Less Policing, Less Problems: The Impact of Strategic Depolicing

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Abstract

What are the consequences of depolicing? We forward a theory of "strategic depolicing" that posits depolicing may not have deleterious consequences if it reduces unnecessary, ineffective police intrusions. We test the theory by exploiting an exogenous decrease in policing effort after the publication of an expert report finding Metropolitan Nashville Police Department tactics were racially biased and unimportant for public safety. We find police significantly reduced traffic stops, particularly for minor violations (e.g. order maintenance), after the report. Traffic stops became more efficient. Police were more likely to issue citations or make arrests and less likely to give warnings conditional on a stop. We do not find depolicing increased crime or traffic violations, and find some evidence "disorder" crimes decrease (e.g. drug possession, disorderly conduct). The findings suggest expert data analysis can motivate depolicing without the negative consequences typically associated with reductions in policing effort.

Introduction

Police misconduct across the U.S. has led to calls for departments to scale back activity. Some have cautioned criticism may cause officers to avoid policing tasks due to fear of igniting controversy, with deleterious effects on crime. This phenomenon is known as *depolicing*.[1]

What are the consequences of depolicing? Empirical research has arrived at conflicting conclusions. Some find no effect on violent crime after depolicing (Rosenfeld and Wallman 2019; Shjarback et al. 2017; Pyrooz et al. 2016), while others show crime and inefficiency increase due to officer shirking (Rushin and Edwards 2016; Shi 2009).

We argue diverging findings are a byproduct of different forms of depolicing. Police can reduce effort in a variety of ways. Officers could work less diligently after a high profile incident to avoid controversy (Chanin and Sheats 2018) or the department hierarchy could take a more organized approach and reduce specific tasks unnecessary for preventing crime. The former may lead police to avoid necessary work, undermining public safety. The latter may lead police to reduce inefficient activity, allowing police to avoid meritless intrusions while maintaining public security. Prior work often treats depolicing monolithically, obscuring the distinction between ad hoc shirking and strategic retrenchment.

We study a case of strategic depolicing in Nashville. An expert report commissioned by Nashville's mayor from the Policing Project (PP) led officers to refocus efforts away from traffic stops. The report found the Metropolitan Nashville Police Department's (MNPD) traffic stops were disproportionately targeting black drivers and unnecessary for preventing crime. The report argued MNPD resources could be directed elsewhere to improve policing quality. Although the report did not cause elected officials or the department to formally change policy, the MNPD Chief responded with a public commitment to reduce traffic stops and 'rededicate' the department to the community. [2]

[1] Rosen 2005 defines depolicing as "a decline in support of the efforts of law enforcement from municipal authorities, usually as a reflection of worsening popular perception of a local police department."

[2] "Nashville's strategy of more traffic stops to reduce crime hasn't worked, new Policing Project study says." *The Tennessean*. Nov 19, 2018. `https://tinyurl.com/y3ok6p8z`

Using a regression discontinuity design, we find police immediately responded to the report by reducing traffic stops. The decrease was driven by reducing minor "non-moving violations," such as broken taillights or vehicle maintenance problems, suggesting police reduced effort by shifting from order-maintenance policing. Policing becomes more efficient. Arrest and citation rates increase, warning rates decline. Assuming the driver pool was constant immediately after the report,[3] the increase in ticketable/arrestable stops suggests stops after depolicing had a stronger legal rationale. Moreover, we find depolicing did not increase crime or traffic violations, and may have even *decreased* crime in some circumstances. But, we find no change in racial disparities.

Our study makes two contributions. First, it clarifies how depolicing affects the distribution of coercion, particularly among race-class subjugated (RCS) communities. If one aims to understand state power's imposition, it is essential to confront the decisions police make. Otherwise, ignoring police behavior, particularly in response to a highly salient behavioral mechanism like depolicing, distorts an effective understanding of governance in the U.S. (Soss and Weaver 2017).

A nascent political science literature demonstrates overpolicing has significant political consequences on participation and trust (Lerman and Weaver 2014; Laniyonu 2019; Walker 2020). However, with exceptions (Mummolo 2018), there is limited research on the impact of reduced police presence. Whereas the cost of over-policing has been made clear, the lacuna of evidence regarding reduced presence provides an incomplete picture of the effect of switching from aggressive to passive police tactics. We show when police stop activity that provides little deterrent benefit, law enforcement quality improves without sacrificing public safety.

Second, we highlight the varieties of depolicing. MNPD strategically reduced order maintenance tasks in response to empirical evidence their activity is unnecessary, reducing needless interactions without increasing disorder. Whereas depolicing is often a negative term used to describe officer shirking, we show how less police effort in the absence of formal pol-

[3]It is unlikely driver composition changed as soon as the PP report was released. We test for endogenous driving behavior using traffic data and find no driver behavioral changes (Appendix, Section J).

icy during controversy can improve the quality of law enforcement. The distinction suggests depolicing is more nuanced than previously thought.

Reducing Excessive Policing

Police must navigate between being "warriors" and "guardians" (Owens 2019); a fragile balance which has been called an "impossible mandate" (Manning 1977). If police are not proactive, criminal activity may evade detection (Becker 1968; Nagin et al. 2013). But, if police are overly aggressive, many police-community interactions will be unnecessary, reducing public trust and departmental efficiency (Owens 2019; Tyler, Jackson, and Mentovich 2015).

One method police may use to strike a balance between competing roles is to engage in strategic depolicing, retrenching from activity which is known to be unimportant for deterring and incapacitating criminals. Instead of initiating contact with civilians on a regular basis in certain domains, police reign in excess stops in response to criticism of undue contact.

Increased caution before making stops may increase efficiency by reducing the rate of uncalled-for police contact with civilians. When stops are made, they are more likely to justify a ticket or arrest when officers apply a more strict threshold before initiating a stop. Thus, strategic depolicing should cause more citations and arrests per stop while reducing the warning rate.[4]

Some suggest depolicing will harm deterrence and incapacitation of criminals (Mac Donald 2017; Rushin and Edwards 2016; Shi 2009) by placing too little effort on reducing crime. Yet, this argument relies on two assumptions. First, it assumes police reduce tasks effectively deterring crime. Second, it assumes officers fail to stop crime when they should have.

Depolicing may not increase crime if it is strategic, that is, police effort declines in areas not effectively preventing or deterring crime. In this case, the change in law enforcement

[4]The outcomes are up to officer discretion. However, outside of the pressure from the report, officer discretion is likely to vary smoothly at the discontinuity.

behavior can balance competing objectives by avoiding unnecessary contact while not inviting more law breaking than would have occurred otherwise.

An Expert Report on Policing

We study a case of strategic depolicing in Nashville, Tennessee. After a series of high profile police killings during traffic stops, MNPD's traffic stop racial disparities were under increased scrutiny. In response, Nashville Mayor Megan Barry contacted the Policing Project (PP) to study MNPD's traffic stops. The Policing Project published a report on November 19, 2018, finding Black drivers were 44% more likely to be stopped than white drivers. The gap was pronounced for non-moving violations, which include a host of trivial vehicle maintenance violations (e.g. broken taillight). Stops were not correlated with less crime, undercutting the rationale for proactive stops. The report predicted a reduction in stops could reduce racial disparities without inviting crime (Chohlas-Wood et al. 2018).

The findings were well received by MNPD managers. Although *no formal policy changes were made*, the MNPD chief and precinct commanders pledged responsiveness to the report.[5] MNPD's police chief indicated they appreciated the finding non-moving equipment violation stops "do not appear to be having a significant impact on crime trends." The chief further stated the department would "closely review the recommendations presented by The Policing Project..."[6] The department has stated there has been no macrolevel change in strategy within the department or handed down by city government. However, precinct commanders responded to the report by putting less officers on traffic patrols.[7]

The report induced a decline in police effort after public controversies, fitting into the category of depolicing (Rosen 2005). However, the reduction in effort, specifically order-

[5] "Nashville's strategy of more traffic stops to reduce crime hasn't worked, new Policing Project study says." *The Tennessean*. Nov 19, 2018. `https://tinyurl.com/y3ok6p8z`

[6] "MNPD Chief Steve Anderson responds to Policing Project study on traffic stops." USA Today Network - Tennessee. November 19, 2018. `https://tinyurl.com/y57pyldx`

[7] "After years of debate, police make major change in number of Nashville traffic stops." April 17, 2019. The Tennessean. `https://tinyurl.com/y3zugmtz`

maintenance tasks, was evidence-based and directed by department supervisors. Thus, MNPD's strategic depolicing may have improved police efficiency by pouring less effort into unproductive activity. More details on background of the Nashville PP report in Supporting Information (SI) A.

Design

Data

We use data on 463,903 MNPD traffic stops between 2017-01-01 and 2019-03-24.[8] To evaluate if the PP report reduced stops, we aggregate the stop data to the day-level to determine the number of stops on a given day. To evaluate if depolicing increased efficiency, we use information on whether each stop led to a citation, warning, or arrest. We generate indicators for whether a stop led to these outcomes. A decrease in the warning rate, concomitant with an increase in the citation or arrest rate, suggests stops after the report have a stronger legal basis since they resulted in identification of ticketable and/or arrestable offenses.

To assess if the PP report increased efficiency by reducing order-maintenance stops, we use indicators for the violation a stop was initialized for. These include moving violations (e.g. speeding), non-moving violations (e.g. broken mufflers, taillights), investigative stops, seatbelt violations, parking violations, and child seat violations. Unfortunately, violation indicators cannot be disaggregated further. However, the PP report noted MNPD initialized an excessive amount of non-moving violation stops related to car maintenance that do not implicate traffic safety. Given the low citation rate associated with these stops, they characterize order-maintenance policing.[9]

To demonstrate the effect of de-policing on traffic outcomes or crime, we use two datasets.[10]

[8] From the Stanford Open Policing Project.

[9] For instance, the warning rate for non-moving violations was 87%, higher than moving violations, the next most common set of stops, at 64% (SI G, Table G4).

[10] From Nashville's Open Data website: `https://data.nashville.gov/`

First, we use data on traffic accidents. We aggregate the count of accidents to the day-level to determine if depolicing increased accidents. Moreover, we use the accident-level data to identify if accidents were more likely to lead to hit-and-runs after depolicing. Second, we use crime data from the MNPD. We aggregate the count of crime incidents to the daily level to determine if depolicing increased crime. We disaggregate the crime incidents using preexisting FBI National Incident-Based Reporting System classifications to evaluate if depolicing increased particular kinds of crimes.[11]

Estimation strategy

To assess the effect of the Policing Project report, we estimate the following:

$$Y_i = \alpha_0 + \tau \text{Report}_i + f_p(\text{d}_i) + \varepsilon_i \tag{1}$$

Y_i is the outcome for stop i (e.g. citation, warning, arrest). Report_i is an indicator for the day after the PP report was released (November 20, 2018). τ is the effect of the policy report at the point the report was released. $f(\text{d}_i)$ are functions modeling days from the report's release (d_i at different polynomial degrees $p \in \{0, 1, 2, 3\}$). ε_i are robust errors.[12]

We present two sets of estimates. The first set characterizes the discontinuous effect of the report using all stop data. The second set uses stop data near the day the report was released (0-100 days). Although the second set of estimates may suffer from reduced statistical power, they are less likely to be confounded by intervening variables distant from the moment the report was released.

τ has a causal interpretation if unobservable covariates vary smoothly at the discontinuity. This assumption is reasonable since determinants of traffic violations, crime, and police behavior are unlikely to jump day-to-day.

[11] These are against-person crimes (assault), against property crimes (theft), and against society crimes (prostitution, drug possession).

[12] Results are the same using block group and reporting area (i.e. police beat) clustered standard errors (SI O).

Results

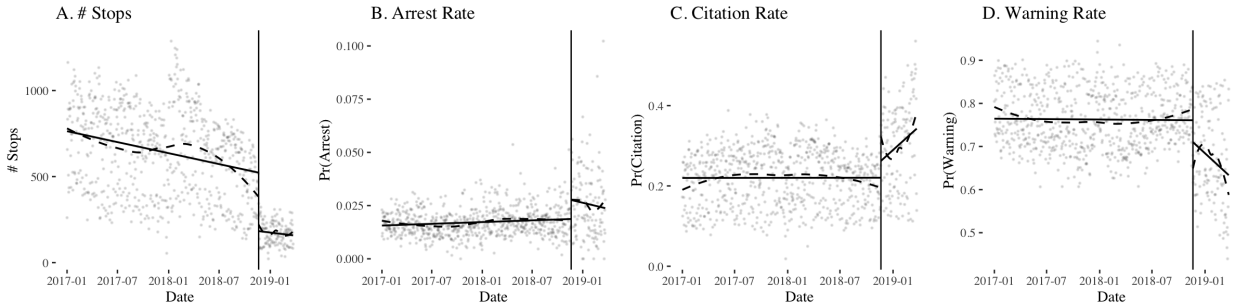

Figure 1: Outcomes (y-axis) over time (x-axis). Panels A, B, C, and D display the stop count, arrest rate, citation rate, and warning rate respectively. Vertical black line denotes the moment the expert report was published. Solid black lines and dashed black lines are linear and loess models fit on each side the moment the expert report was published.

We visualize the efficiency effect in Figure 1. A large drop in stops is evident immediately after the report, along with higher arrest and citation rates with lower warning rates. Table 1 shows the effect of the report on police efficiency from Equation (1) using all stop data. Columns 1-4 vary time polynomials without additional covariate adjustment. Columns 5-8 include temporal fixed effects in addition to the lagged outcome to adjust for time trends and auto-correlation. Our most conservative estimate suggests the report led to 125 less stops, a 20% decrease from the pre-report mean (Panel A, Column 7). The most conservative estimate suggests the report increased citation rates by 21% relative to the pre-report mean (Panel B). Arrests follow a similar pattern (Panel C), but the estimates attenuate in some specifications.[13] The warning rate declines by 7% after the report (Panel D). Figure 2 shows these estimates are insensitive to bandwidths near the discontinuity.[14] Effects are constant across precincts (SI P).

We use violation types to understand mechanisms. The report highlighted non-moving violation stops were unnecessary. Consistent with the report's advice, non-moving violations

[13]Smaller effects might be because arrests are rarer than citations. Re-estimating results removing citation stops generates more precise positive effects relative to warning stops, the relevant reference category (SI L).

[14]We also evaluate the effect of the report on contraband hit rates, an alternative measure of efficiency (SI E). Additionally, we demonstrate results derived from data near the discontinuity is insensitive to the polynomial choice for the running variable (SI F).

Table 1: Effect of expert report on outcomes of interest using all stop data

Panel A: Stop #	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	-472.63^{***}	-339.91^{***}	-269.63^{***}	-169.49^{*}	-178.41^{***}	-189.86^{***}	-125.31^{**}	-155.30^{**}
	(22.42)	(42.92)	(63.58)	(83.09)	(27.39)	(29.64)	(41.83)	(49.76)
Pre-Report μ	643	643	643	643	643	643	643	643
N	813	813	813	813	812	812	812	812
Panel B: Citation Rate	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	0.09^{***}	0.05^{***}	0.13^{***}	0.13^{***}	0.07^{***}	0.05^{***}	0.12^{***}	0.11^{***}
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Pre-Report μ	0.24	0.24	0.24	0.24	0.24	0.24	0.24	0.24
Panel C: Arrest Rate	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	0.007^{***}	0.007^{***}	0.006^{*}	0.006	0.007^{***}	0.007^{***}	0.004	0.003
	(0.001)	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)	(0.004)
Pre-Report μ	0.017	0.017	0.017	0.017	0.017	0.017	0.017	0.017
Panel D: Warning Rate	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	-0.10^{***}	-0.05^{***}	-0.13^{***}	-0.14^{***}	-0.07^{***}	-0.06^{***}	-0.12^{***}	-0.11^{***}
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Pre-Report μ	0.74	0.74	0.74	0.74	0.74	0.74	0.74	0.74
N	463903	463903	463903	463903	463903	463903	463903	463903
Degree	0	1	2	3	0	1	2	3
Controls	N	N	N	N	Y	Y	Y	Y

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. Models 1-4 assess the discontinuous effect of the PP report on the stop number (Panel A), citation rate (Panel B), arrest rate (Panel C), and warning rate (Panel D) without controls using all MNPD stop data. Models 5-8 do the same adjusting for year, month, day-of-week fixed effects in addition to the lagged outcome. All models specify varying degrees for the running variable from 0 (difference-in-means) to 3. Robust standard errors in parentheses.

decline significantly post-report (SI G, Figure G10). Meanwhile, the moving violation stop rate increases. Officers concentrated less on order-maintenance in favor of enforcing rules of the road, which may be more linked to public safety.

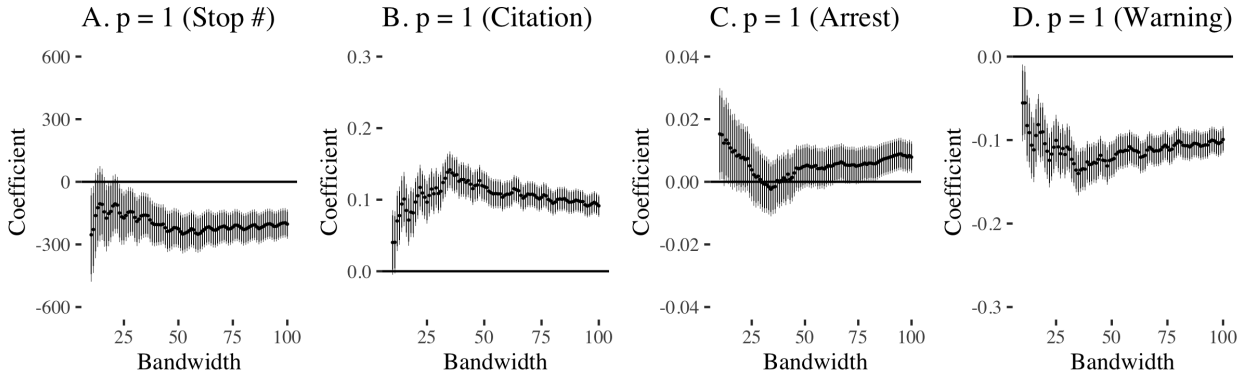

Figure 2: Effect of expert report on relevant outcomes (y-axis) across bandwidths (x-axis). All panels use the running variable to the first degree.

We do not find the report increased crime against persons or property. We do find "against society crimes (e.g. drug use, disorderly conduct)," decrease (Figure 3, Panel D).[15] Three channels could explain the reduction. First, reducing inefficient policing may increase the legal system's legitimacy, reducing lawbreaking and increasing community cooperation (Tyler, Jackson, and Mentovich 2015).[16]. Second, the shift from order maintenance to community engagement could have deterred "disorder" crime (Braga, Welsh, and Schnell 2015). Third, against society crimes were less likely to be identified because order maintenance policing was reduced.[17] Although we lack the ability to probe mechanisms, the result suggests reduced effort need not increase crime, and could lead to a reduction. Concomitantly, strategic depolicing did not increase traffic accidents or the rate of hit-and-runs (SI J).

Notably, depolicing's effects are homogenous across ethno-racial groups. Although stops/efficiency

[15]See Appendix J for a full empirical analysis of the crime outcomes

[16]This channel is consistent with theoretical models finding overpolicing increases equilibrium crime (Owens 2020). Other evidence suggests order maintenance depolicing reduces crime (Sullivan and O'Keeffe 2017).

[17]This channel may not explain the results given most against society crimes are unrelated to traffic and there was no increase in drug contraband recovery after depolicing (SI E).

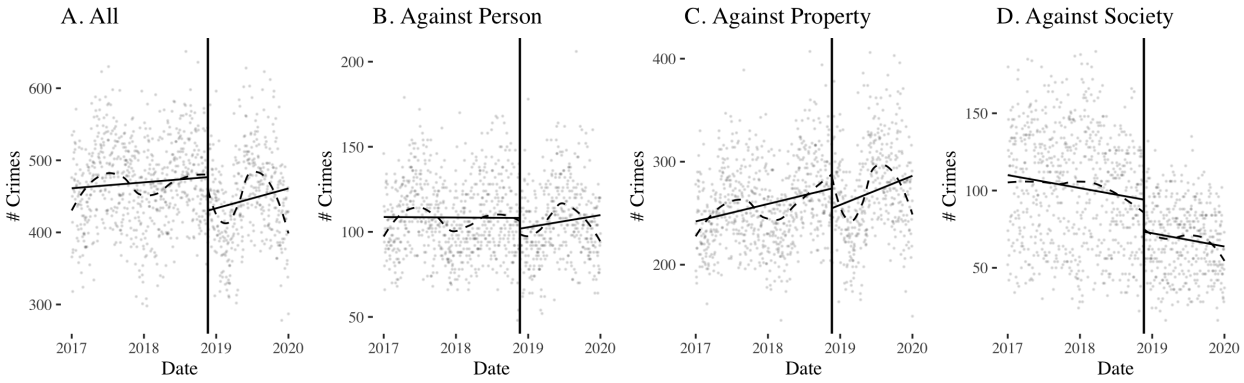

Figure 3: Crime outcomes (y-axis) over time (x-axis). Panels A, B, C, and D display all crimes, crimes against persons, crimes against property, and crimes against society respectively (using NIBRS offense categories). Vertical black line denotes report publication. Solid/dashed are linear/loess models fit on each side the moment the report was published.

decreased/increased for *all* groups, disparities remain (see Appendix G, H).[18] One explanation for the persistence of disparities is officers believe effort ought be reduced in a race-neutral fashion, without attempting to proactively reverse past discrimination (Appendix I and Q).

Conclusion

Our theoretical contribution is important for two reasons. First, it suggests scholars should investigate the political context surrounding reductions in police presence. Recent studies show police respond poorly to viral controversies compared to oversight regimes inaugurated under less politicized conditions (Ba and Rivera 2019; Devi and Fryer Jr 2020). Our theory provides a general framework for explaining the pattern: voluntary reductions in effort can boost efficiency. Our finding differs from prior work since no formal institutional change was necessary to modify police behavior. Second, we show that to understand whether depolicing harms deterrence or improves officer-civilian interactions, one must consider the deterrent effect of the activity police reduce. As calls for reduced police presence grow, an important

[18]Results available upon request are similar testing for hit rates by race.

exercise is identifying which policing tasks deter or do not. Identifying unnecessary policing tasks could enable strategic depolicing, improving welfare.

Our results highlight avenues for future theoretical and empirical work. First, our finding suggests future documentation of bias in police practice could result in rapid departmental change when supervisors are receptive to the information. Researchers that engage departments like the Policing Project can have a large influence on policing in American cities.

Second, the Policing Project report was a sufficient condition to achieve efficient reductions in proactive policing, suggesting formal policy changes across the city were needed to limit law enforcement activity. However, supervisors and policymakers were supportive of the report, meaning similar reports may not induce changes in police behavior when managers oppose tactical changes.[19] One task for future researchers is understanding why depolicing is sometimes strategic and other times officer shirking of responsibility. Researchers should consider when and why officers are more receptive to some reports and not others, as criticism can have a positive impact.

Finally, although the Policing Project report noted the existence of racial disparities, they did not decrease after depolicing. Reducing the level of policing may not eliminate ethno-racial disparities in police exposure. Since the reduction was voluntary, officer attitudes towards resolving racial inequity may drive the continuity. The null finding for racial disparities in stops forces the question: why does reducing a racially biased policing practice not remove racial bias? Answering this question is an important task for theoretical work to prevent discrimination in the criminal justice system.

[19]MNPD did not respond to a similar report on MNPD traffic stops by Gideon's Army, a Black-led activist organization, in 2016.

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Appendices

Supplementary Information (SI) For Online Publication

Contents

A Description of Nashville Context

A.1 The beginning of order maintenance policing

In 2004, the Metropolitan Nashville Police Department's (MNPD) new police chief, Ronal Serpas, implemented an order-maintenance policing regime that increased the number of traffic stops across Davidson County. Order-maintenance policing regimes originated from New York City and are informed by the theory cracking down on minor crime deters serious crime (Wilson and Kelling 1982). In NYC, order-maintenance policing took the form of an increase in "stop-and-frisks," where individuals were stopped on weak legal bases, ostensibly to recover drugs, contraband, or prior warrants. In car-oriented cities like Nashville, police-civilian contact typically occurs through vehicular stops. In terms of order maintenance policing, police will identify minor violations in order to stop cars on the chance a motorist is engaged in criminal activity (Armenta 2017).

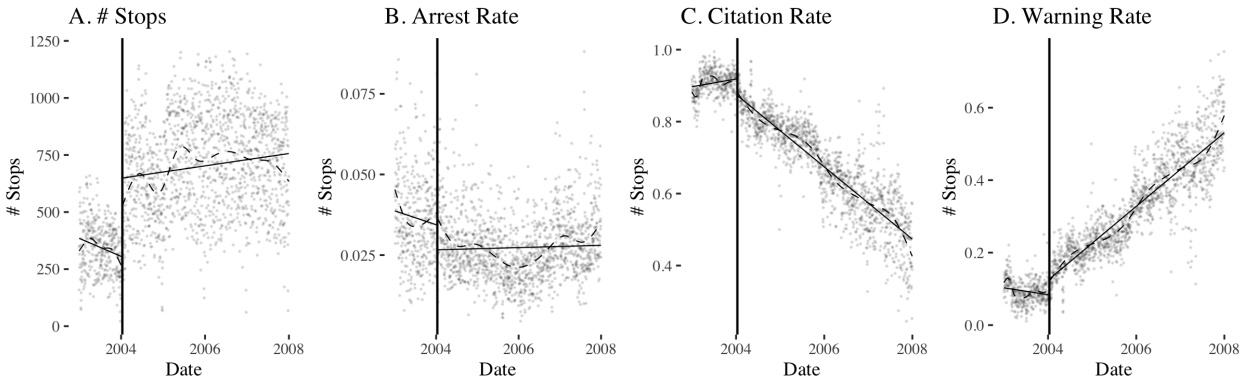


Figure A1: Policing outcomes (y-axis) over time between 2003-01-01 and 2008-01-01 (x-axis) before and after Ronal Serpas tenure as MNPD police chief (vertical black line). Solid/dashed are linear/loess models fit on each side the moment Serpas was instated as police chief. Data are from a public records request to the MNPD.

Under Serpas, traffic stops increased by 42% relative to his predecessor, Emmett Turner (1996-2004) (Gideon's Army 2016; Alcorn 2019). Serpas implemented policies like "Mission 1," which assigned officers not otherwise assigned to patrol to spend 1 night a month in high-crime areas. Figure A1 displays the number of stops, arrest rate, citation rate, and warning rate between 2003-01-01 and 2008-01-01. The vertical line indicates when Serpas took over as police chief. After Serpas, stops discontinuously increased over 100%. Traffic stop efficiency also decreased despite the increase in policing effort. Arrest and citation rates decreased while warning rates increased, suggesting an increase in the rate of unnecessary stops.

A.2 Chief Anderson and the "Driving While Black" report

After Serpas, Chief Steve Anderson took over in 2010. Under Anderson, the number of traffic stops increased to a peak of just over 450,000 in 2012 before beginning to decrease slightly

(Gideon's Army 2016). The order maintenance strategy began taking its toll, particularly on Black communities throughout Nashville. Between 2011-2015, the MNPD initialized traffic stops at a rate 8 times the national average (Gideon's Army 2016). There were 2 million stops, roughly 400,000 stops per year. The rate of stops was 786 for every 1000 drivers, 75% of the driving age population. This rate was also 3.4-6.8x greater than comparable cities such as Charlotte and Louisville. Additionally, during this time period, the MNPD initialized more stops of black people than the number of black people who were driving age living in Davidson County (Gideon's Army 2016). The rate of traffic stops for black drivers was 1122 stops for every 1000 black drivers (Gideon's Army 2016).

The aforementioned statistics were produced in a report titled "Driving While Black: A Report on Racial Profiling in Metro Nashville Police Department Traffic Stops." The report was released by Gideon's Army on October 2016, a Nashville-based Black-led activist organization (Gideon's Army 2016).[20] In addition to analyzing MNPD traffic stop data, they also provided a number of field interviews and personal narratives from Black Nashvillians explicating the excessive level of policing on part of MNPD against Black people and the Black community writ large. The report summarized the interviews, indicating the excessive level of traffic stops imposed by MNPD made Black Nashvillians "fearful, angry, anxious, dehumanized, and traumatized (Gideon's Army 2016)."

The report was turned over to the Department of Justice in order to generate a basis for a Federal investigation (WPLN News 2016). Mayor Megan Barry took no action in response to the report. MNPD Chief Anderson slammed the report, indicating it was "morally disingenuous." Anderson argued the reason there were more traffic stops in Black communities was because of Black communities had more crime.[21]

A.3 Jocques Clemmons and Daniel Hambrick

On February 2017, Jocques Clemmons, a 31-year old black man, was killed by a white MNPD officer after a traffic stop.[22] The murder increased focus on MNPD traffic stop racial disparities. At this point, Megan Barry began meetings with the Policing Project in order to conduct another study on MNPD's policing practices.[23] After the murder and initial meetings with the Policing Project, MNPD and the Nashville government took no action on traffic stops.

On July 2018, Daniel Hambrick, a 25-year old black man was murdered by another MNPD officer after a traffic stop. The MNPD officer shot Hambrick 3 times in the back as he was running away. The murder precipitated another public inquiry into MNPD traffic stop behavior.[24] The new Nashville Mayor, David Briley, announced a review of MNPD traffic stops was to be released later in the year on August 2018. This announcement was

[20] The report can be accessed at this URL: `https://tinyurl.com/y5qbz93w`

[21] "Gideon's Army Responds to Police Chief's Criticism of 'Driving While Black' Report." Nashville Scene. March 7, 2017. `https://tinyurl.com/y3343ab6`

[22] "Nashville's strategy of more traffic stops to reduce crime hasn't worked, new Policing Project study says." *The Tennessean*. Nov 19, 2018. `https://tinyurl.com/y3ok6p8z`

[23] "Barry Administration Leans Toward Collaboration With NYU's 'Policing Project" Nashville Scene. July 18, 2017. `https://tinyurl.com/yyeveh3o`

[24] "Prosecutors file homicide charge against officer in Daniel Hambrick's death" The Tennessean. September 27, 2018. `https://tinyurl.com/y93wku85`

slammed by Community Oversight Now, a group composed of members of Gideon's Army fighting for a civilian review board of the police. They indicated:

> In response to the shooting, the mayor reintroduced the NYU Policing Project to Nashville. The Policing Project was first introduced by Mayor Megan Barry and her purposeful attempt to counter the Community Oversight Board proposal that was developed after the killing of Jocques Clemmons in February 2017. It was brought to Nashville with no vetting, little transparency, and mostly by advocates with little experience working and living in heavily-policed communities. The fact that Mayor Briley continues to use the Policing Project to counter local grassroots activists in Nashville's black community is insulting and smacks of racial paternalism.
>
> The Policing Project has nothing to do with police accountability. It only proposes a cost-benefit analysis of traffic stops and related matters - that is to study a problem that has already been studied in the "Driving While Black" report produced by Gideon's Army. It is an initiative that will be managed and controlled by Chief Steve Anderson and discarded in a couple of years. Briley's embrace of the Policing Project is a cynical display of governance. It intends to disempower local activists, particularly African Americans heavily invested in police accountability.[25]

A.4 The Policing Project report and MNPD response

The Policing Project report was released on November 19, 2018. Like the Gideon's Army report, the report found traffic stops were not correlated with crime and there were a significant disparity in traffic stop rates between white and Black drivers. The report made a number of recommendations, including reducing the number of traffic stops, monitoring racial disparities, redeployment of officer resources toward more effective crime-fighting tools, and the adoption of a Neighborhood Policing Strategy (Chohlas-Wood et al. 2018). Specifically, the report indicating non-moving violation stops, that is, order-maintenance stops for minor vehicular violations, were excessive and had particularly disparate impact on Black drivers. The report was well received at the highest levels of the MNPD. The Chief of Police, Steve Anderson, issued a long statement reflecting on the report's findings and how the police could adjust their behavior in light of its findings:

> "Nevertheless, I appreciate the finding of the researchers that vehicle stops for non-moving equipment violations and the police visibility related to them do not appear to be having a significant impact on short-term or long-term crime trends. Just as important, is the concern for trust with the police department by citizens in areas where these stops most often occur. Just as Nashville continues

[25] "Nashville mayor announces review of policing strategies in wake of fatal shooting" Fox - Nashville. August 8, 2018. `https://tinyurl.com/y3vlzmjm`

> to evolve, so must our police department's strategies and partnership efforts to best serve all of our communities."[26]

However, Chief Anderson did dismiss the findings on racial disparities, indicating they were a function of differential criminal offending across communities.[27] After the report, traffic stops appear to have decreased precipitously. The MNPD also shifted some officers from conducting traffic stops to conducting neighborhood patrols in order to increase community relationships (Alcorn 2019).

[26] "MNPD Chief Steve Anderson responds to Policing Project study on traffic stops." USA Today Network - Tennessee. November 19, 2018. `https://tinyurl.com/y57pyldx`

[27] "Study finds 'notable racial disparities in traffic stops' in Nashville." News Channel 5 - Nashville. November 19, 2018. `https://tinyurl.com/y2aw9ngc`

B Effect of report on stops across bandwidths

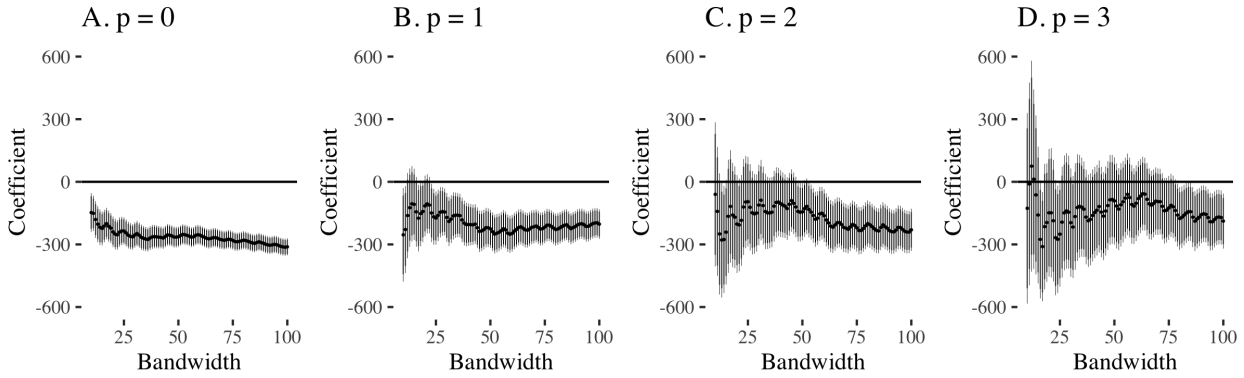


Figure B2: Effect of expert report on stop counts (y-axis) across bandwidths (x-axis). Panels A, B, C, and D vary the degree the running variable is at from 0-3 respectively.

Figure B2 displays the effect of the expert report on stop counts along data bandwidths near the release of the report between 10-100. Panels A, B, C, and D vary the degree of the running variable from 0-3. The estimates consistently demonstrate the report reducing the overall number of traffic stops initialized by MNPD.

C Effect of report on outcome counts

This section displays the effect of the report on outcome *counts* as opposed to *rates*. Figure C3 illustrates the report's effect on arrest, citation, and warning counts (Panels A, B and C respectively). Although the linear models fit on each side of the moment the report was released demonstrate the report discontinuously decreased arrest, citation, and warning stops, the loess models demonstrate arrest and citation counts are smooth near the discontinuity. However, warning counts decrease discontinuously regardless of estimation strategy, suggesting police did not decrease effort against citeable/arrestable offenses, only violations that had a lower legal threshold. Table C1 and Figure C4 corroborates the descriptive findings, demonstrating warning counts decreased much more than citations or arrests.

C.1 Descriptive analysis

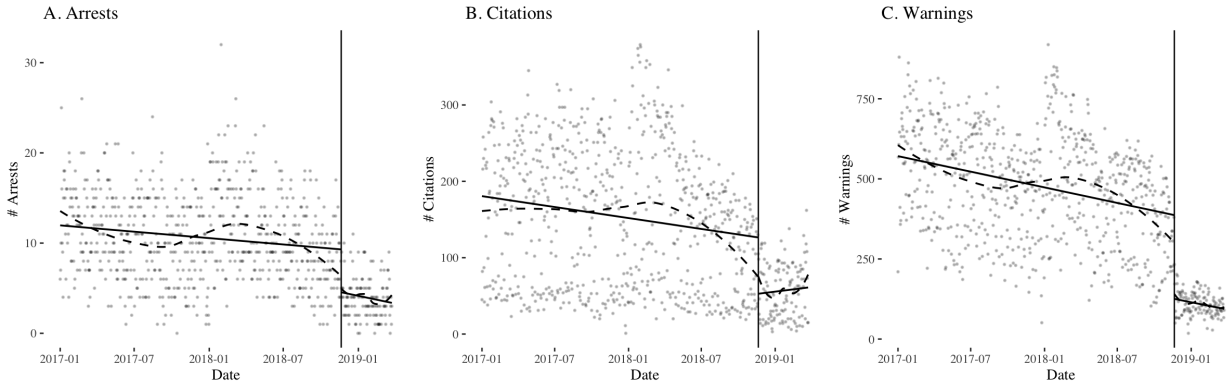


Figure C3: Relevant outcome counts (y-axis) over time (x-axis). Panels A, B, and C display the citation count, arrest count, and warning count respectively. Vertical black line denotes the moment the expert report was published. Solid black lines and dashed black lines are linear and loess models fit on each side the moment the expert report was published.

C.2 All data

Table C1: Effect of expert report on count outcomes of interest using all stop data

Panel A: Citation Count	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	-96.69^{***}	-73.70^{***}	-28.25	-6.53	-35.40^{***}	-38.47^{***}	-5.93	-18.41
	(8.06)	(15.81)	(23.23)	(30.58)	(10.69)	(11.63)	(16.81)	(20.02)
Pre-Report μ	154	154	154	154	154	154	154	154
Panel B: Arrest Count	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	-6.67^{***}	-4.75^{***}	-3.83^{**}	-1.42	-3.72^{***}	-3.87^{***}	-2.36	-2.90^{*}
	(0.44)	(0.87)	(1.30)	(1.68)	(0.76)	(0.83)	(1.22)	(1.45)
Pre-Report μ	11	11	11	11	11	11	11	11
Panel C: Warning Count	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	-369.27^{***}	-261.46^{***}	-237.54^{***}	-161.55^{**}	-148.15^{***}	-157.14^{***}	-119.05^{***}	-137.57^{***}
	(14.91)	(28.14)	(41.83)	(54.46)	(19.23)	(20.74)	(29.10)	(34.50)
Pre-Report μ	479	479	479	479	479	479	479	479
N	812	812	812	812	812	812	812	812
Degree	0	1	2	3	0	1	2	3
Controls	N	N	N	N	Y	Y	Y	Y

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

C.3 Analysis near bandwidth

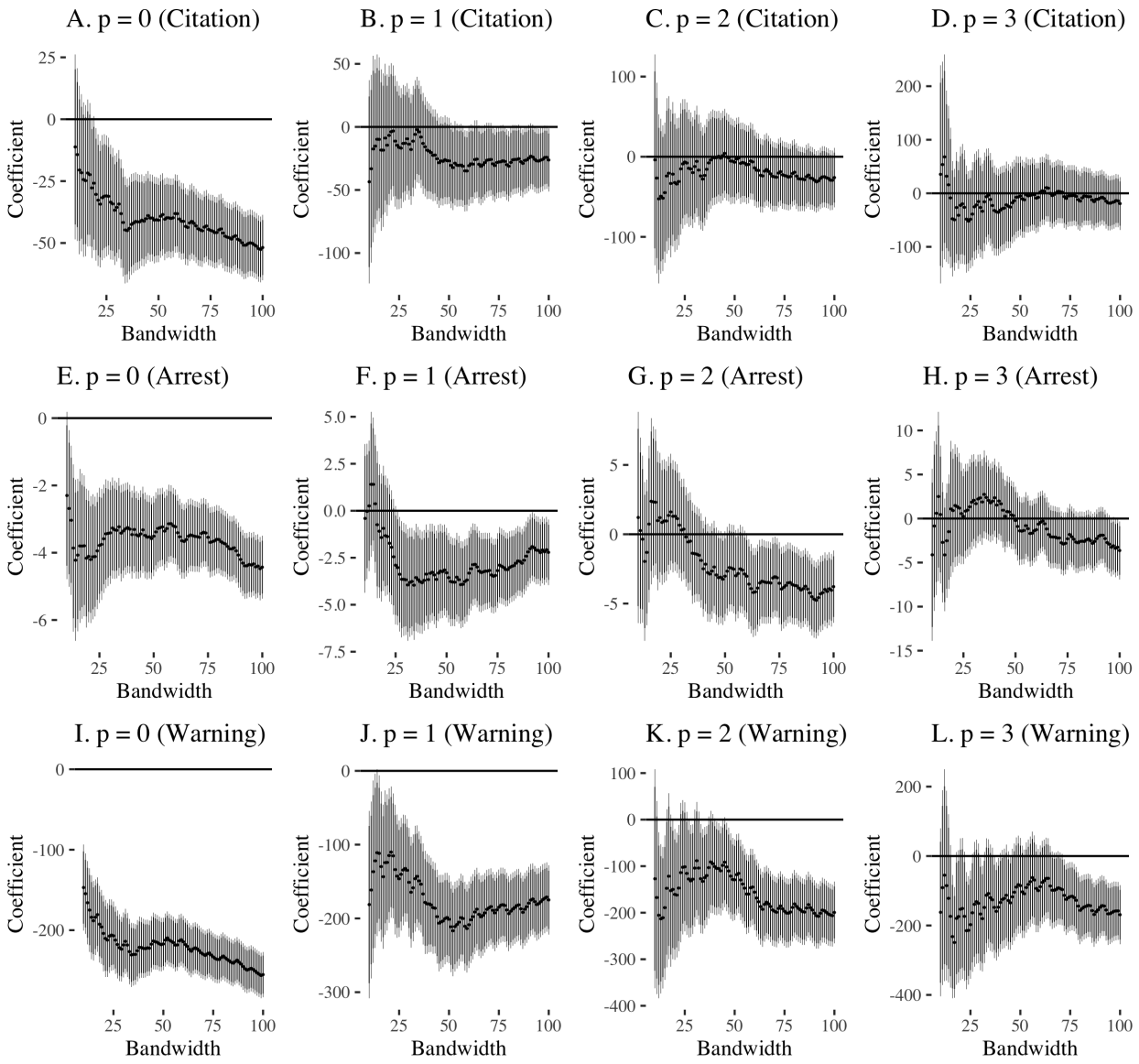


Figure C4: Effect of expert report on citation (y-axis, first 4 panels), arrest (y-axis, middle 4 panels), and warning counts (y-axis, last 4 panels) across bandwidths (x-axis). Every 4 panels varies the degree of the running variable from 0-3.

D Effect of report on search outcomes

Here, we evaluate the effect of the report on search outcomes (counts and rates). Figures D5, D6 and Table D2 show search counts decrease, consistent with a decrease in overall policing effort. However, there is no commensurate change in search rates, suggesting the legal threshold for initializing a search does not change discontinuously after the Policing Project report was released. These findings suggest the depolicing initiative implemented by MNPD may have changed factors that determine decisions to initialize a stop, but not factors that determine decisions to initialize a search. The null effect of the report on search rates may be because the policing project report did not explicitly touch on search rates, and instead focused on analyzing overall traffic stops and non-moving violation stops (Chohlas-Wood et al. 2018).

D.1 Descriptive Analysis

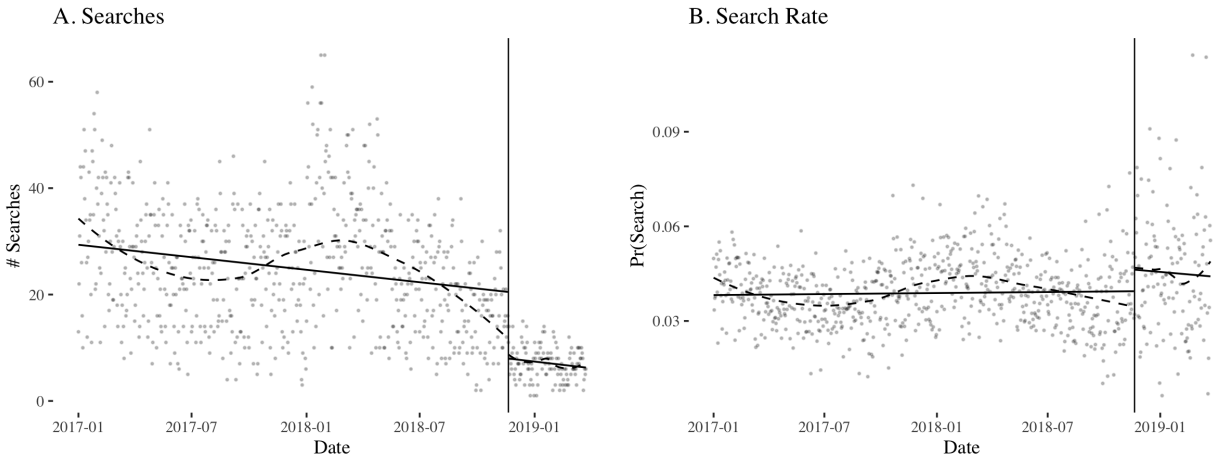


Figure D5: Searches (y-axis, Panel A) and search rates (y-axis, Panel B) over time (x-axis). Vertical line denotes the time the expert report is released. Loess (dashed) and linear (solid) models fit on each side the moment the report is released.

D.2 Using all data

Table D2: Effect of expert report on search outcomes

Panel A: Search Rates	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	0.00*	0.00	0.00	0.01*	0.00	0.00	−0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
N	463903	463903	463903	463903	463903	463903	463903	463903
Panel B: Searches	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	−17.80***	−12.51***	−9.00**	−1.07	−8.90***	−9.76***	−5.68*	−5.64*
	(1.04)	(2.02)	(2.98)	(3.79)	(1.50)	(1.63)	(2.31)	(2.74)
N	813	813	813	813	812	812	812	812
Degree	0	1	2	3	0	1	2	3
Controls	N	N	N	N	Y	Y	Y	Y

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. Models 1-4 assess the discontinuous effect of the project policing report on the search rate (Panel A) and the search count (Panel B) without controls using all Nashville PD stop data after 2017-01-01. Models 5-8 do the same adjusting for year, month, day-of-week fixed effects in addition to the lagged outcome. All models specify varying degrees for the running variable from 0 (a simple difference-in-means) to 3. Robust standard errors in parentheses.

D.3 Analysis near bandwidth

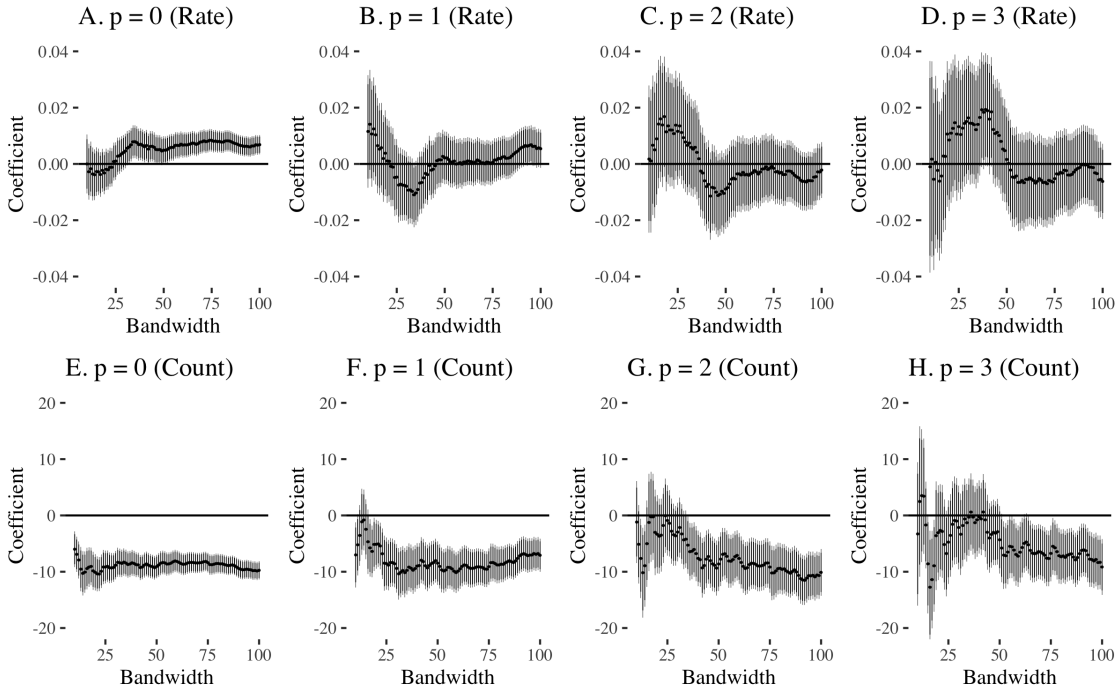


Figure D6: Effect of expert report on search rates (top 4 panels, y-axis) and search counts (bottom 4 panels, y-axis) across bandwidths (x-axis). Every 4 panels varies the degree of the running variable from 0-3.

E Effect of report on search hit outcomes

Here, we analyze the effect of the policing project report on contraband hit counts (e.g. recovery of drugs, weapons during stops) and contraband hit rates. After the report, the number of hits decreases (Figures E7, E8, Table E3). However, the hit rate remains unchanged. Again, consistent with the analysis on Section D, these findings demonstrate police are not changing their thresholds for initializing a search after depolicing, only their thresholds for initializing a stop. The absence of a reduction in search and hit rates post-report may be a limitation of the depolicing initiative, given the hit rate immediately prior to the report is relatively inefficient (roughly 25 percentage points).

E.1 Descriptive analysis

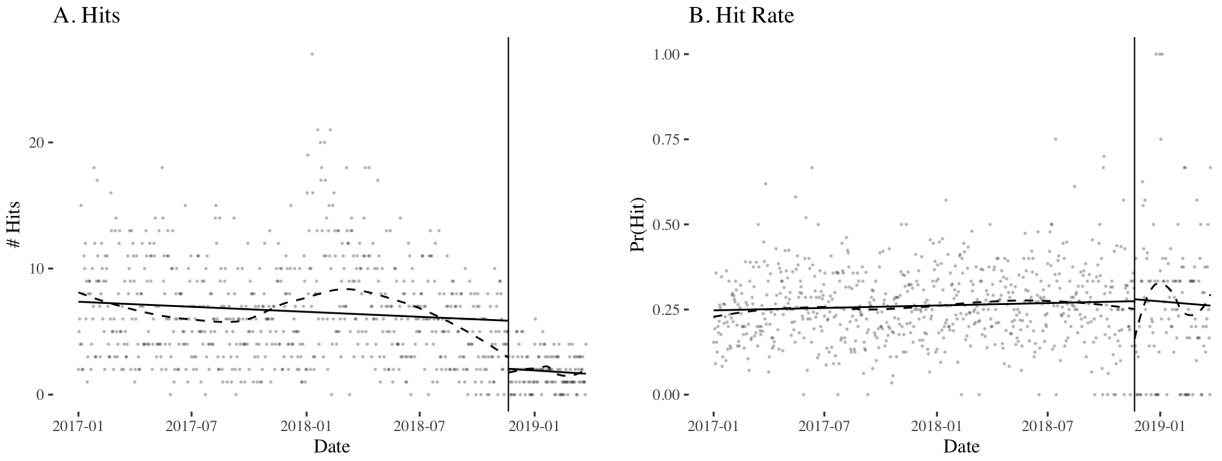


Figure E7: Hits (y-axis, Panel A) and hit rates (y-axis, Panel B) over time (x-axis). Vertical line denotes the time the expert report is released. Loess (dashed) and linear (solid) models fit on each side the moment the report is released.

E.2 Using all data

Table E3: Effect of expert report on search hit outcomes

Panel A: Hit Rates	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	−0.00	−0.03	−0.04	−0.09	−0.05	−0.03	−0.01	−0.10
	(0.02)	(0.03)	(0.04)	(0.06)	(0.03)	(0.03)	(0.05)	(0.06)
N	18040	18040	18040	18040	18040	18040	18040	18040
Panel B: Hits	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	−4.76***	−3.81***	−2.73*	−1.09	−3.77***	−3.83***	−2.20*	−3.23**
	(0.37)	(0.73)	(1.08)	(1.40)	(0.63)	(0.68)	(0.99)	(1.17)
N	813	813	813	813	812	812	812	812
Degree	0	1	2	3	0	1	2	3
Controls	N	N	N	N	Y	Y	Y	Y

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. Models 1-4 assess the discontinuous effect of the project policing report on the stop-and-search hit rate (Panel A) and the hit count (Panel B) without controls using all Nashville PD stop data after 2017-01-01. Models 5-8 do the same adjusting for year, month, day-of-week fixed effects in addition to the lagged outcome. All models specify varying degrees for the running variable from 0 (a simple difference-in-means) to 3. Robust standard errors in parentheses.

E.3 Analysis near bandwidth

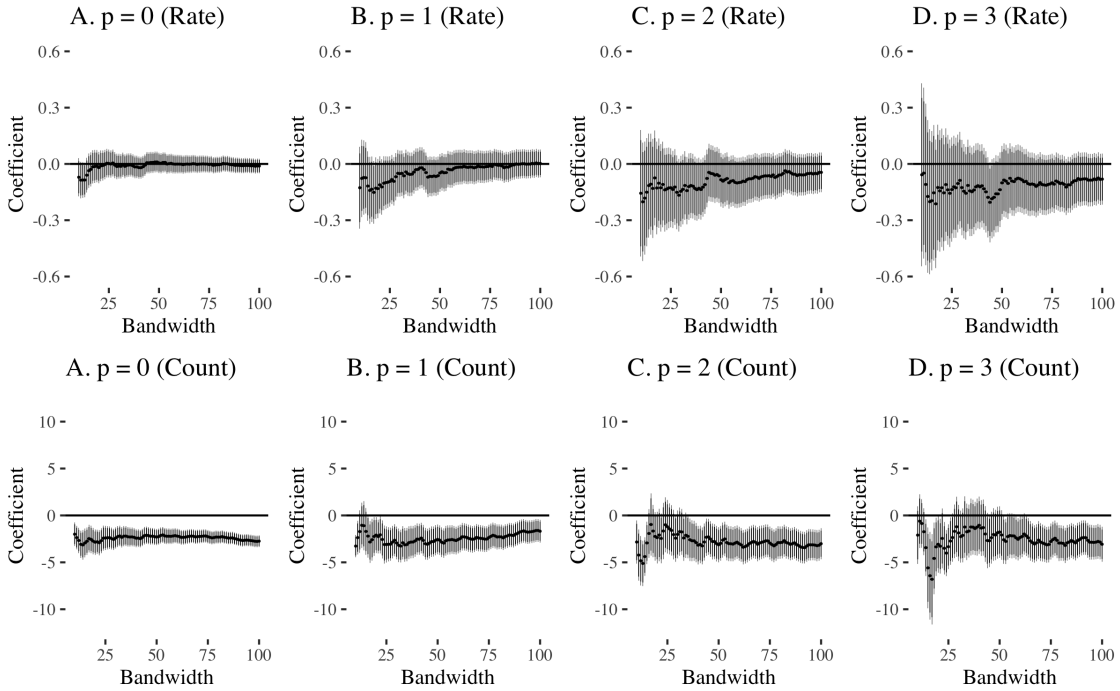


Figure E8: Effect of expert report on hit rates (top 4 panels, y-axis) and hit counts (bottom 4 panels, y-axis) across bandwidths (x-axis). Every 4 panels varies the degree of the running variable from 0-3.

F Main results near bandwidth by different polynomials

Here, we re-estimate equation (1) for bandwidths near the report's release across different polynomials degrees (0-3). The estimates presented here are consistent with the findings presented on Figure 2 in the main text, which only use the running variable to the first degree.

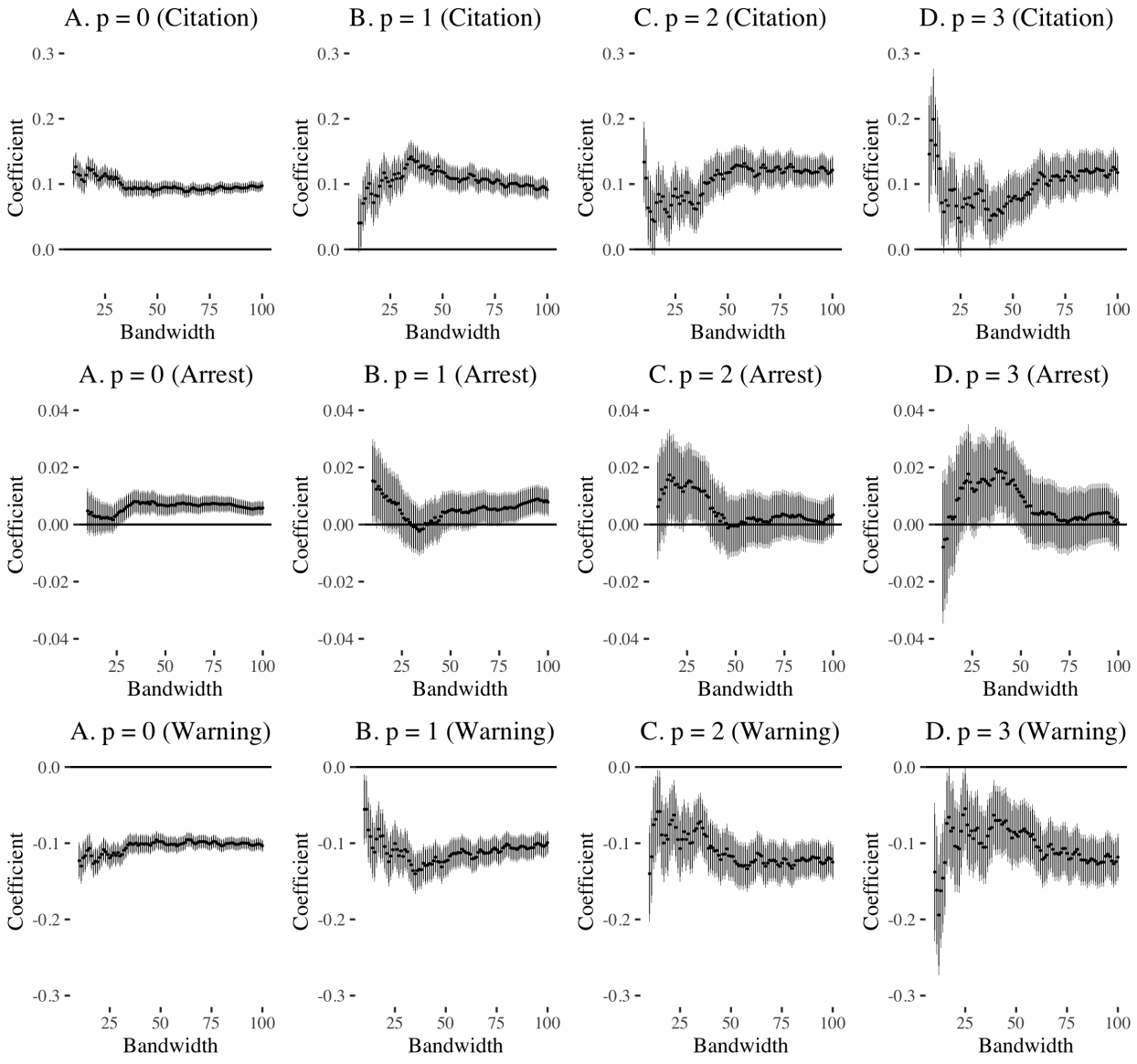


Figure F9: Effect of expert report on relevant outcomes (y-axis) across bandwidths (x-axis). Panels A, B, C, and D vary the degree the running variable is at from 0-3 respectively. The top 4 panels have the citation rate as the outcome. The middle 4 panels have the arrest rate as the outcome. The bottom 4 panels have the warning rate as the outcome.

G Effect of report on violation type

Here, we display the effect of the report on the violation a traffic stop was for. Prior to the report, non-moving stops had the highest warning rate, suggesting these stops had a low legal threshold for initiation or were unnecessary (Table G4).[28] It is important to note moving and non-moving violation stops essentially make up 96% of traffic stops during the temporal domain (Figure G10). Consistent with the recommendations of the report, the proportion of stops that were non-moving violation stops decreased, while the proportion of stops that were moving violation stops increased (e.g. speeding) (Figure G10, Table G5). These results are insensitive to bandwidths near the discontinuity (Figure G11).

G.1 Descriptive analysis

Table G4: Average warning rates and arrest rates by violation type prior to report

Violation Type	**Warning Rate**	**Arrest Rate**
Moving	0.64	0.01
Non-Moving	0.87	0.02
Investigatory	0.69	0.13
Seatbelt	0.67	0.02
Parking	0.75	0.01
Child Seat	0.44	0.01

[28] Consistent with Chohlas-Wood et al. (2018), non-moving violation stops are stops based on vehicle equipment violations, safety violations, and registration violations. According to narrative descriptions associated with the stop data, safety violations appear to be the same as vehicle equipment violations.

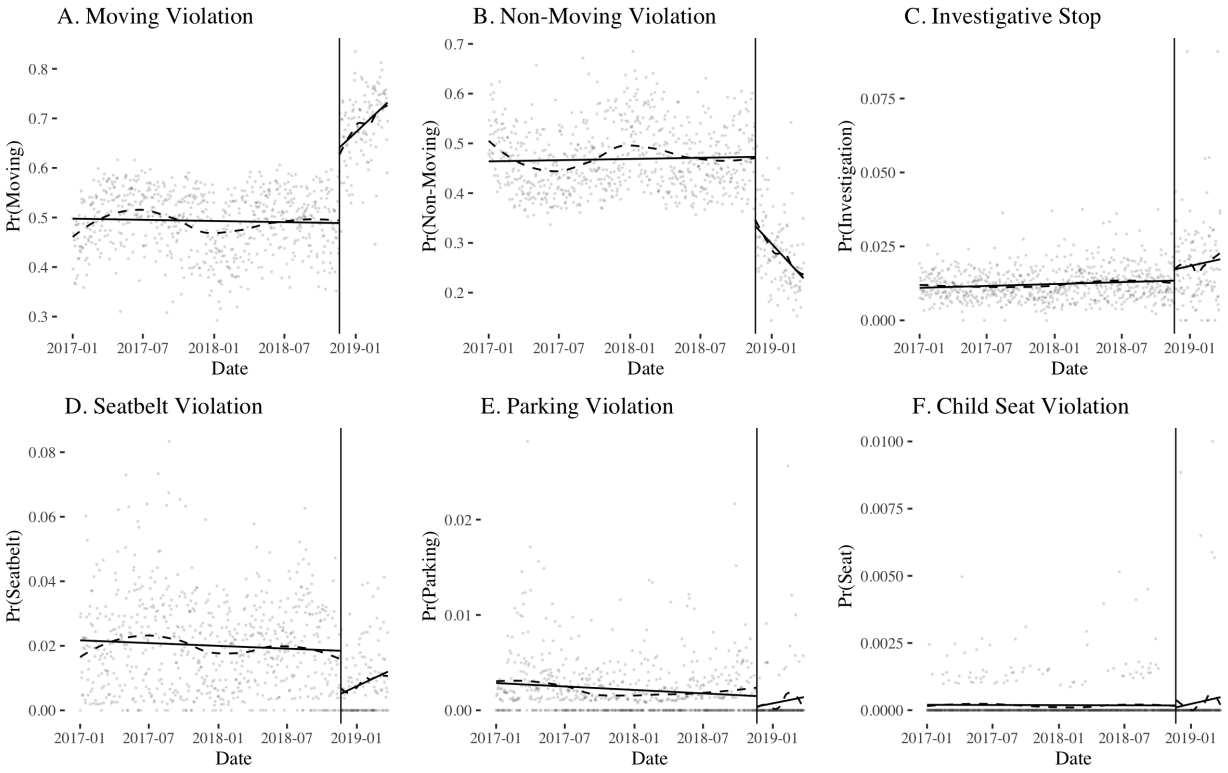


Figure G10: Proportion of stops that are of a particular violation (y-axis) over time (x-axis). Panels A, B, C, D, E, and F correspond to moving, non-moving, investigative, seat belt, parking, and child seat violation stops respectively. The vertical black line denotes the moment the expert report was published. Solid black lines and dashed black lines are linear and loess models fit on each side the moment the expert report was published.

G.2 Using all data

Table G5: Effect of expert report on citation rates using all stop data

Panel A: Moving	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	0.19***	0.15***	0.15***	0.12***	0.16***	0.16***	0.12***	0.12***
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Panel B: Non-Moving	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	−0.18***	−0.14***	−0.13***	−0.11***	−0.15***	−0.15***	−0.11***	−0.11***
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Panel C: Investigative	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	0.006***	0.003	0.002	0.004	0.005***	0.005**	0.003	0.003
	(0.001)	(0.001)	(0.002)	(0.003)	(0.001)	(0.002)	(0.003)	(0.003)
Panel D: Seatbelt	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	−0.013***	−0.014***	−0.014***	−0.015***	−0.008***	−0.008***	−0.008*	−0.007
	(0.001)	(0.002)	(0.003)	(0.004)	(0.002)	(0.002)	(0.003)	(0.004)
Panel E: Parking	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	−0.00***	−0.00	−0.00	−0.00	−0.00	0.00	−0.00	−0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Panel F: Child Seat	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	0.00	−0.00	0.00	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
N	463903	463903	463903	463903	463903	463903	463903	463903
Degree	0	1	2	3	0	1	2	3
Controls	N	N	N	N	Y	Y	Y	Y

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

G.3 Close to bandwidth

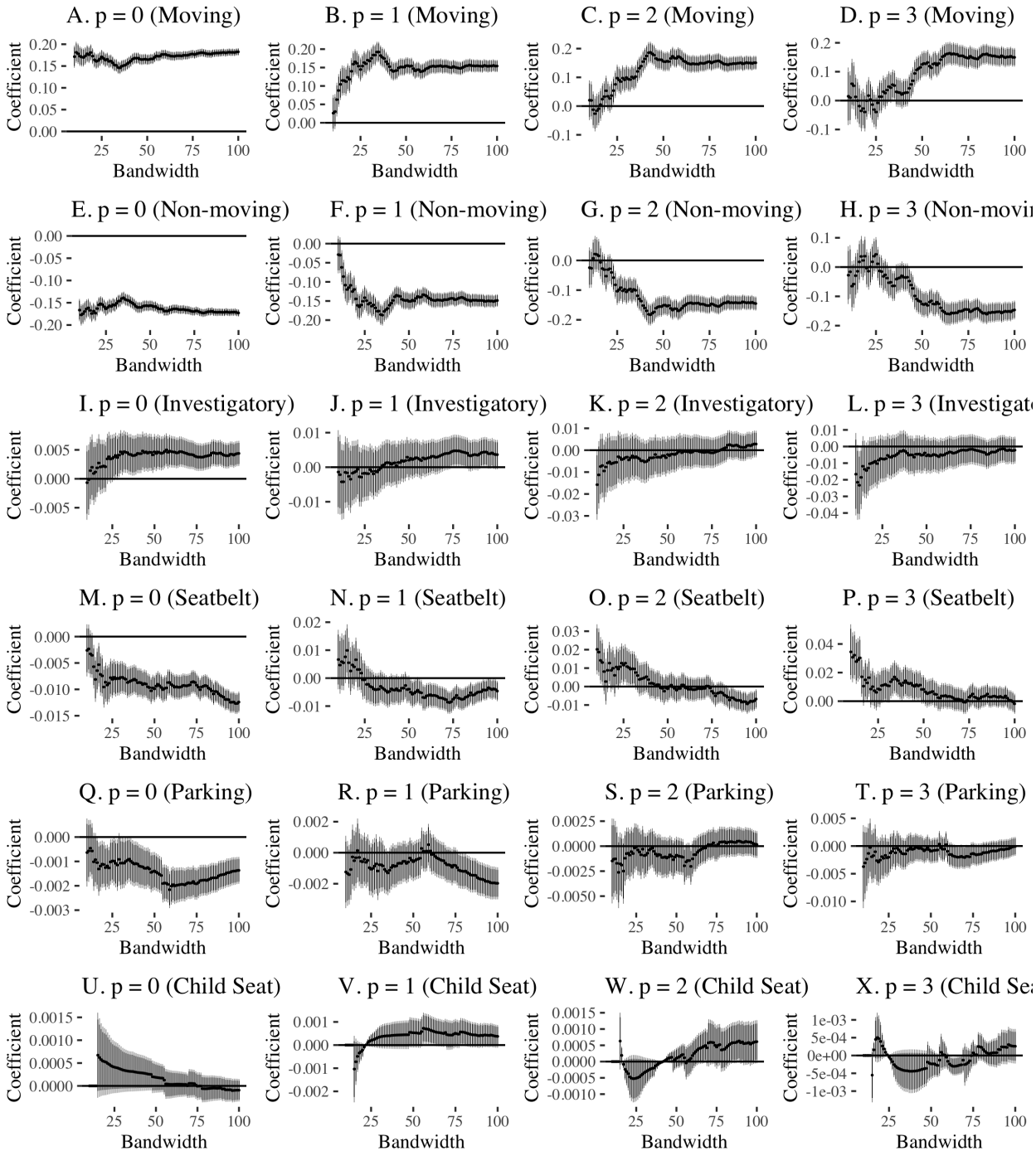


Figure G11: Effect of expert report on violation types (y-axis) across bandwidths (x-axis).

H Heterogeneity by race

Here, we assess the effect of the report on outcomes of interest by ethno-race. We estimate the effect of the report conditional on the driver being white, and use 3 different reference categories: non-white, Black, and Latinx. Tables H6, H7, and H8 along with Figures H12, H13, and H14 demonstrate the report did not have heterogenous effects on the outcome by driver race. These findings suggest the rate of unnecessary stops reduced across the board for all ethno-racial groups, but deferentially reduce the rate of unnecessary stops for one group over another.

The absence of heterogeneous effects may be because disparities in some stop outcomes of interest did not exist prior to the report, at least for some groups. For example, the warning rate for whites, non-whites, Black people, and Latinxs was 76%, 73%, 74%, and 63% respectively.[29]

H.1 Using all data

H.1.1 Citation rates

Table H6: Effect of expert report on citation rates using all stop data

Panel A: Non-White Reference	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report x White	0.05***	−0.01	0.01	−0.02	0.04***	−0.02	0.01	−0.02
	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)
N	463903	463903	463903	463903	463903	463903	463903	463903
Panel B: Black Reference	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report x White	0.05***	−0.01	0.01	−0.02	0.05***	−0.01	0.01	−0.02
	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)
N	420178	420178	420178	420178	420178	420178	420178	420178
Panel C: Latinx Reference	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report x White	0.03*	−0.04	0.00	−0.01	0.03*	−0.05*	−0.00	−0.02
	(0.01)	(0.02)	(0.03)	(0.04)	(0.01)	(0.02)	(0.03)	(0.04)
N	280066	280066	280066	280066	280066	280066	280066	280066
Degree	0	1	2	3	0	1	2	3
Controls	N	N	N	N	Y	Y	Y	Y

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

[29]However, the arrest rate by ethno-racial subgroup prior to the report was 1%, 2.4%, 2.5% and 2.5% for whites, non-whites, Black people, and Latinxs respectively. These disparities between whites and non-whites in pre-report arrest marginals should not influence the result, however, given the report generated an efficiency effect that effectively increases the arrest rate for traffic stops.

H.1.2 Arrest rates

Table H7: Effect of expert report on arrest rates using all stop data

Panel A: Non-White Reference	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report x White	−0.00*	−0.01	−0.00	−0.01	−0.00*	−0.01	−0.00	−0.01
	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)
N	463903	463903	463903	463903	463903	463903	463903	463903
Panel B: Black Reference	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report x White	−0.01**	−0.01	−0.00	−0.01	−0.00**	−0.01	−0.00	−0.01
	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)
N	420178	420178	420178	420178	420178	420178	420178	420178
Panel C: Latinx Reference	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report x White	−0.01	−0.01	0.01	0.00	−0.01	−0.01	0.01	0.00
	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)
N	280066	280066	280066	280066	280066	280066	280066	280066
Degree	0	1	2	3	0	1	2	3
Controls	N	N	N	N	Y	Y	Y	Y

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

H.1.3 Warning rates

Table H8: Effect of expert report on warning rates using all stop data

Panel A: Non-White Reference	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report x White	−0.04***	0.02	−0.00	0.02	−0.04***	0.02	−0.00	0.03
	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)
N	463903	463903	463903	463903	463903	463903	463903	463903
Panel B: Black Reference	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report x White	−0.05***	0.02	−0.00	0.03	−0.05***	0.02	−0.00	0.03
	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)
N	420178	420178	420178	420178	420178	420178	420178	420178
Panel C: Latinx Reference	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report x White	−0.02*	0.05*	−0.01	0.01	−0.02	0.06*	−0.01	0.01
	(0.01)	(0.02)	(0.03)	(0.04)	(0.01)	(0.02)	(0.03)	(0.04)
N	280066	280066	280066	280066	280066	280066	280066	280066
Degree	0	1	2	3	0	1	2	3
Controls	N	N	N	N	Y	Y	Y	Y

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

H.2 Data near bandwidth

H.2.1 Citation rates

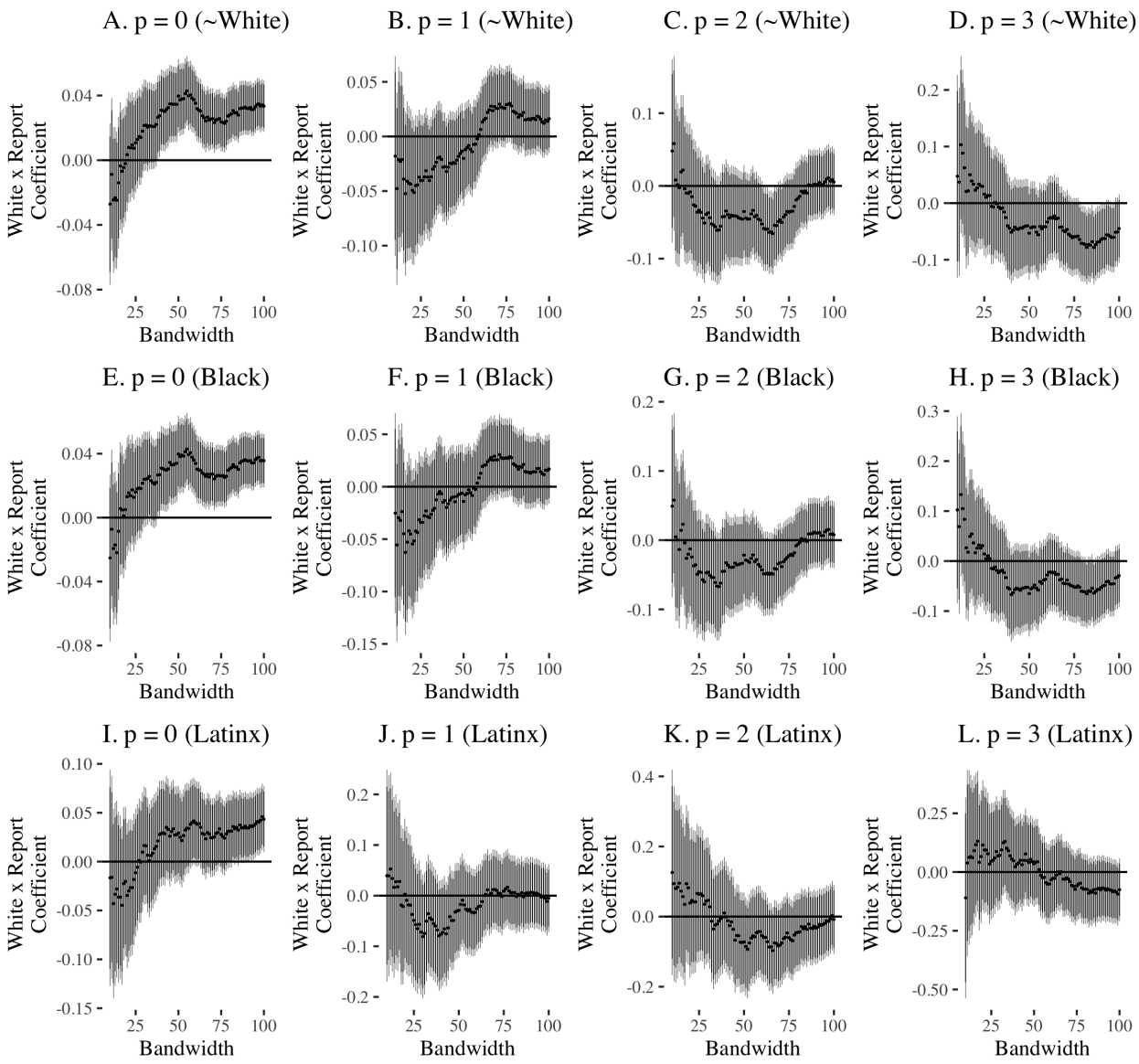


Figure H12: Heterogeneous effect of expert report on citation rates conditional on white driver race (y-axis) across bandwidths (x-axis). Every 4 panels varies the degree of the running variable from 0-3. The top 4 panels have non-whites as the reference category. The middle 4 panels have Black people as the reference category. The bottom 4 panels have Latinxs as the reference category.

H.2.2 Arrest rates

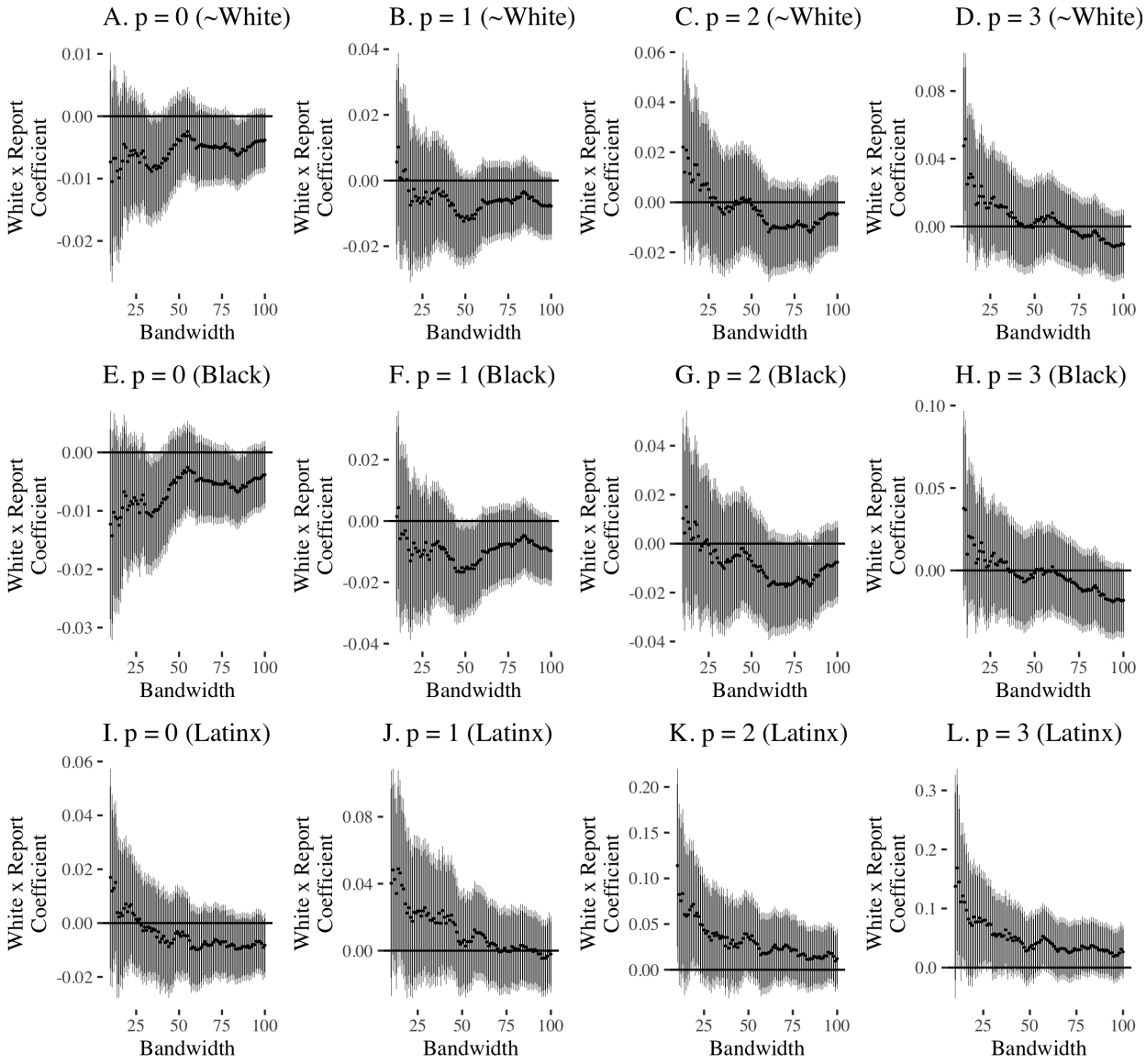


Figure H13: Heterogeneous effect of expert report on arrest rates conditional on white driver race (y-axis) across bandwidths (x-axis). Every 4 panels varies the degree of the running variable from 0-3. The top 4 panels have non-whites as the reference category. The middle 4 panels have Black people as the reference category. The bottom 4 panels have Latinxs as the reference category.

H.2.3 Warning rates

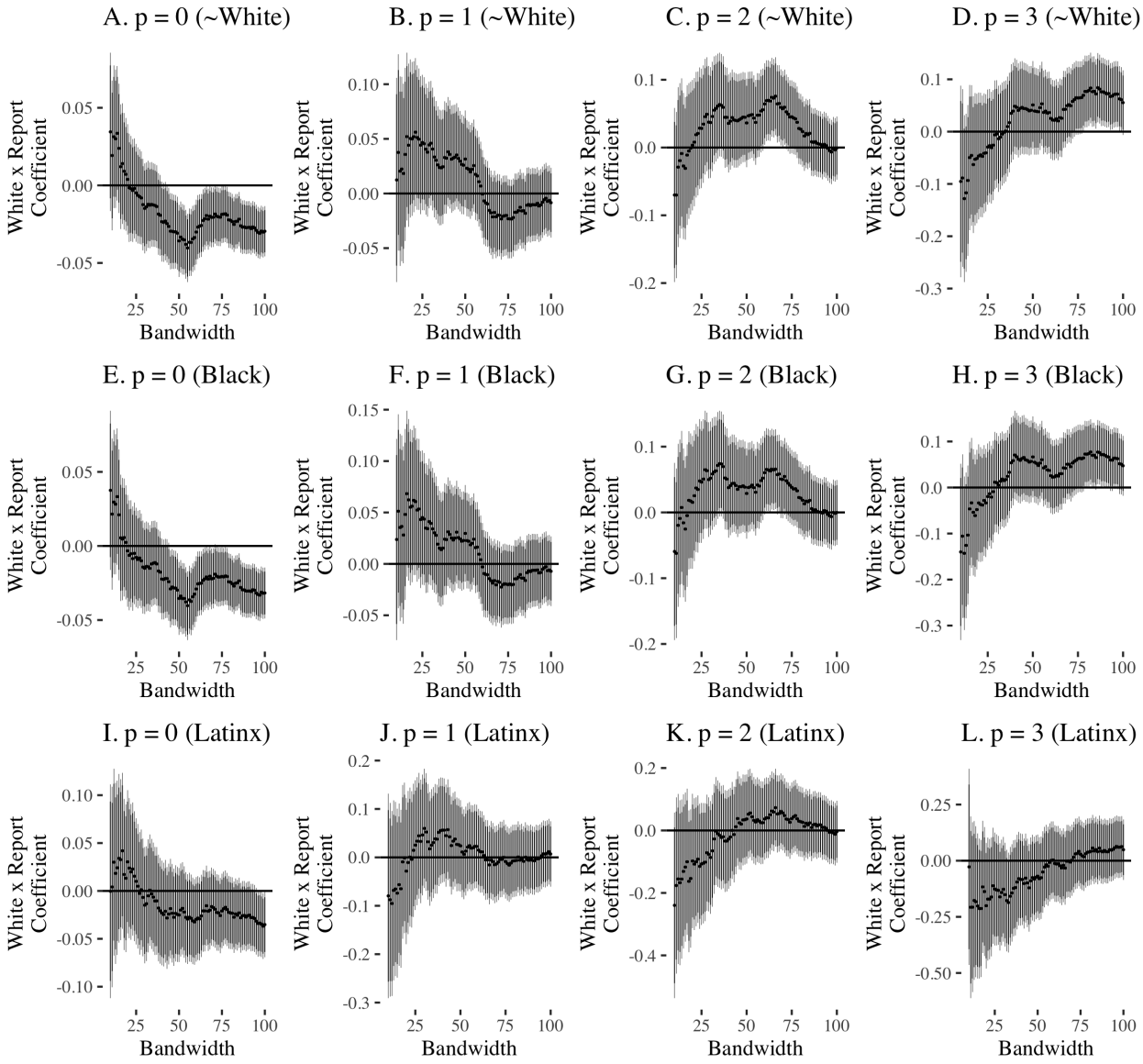


Figure H14: Heterogeneous effect of expert report on arrest rates conditional on white driver race (y-axis) across bandwidths (x-axis). Every 4 panels varies the degree of the running variable from 0-3. The top 4 panels have non-whites as the reference category. The middle 4 panels have Black people as the reference category. The bottom 4 panels have Latinxs as the reference category.

I Stop rates by ethno-racial group

To estimate the effect of the report on stop rates by ethno-racial group, we geocode each traffic stop to its respective census block group in Davidson County (MNPD's jurisdiction) using latitude/longtiude information in the stop data. We construct a block-group/day dataset and generate stop-rates for each ethno-racial group (white, Black, Latinx) per day, equivalent to the number of stops divided by the driving age population for a respective ethno-racial group. Given there are no driving age data by race, we use a strategy similar to Chohlas-Wood et al. (2018), where we multiply the driving age population by the overall race proportions.

Aggregating the stop rate data to the daily levels, we find stop rates appear to decrease across the board for *all* ethno-racial groups (Figures I17 and I18, Table I11). We generate daily rate ratios between Black people and whites (Figure I16, Panel A) in addition to Latinxs and whites (Figure I16, Panel B). Descriptively, there appears to be no decrease in the rate ratio after the report (Figure I16). Likewise, regression discontinuity estimates suggest there was no decrease in the stop rate ratio between whites and Black people or whites and Latinxs (Table I10). These findings suggest that although stop rates decreased overall, they did not reduce stop rate disparities between white and non-white drivers.

It is possible MNPD understood it was important to reduce the level of policing, but not the ethno-racial disparity in policing. MNPD police chief Steve Anderson recognized the need to reduce non-moving violation stops in response to the report, indicating,

> "Nevertheless, I appreciate the finding of the researchers that vehicle stops for non-moving equipment violations and the police visibility related to them do not appear to be having a significant impact on short-term or long-term crime trends."[30]

However, the chief dismissed findings on racial disparities, making the case that more policing occurs in non-white communities because more crime occurs in those communities, indicating

> "The MNPD has consistently acknowledged for years that black drivers have been stopped at a higher rate than white drivers relative to the respective black and white driving age populations. Officer deployment, that is assigning larger numbers of MNPD officers in high crime areas and crime hotspots, is certainly a factor in the disparity. And, as today's report observes, "Disparity... is not necessarily evidence of discrimination." Weekly data-driven analyses inform our eight precinct commanders where and when crime is occurring, along with where and when citizens are calling for MNPD assistance. Based on these analyses, this police department prioritizes and assigns our finite resources to areas of need, regardless of racial demographics or socio-economic status. So long as crime victimizes citizens and families in some of our city's most vulnerable areas– communities most often in transition and impacted by gentrification, housing

[30] "MNPD Chief Steve Anderson responds to Policing Project study on traffic stops." USA Today Network - Tennessee. November 19, 2018. `https://tinyurl.com/y57pyldx`

issues or lack of economic development–the police will necessarily be staffed in these areas with the primary goal of reducing victimization."[31]

Although the Policing Project report certainly notes, "Disparity... is not necessarily evidence of discrimination," it appears that MNPD chief Anderson missed the report also indicates that, "even controlling for crime, unexplained racial disparity still remains." Therefore, perhaps the reason there is a decrease in the level of stops but not stop disparities by ethno-racial group is because the MNPD was willing to acknowledge the level of stops was excessive, but refuted the notion the disparity in stops was not justifiable on the basis of differential crime rates across communities.

I.1 Descriptive statistics, stop rate

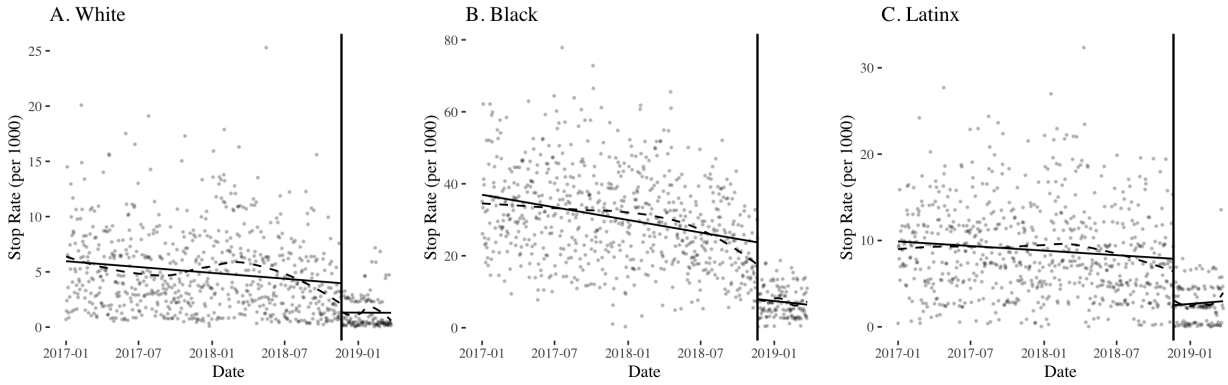


Figure I15: Traffic stop rate (y-axis per 1,000 driving-age individuals from a specific group) over time (x-axis). Panels A, B and C denote the white, Black, and Latinx stop rate respectively. Vertical line denotes the time the expert report is released. Loess (dashed) and linear (solid) models fit on each side the moment the report is released.

[31] "Study finds 'notable racial disparities in traffic stops' in Nashville." News Channel 5 - Nashville. November 19, 2018. `https://tinyurl.com/y2aw9ngc`

I.2 Descriptive statistics, stop rate ratio

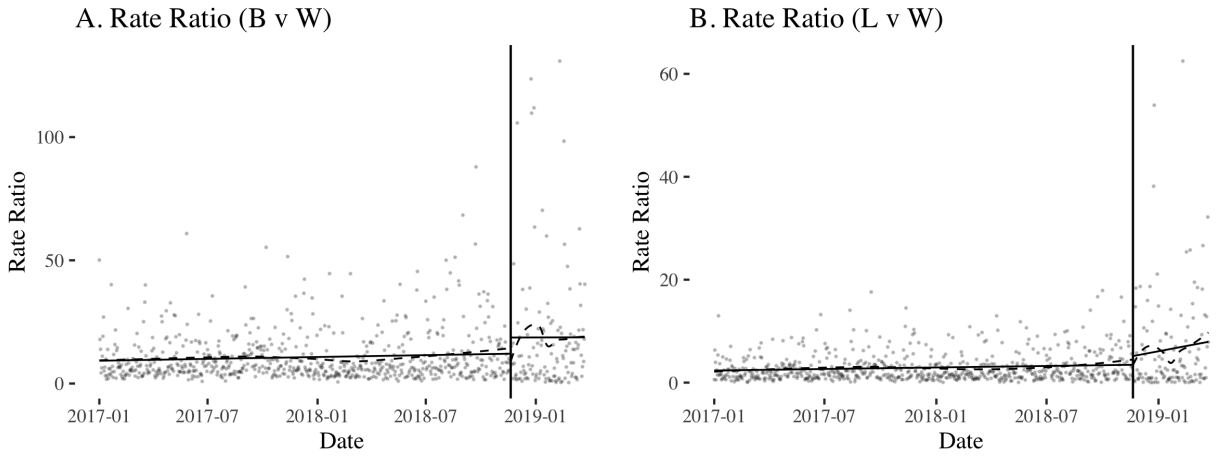


Figure I16: Traffic stop rate ratio (y-axis) over time (x-axis). Panels A, B and C denote the Black/white, and Latinx/white stop rate ratio respectively. Vertical line denotes the time the expert report is released. Loess (dashed) and linear (solid) models fit on each side the moment the report is released.

I.3 Descriptive statistics, stop rate difference

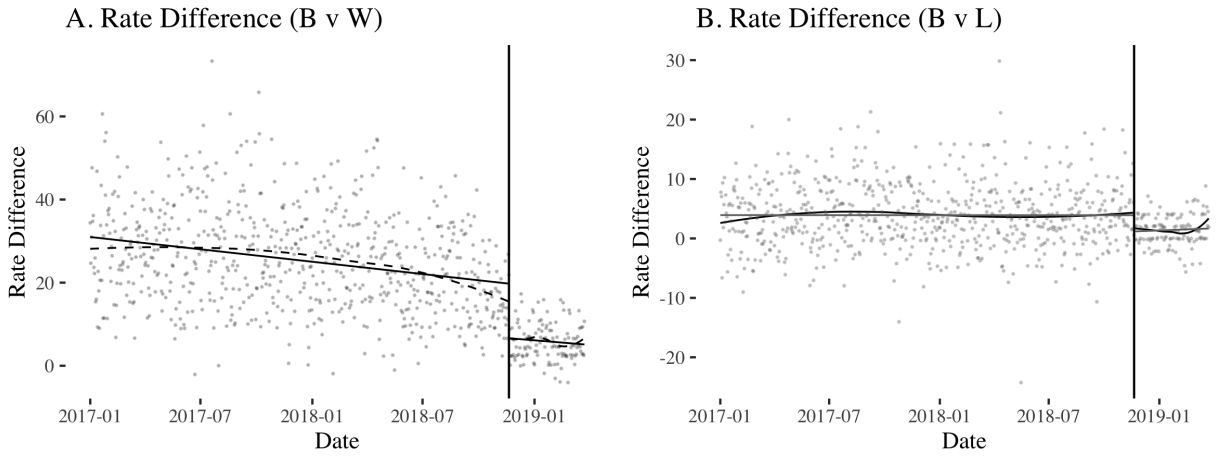


Figure I17: Traffic stop difference (y-axis) over time (x-axis). Panels A, B and C denote the Black - white, and Latinx - white stop rate difference respectively. Vertical line denotes the time the expert report is released. Loess (dashed) and linear (solid) models fit on each side the moment the report is released.

I.4 Full data analysis, stop rates

Table I9: Effect of expert report on stop rates by group using all stop data

Panel A: White stop rate	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	-3.66^{*}	-2.65^{**}	-2.33^{***}	-0.54	-2.50^{*}	-2.60^{*}	-1.14^{***}	-1.36^{***}
	(1.59)	(0.94)	(0.70)	(0.63)	(1.08)	(1.08)	(0.29)	(0.41)
Pre-Report μ	4.97	4.97	4.97	4.97	4.97	4.97	4.97	4.97
Panel B: Black stop rate	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	-23.16^{**}	-15.78^{**}	-11.93^{**}	-9.91^{*}	-12.97^{**}	-13.08^{**}	-7.57^{*}	-7.88^{**}
	(8.12)	(5.14)	(3.91)	(4.33)	(4.42)	(4.78)	(2.97)	(2.99)
Pre-Report μ	30.34	30.34	30.34	30.34	30.34	30.34	30.34	30.34
Panel C: Latinx stop rate	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	-6.15^{***}	-5.39^{***}	-3.68^{***}	-3.71^{**}	-3.97^{***}	-4.30^{***}	-2.39^{**}	-4.60^{***}
	(1.16)	(1.06)	(0.93)	(1.20)	(0.94)	(1.00)	(0.91)	(1.20)
Pre-Report μ	8.9	8.9	8.9	8.9	8.9	8.9	8.9	8.9
N	382110	382110	382110	382110	382110	382110	382110	382110
Degree	0	1	2	3	0	1	2	3
Controls	N	N	N	N	Y	Y	Y	Y

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. Models 1-4 assess the discontinuous effect of the project policing report on the stop rates per 1000 people for white, Black, and Latinx subgroups respectively (Panel A, B and C) without controls using all Nashville PD stop data after 2017-01-01. Models 5-8 do the same adjusting for year, month, day-of-week fixed effects in addition to the lagged outcome. All models specify varying degrees for the running variable from 0 (a simple difference-in-means) to 3. Robust standard errors clustered at block group level in parentheses.

I.5 Full data analysis, stop rate ratios

Table I10: Effect of expert report on stop rate ratios by group using all stop data

Panel A: Black/white rate ratio	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	8.04***	6.57*	3.19	−3.70	6.57*	3.93	−4.83	−4.48
	(1.35)	(2.69)	(4.00)	(5.27)	(2.93)	(3.17)	(4.72)	(5.62)
Panel B: Latinx/white rate ratio	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	3.64***	1.69	2.71*	−0.29	2.92**	1.45	−0.02	−1.49
	(0.45)	(0.89)	(1.33)	(1.75)	(0.98)	(1.06)	(1.58)	(1.87)
N	813	813	813	813	813	813	813	813
Degree	0	1	2	3	0	1	2	3
Controls	N	N	N	N	Y	Y	Y	Y

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. Models 1-4 assess the discontinuous effect of the project policing report on the stop rate ratios for Black people and whites, and Latinxs and whites. (Panels A and B) without controls using all Nashville PD stop data after 2017-01-01. Models 5-8 do the same adjusting for year, month, day-of-week fixed effects in addition to the lagged outcome. All models specify varying degrees for the running variable from 0 (a simple difference-in-means) to 3. Robust standard errors in parentheses.

I.6 Full data analysis, stop rate differences

Table I11: Effect of expert report on stop rate differences by group using all stop data

Panel A: Black/white rate difference	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	-19.50^{***}	-13.13^{***}	-9.60^{**}	-9.36^{*}	-10.60^{***}	-10.44^{***}	-6.00	-5.95
	(1.09)	(2.10)	(3.11)	(4.10)	(2.32)	(2.52)	(3.69)	(4.40)
Panel B: Latinx/white rate difference	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	-17.00^{***}	-10.39^{***}	-8.25^{**}	-6.19	-9.35^{***}	-8.96^{***}	-4.95	-2.87
	(1.09)	(2.09)	(3.09)	(4.07)	(2.31)	(2.51)	(3.69)	(4.39)
N	813	813	813	813	813	813	813	813
Degree	0	1	2	3	0	1	2	3
Controls	N	N	N	N	Y	Y	Y	Y

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. Models 1-4 assess the discontinuous effect of the project policing report on the stop rate differences for Black people and whites, and Latinxs and whites. (Panels A and B) without controls using all Nashville PD stop data after 2017-01-01. Models 5-8 do the same adjusting for year, month, day-of-week fixed effects in addition to the lagged outcome. All models specify varying degrees for the running variable from 0 (a simple difference-in-means) to 3. Robust standard errors in parentheses.

I.7 Analysis near bandwidth, stop rates

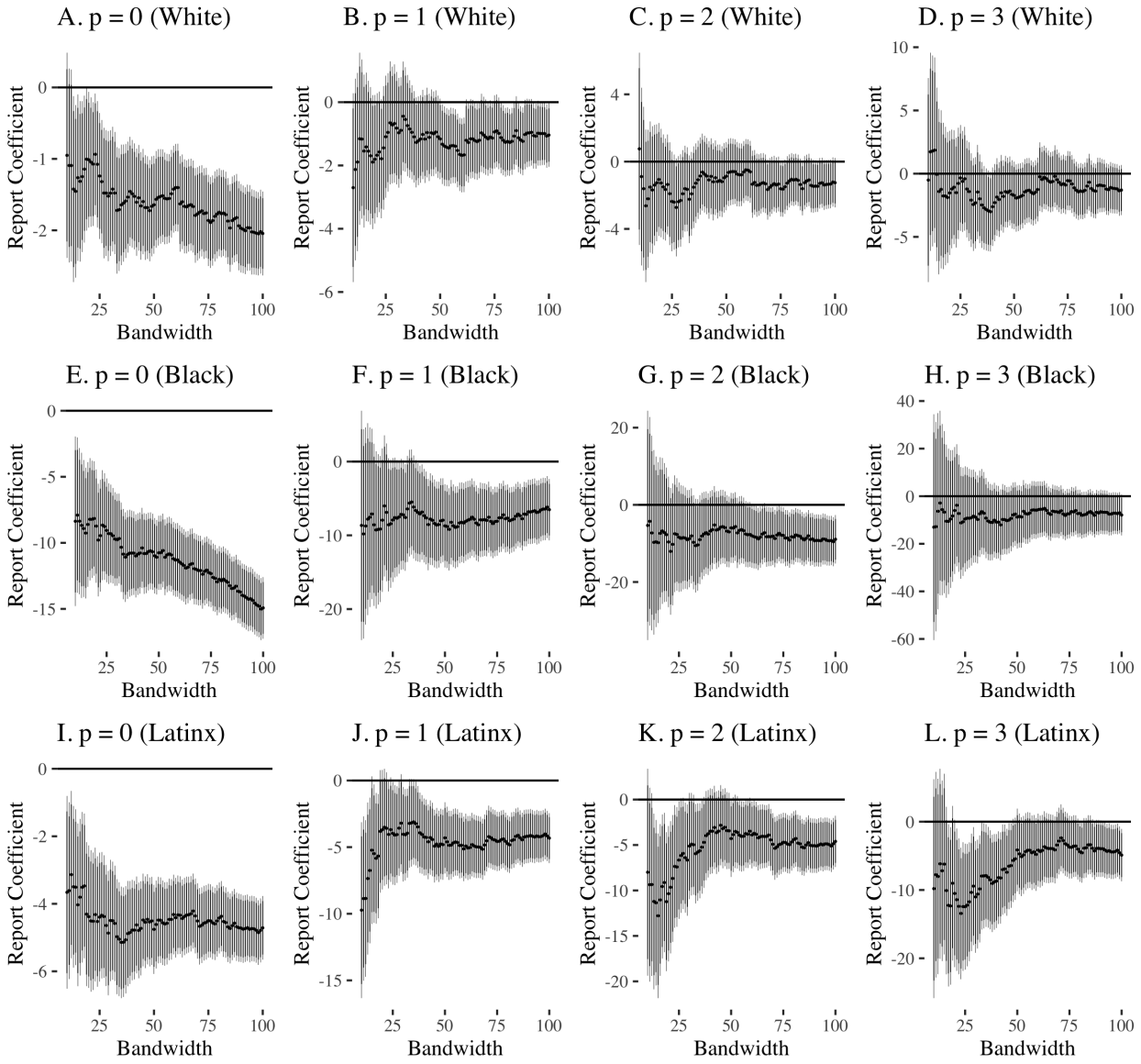


Figure I18: Effect of expert report on stop rates by group (y-axis) across bandwidths (x-axis). Every 4 panels varies the degree of the running variable from 0-3. The top 4 panels are stop rates for whites. The middle 4 panels are stop rates for Black people. The bottom 4 panels are stop rates for Latinxs. Confidence intervals are from robust standard errors clustered at the block group level.

J Traffic outcomes

Here, we display the effect of the Policing Project report on two outcomes, traffic accidents and the rate of hit-and-runs conditional on an accident. Figure J19 graphically displays the report did not change the number of traffic accidents nor the hit-and-run rate. Regression discontinuity estimates using all accident data and accident data near the time the Policing Project report was released demonstrates the report, and subsequent depolicing, had no negative consequences on traffic outcomes (Table J12 and Figure J20).

These findings not only demonstrate depolicing in the form of reduced traffic stops did not increase public safety risk, they also demonstrate endogenous driver behavior in response to the report is not a confounder. Drivers were not incognizant of excessive traffic stops before the report. If driver behavior was becoming more responsible afterwards, we ought to detect less hit and runs and accidents. We find both hit and runs and accidents are not affected at the discontinuity. Thus, the reduction in stops does not appear to be a function of more cautious motorists.

J.1 Graphical representation

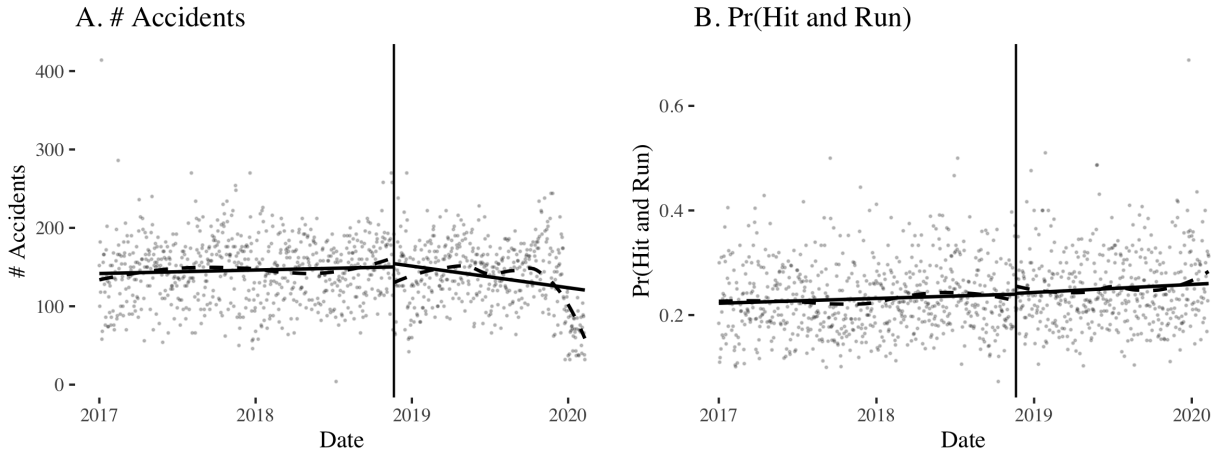


Figure J19: Traffic counts (y-axis, Panel B) and hit and run rates (y-axis, Panel B) over time (x-axis). Vertical line denotes the time the expert report is released. Loess (dashed) and linear (solid) models fit on each side the moment the report is released.

J.2 Full data analysis

Table J12: Effect of expert report on traffic accident counts

Panel A: Traffic Accidents	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	9.25***	−3.94	−28.22***	2.39	−8.03	−7.76	−9.73	−7.51
	(2.13)	(3.91)	(5.77)	(7.62)	(4.80)	(4.79)	(5.28)	(6.31)
Panel B: Pr(Hit and Run)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	0.04***	0.01	0.01	0.01	0.01	0.01	0.01	0.01
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
N	2962	2962	2962	2962	2961	2961	2961	2961
Degree	0	1	2	3	0	1	2	3
Controls	N	N	N	N	Y	Y	Y	Y

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. Models 1-4 assess the discontinuous effect of the project policing report on the raw number of traffic accidents (Panel A) and the probability of a hit and run (Panel B) without controls using all Nashville PD stop data after 2017-01-01. Models 5-8 do the same adjusting for year, month, day-of-week fixed effects in addition to the lagged outcome. All models specify varying degrees for the running variable from 0 (a simple difference-in-means) to 3. Robust standard errors in parentheses.

J.3 Analysis near bandwidth

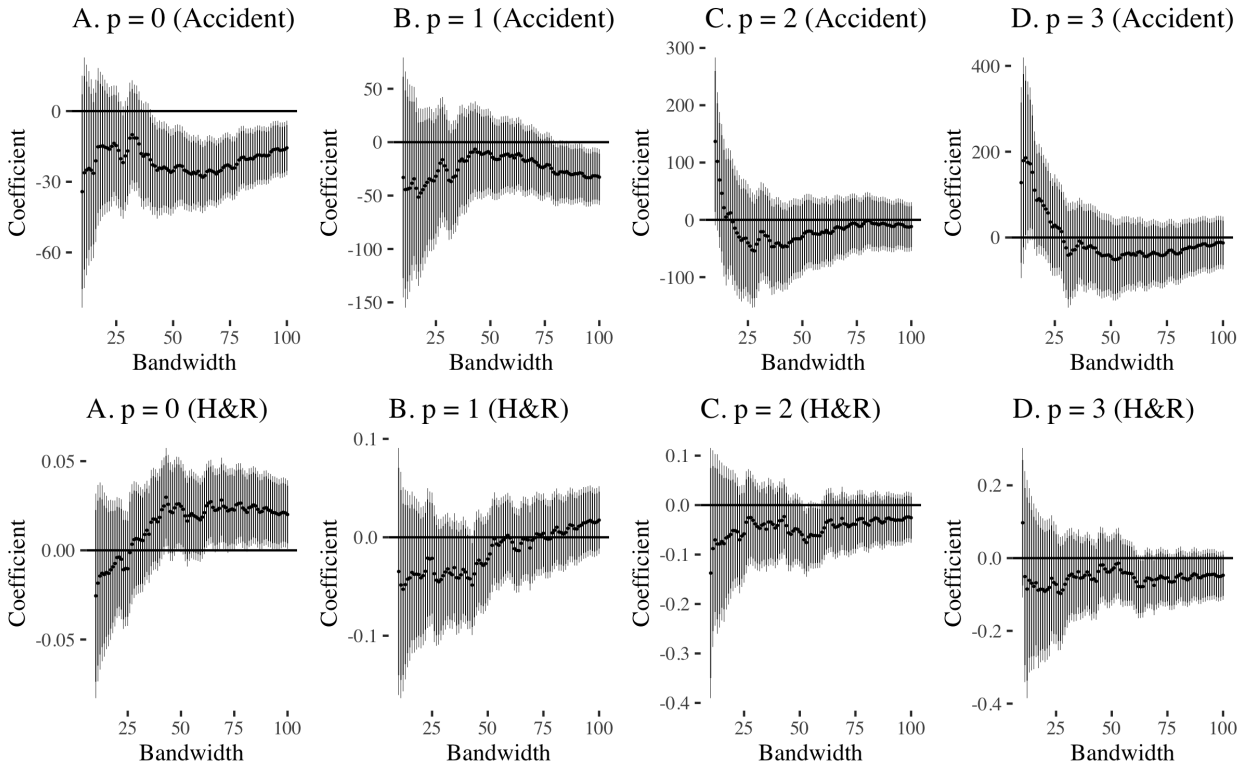


Figure J20: Effect of expert report on accident counts (y-axis, top 4 panels) and hit and runs (y-axis, bottom 4 panels) across bandwidths (x-axis). Panels A, B, C, and D vary the degree the running variable is at from 0-3 respectively.

K Crime outcomes

Here we display regression discontinuity estimates on the effect of the policing project report on crime. Table K13 demonstrates the policing project report did not increase or decrease crime using all of the crime data under the most conservative specifications (i.e. those with temporal fixed effects), with the exception of against society crime. Estimates using only data close to the discontinuity suggest similar conclusions (Figure K21).

K.1 Full data analysis

Table K13: Effect of expert report on crime

Panel A: All crimes	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	-23.31^{***}	-46.42^{***}	-78.12^{***}	-40.83^{**}	-0.22	6.75	-5.37	-14.69
	(3.73)	(7.43)	(11.02)	(14.23)	(10.02)	(10.23)	(12.69)	(15.01)
Panel B: Against person	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	-2.58	-6.28^{*}	-19.56^{***}	-17.26^{**}	4.21	6.42	5.39	0.13
	(1.37)	(2.74)	(4.06)	(5.37)	(3.55)	(3.63)	(4.51)	(5.33)
Panel C: Property	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	12.58^{***}	-19.15^{***}	-40.54^{***}	-14.26	11.83	14.67^{*}	-2.58	0.09
	(2.60)	(5.06)	(7.54)	(9.68)	(6.94)	(7.12)	(8.79)	(10.40)
Panel D: Society	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	-33.51^{***}	-20.78^{***}	-18.01^{**}	-9.40	-14.25^{***}	-12.31^{**}	-7.22	-14.04^{*}
	(2.02)	(4.01)	(5.99)	(7.97)	(4.21)	(4.29)	(5.29)	(6.26)
N	1096	1096	1096	1096	1096	1096	1096	1096
Degree	0	1	2	3	0	1	2	3
Controls	N	N	N	N	Y	Y	Y	Y

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. Models 1-4 assess the discontinuous effect of the project policing report on the raw number of crimes without controls using all Nashville PD crime data after 2017-01-01. Models 5-8 do the same adjusting for year, month, day-of-week fixed effects in addition to the lagged outcome count. All models specify varying degrees for the running variable from 0 (a simple difference-in-means) to 3. Robust standard errors in parentheses.

K.2 Analysis near bandwidth

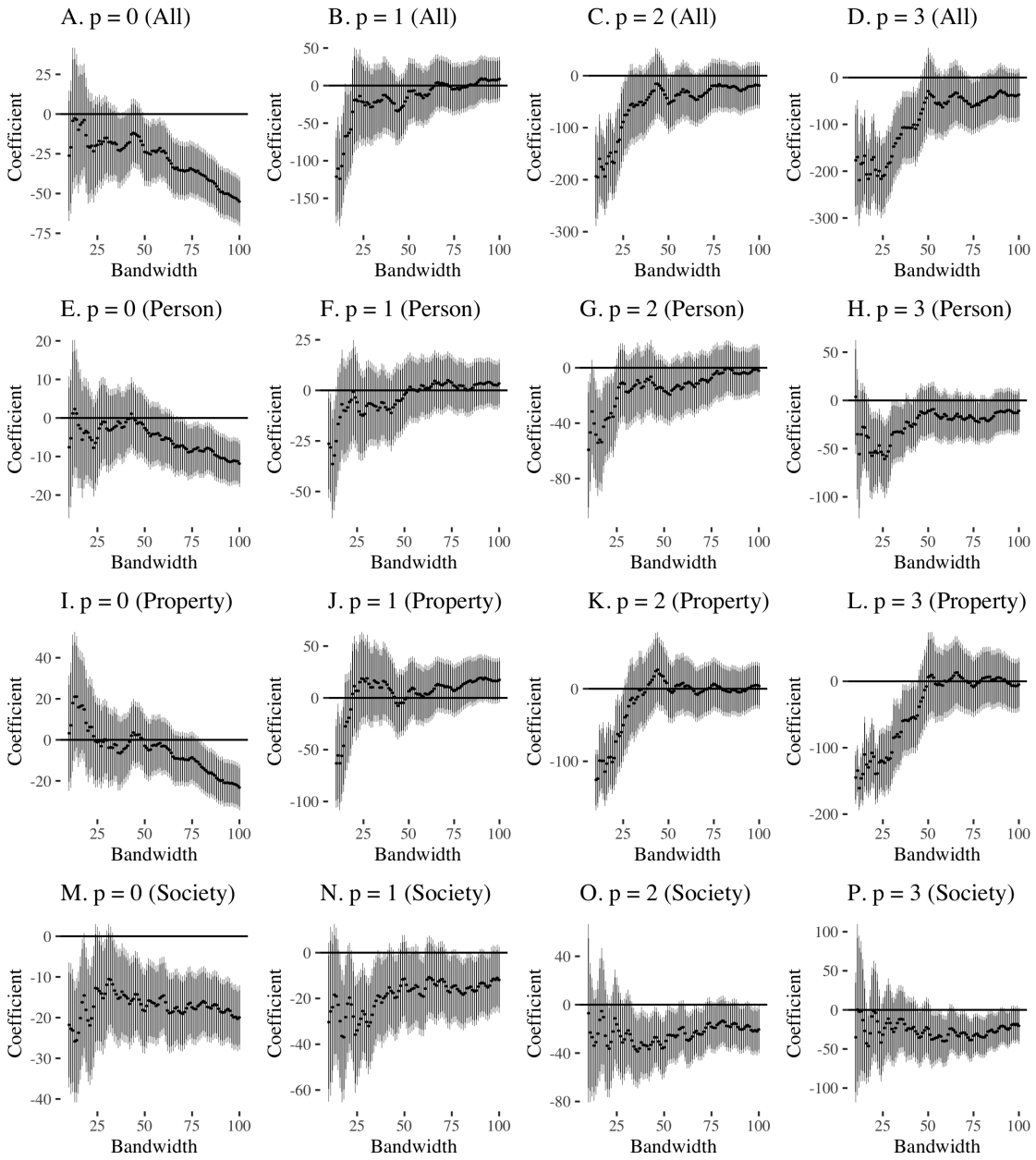


Figure K21: Effect of expert report on crimes (y-axis) across bandwidths (x-axis). Every 4 panels varies the degree of the running variable from 0-3. The top 4 panels have all crimes as the outcome. The next 4 panels have crimes against persons as the outcome. The next 4 panels have crimes against property as the outcome. The last 4 panels have crimes against society as the outcome.

L Reanalyzing arrests without citations

L.1 Using all data

Table L14: Effect of expert report on outcomes of interest using all stop data

	Arrest Rate							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	0.013***	0.012***	0.014***	0.013**	0.013***	0.012***	0.011**	0.010*
	(0.001)	(0.002)	(0.004)	(0.005)	(0.002)	(0.003)	(0.004)	(0.005)
Pre-Report μ	0.02	0.02	0.02	0.02	0.02	0.02	0.02	0.02
N	351129	351129	351129	351129	351129	351129	351129	351129
Degree	0	1	2	3	0	1	2	3
Controls	N	N	N	N	Y	Y	Y	Y

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. Models 1-4 assess the discontinuous effect of the project policing report on the arrest rate without controls using all Nashville PD stop data after 2017-01-01 excluding citation stops. Models 5-8 do the same adjusting for year, month, day-of-week fixed effects in addition to the lagged outcome. All models specify varying degrees for the running variable from 0 (a simple difference-in-means) to 3. Robust standard errors in parentheses.

L.2 Analysis near bandwidth

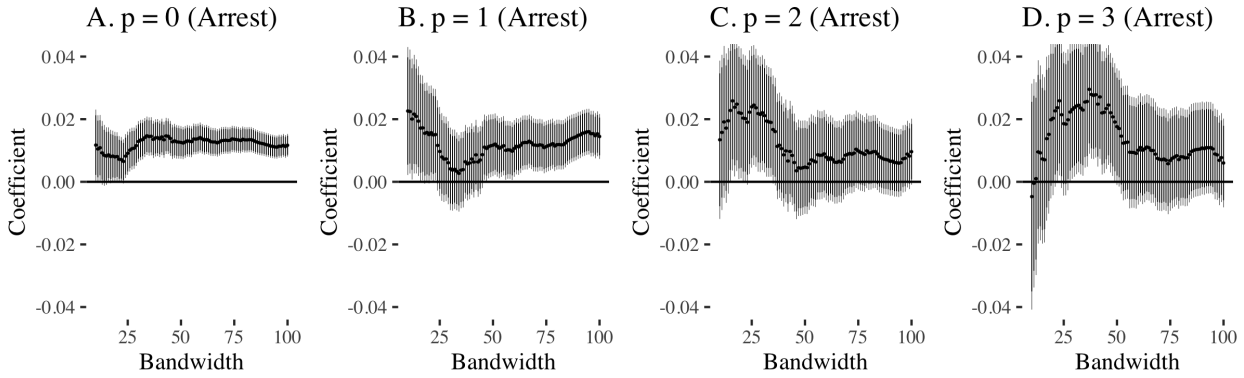


Figure L22: Effect of expert report on arrest rates (y-axis) across bandwidths (x-axis) (excluding citation data). Panels A, B, C, and D vary the degree the running variable is at from 0-3 respectively.

M Temporal placebo tests

Here, we determine whether the effects we derive for the report are different than a random distribution of discontinuous effects by estimating the discontinuous effect of placebo estimates for each day prior to the release of the report. When we use all of the data (Figure M23), we estimate the effect of all days between 30 days after January 1, 2017 and 30 days prior to November 20, 2018 while censoring all post-treatment data after November 20, 2018 to prevent confounding based on the effects of the report. When we derive placebo estimates with a 50 day bandwidth, we conduct the same exercise, but instead estimate the effect of all days between 50 days after January 1, 2017 and 50 days prior to November 20, 2018 while censoring all post-treatment data after November 20, 2018. Finally, when we derive placebo estimates with a 25 day bandwidth, we conduct the same exercise, but instead estimate the effect of all days between 25 days after January 1, 2017 and 25 days prior to November 20, 2018 while censoring all post-treatment data after November 20, 2018.

With few exceptions, the effect of the report with respect to the citation, warning, and arrest rate is almost always outside of the 95% confidence intervals that characterize the distribution of placebo effects. This suggests the effects of the report are distinct from random chance and that the day we use as the onset of the report's effects on depolicing (November 20, 2018) reasonable captures the point at which the report affects police behavior.

M.1 Using all data

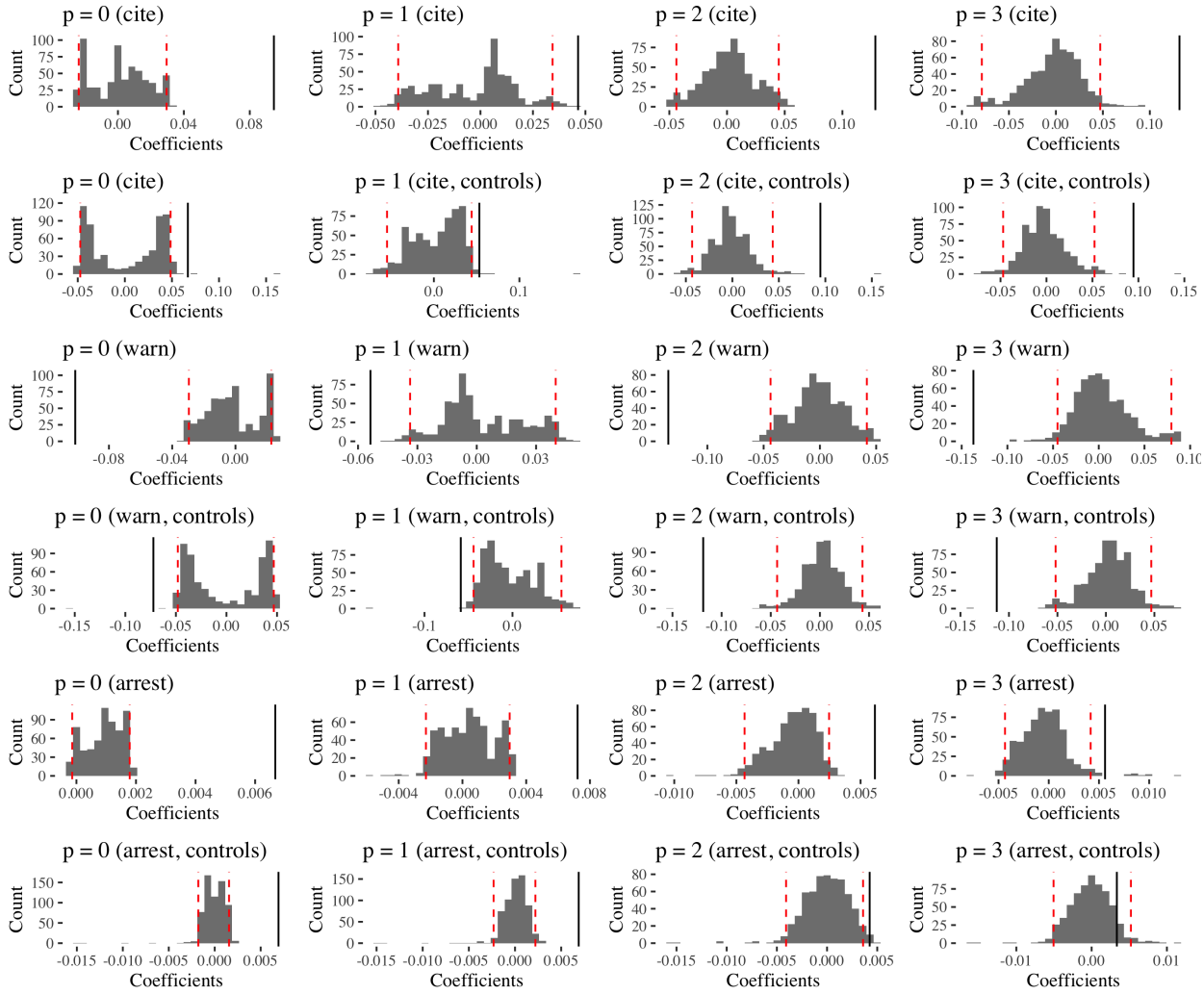


Figure M23: Pre-report temporal placebo tests across outcomes, polynomial degrees, and the inclusion of control covariates using all stop data. The first two rows display the distribution of temporal placebo estimates for the citation outcome. The third and fourth rows display the distribution of temporal placebo estimates for the warning outcome. The fifth and sixth rows display the distribution of temporal placebo estimates for the arrest outcome. The vertical black line is the true estimate of the effect. The dashed red lines characterizes the 95% confidence interval for the placebo estimates.

M.2 Using data 50 days from discontinuity

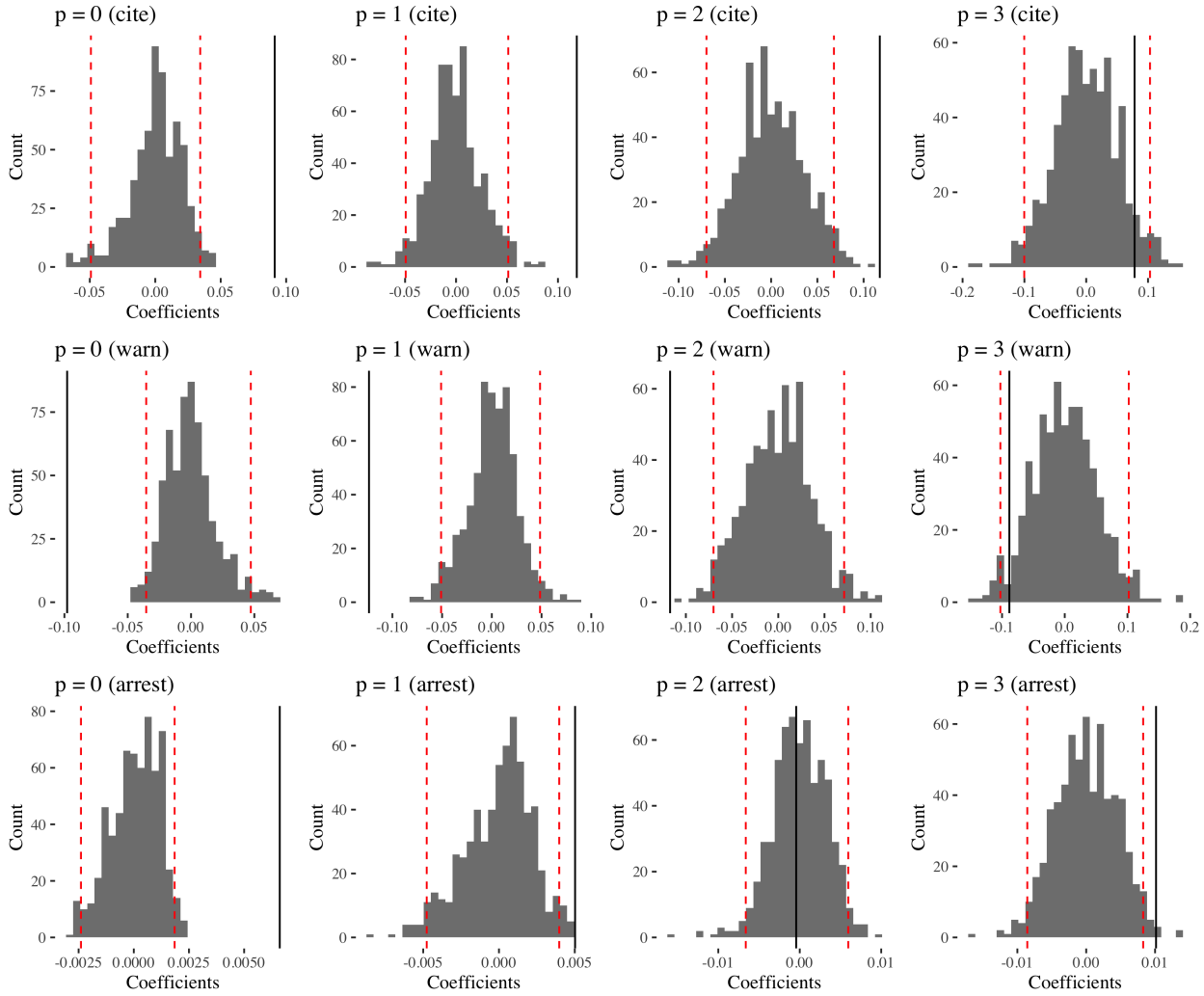


Figure M24: Pre-report temporal placebo tests across outcomes, polynomial degrees, and the inclusion of control covariates using stop data 50 days from the report's release. The first, second, and third rows display the distribution of temporal placebo estimates for the citation, warning, and arrest outcomes respectively. The vertical black line is the true estimate of the effect. The dashed red lines characterizes the 95% confidence interval for the placebo estimates.

M.3 Using data 25 days from discontinuity

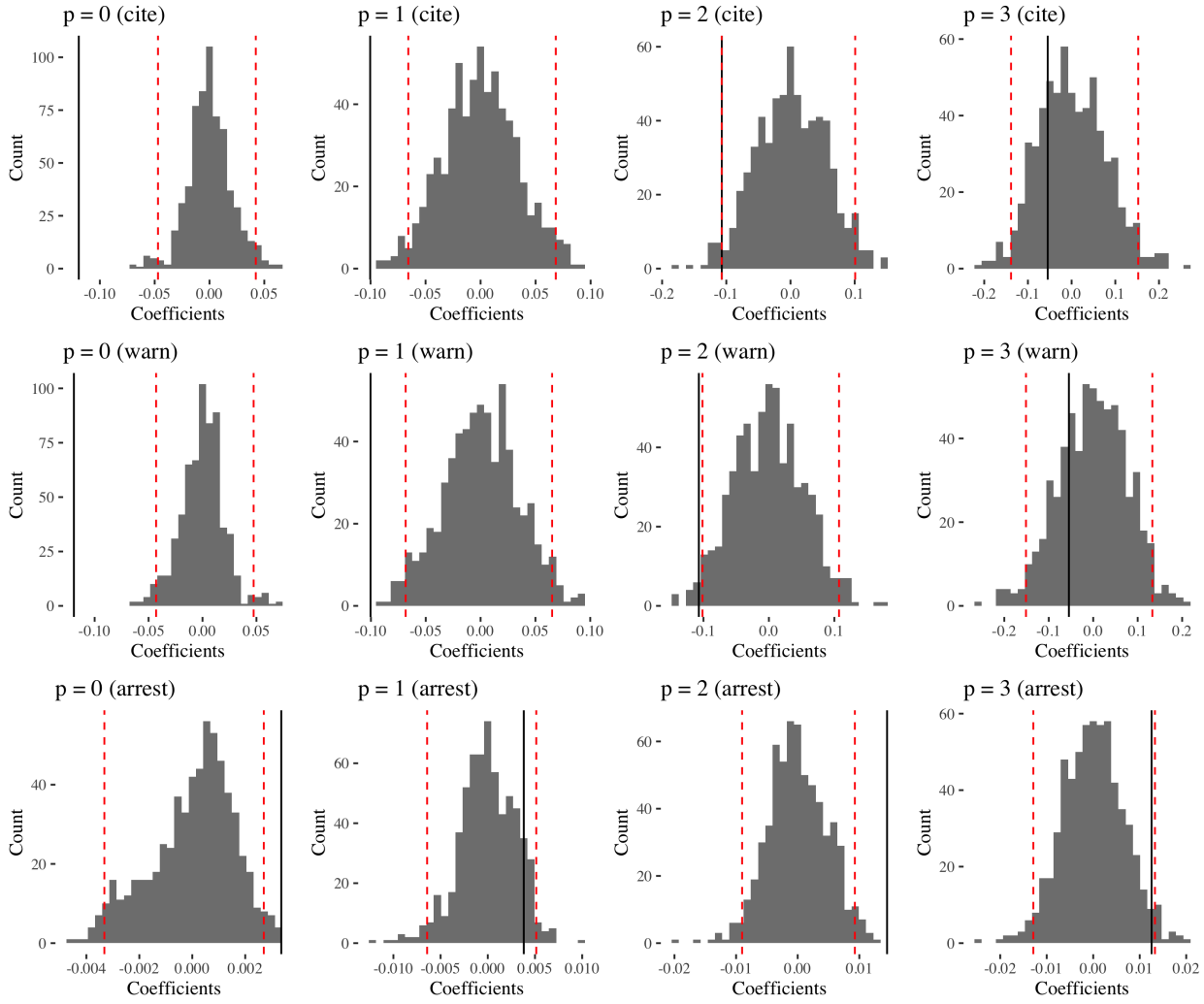


Figure M25: Pre-report temporal placebo tests across outcomes, polynomial degrees, and the inclusion of control covariates using stop data 25 days from the report's release. The first, second, and third rows display the distribution of temporal placebo estimates for the citation, warning, and arrest outcomes respectively. The vertical black line is the true estimate of the effect. The dashed red lines characterizes the 95% confidence interval for the placebo estimates.

N Ruling out anticipatory effects

Here, we attempt to rule out anticipatory effects. Police, anticipating the report, may have begun depolicing behavior prior to the report's release. This is a plausible alternative explanation for the results we derive given the Mayor of Nashville had commissioned the report some months prior to its release and the MNPD knew the report would released on November 2018.

We engage in two exercises to rule out anticipatory effects. First, we visualize the relationship between date and the outcomes of interest 60 days before and after the report was released (Figure N26). Loess models fit on each side of the discontinuity suggest the outcomes of interest were not significantly moving in a direction consistent with the results we derive in the main text, that is, arrest/citation rates do not appear to be precipitously increasing prior to the report's release, and warning rates do not appear to be precipitously decreasing prior to the report's release.

In the second exercise, we estimate a series of donut hole regression discontinuity estimates. We remove data near the discontinuity most likely to be affected by anticipatory effects (between 0-25 days). We implement this exercise with respect to our outcomes of interest using all stop data (Section N.2), data 50 days from the fake discontinuity (Section N.3), and data 25 days from the fake discontinuity (Section N.4). We do not find that removing data most likely to be confounded by anticipatory behavior changes the main results.

The absence of anticipatory effects despite MNPD's knowledge their traffic stop behavior was being analyzed might make sense. For instance, Barry Friedman, the faculty director of the Policing Project, briefed the MNPD about the findings and indicated, "They were, I think a bit surprised to find the traffic stops weren't working."[32] Friedman's quote suggests MNPD knew their traffic stops were under scrutiny, but did not expect scrutiny to result in a negative characterization of their policing behavior. Therefore, the expectation of a report may have not changed police behavior, consistent with the empirical evidence.

[32] "Review Of Nashville Traffic Stops Casts Doubt On Effectiveness In Fighting Crime" WPLN News - Nashville. November 19, 2018. `https://tinyurl.com/y5ckruvb`

N.1 Visualization near discontinuity

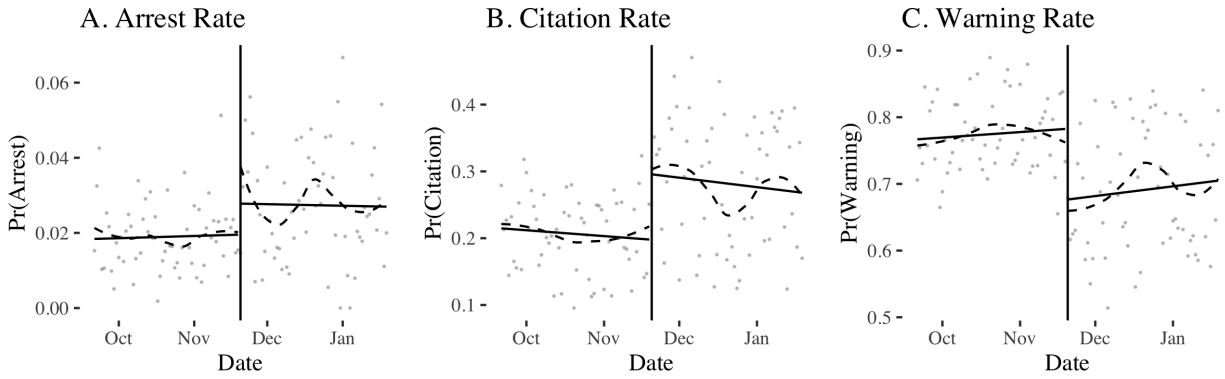


Figure N26: Arrest, citation, and warning rates (y-axis) over time in days (x-axis). The dark vertical line is the time when the Policing Project report was released. The solid lines characterize linear models fit on each side of the discontinuity. The dashed lines characterize loess models fit on each side of the discontinuity.

N.2 Donut RD (all data)

N.2.1 Citations

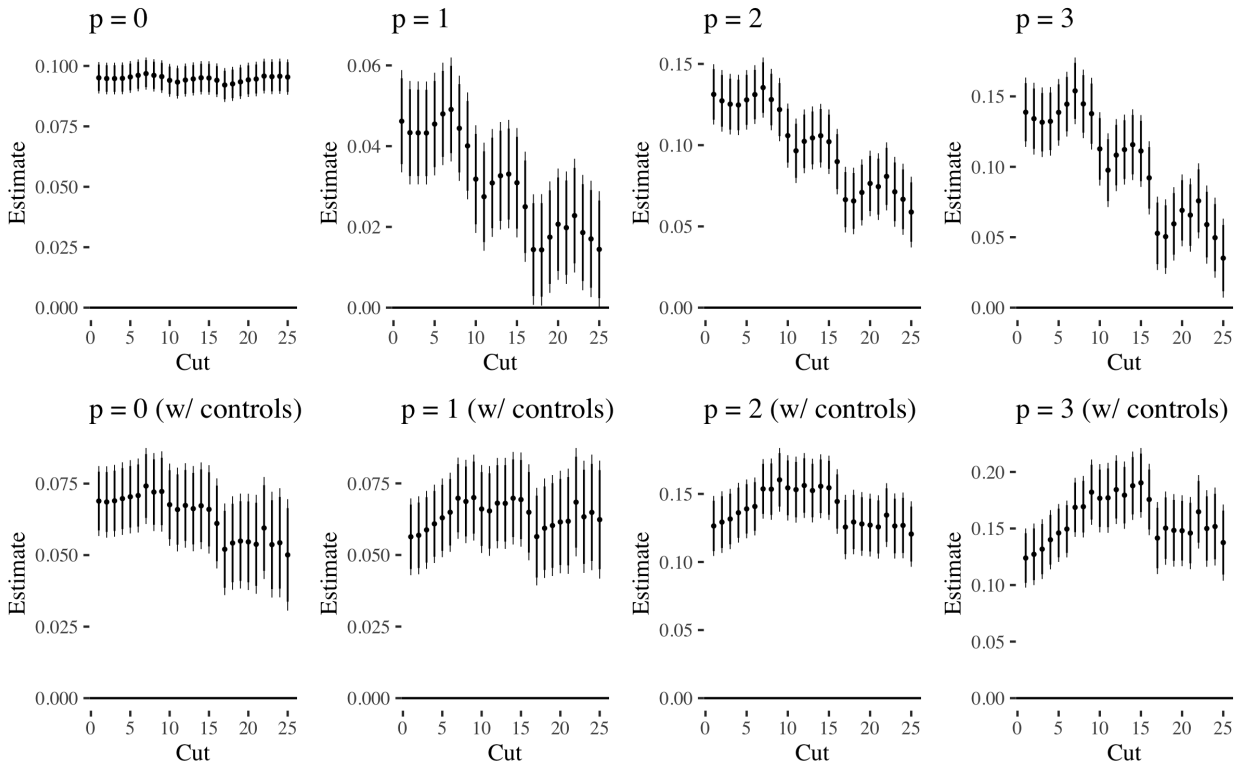


Figure N27: Donut RD estimates using all stop data and the citation outcome. The x-axis is the number of days cut from the data near the discontinuity (0-25). The y-axis is the coefficient after cutting a certain number of days near the discontinuity. Each column varies the polynomial degree for the running variable from 0-3. The top row does not adjust for control covariates, the bototm row does.

N.2.2 Warnings

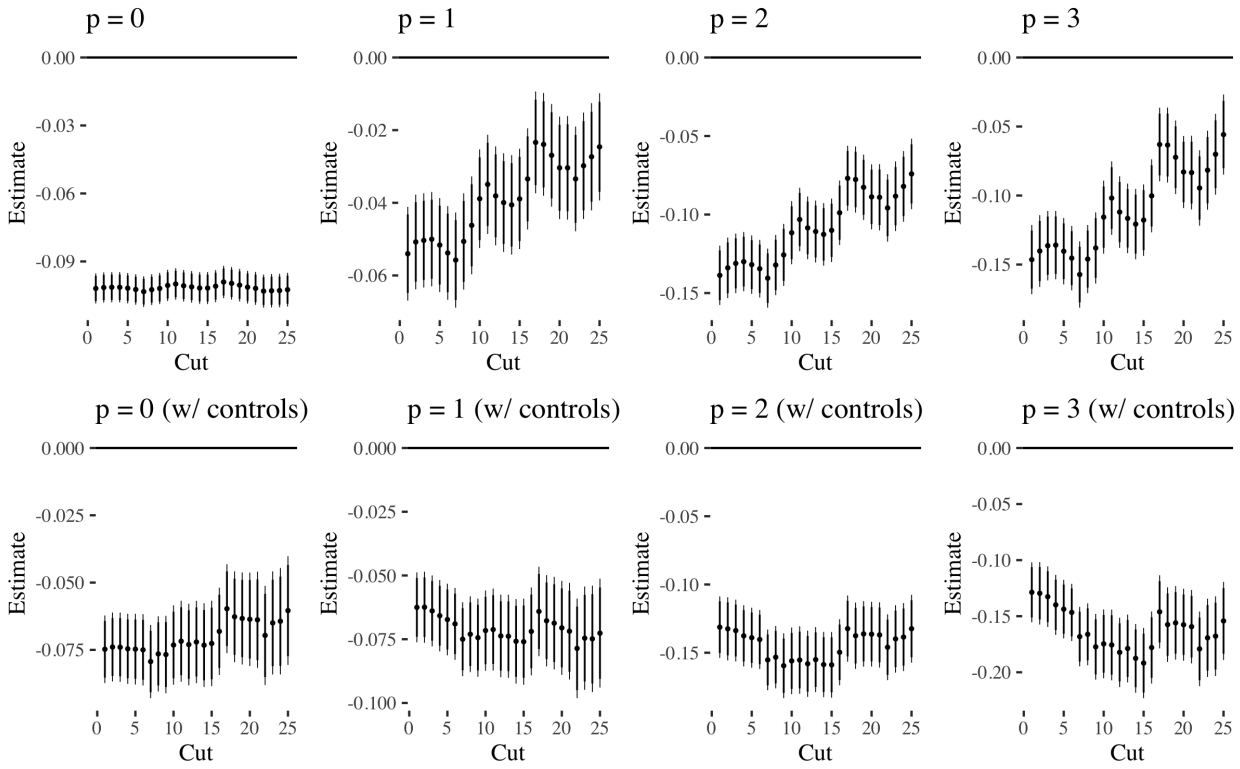


Figure N28: Donut RD estimates using all stop data and the warning outcome. The x-axis is the number of days cut from the data near the discontinuity (0-25). The y-axis is the coefficient after cutting a certain number of days near the discontinuity. Each column varies the polynomial degree for the running variable from 0-3. The top row does not adjust for control covariates, the bototm row does.

N.2.3 Arrests

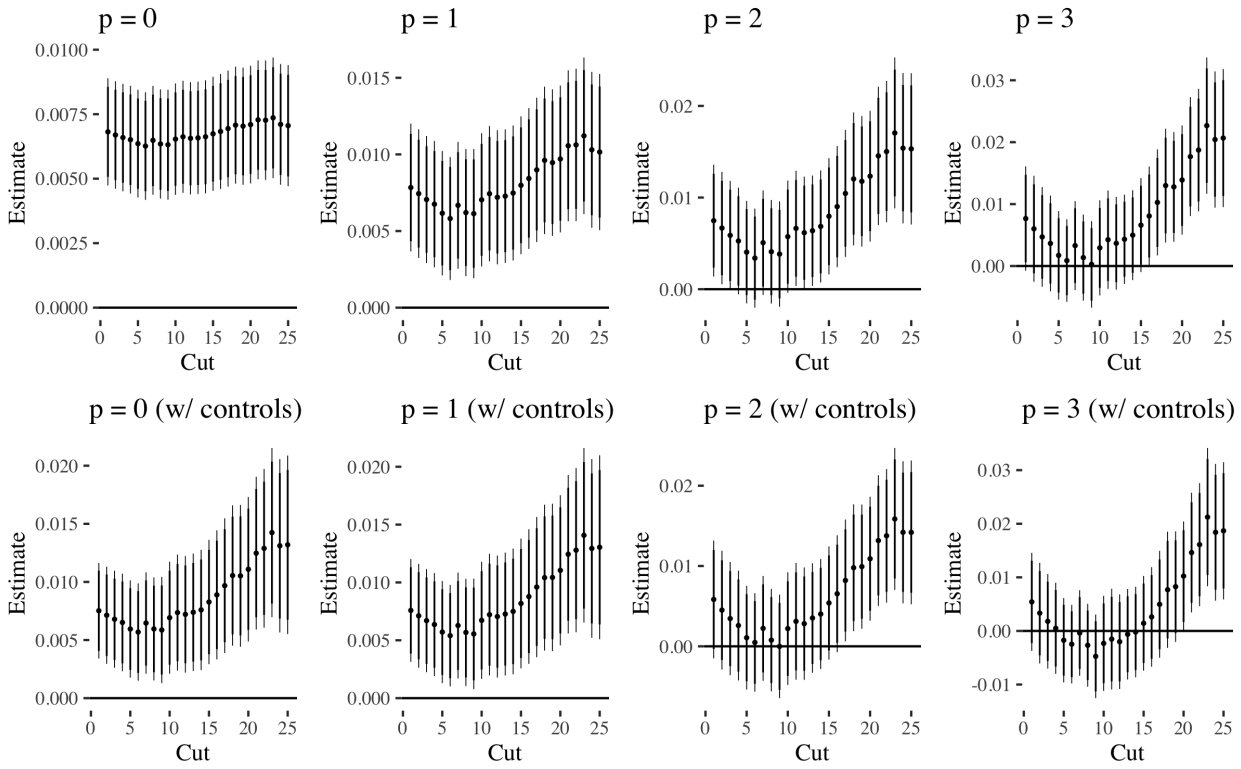


Figure N29: Donut RD estimates using all stop data and the arrest outcome. The x-axis is the number of days cut from the data near the discontinuity (0-25). The y-axis is the coefficient after cutting a certain number of days near the discontinuity. Each column varies the polynomial degree for the running variable from 0-3. The top row does not adjust for control covariates, the bototm row does.

N.3 Donut RD (50 days from discontinuity)

N.3.1 Citations

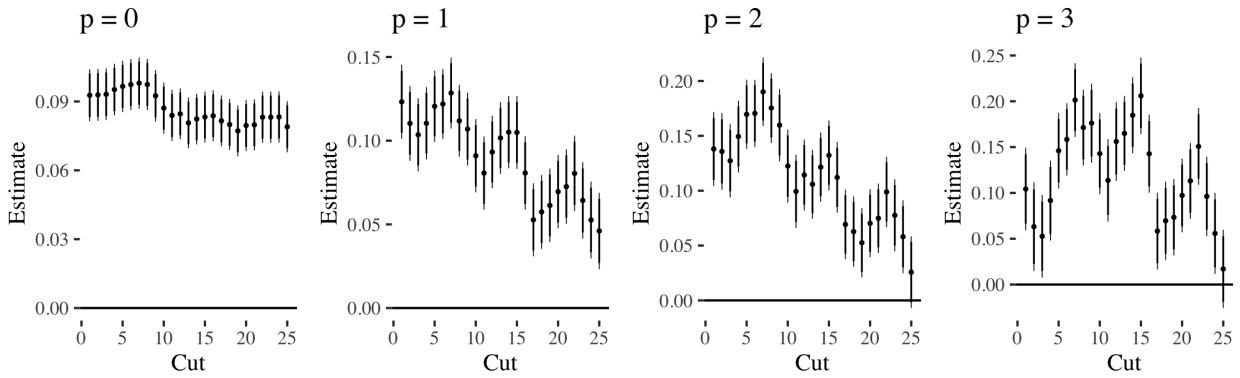


Figure N30: Donut RD estimates using stop data 50 days from the discontinuity and the citation outcome. The x-axis is the number of days cut from the data near the discontinuity (0-25). The y-axis is the coefficient after cutting a certain number of days near the discontinuity. Each column varies the polynomial degree for the running variable from 0-3. The top row does not adjust for control covariates, the bototm row does.

N.3.2 Warnings

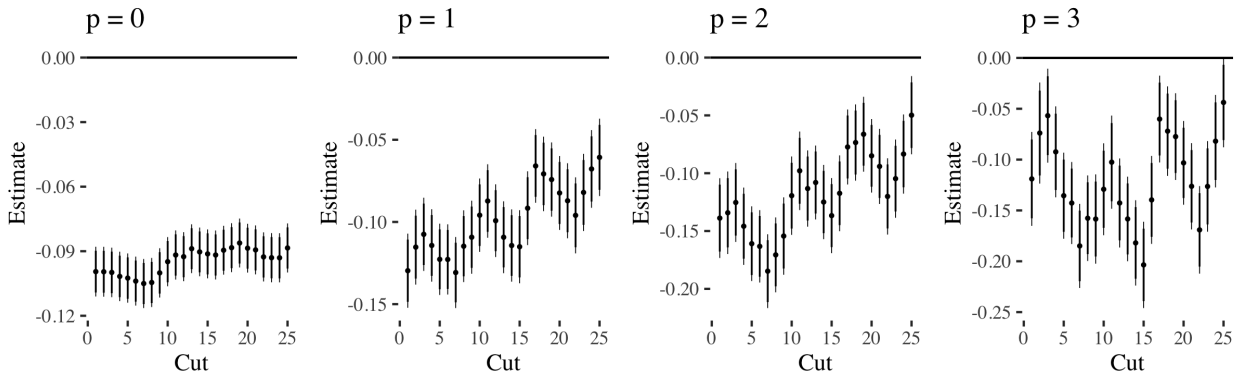


Figure N31: Donut RD estimates using stop data 50 days from the discontinuity and the warning outcome. The x-axis is the number of days cut from the data near the discontinuity (0-25). The y-axis is the coefficient after cutting a certain number of days near the discontinuity. Each column varies the polynomial degree for the running variable from 0-3. The top row does not adjust for control covariates, the bototm row does.

N.3.3 Arrests

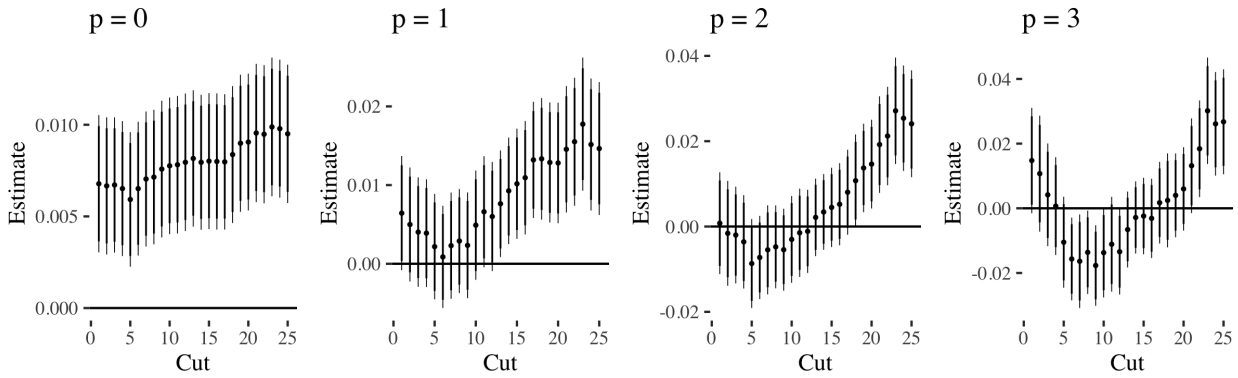


Figure N32: Donut RD estimates using stop data 50 days from the discontinuity and the arrest outcome. The x-axis is the number of days cut from the data near the discontinuity (0-25). The y-axis is the coefficient after cutting a certain number of days near the discontinuity. Each column varies the polynomial degree for the running variable from 0-3. The top row does not adjust for control covariates, the bototm row does.

N.4 Donut RD (25 days from discontinuity)

N.4.1 Citations

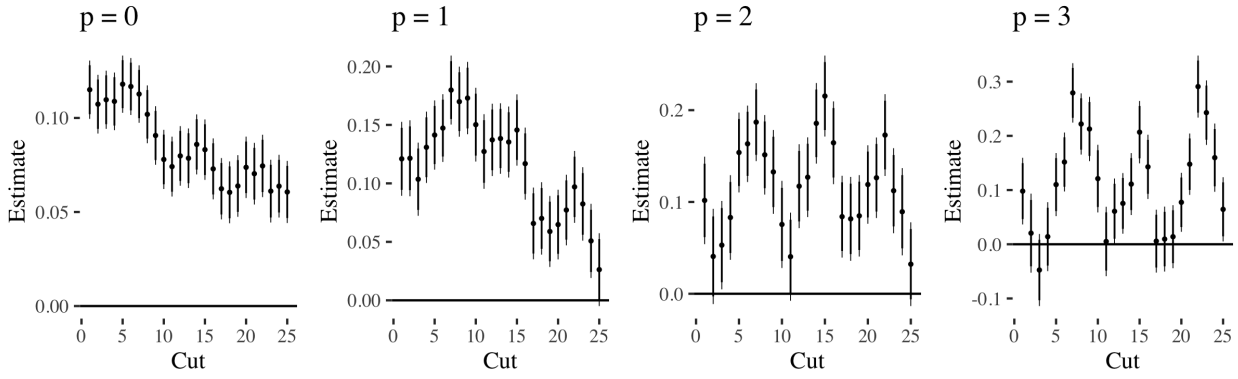


Figure N33: Donut RD estimates using stop data 25 days from the discontinuity and the citation outcome. The x-axis is the number of days cut from the data near the discontinuity (0-25). The y-axis is the coefficient after cutting a certain number of days near the discontinuity. Each column varies the polynomial degree for the running variable from 0-3. The top row does not adjust for control covariates, the bototm row does.

N.4.2 Warnings

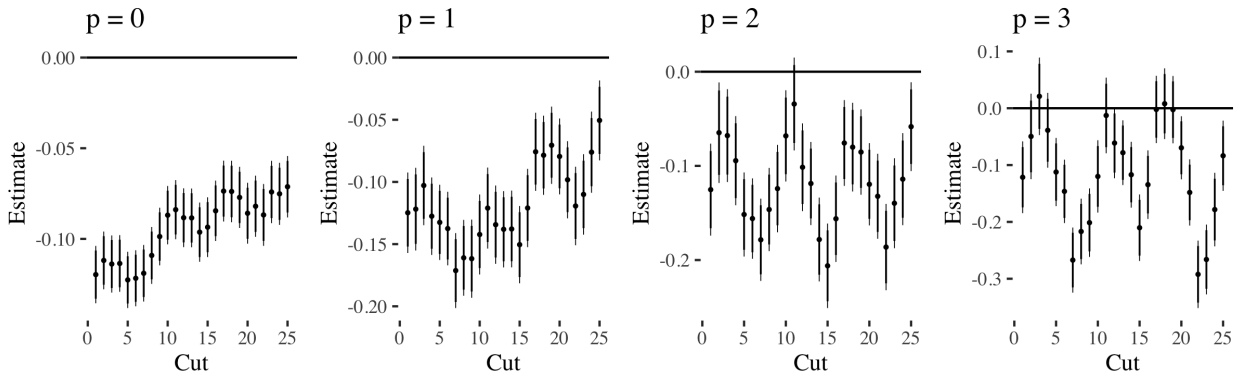


Figure N34: Donut RD estimates using stop data 25 days from the discontinuity and the warning outcome. The x-axis is the number of days cut from the data near the discontinuity (0-25). The y-axis is the coefficient after cutting a certain number of days near the discontinuity. Each column varies the polynomial degree for the running variable from 0-3. The top row does not adjust for control covariates, the bototm row does.

N.4.3 Arrests

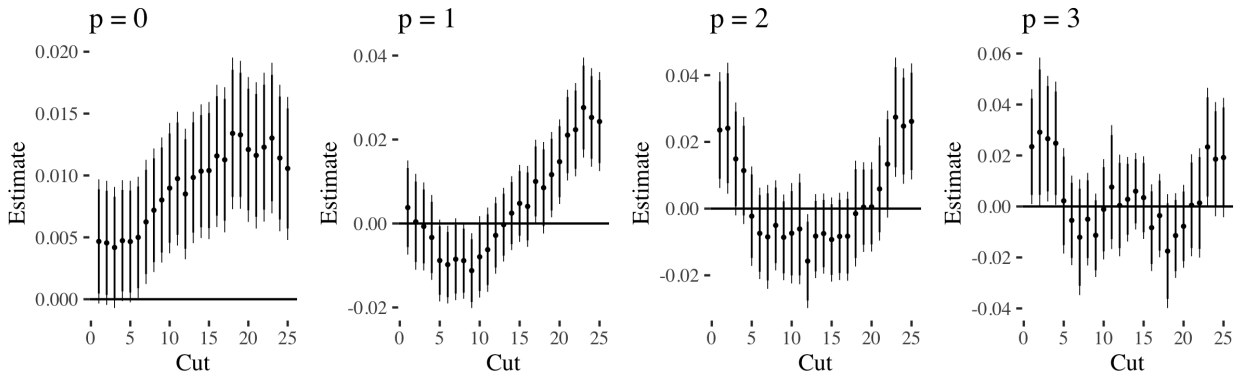


Figure N35: Donut RD estimates using stop data 25 days from the discontinuity and the arrest outcome. The x-axis is the number of days cut from the data near the discontinuity (0-25). The y-axis is the coefficient after cutting a certain number of days near the discontinuity. Each column varies the polynomial degree for the running variable from 0-3. The top row does not adjust for control covariates, the bototm row does.

O Alternative standard errors

O.1 All data, reporting areas

Table O15: Effect of expert report on outcomes of interest using all stop data (reporting area standard errors)

Panel A: Citation Rate	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	0.09***	0.05**	0.13***	0.13***	0.07***	0.05***	0.12***	0.11***
	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)
Panel B: Arrest Rate	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	0.007***	0.007***	0.006*	0.006	0.007**	0.007**	0.004	0.003
	(0.001)	(0.002)	(0.003)	(0.004)	(0.002)	(0.002)	(0.004)	(0.004)
Panel C: Warning Rate	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	−0.10***	−0.05***	−0.13***	−0.14***	−0.07***	−0.06***	−0.12***	−0.11***
	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)
N	463716	463716	463716	463716	463716	463716	463716	463716
Clusters	2037	2037	2037	2037	2037	2037	2037	2037
Degree	0	1	2	3	0	1	2	3
Controls	N	N	N	N	Y	Y	Y	Y

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. Models 1-4 assess the discontinuous effect of the project policing report on the citation rate (Panel A), the arrest rate (Panel B), and the warning rate (Panel C) without controls using all Nashville PD stop data after 2017-01-01. Models 5-8 do the same adjusting for year, month, day-of-week fixed effects in addition to the lagged outcome. All models specify varying degrees for the running variable from 0 (a simple difference-in-means) to 3. Robust standard errors clustered by reporting area in parentheses. Discrepancies in sample size between this table and the main results are due to missingness in the reporting area geography.

O.2 All data, block groups

Table O16: Effect of expert report on outcomes of interest using all stop data (block group standard errors)

Panel A: Citation Rate	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	0.08^{***}	0.05^{*}	0.12^{***}	0.11^{***}	0.06^{***}	0.06^{**}	0.11^{***}	0.10^{***}
	(0.01)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)
Panel B: Arrest Rate	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	0.008^{***}	0.007^{**}	0.005	0.006	0.007^{**}	0.006^{*}	0.002	0.003
	(0.001)	(0.002)	(0.003)	(0.005)	(0.003)	(0.003)	(0.004)	(0.005)
Panel C: Warning Rate	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	-0.08^{***}	-0.06^{**}	-0.12^{***}	-0.12^{***}	-0.07^{***}	-0.06^{**}	-0.11^{***}	-0.10^{***}
	(0.01)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.03)
N	422492	422492	422492	422492	422492	422492	422492	422492
Clusters	474	474	474	474	474	474	474	474
Degree	0	1	2	3	0	1	2	3
Controls	N	N	N	N	Y	Y	Y	Y

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. Models 1-4 assess the discontinuous effect of the project policing report on the citation rate (Panel A), the arrest rate (Panel B), and the warning rate (Panel C) without controls using all Nashville PD stop data after 2017-01-01. Models 5-8 do the same adjusting for year, month, day-of-week fixed effects in addition to the lagged outcome. All models specify varying degrees for the running variable from 0 (a simple difference-in-means) to 3. Robust standard errors clustered by census block group in parentheses. Discrepancies in sample size between this table and the main results are due to missingness in the block group geography.

O.3 Data near discontinuity, reporting areas

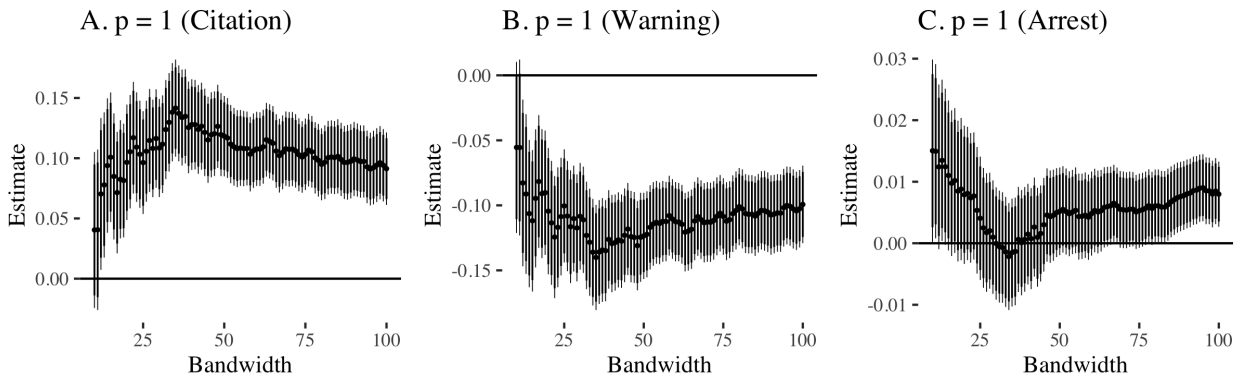


Figure O36: Effect of expert report on relevant outcomes (y-axis) across bandwidths (x-axis). All panels use the running variable to the first degree. Standard errors clustered at the reporting area level.

O.4 Data near discontinuity, block groups

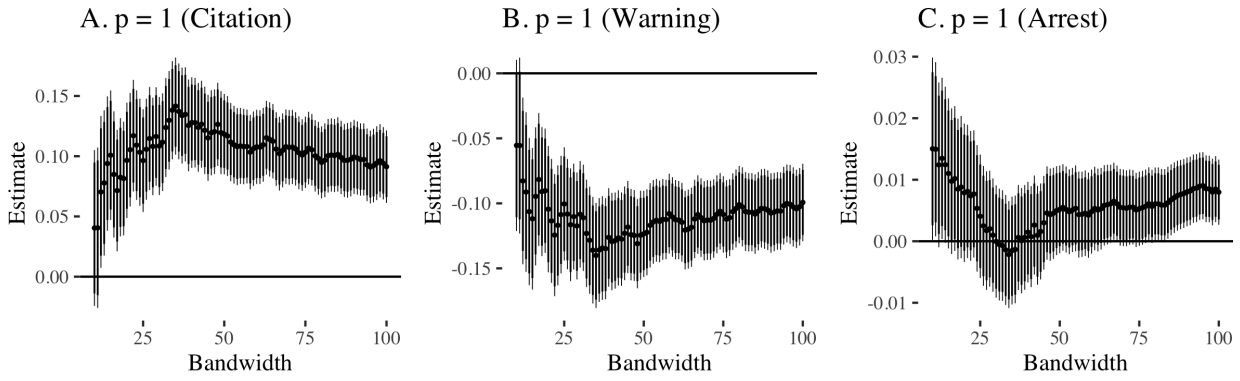


Figure O37: Effect of expert report on relevant outcomes (y-axis) across bandwidths (x-axis). All panels use the running variable to the first degree. Standard errors clustered at the block group level.

P Evaluating effects on relevant outcomes by precinct

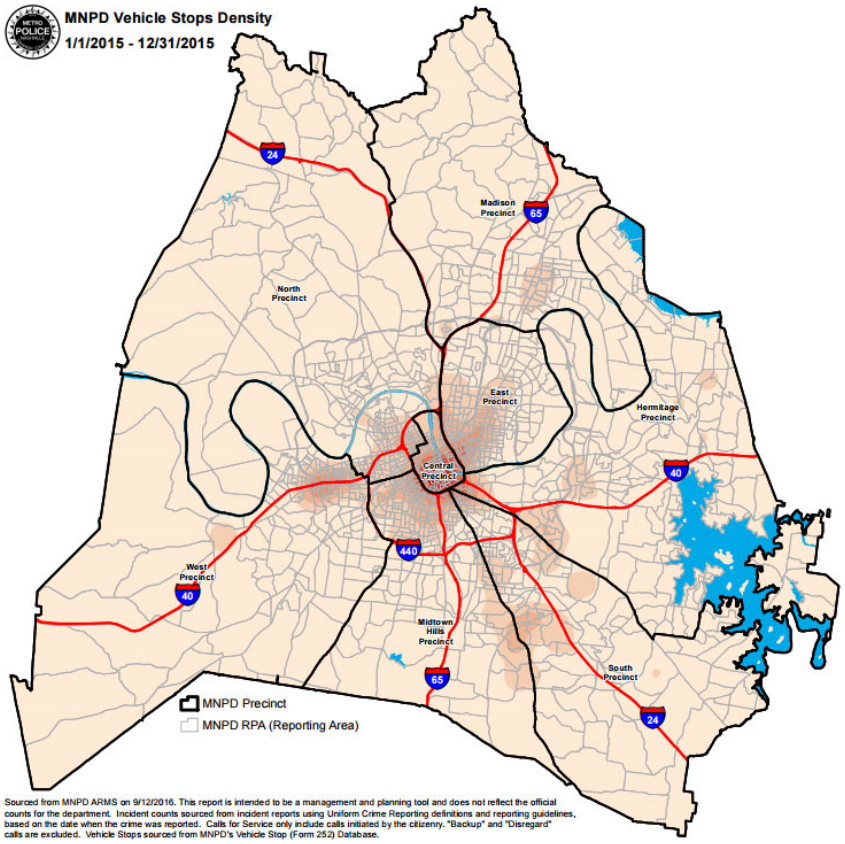


Figure P38: Metropolitan Nashville Police Department precincts (denoted by black borders).

Here we evaluate whether depolicing occurs uniformly across Nashville's 8 police precincts. This exercise helps to identify if strategic depolicing was concentrated among a particular geographic area or was the result of mandates from certain precinct commanders and not others. Since estimates are derived from a smaller pool of data within each precinct subset, we display effect estimates of the Policing Project report within each precinct using the full temporal domain between 2017-01-01 and 2019-03-24.

Table P17: MNPD pre-report precinct characteristics

Precinct Name	Citation Rate	Arrest Rate	Warning Rate	% Stops White	% Stops Black	% Stops Latinx	N Stops
West	0.21	0.01	0.78	0.71	0.23	0.03	304394.00
East	0.14	0.02	0.83	0.49	0.47	0.03	377359.00
South	0.23	0.01	0.75	0.46	0.37	0.12	421770.00
Central	0.28	0.01	0.71	0.61	0.33	0.03	234166.00
Hermitage	0.27	0.02	0.72	0.60	0.32	0.05	447802.00
North	0.20	0.02	0.78	0.25	0.73	0.01	283949.00
Madison	0.21	0.02	0.77	0.49	0.44	0.06	193669.00
Midtown Hills	0.22	0.01	0.77	0.66	0.24	0.05	416207.00

Across all precincts, the expert report appears to have motivated a discontinuous decrease in the raw number of stops (Table P18). Although the reduction in statistical power (e.g. the number of stops) might decrease the prospect of detecting effects across specifications, all coefficients across the different specifications assessing the discontinuous effect of the Policing Project report are negative, and most of them are statistically significant. In general, the Policing Project report appears to have motivated a reduction in Metropolitan Nashville Police Department traffic stops across all precincts.

Findings with respect to the other outcomes of interest are somewhat mixed across precincts. With the exception of the Central precinct (Panel D), at least 1 of the specifications assessing the discontinuous effect of the expert report on the citation rate is statistically significant and positive (see Table P19). Importantly, none of the coefficients are statistically significant and in the opposite direction of the main results that bundle all data across precincts. The lack of statistically significant and positive effects of the expert report for the Central Precinct does not have an obvious explanation. Table P17 displays pre-report precinct outcome characteristics and illustrates the Central Precinct is not distinct from other precincts, such as Hermitage, in terms of citation rates, warning rates, and the demographic composition of who is being stopped. Yet the Policing Project report does have a fairly substantial positive effect on the citation rate in Hermitage. One explanation could be differences in precinct commander priorities. However, it is important to note the Central Precinct still experiences a negative discontinuous decrease in the raw number of traffic stops after the expert report was released, suggesting the precinct commander did not necessarily disfavor depolicing. Perhaps, for the Central Precinct, by virtue of forces unknown, a decrease in policing did not necessarily translate into gains in efficiency. However, the MNPD appeared to increase efficiency across the board. Many of our estimates still demonstrate the Policing Project report increased efficiency via higher citation rates across most precincts.

Findings with respect to arrest rates are highly indeterminate (Table P20). We suspect the problem lies in the low number of arrests in a given day, undermining the necessary statistical power to evaluate a discontinuous effect using daily-level data across small jurisdictions. The warning rate findings are similar to the citation rate findings. For the most part, there is at least 1 statistically significant coefficient in a direction consistent with the main results for each precinct with the exception of the Central Precinct (Table P21).

P.1 Stops

Table P18: Effect of expert report on stop count by precinct

Panel A: West Precinct	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	−71.94***	−60.97***	−54.68***	−42.69**	−29.95***	−30.99***	−23.24*	−26.15*
	(4.03)	(7.91)	(11.78)	(15.43)	(6.28)	(6.81)	(10.02)	(11.87)
N	813	813	813	813	812	812	812	812
R^2	0.28	0.30	0.30	0.31	0.66	0.66	0.66	0.67
Panel B: East Precinct	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	−74.15***	−52.97***	−46.39***	−33.13**	−48.03***	−47.79***	−36.75***	−35.59***
	(2.83)	(5.48)	(8.15)	(10.68)	(5.46)	(5.88)	(8.38)	(9.92)
N	813	813	813	813	813	813	813	813
R^2	0.46	0.49	0.49	0.50	0.64	0.64	0.65	0.65
Panel C: South Precinct	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	−69.11***	−32.11***	−26.38	−19.29	−21.91**	−24.84***	−13.29	−28.37*
	(4.88)	(9.06)	(13.48)	(17.79)	(6.78)	(7.38)	(10.89)	(12.89)
N	813	813	813	813	813	813	813	813
R^2	0.20	0.30	0.31	0.31	0.69	0.69	0.69	0.70
Panel D: Central Precinct	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	−44.93***	−28.03***	−23.73***	−26.54**	−21.99***	−22.41***	−21.91**	−28.45***
	(2.43)	(4.55)	(6.76)	(8.92)	(4.38)	(4.74)	(6.97)	(8.30)
N	813	813	813	813	813	813	813	813
R^2	0.30	0.38	0.38	0.38	0.56	0.56	0.56	0.56
Panel E: Hermitage Precinct	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	−76.37***	−54.96***	−40.90**	−25.53	−31.17***	−32.40***	−14.17	−26.77*
	(4.66)	(9.04)	(13.34)	(17.53)	(6.72)	(7.29)	(10.53)	(12.52)
N	813	813	813	813	813	813	813	813
R^2	0.25	0.29	0.30	0.31	0.70	0.70	0.71	0.71
Panel F: North Precinct	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	−36.39***	−25.90***	−31.81***	−13.36	−13.02***	−14.62***	−18.04**	−17.51*
	(2.51)	(5.00)	(7.47)	(9.53)	(3.92)	(4.25)	(6.37)	(7.57)
N	810	810	810	810	810	810	810	810
R^2	0.21	0.22	0.23	0.29	0.63	0.63	0.63	0.63
Panel G: Madison Precinct	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	−38.56***	−33.03***	−18.73**	−3.54	−21.96***	−23.67***	−9.18	−6.95
	(2.47)	(4.89)	(7.16)	(9.25)	(3.92)	(4.26)	(5.99)	(7.12)
N	812	812	812	812	812	812	812	812
R^2	0.23	0.24	0.26	0.29	0.63	0.63	0.65	0.65
Panel H: Midtown Hills Precinct	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	−59.93***	−51.06***	−26.17*	−4.40	−33.18***	−36.77***	−14.63	−17.96
	(3.89)	(7.62)	(11.18)	(14.56)	(6.03)	(6.56)	(9.30)	(11.07)
N	812	812	812	812	812	812	812	812
R^2	0.23	0.25	0.27	0.29	0.64	0.64	0.66	0.66
Degree	0	1	2	3	0	1	2	3
Controls	N	N	N	N	Y	Y	Y	Y

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

P.2 Citations

Table P19: Effect of expert report on citation rate by precinct

Panel A: West Precinct	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	0.16***	0.03	0.11***	0.08*	0.11***	0.05**	0.04	0.02
	(0.01)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)	(0.03)
N	62438	62438	62438	62438	62438	62438	62438	62438
R^2	0.01	0.01	0.01	0.01	0.02	0.02	0.02	0.02
Panel B: East Precinct	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	0.01	−0.02	0.02	0.02	−0.00	−0.00	0.06**	0.05*
	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)
N	71891	71891	71891	71891	71891	71891	71891	71891
R^2	0.00	0.00	0.00	0.00	0.01	0.01	0.01	0.01
Panel C: South Precinct	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	0.15***	0.16***	0.15***	0.11***	0.16***	0.18***	0.14***	0.10***
	(0.01)	(0.01)	(0.02)	(0.03)	(0.01)	(0.02)	(0.02)	(0.03)
N	75518	75518	75518	75518	75518	75518	75518	75518
R^2	0.01	0.01	0.01	0.01	0.02	0.02	0.02	0.03
Panel D: Central Precinct	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	−0.00	−0.01	0.03	0.04	0.01	0.01	−0.07	−0.03
	(0.01)	(0.02)	(0.03)	(0.05)	(0.02)	(0.02)	(0.04)	(0.05)
N	42382	42382	42382	42382	42382	42382	42382	42382
R^2	0.00	0.00	0.00	0.00	0.02	0.02	0.02	0.02
Panel E: Hermitage Precinct	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	0.09***	0.09***	0.19***	0.25***	0.10***	0.12***	0.24***	0.23***
	(0.01)	(0.01)	(0.02)	(0.03)	(0.01)	(0.01)	(0.02)	(0.03)
N	79186	79186	79186	79186	79186	79186	79186	79186
R^2	0.00	0.00	0.01	0.01	0.01	0.01	0.01	0.02
Panel F: North Precinct	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	−0.02	0.05*	0.21***	0.18***	−0.00	−0.02	0.16***	0.19***
	(0.01)	(0.02)	(0.03)	(0.04)	(0.02)	(0.02)	(0.04)	(0.05)
N	36113	36113	36113	36113	36113	36113	36113	36113
R^2	0.00	0.00	0.01	0.01	0.02	0.02	0.03	0.03
Panel G: Madison Precinct	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	0.06***	0.01	0.19***	0.22***	0.00	−0.01	0.18***	0.19***
	(0.01)	(0.02)	(0.03)	(0.04)	(0.02)	(0.03)	(0.04)	(0.05)
N	33884	33884	33884	33884	33884	33884	33884	33884
R^2	0.00	0.00	0.01	0.01	0.01	0.01	0.02	0.02
Panel H: Midtown Hills Precinct	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	0.18***	−0.02	0.07**	0.10***	0.02	−0.02	0.03	0.08*
	(0.01)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)	(0.03)
N	61894	61894	61894	61894	61894	61894	61894	61894
R^2	0.01	0.01	0.01	0.01	0.03	0.03	0.03	0.03
Degree	0	1	2	3	0	1	2	3
Controls	N	N	N	N	Y	Y	Y	Y

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

P.3 Arrests

Table P20: Effect of expert report on arrest rate by precinct

Panel A: West Precinct	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	−0.00	0.01	0.01*	0.02*	0.01*	0.01	0.01	0.01
	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)
N	62438	62438	62438	62438	62438	62438	62438	62438
R^2	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Panel B: East Precinct	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	0.01***	0.00	0.01	0.01	0.01	0.00	0.01	0.00
	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)
N	71891	71891	71891	71891	71891	71891	71891	71891
R^2	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Panel C: South Precinct	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	0.01***	0.01*	0.00	0.00	0.01	0.01	0.01	0.01
	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)
N	75518	75518	75518	75518	75518	75518	75518	75518
R^2	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Panel D: Central Precinct	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	0.01***	0.00	0.01	0.00	0.00	−0.00	0.01	0.00
	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)
N	42382	42382	42382	42382	42382	42382	42382	42382
R^2	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Panel E: Hermitage Precinct	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	0.01**	0.01	0.00	0.01	0.01	0.01	0.00	0.00
	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)
N	79186	79186	79186	79186	79186	79186	79186	79186
R^2	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Panel F: North Precinct	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	−0.00	−0.00	−0.01	−0.02	−0.00	−0.00	−0.01	−0.02
	(0.00)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)
N	36113	36113	36113	36113	36113	36113	36113	36113
R^2	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Panel G: Madison Precinct	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	0.02***	0.03**	0.04**	0.02	0.04***	0.03**	0.02	0.01
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)
N	33884	33884	33884	33884	33884	33884	33884	33884
R^2	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Panel H: Midtown Hills Precinct	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	−0.00	0.00	0.00	0.00	0.00	0.00	−0.01	−0.01
	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)
N	61894	61894	61894	61894	61894	61894	61894	61894
R^2	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Degree	0	1	2	3	0	1	2	3
Controls	N	N	N	N	Y	Y	Y	Y

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

P.4 Warnings

Table P21: Effect of expert report on warning rate by precinct

Panel A: West Precinct	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	-0.16^{***}	-0.03	-0.12^{***}	-0.10^{**}	-0.12^{***}	-0.06^{**}	-0.05	-0.03
	(0.01)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)	(0.04)
N	62438	62438	62438	62438	62438	62438	62438	62438
R^2	0.00	0.01	0.01	0.01	0.02	0.02	0.02	0.02
Panel B: East Precinct	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	-0.02^{**}	0.01	-0.03	-0.03	-0.01	-0.00	-0.06^{**}	-0.05^{*}
	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)
N	71891	71891	71891	71891	71891	71891	71891	71891
R^2	0.00	0.00	0.00	0.00	0.01	0.01	0.01	0.01
Panel C: South Precinct	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	-0.16^{***}	-0.17^{***}	-0.16^{***}	-0.12^{***}	-0.17^{***}	-0.18^{***}	-0.15^{***}	-0.10^{***}
	(0.01)	(0.01)	(0.02)	(0.03)	(0.01)	(0.02)	(0.02)	(0.03)
N	75518	75518	75518	75518	75518	75518	75518	75518
R^2	0.01	0.01	0.01	0.01	0.02	0.02	0.02	0.02
Panel D: Central Precinct	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	-0.01	0.01	-0.04	-0.04	-0.01	-0.01	0.06	0.03
	(0.01)	(0.02)	(0.03)	(0.05)	(0.02)	(0.02)	(0.04)	(0.05)
N	42382	42382	42382	42382	42382	42382	42382	42382
R^2	0.00	0.00	0.00	0.00	0.02	0.02	0.02	0.02
Panel E: Hermitage Precinct	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	-0.09^{***}	-0.10^{***}	-0.20^{***}	-0.25^{***}	-0.10^{***}	-0.12^{***}	-0.24^{***}	-0.23^{***}
	(0.01)	(0.01)	(0.02)	(0.03)	(0.01)	(0.01)	(0.02)	(0.03)
N	79186	79186	79186	79186	79186	79186	79186	79186
R^2	0.00	0.00	0.01	0.01	0.01	0.01	0.01	0.01
Panel F: North Precinct	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	0.02	-0.04^{*}	-0.20^{***}	-0.16^{***}	0.01	0.02	-0.15^{***}	-0.17^{***}
	(0.01)	(0.02)	(0.03)	(0.04)	(0.02)	(0.02)	(0.04)	(0.05)
N	36113	36113	36113	36113	36113	36113	36113	36113
R^2	0.00	0.00	0.01	0.01	0.02	0.02	0.02	0.02
Panel G: Madison Precinct	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	-0.08^{***}	-0.04	-0.23^{***}	-0.24^{***}	-0.04	-0.02	-0.19^{***}	-0.21^{***}
	(0.01)	(0.02)	(0.04)	(0.04)	(0.02)	(0.03)	(0.04)	(0.05)
N	33884	33884	33884	33884	33884	33884	33884	33884
R^2	0.00	0.00	0.01	0.01	0.01	0.01	0.02	0.02
Panel H: Midtown Hills Precinct	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report	-0.17^{***}	0.02	-0.07^{**}	-0.11^{***}	-0.02	0.02	-0.02	-0.07^{*}
	(0.01)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)	(0.03)
N	61894	61894	61894	61894	61894	61894	61894	61894
R^2	0.01	0.01	0.01	0.01	0.03	0.03	0.03	0.03
Degree	0	1	2	3	0	1	2	3
Controls	N	N	N	N	Y	Y	Y	Y

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Q Racial Attitudes of Police

We posit the racial attitudes of police officers cause discriminatory behavior to persist after voluntary reductions in effort. The intention behind police retrenchment was not to undo the greater frequency with which non-white drivers were stopped, instead, the police reduced their effort across the board in a race neutral fashion. Rather than implementing an explicitly anti-racist program, the department opted to adopt a "color-blind" approach to address discrimination.

We use data from the General Social Survey (GSS) to test whether law enforcement officers generally believe that discrimination can be confronted with race-neutrality rather than explicit anti-discrimination reforms. We construct a binary indicator Police Officer$_i$ which is unity when a respondent is a law enforcement officer and zero otherwise. Due to the low number of police officers in the survey, we include survey years from 2000-2018 but include survey wave fixed effects to avoid capturing temporal trends in officers surveyed and attitudes.

We use two attitudinal measures that capture the degree to which individuals believe neutrality is the optimal way to overcome racial discrimination. First, we use a question regarding whether Black Americans should "work their way up" to overcome racism in the United States:

> Irish, Italians, Jewish and many other minorities overcame prejudice and worked their way up. Blacks should do the same without special favors. (Agree Strongly 1 - Disagree Strongly 5)

the question ranges from 1-5 where 1 is most strongly agree and 5 is most strongly disagree. Next, we use a question regarding affirmative action:

> Some people say that because of past discrimination, blacks should be given preference in hiring and promotion. Others say that such preference in hiring and promotion of blacks is wrong because it discriminates against whites. What about your opinion – are you for or against preferential hiring and promotion of blacks? (Strongly Favor 1 - Strongly Oppose 4)

We estimate the following equation with least squares:

$$Attitude_i = \alpha + \delta \text{Police Officer}_i + \sum_{k=1}^{k} X_i + \pi_y + \varepsilon_i$$

$Attitude_i$ is the respective attitude outcome, $\sum_{k=1}^{k} X_i$ include age and its square, sex, race, and income of the respondent, π_y is a survey wave fixed effect, and ε_i is the robust error term. δ captures the average difference between police officers and non-police officers, net of covariate adjustment. We expect $\delta > 0$ for the both outcomes. We caution that δ cannot be interpreted causally - occupations are selected into by respondents. However, it need not be the case that being a police officer causes one to believe in race neutral approaches to address past racial injustice for the proposed mechanism to be in play - so long as police officers

Table Q22: Police Officer Racial Attitudes

Outcome	Aff. Action		Work Way Up	
	(1)	(2)	(3)	(4)
𝟙 Police	0.37**	0.42**	0.36***	0.36***
	(0.13)	(0.13)	(0.10)	(0.09)
N	12249	10803	11773	10427
Covariates?	-	Yes	-	Yes
μ DV	3.84	3.84	3.21	3.21

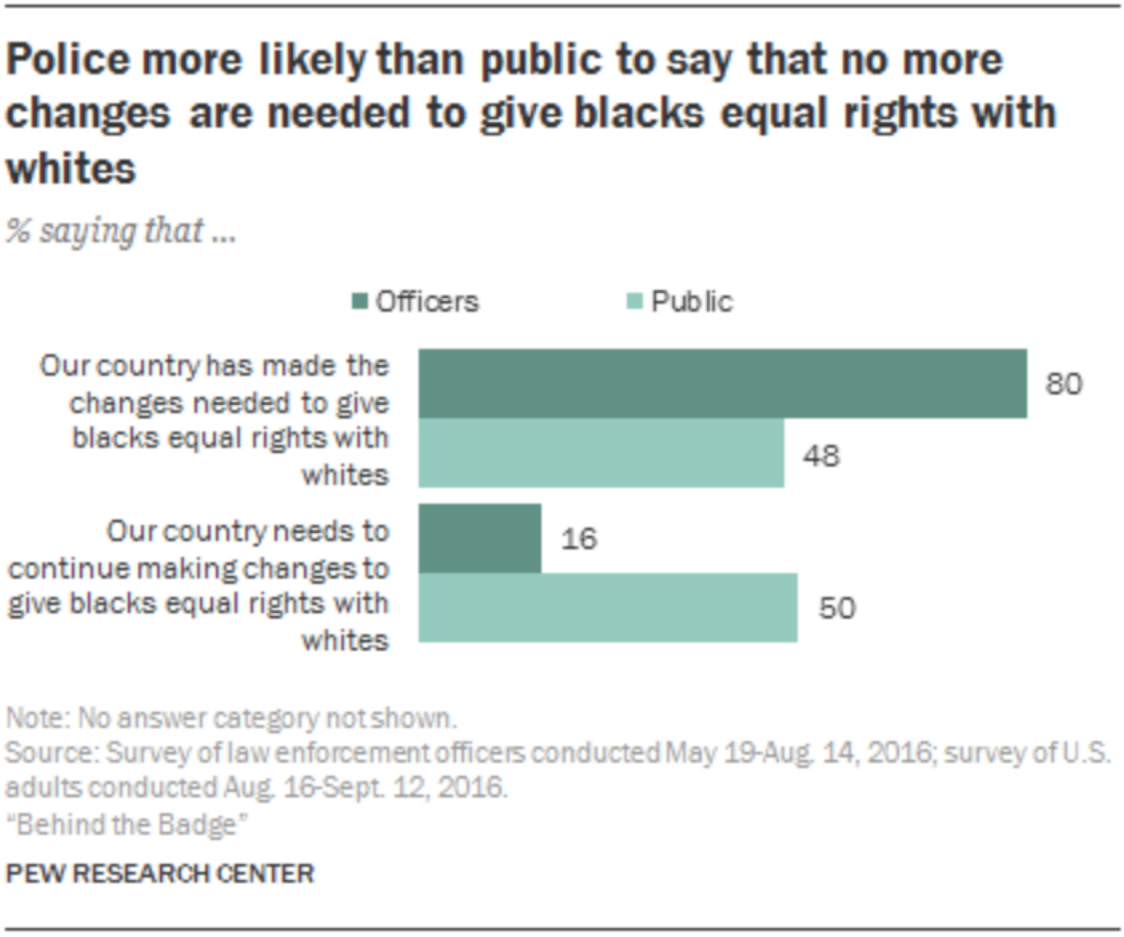
$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

do value race-neutral approaches, the explanation that officer attitudes shaped voluntary reductions in a way that did not address the root of discrimination is plausible.

Results in Table Q22 suggest police officers are more likely to oppose affirmative action and more likely to believe that Black Americans can overcome racial discrimination through hard work and individual actions without redistribution. The result suggests police officers are more likely to support race neutral approaches to discrimination compared to other members of society. The belief that officers should not extend any "special treatment" based on race may explain why the pattern of traffic stops still suggests Black drivers make up a disproportionate share of traffic stops even after the Policing Project report. Because officers sought to reduce effort in general without taking into account that unnecessary stops are concentrated unevenly across racial groups, discriminating persists after depolicing.

Our findings are consistent with a recent Pew Survey of police, which finds officers are more likely than the public to believe that no fundamental changes are required in order to achieve racial equality (Figure Q39).

Figure Q39: Pew Poll of Police Views



Note: source Pew Research Center `https://www.pewsocialtrends.org/2017/01/11/police-views-public-views/`

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