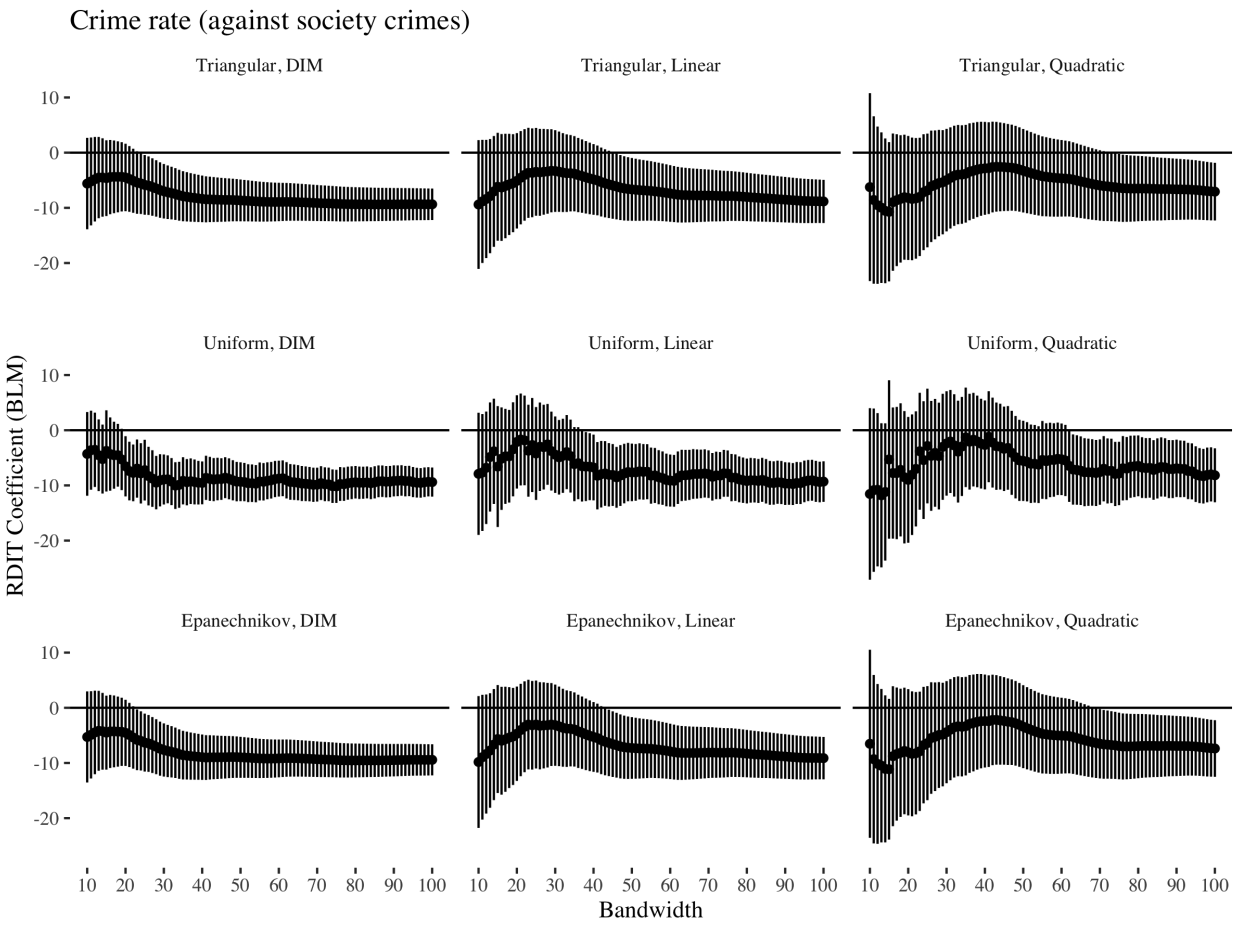
B.3 Racial Disparities

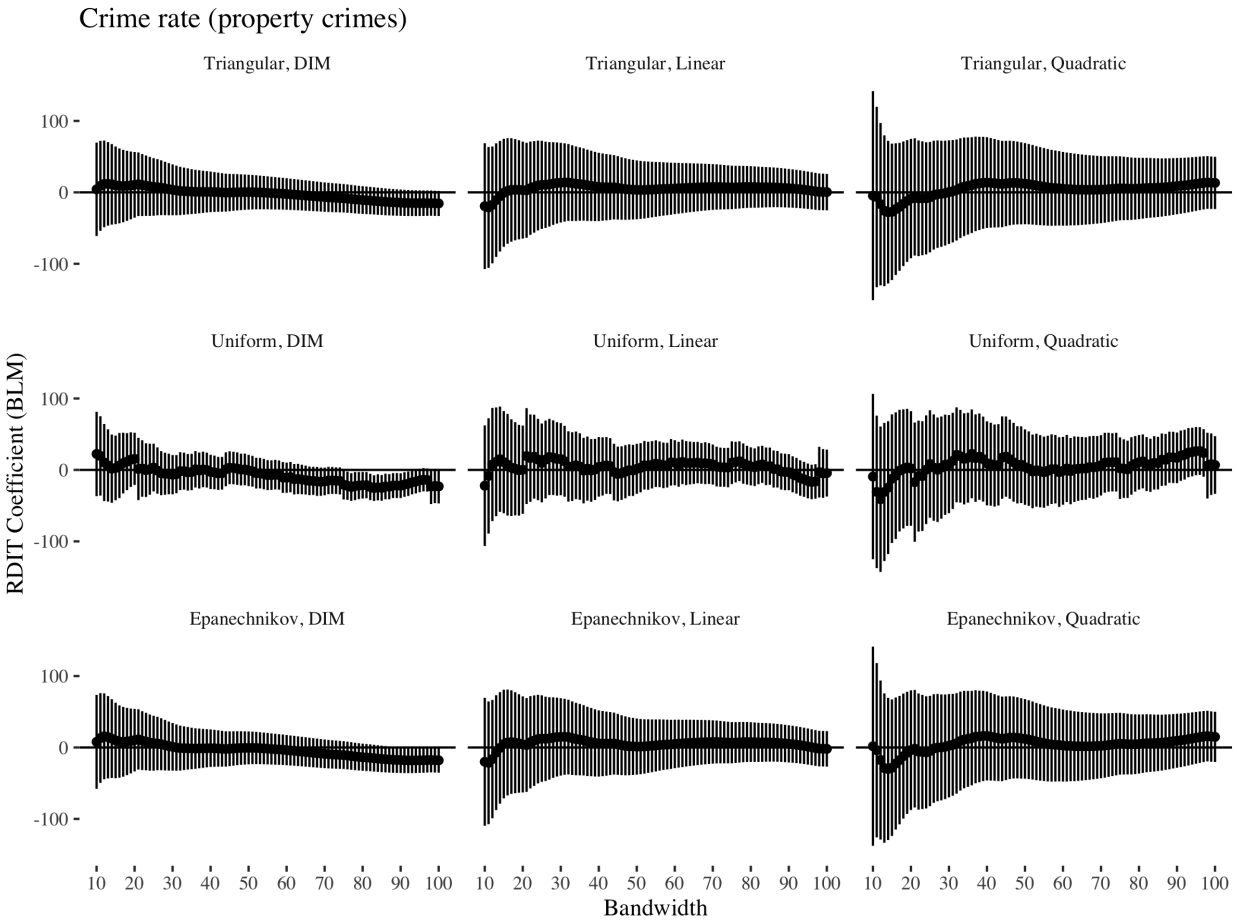


B.4 Crime


Crime rate (all crimes)
Triangular, DIM
Triangular, Linear
Triangular, Quadratic
Uniform, DIM
Uniform, Linear
Uniform, Quadratic
Epanechnikov, DIM
Epanechnikov, Linear
Epanechnikov, Quadratic
RDIT Coefficient (BLM)
150
100
50
0
-50
-100
-150
10 20 30 40 50 60 70 80 90 100
Bandwidth


Crime rate (against person crimes)
Triangular, DIM
Triangular, Linear
Triangular, Quadratic
Uniform, DIM
Uniform, Linear
Uniform, Quadratic
Epanechnikov, DIM
Epanechnikov, Linear
Epanechnikov, Quadratic
RDIT Coefficient (BLM)
40
30
20
10
0
-10
10 20 30 40 50 60 70 80 90 100
Bandwidth

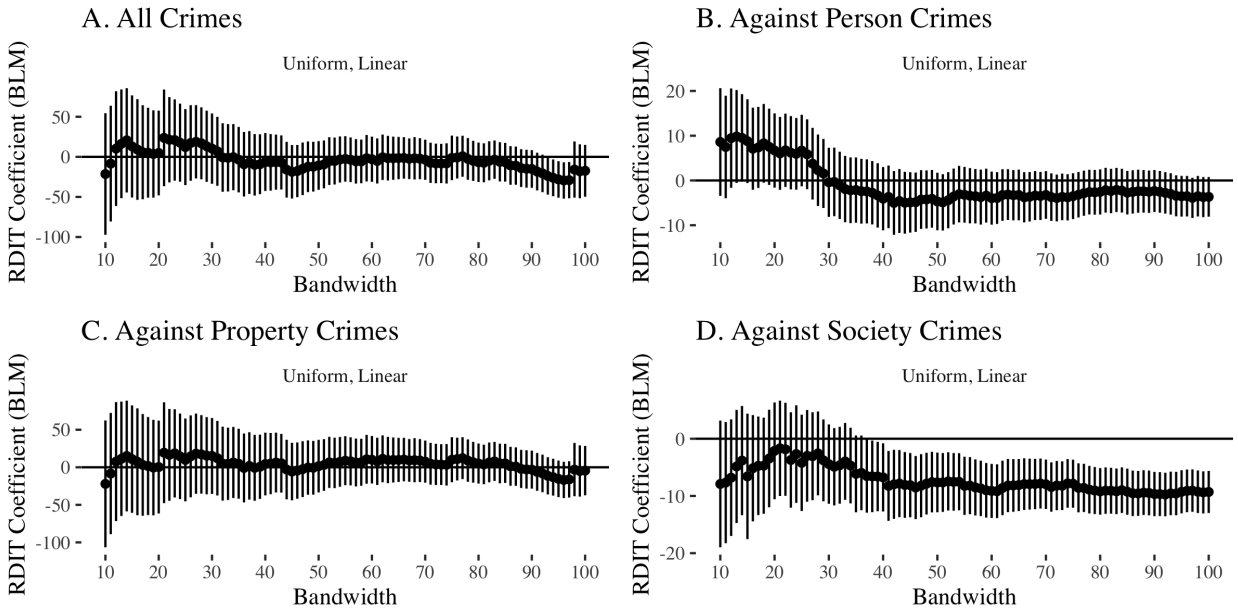

Crime rate (against society crimes)
Triangular, DIM
Triangular, Linear
Triangular, Quadratic
Uniform, DIM
Uniform, Linear
Uniform, Quadratic
Epanechnikov, DIM
Epanechnikov, Linear
Epanechnikov, Quadratic
RDIT Coefficient (BLM)
10
0
-10
-20
10 20 30 40 50 60 70 80 90 100
Bandwidth



C Descriptive Statistics

C.1 Discretionary Policing

Figure C2: Coefficients characterizing the RDiT effect of BLM protests on crime, varying bandwidths



Note: Uniform kernel, linear polynomial, bandwidths ranging from 10-100

D RDiT Effect Persistence

D.1 Discretionary Policing

D.1.1 Terry Stops

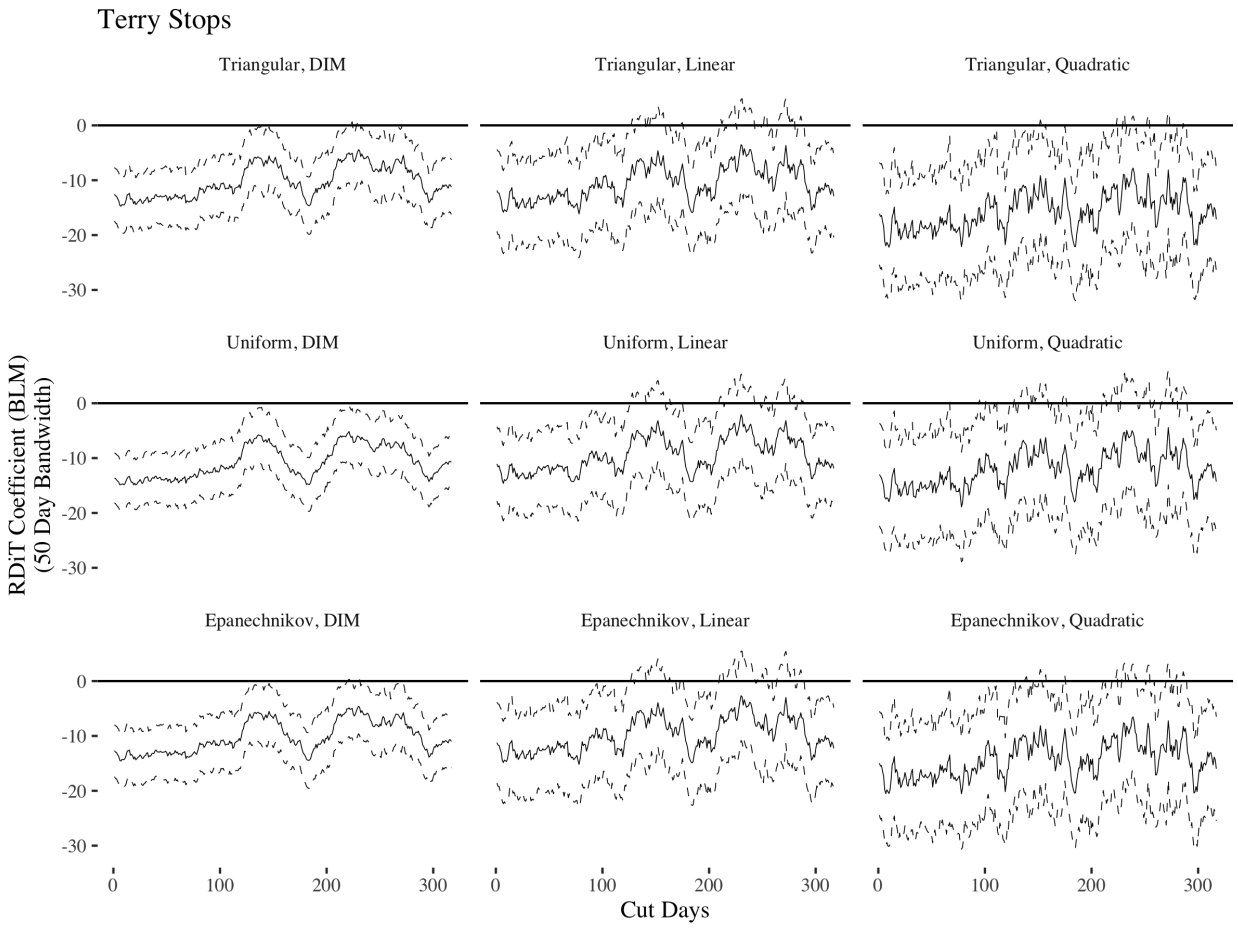


Figure D3: Right-Sided Donut Hole RDiT Estimates (Terry Stop Outcome). The x-axis is the number of days cut from the right side of the discontinuity (the post-*BLM protest* side). The y-axis is the 50-day bandwidth RDiT coefficient. Panels denote different kernel and running variable polynomial degree specifications. 95% CIs displayed derived from robust SEs.

D.1.2 Police-Initiated 911 Calls

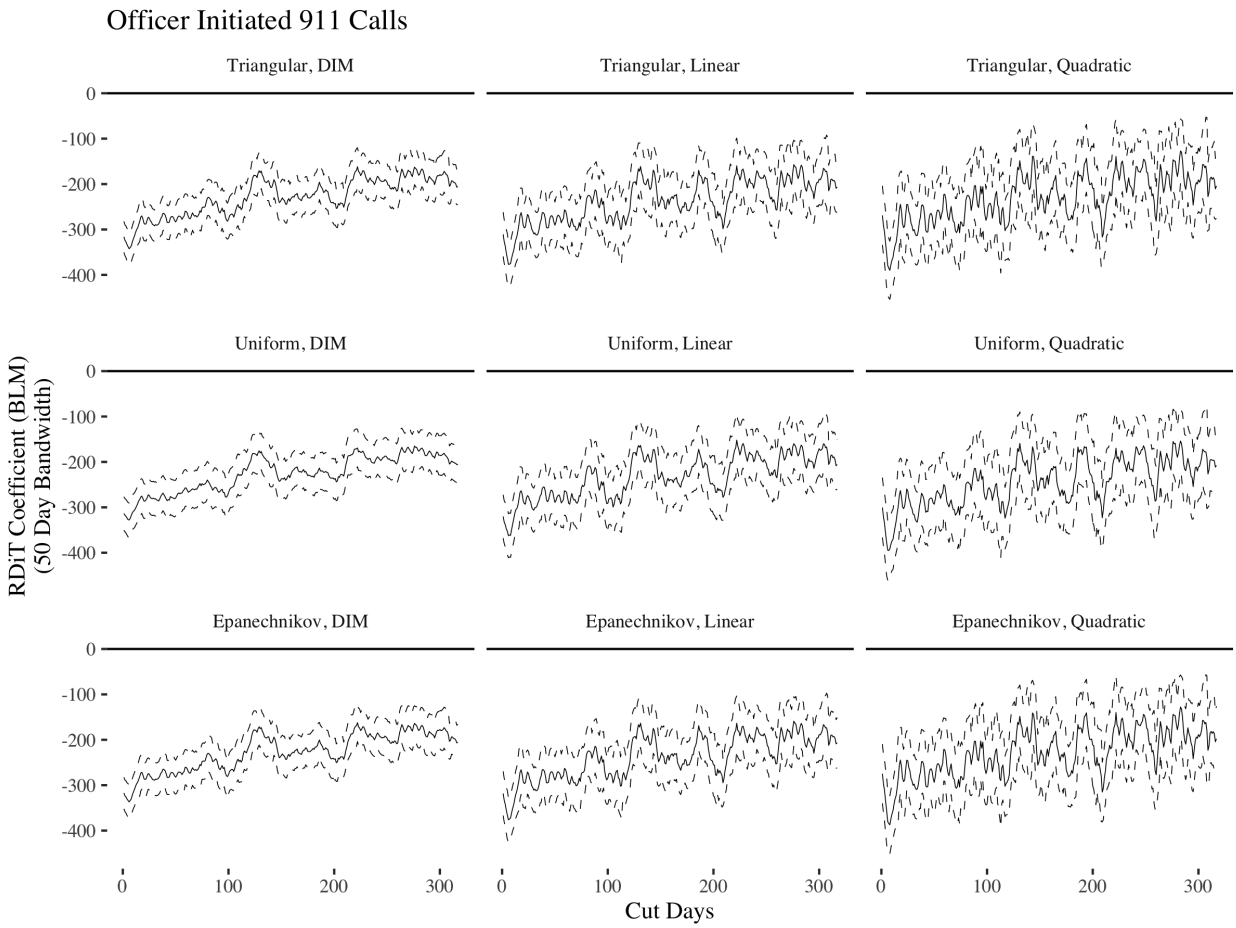


Figure D4: Right-Sided Donut Hole RDiT Estimates (Police-Initiated 911 Calls Outcome). The x-axis is the number of days cut from the right side of the discontinuity (the post-*BLM protest* side). The y-axis is the 50-day bandwidth RDiT coefficient. Panels denote different kernel and running variable polynomial degree specifications. 95% CIs displayed derived from robust SEs.

D.2 Quality

D.2.1 Response Times

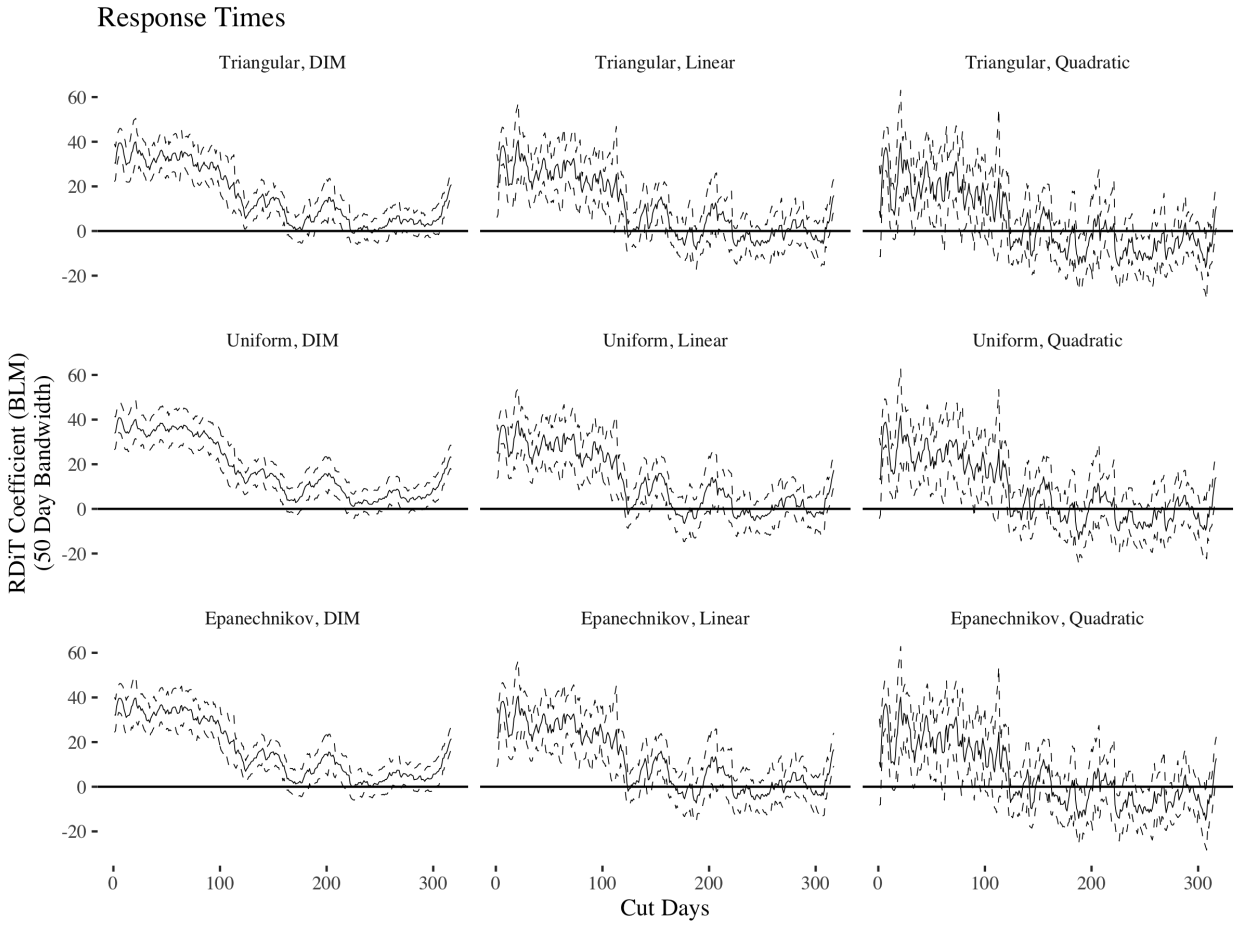


Figure D5: Right-Sided Donut Hole RDiT Estimates (Response Time Outcome). The x-axis is the number of days cut from the right side of the discontinuity (the post-*BLM protest* side). The y-axis is the 50-day bandwidth RDiT coefficient. Panels denote different kernel and running variable polynomial degree specifications. 95% CIs displayed derived from robust SEs.

D.2.2 Hit Rates

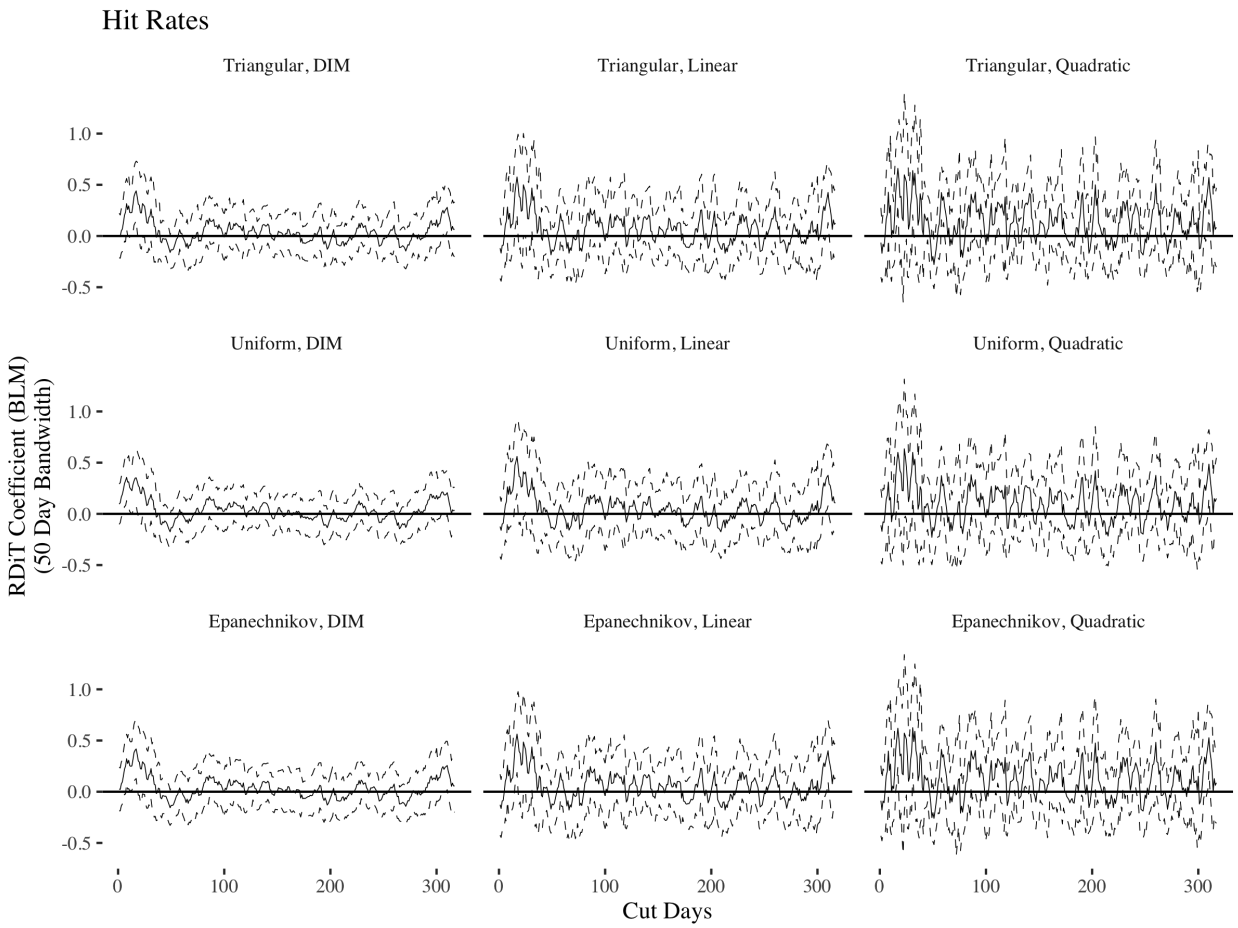


Figure D6: Right-Sided Donut Hole RDiT Estimates (Hit Rate Outcome). The x-axis is the number of days cut from the right side of the discontinuity (the post-*BLM protest* side). The y-axis is the 50-day bandwidth RDiT coefficient. Panels denote different kernel and running variable polynomial degree specifications. 95% CIs displayed derived from robust SEs.

D.2.3 Arrest Rates

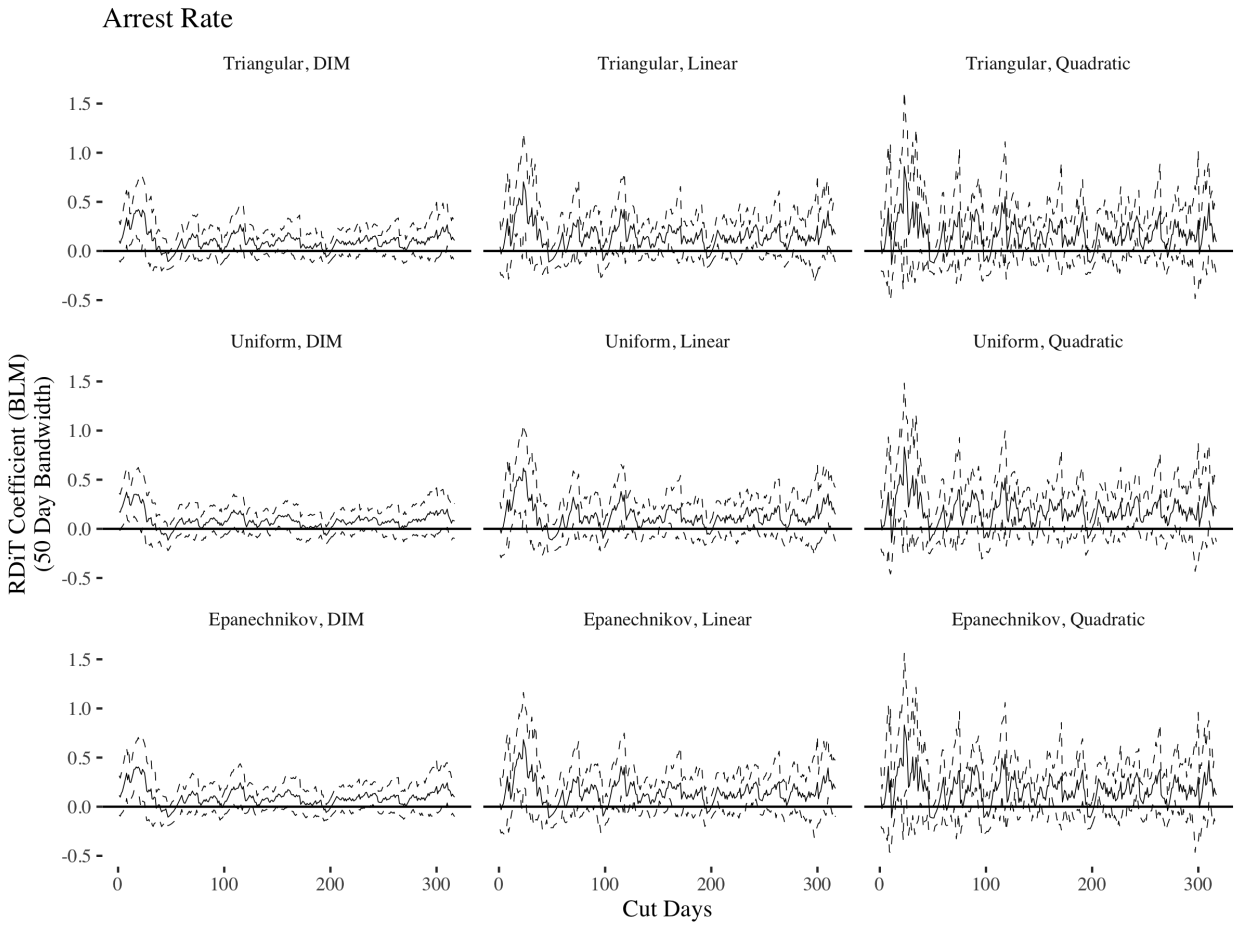


Figure D7: Right-Sided Donut Hole RDiT Estimates (Arrest Rate Outcome). The x-axis is the number of days cut from the right side of the discontinuity (the post-*BLM protest* side). The y-axis is the 50-day bandwidth RDiT coefficient. Panels denote different kernel and running variable polynomial degree specifications. 95% CIs displayed derived from robust SEs.

D.2.4 Rate Ratio

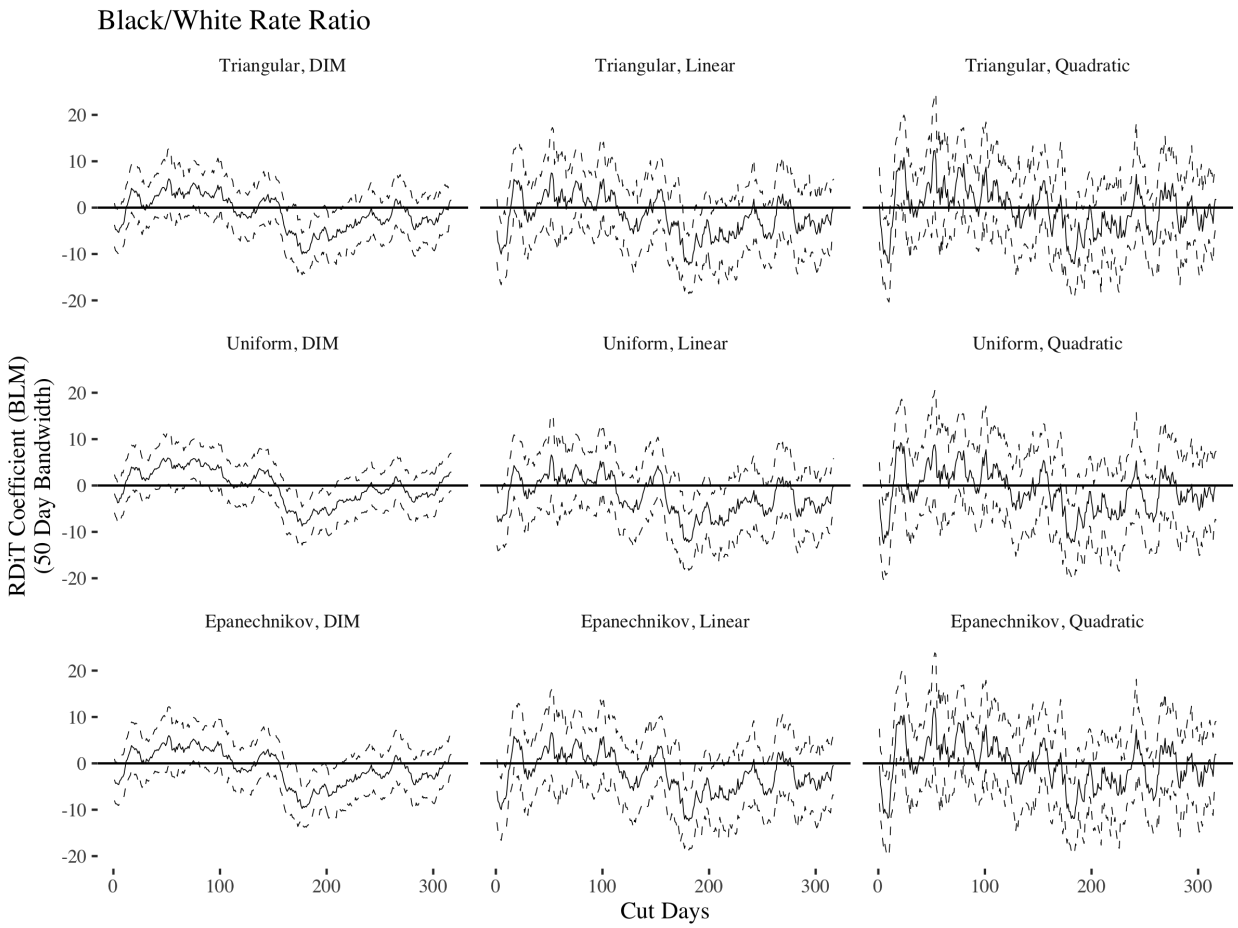


Figure D8: Right-Sided Donut Hole RDiT Estimates (Rate Ratio Outcome). The x-axis is the number of days cut from the right side of the discontinuity (the post-*BLM protest* side). The y-axis is the 50-day bandwidth RDiT coefficient. Panels denote different kernel and running variable polynomial degree specifications. 95% CIs displayed derived from robust SEs.

D.3 Against Person Crimes

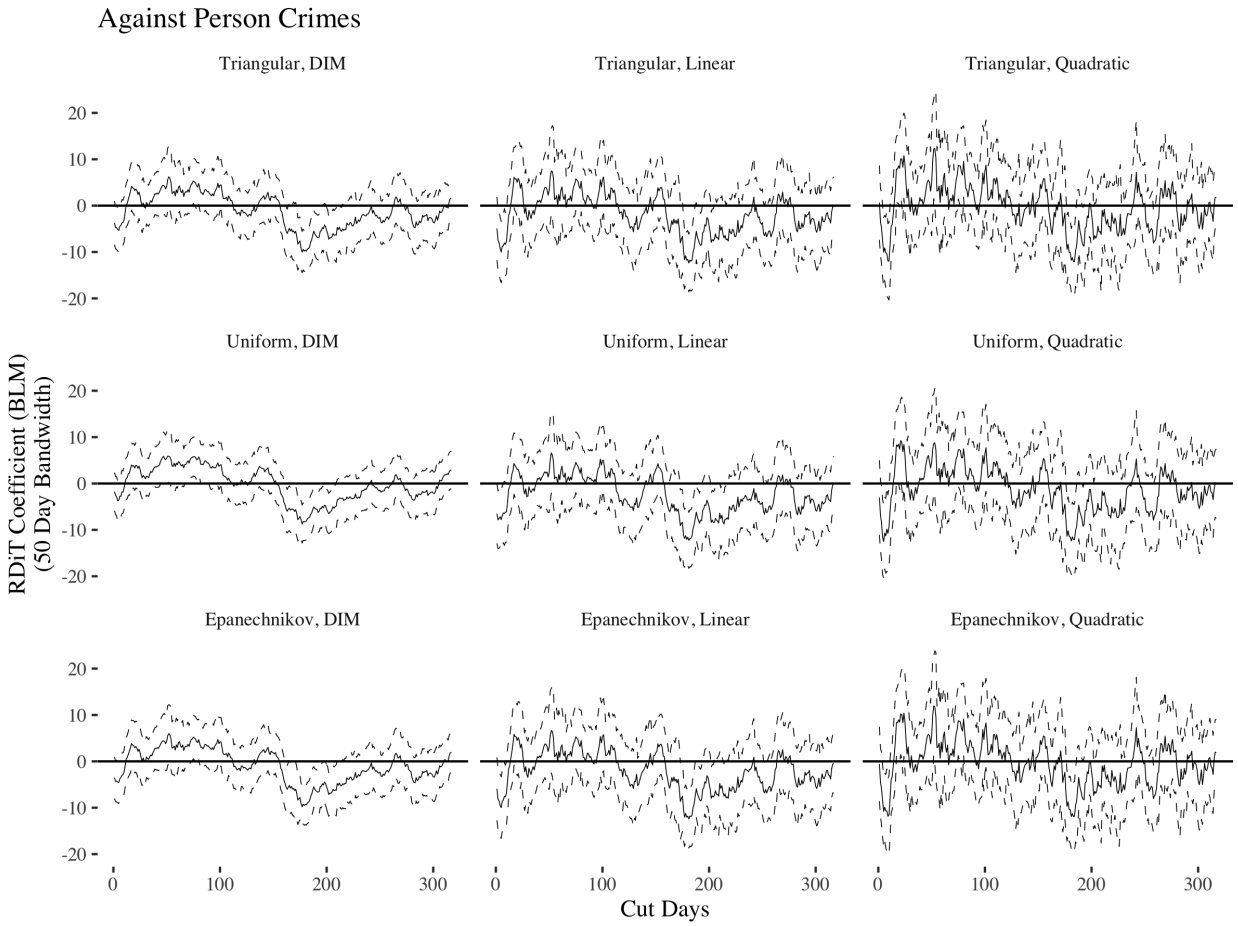


Figure D9: Right-Sided Donut Hole RDiT Estimates (Against Person Outcome). The x-axis is the number of days cut from the right side of the discontinuity (the post-*BLM protest* side). The y-axis is the 50-day bandwidth RDiT coefficient. Panels denote different kernel and running variable polynomial degree specifications. 95% CIs displayed derived from robust SEs.

E Long-Term Effects

E.1 Discretionary Policing

E.1.1 Terry Stops

Table E1: Difference-in-Differences Estimates Characterizing Effect of BLM Protest on Terry Stops

	Terry Stops	Log(Terry Stops + 1)
	(1)	(2)
2020 x BLM Protest	-26.69^{***}	-0.75^{***}
	(5.24)	(0.11)
2020	-18.37^{***}	-0.26^{***}
	(4.40)	(0.06)
BLM Protest	-12.49^{***}	-0.21^{***}
	(2.75)	(0.04)
R^2	0.44	0.51
N	265	265

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. HC2 robust SEs in parentheses

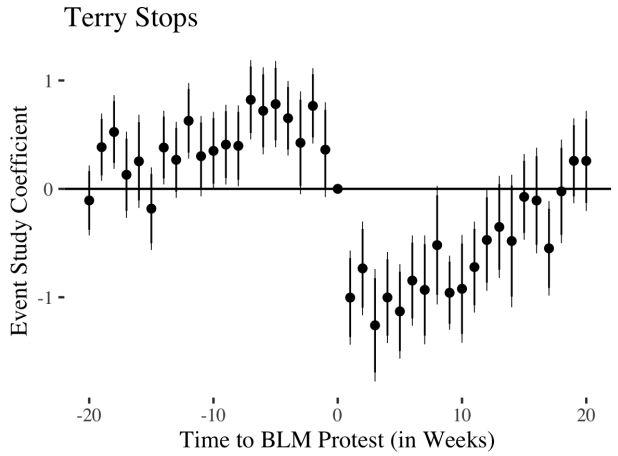


Figure E10: Event Study Characterizing Effect of *BLM Protest* on Terry Stops. The y-axis is the 2020 coefficient for the 20 weeks before and after the onset of the 2020 BLM protest (the 20-week indicators prior and after the *BLM protest* includes all weeks before and after). 95% CIs displayed from HC2 robust SEs.

Table E2: Monthly Time Series Analysis Assessing Effect of BLM Protest on Terry Stops

	Terry Stops		Log(Terry Stops + 1)	
	(1)	(2)	(3)	(4)
BLM Protest	-206.84^{***}	-176.34^{***}	-1.09^{***}	-0.99^{***}
	(28.14)	(37.64)	(0.12)	(0.20)
R^2	0.71	0.80	0.78	0.83
N	72	72	72	72
Year FE	N	Y	N	Y
Month Trend	N	Y	N	Y

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. HC2 robust SEs in parentheses

E.1.2 Police-Initiated 911 Calls

Table E3: Difference-in-Differences Estimates Characterizing Effect of BLM Protest on Police-Initiated 911 Calls

	Police Calls	Log(Police Calls + 1)
	(1)	(2)
2020 x BLM Protest	-1935.12^{***}	-0.87^{***}
	(102.35)	(0.06)
2020	147.09	0.05^{*}
	(75.02)	(0.02)
BLM Protest	-95.95^{*}	-0.06^{**}
	(47.61)	(0.02)
R^2	0.33	0.30
N	615	615

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. HC2 robust SEs in parentheses

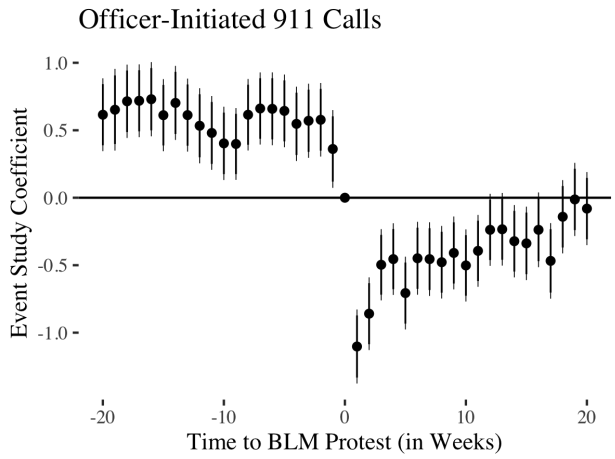


Figure E11: Event Study Characterizing Effect of *BLM Protest* on Officer-Initiated 911 calls. The y-axis is the 2020 coefficient for the 20 weeks before and after the onset of the 2020 BLM protest (the 20-week indicators prior and after the *BLM protest* includes all weeks before and after). 95% CIs displayed from HC2 robust SEs.

Table E4: Monthly Time Series Analysis Assessing Effect of BLM Protest on Officer-Initiated 911 Calls

	Officer Calls		**Log(Officer Calls + 1)**	
	(1)	(2)	(3)	(4)
BLM Protest	-7859.74^{***}	-8317.97^{***}	-1.00^{***}	-0.85^{***}
	(855.43)	(716.20)	(0.25)	(0.09)
R^2	0.60	0.78	0.49	0.56
N	145	145	145	145
Shutdown	Y	Y	Y	Y
Year FE	N	Y	N	Y
Month Trend	N	Y	N	Y

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. HC2 robust SEs in parentheses

E.2 Quality

E.2.1 Response Time

Table E5: Difference-in-Differences Estimates Characterizing Effect of BLM Protest on Response Time

	Response Time
	(1)
2020 x BLM Protest	2.64
	(3.94)
2020	−0.14
	(2.83)
BLM Protest	11.04***
	(1.38)
R^2	0.34
N	689

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. HC2 robust SEs in parentheses

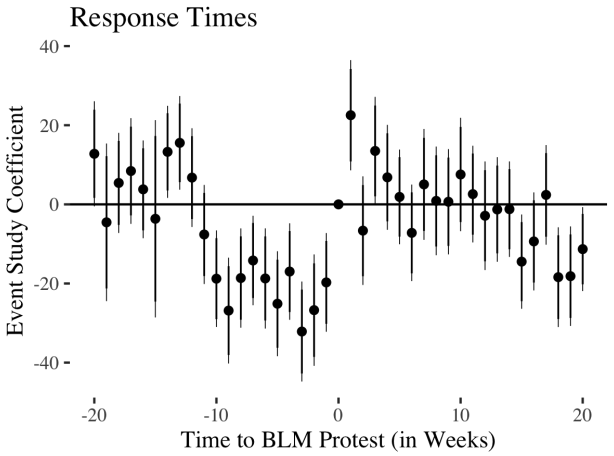


Figure E12: Event Study Characterizing Effect of *BLM Protest* on Response Times. The y-axis is the 2020 coefficient for the 20 weeks before and after the onset of the 2020 BLM protest (the 20-week indicators prior and after the *BLM protest* includes all weeks before and after). 95% CIs displayed from HC2 robust SEs.

Table E6: Monthly Time Series Analysis Assessing Effect of BLM Protest on Response Times

	Response Time	
	(1)	(2)
BLM Protest	14.84**	17.79**
	(4.40)	(6.59)
R^2	0.71	0.80
N	72	72
Shutdown	Y	Y
Year FE	N	Y
Month Trend	N	Y

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. HC2 robust SEs in parentheses

E.2.2 Hit Rate

Table E7: Difference-in-Differences Estimates Characterizing Effect of BLM Protest on Hit Rates

	Response Time
	(1)
2020 x BLM Protest	0.07*
	(0.03)
2020	−0.07***
	(0.02)
BLM Protest	0.01
	(0.01)
R^2	0.05
N	265

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. HC2 robust SEs in parentheses

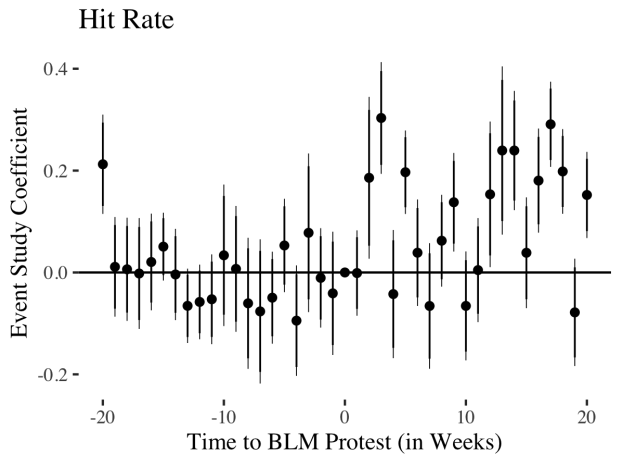


Figure E13: Event Study Characterizing Effect of *BLM Protest* on Hit Rates. The y-axis is the 2020 coefficient for the 20 weeks before and after the onset of the 2020 BLM protest (the 20-week indicators prior and after the *BLM protest* includes all weeks before and after). 95% CIs displayed from HC2 robust SEs.

Table E8: Monthly Time Series Analysis Assessing Effect of BLM Protest on Hit Rates

	Hit Rate	
	(1)	(2)
BLM Protest	0.07**	0.10**
	(0.02)	(0.03)
R^2	0.10	0.28
N	72	72
Shutdown	Y	Y
Year FE	N	Y
Month Trend	N	Y

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. HC2 robust SEs in parentheses

E.2.3 Rate Ratio

Table E9: Difference-in-Differences Estimates Characterizing Effect of BLM Protest on Black/White Rate Ratios

	Rate Ratio
	(1)
2020 x BLM Protest	−1.06
	(1.14)
2020	1.52***
	(0.44)
BLM Protest	0.44
	(0.25)
R^2	0.02
N	265

Note: ***$p < 0.001$, **$p < 0.01$, *$p < 0.05$. HC2 robust SEs in parentheses

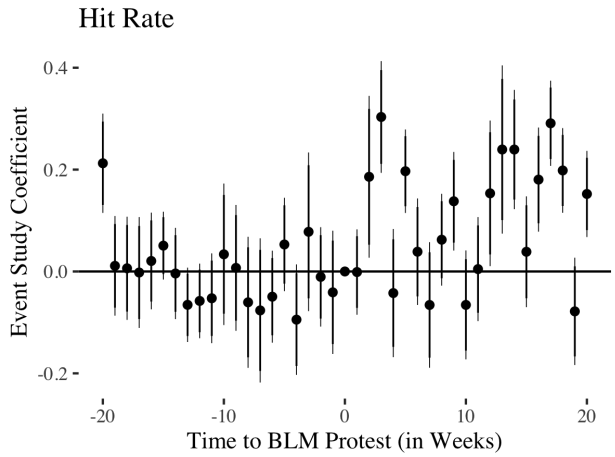


Figure E14: Event Study Characterizing Effect of *BLM Protest* on Rate Ratio. The y-axis is the 2020 coefficient for the 20 weeks before and after the onset of the 2020 BLM protest (the 20-week indicators prior and after the *BLM protest* includes all weeks before and after). 95% CIs displayed from HC2 robust SEs.

Table E10: Monthly Time Series Analysis Assessing Effect of BLM Protest on Black/White Rate Ratio

	Rate Ratio	
	(1)	(2)
BLM Protest	−0.51	−0.47
	(0.63)	(1.17)
R^2	0.10	0.28
N	72	72
Shutdown	Y	Y
Year FE	N	Y
Month Trend	N	Y

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. HC2 robust SEs in parentheses

E.3 Against Person Crimes

Table E11: Difference-in-Differences Estimates Characterizing Effect of BLM Protest on Against Person Crime

	Against Person Crimes	Log(Against Person Crimes + 1)
	(1)	(2)
2020 x BLM Protest	−10.08	−0.05
	(8.46)	(0.05)
2020	26.24***	0.19***
	(4.01)	(0.03)
BLM Protest	7.85**	0.16***
	(2.65)	(0.02)
R^2	0.34	0.03
N	689	689

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. HC2 robust SEs in parentheses

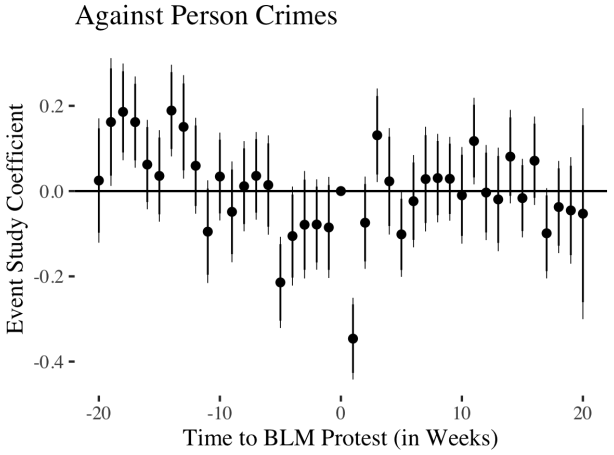


Figure E15: Event Study Characterizing Effect of *BLM Protest* on Against Person Crimes. The y-axis is the 2020 coefficient for the 20 weeks before and after the onset of the 2020 BLM protest (the 20-week indicators prior and after the *BLM protest* includes all weeks before and after). 95% CIs displayed from HC2 robust SEs.

Table E12: Monthly Time Series Analysis Assessing Effect of BLM Protest on Against Person Crimes

	Against Person Crimes		**Log(Against Person Crimes + 1)**	
	(1)	(2)	(3)	(4)
BLM Protest	68.58*	5.44	0.07	−0.00
	(33.82)	(49.64)	(0.04)	(0.06)
R^2	0.11	0.63	0.11	0.62
N	168	168	168	168
Shutdown	Y	Y	Y	Y
Year FE	N	Y	N	Y
Month Trend	N	Y	N	Y

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. HC2 robust SEs in parentheses

F Ruling Out Civilian Behavior

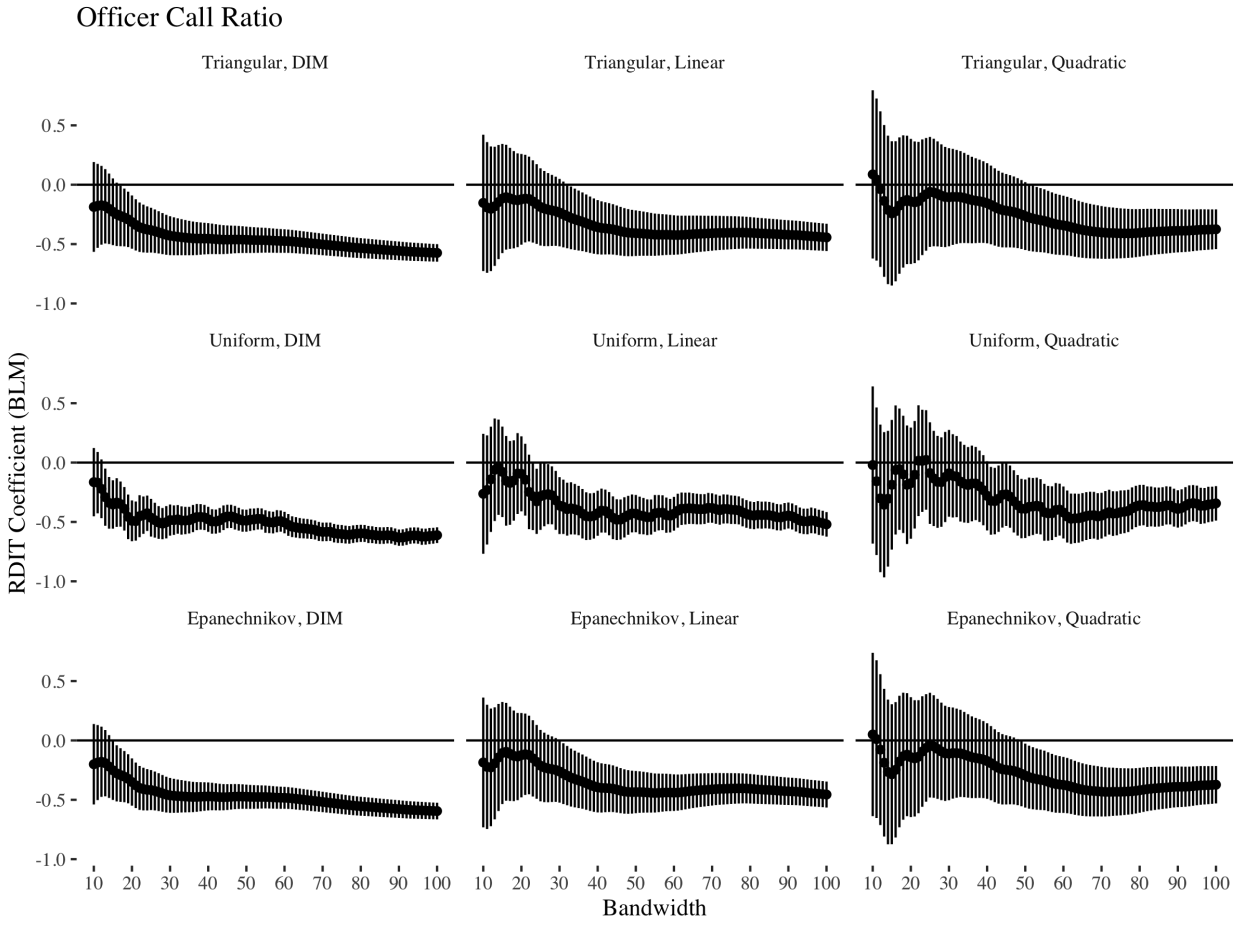


Figure F16: Effect of BLM protest on Officer/Civilian 911 Call Ratio. Panels denote different kernel and running variable polynomial degree specifications. 95% CIs displayed derived from robust SEs.

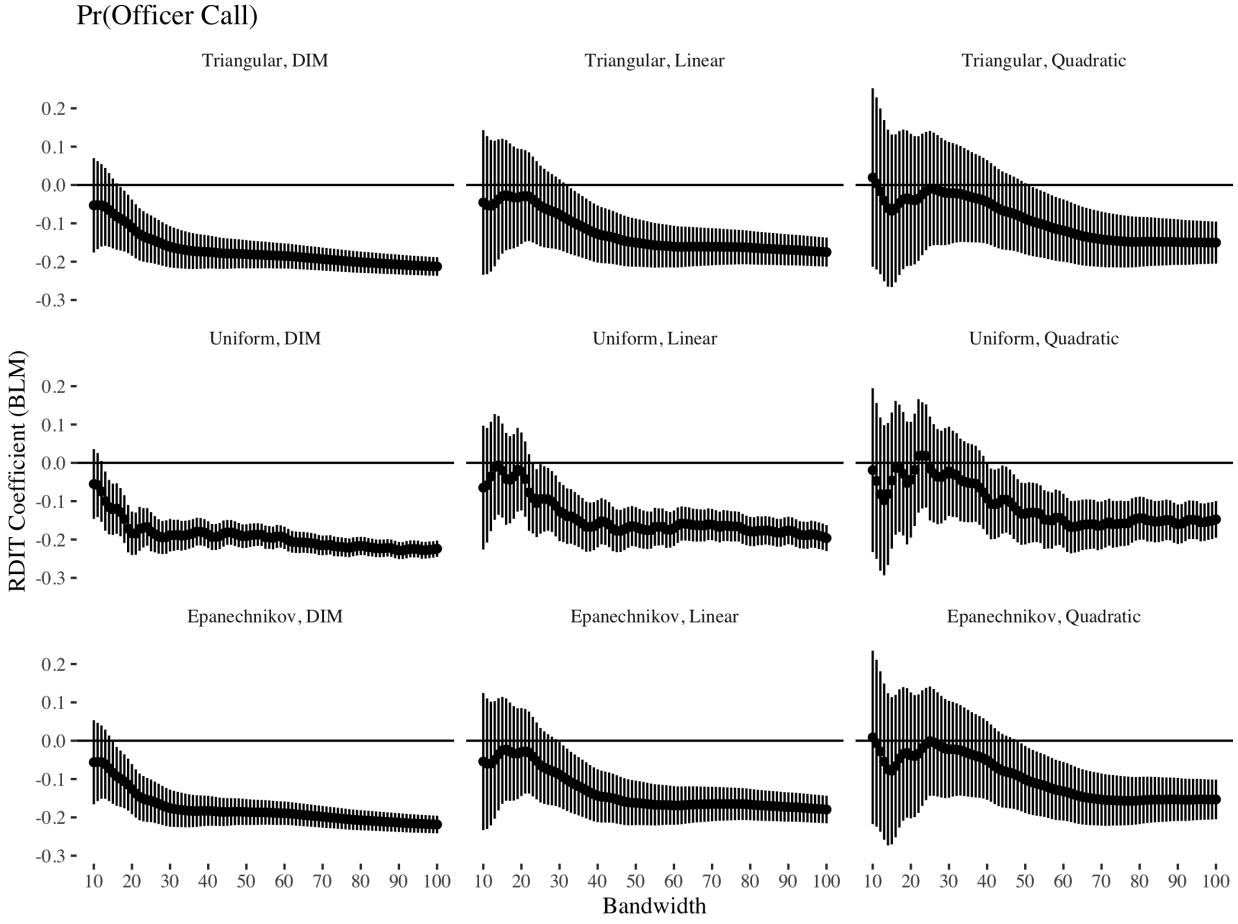


Figure F17: Effect of BLM protest on Pr(Officer Call). Panels denote different kernel and running variable polynomial degree specifications. 95% CIs displayed derived from robust SEs.