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DRIVING CHANGE: EVALUATING CONNECTICUT'S COLLABORATIVE APPROACH TO REDUCING RACIAL DISPARITIES IN POLICING

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ABSTRACT

We examine a statewide program that identifies police departments with large racial disparities in traffic stops and works with identified departments to reduce disparities. The intervention caused large (23.56%) and persistent (at least 12 months) reductions in the number of minorities involved in traffic stops, with no impact on stops of white drivers. Reductions in traffic stops involving minority drivers primarily result from fewer pretextual stops (85%) for lighting violations and non-moving violations. We find relative declines of approximately 30% for stops resulting in a warning or an arrest. Using data on crime and vehicle crashes, we find no evidence that crashes increase after traffic stops fall, but we do find moderate declines in the clearance rates for property crime.

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Introduction

Public concern over racial disparities in police conduct has grown over time, especially in the context of traffic enforcement. Over 21 million drivers or passengers are annually stopped by police, a frequent, seemingly minor police-civilian interaction that has the potential to escalate into more deadly encounters (Levenson 2021; Tapp and Davis 2022). Evidence also suggests that punitive enforcement strategies have eroded police legitimacy and diminished community trust (Tyler and Fagan 2008, 2012; Gau and Brunson 2010; Ang et al. 2021; Mikdash and Zaiour 2023). Against this backdrop, there has been a growing emphasis on initiatives that critically examine and seek to mitigate racial biases in traffic stops.[1] In 2011, Connecticut launched one of the first statewide programs intended to address systemic racial disparities in traffic stops. The Connecticut program emphasizes data-driven decision-making and the voluntary cooperation of police departments. One possible advantage of this approach is empowering law enforcement agencies by providing detailed information on the particular stop patterns that contribute to racial disparities in their jurisdiction, especially for smaller police departments with limited resources for data analysis.

The influence of the Connecticut program (titled the "Connecticut Racial Profiling Prohibition Project" or CTRP3) has reached far beyond the state's borders and has had a national impact on the conversation about police reform. As early as 2015, program staff had provided detailed guidance to states interested in passing data collection laws, analyses, and implementing similar interventions.[2] In 2021, program staff testified before Congress about the initiative (Barone 2021), and the program was subsequently promoted as a model for state reforms by two of the major national traffic safety organization: Mothers Against Drunk Driving (MADD) (Hawkins 2021; MADD 2021) and Governors Highway Safety Association (Sprattler and Statz 2021). These activities dramatically increased the visibility of CTRP3, and the program staff has since provided guidance to 10 additional states regarding policing reform initiatives.[3] The Arnold Foundation has funded the Justice Center at the Council of State Governments to provide detailed technical assistance to the State of Nevada as

[1] 23 states have at some point mandated the collection and analysis of traffic stop data to assess racial differences in police stops. Also see policy initiatives like Obama's Task Force on 21st Century Policing as well as funding made available via the National Highway Safety Traffic Authority (NHTSA). See NHTSA SAFETEA-LU and Fast Act S. 1906 funding for FY 2006 to 2019.

[2] Three early states that consulted with CRPPP when developing legislation and programs were California, Oregon and Rhode Island.

[3] Alabama, Colorado, DC Metro, Maine, Maryland, Minnesota, Nevada, New Jersey, New York and Ohio

well as three additional states in crafting initial legislation based on Connecticut's program.[4] Finally, U.S. Department of Justice (DOJ) enforcement activities have built on the framework created by the CTRP3.[5]

This paper evaluates effects arising from CTRP3 interventions with targeted police departments that had identifiable racial disparities in stops. This particular aspect of the program, i.e. the structured and cooperative intervention process, is unique among states that analyze traffic stops data and is a likely contributor to the longevity of the program.[6] We first examine the impact on racial disparities in traffic stops and the composition of stops. In addition, we explore whether these interventions had any unintended consequences such as increases in accidents or reduced effectiveness of law enforcement. We focus on interventions at the level of town police departments because this provides us with a natural control group of not yet or untreated towns. We estimate so-called "stacked" event studies by examining 29 (of 94) treated Connecticut town police departments identified as having a disparity and underwent an intervention at some point between October 2013 and December 2021. Our empirical design mitigates concerns about bias related to staggered rollout by ensuring all comparisons are drawn between treated agencies and associated control agencies, i.e. untreated and later-treated agencies (see Deshpande and Li 2019; Goodman-Bacon 2021). For each treated town, we select a set of control towns based any never or not yet treated towns among a set of "peer" towns developed by CTRP3 program staff in the period prior to the first intervention in 2013, and we compare outcomes before and after the intervention for treated agencies relative to their controls.

We find that being identified as a high disparity police department and subjected to the intervention reduces traffic stops of both African-American (19%) and Hispanic (20%) motorists, while leaving the volume of white motorist stops

[4] The second-round proposal at Arnold would expand this effort to technical assistance for up to 10 states through the entire process of initial legislation through program implementation.
[5] I.e. DOJ consent decrees with local departments typically require annual assessments of Bias-Free policing and these assessment reports have drawn on the CTRP3 model, see for example the report for the New Orleans Police Department (DOJ, p. 25, 2021).
[6] From FY 2006 to FY 2019, 24 states collected federal 1906 funds to support data collection and analysis on race in traffic stops. However, the average participation was only 2.5 years, with only Connecticut and Rhode Island participating for five or more years. Connecticut and Rhode Island's extended participation may have been in part due to a collaborative intervention process implemented by CTRP3 program staff. Instead of sparking contentious debate, the interventions were intended to complement the publicly available analytical reports by offering a clear set of collaborative next steps for policing agencies to mitigate disparities and address public concerns.

unchanged. In total, our findings equate to a reduction of approximately 20 minority stops per month for a given treated agency or about 234 stops in the year following treatment. Across all our estimates, the volume of stops is flat approaching the provision of the report and the beginning of any interventions with individual policing agencies. The volume of traffic stops drops, primarily due to the drop in stops involving minority motorists, soon after the beginning of the intervention and remains at a reduced level throughout the next year. Given the continuous twelve month cycle of identification and intervention, we are unable to estimate long term effects beyond twelve months but note that our estimates show no evidence of a reversion to pre-treatment means. We also note that our findings are conservative in that our difference-in-differences approach doesn't capture any statewide impacts to all policing agencies due to the process of identifying and publishing the annual report and summary of the interventions.

Nationally, many jurisdictions have recently enacted rules that eliminate traffic stops for low level offenses. These stops are frequently used by law enforcement as a justification for searching for other illegal activities rather than the actual observed traffic offense, often referred to as pretextual stops (Holder 2023; Kirkpatrick 2022).[7] Naddeo and Pulvino (2024), Matsuzawa (2024), and Rushin and Edwards (2021) document a link between the use or elimination of pretextual stops and racial disparities.[8] To examine the effect of the Connecticut program on pretextual stops, we characterize stops based on legal statute violations and identify a subset of statute codes with unusually high rates of warnings and/or discretionary searches.[9] In addition, because Connecticut only records one reason for stop and sometimes the reason listed as a criminal statutory offense that could only have been observed following the stop, we select stops for pre-textual offences that did not result in a successful search or arrest, and describe the resulting sample of stops as unsuccessful pre-textual stops. We then characterize a separate set of successful, likely pre-textual

[7] States include Virginia and California, and cities include Minneapolis, Philadelphia, Pittsburg, Seattle, St. Paul and Los Angeles, Oakland, and San Francisco, prior to the California state ban.

[8] Also see Feigenberg and Miller (2023) for recent analyses of pre-textual stops based on police search decisions, rather than stop volume.

[9] Specifically, we identify pretextual statutes based on a warning rate and discretionary search or arrest rate which is above the global mean. Using the empirical distribution of search/arrest and warning rates per statutory violation as a starting point, we also hand curate our list of pretextual stops using the text description of violations. In particular, we exclude stops that are generally considered to be moving violations and include additional equipment violations that are widely considered to be driven by pretextual enforcement. The full list of citations we label as pretextual is included in the Appendix Table A.2 and we note that it very closely resembles the list identified by the California Racial and Identity Profiling Act.

stops where either the stop was associated with a statute violation that we characterized as pre-textual with a successful search or an arrest or the true reason for the stop is unobserved because the listed violation was a criminal rather than traffic violation and was unlikely to have been observed prior to a stop. We observe reductions in both categories of potentially pretextual stops with a decline of approximately 43% in unsuccessful pretextual stops and a decline of 38% in successful pretextual stops. Consistent with these findings, we also observe declines in the overall volume of warnings and arrests of about 30% respectively.

Finally, we examine similar event studies using monthly vehicle crash and law enforcement outcomes. This analysis is particularly relevant because equipment violations are often justified as a traffic safety measure while pretextual stops are defended as a crime prevention and deterrence tool. We find no evidence that crashes increase in treated towns, i.e. towns where police departments were found to have high disparity rates and so subject to the intervention.[10] While we find no effect on violent or drug related crime, we do find a 14% decrease in the number of cleared cases for property crimes. The decline in property clearances is only about a one-third the size of the decline in number of stops that lead to arrests, suggesting that the overall reduction in arrests from police stops had only a modest effect on overall criminal enforcement. These findings of no to modest effects on crashes and law enforcement are consistent with other work on pretextual traffic enforcement, i.e. Weiss and Freels (1996), Heaton 2010, Campbell (2023), Matsuzawa (2024), and Naddeo and Pulvino (2024). Although, our findings of a reduction in the clearance rate for property crimes is consistent with Josi et al. (2000) who find that policy induced increases in traffic stops are associated with declines in burglary compared to control neighborhoods selected from the same jurisdiction.

This paper also contributes to a broader literature on disparities in the criminal justice system and police reform. While police more often stop and search minority drivers (Pierson et al. 2020; Kalinowski et al. In Press), they often do not find contraband at higher rates among minority motorists (Feigenberg and Miller 2022), and have been shown to issue minority motorists more severe sanctions (Goncalves and Mello 2021). Racial disparities in the criminal justice system extend beyond traffic stops to the likelihood of conviction (Anwar, Bayer, and Hjalmarsson 2012), denial of bail (Arnold, Dobbie, and Yang 2018), and prison sentence length (Rehavi and Starr 2014). Additional literature examines efforts to reform police conduct and reduce

[10] Note that we use the term "crash" as opposed to "accident" throughout this paper because the former is the preferred term in the scientific literature due to the fact that the latter suggests a lack of fault by motorists involved in the incident.

racial disparities in policing by focusing on either officer-level interventions, often in the form of police training, and agency-wide interventions or investigations. Limited evidence exists on the potential for training to reduce racial disparities in policing outcomes (see for example Owens et al. 2018; Dube et al. 2023; Adger et al. 2023; and Mello et al. 2023). Results from agency-wide federal investigations find a range of effects, from no change in racial disparities (Fagan and Geller 2020) to significant reductions in low-level minority arrests (Long 2019; MacDonald and Braga 2019). Less evidence exists on the effects of programs and interventions to reduce racial disparities in outcomes. Heaton (2010) documents the impact of a police scandal and reform effort to reduce racial disparities in traffic stops, finding a substantial reduction in minority arrests for outcomes likely resulting from a police traffic stop with no evidence of changes in the incidence of vehicle crashes. We contribute to this literature by providing evidence from a years-long structured intervention aimed at reducing disparities in targeted jurisdictions using detailed traffic stop, crime, and accident data.

Institutional Background

While Connecticut has had an anti-racial profiling law on the books since 1999, the well-publicized 2011 case of police profiling in East Haven, CT and subsequent DOJ investigation led to a major legislative overhaul later in 2011. Notably, the new law included provisions that granted the executive branch authority to withhold funding from non-compliant police departments. This legislative effort was also bolstered by U.S. Department of Transportation funding for developing comprehensive traffic stop data systems, which the state used to support the development of CRP3.

A key component of the revised law was the oversight of an Advisory Board which consists of policymakers from the legislative and executive branches, leadership from the policing community, advocacy organizations and community stakeholders, and academic scholars who provide technical expertise. This board collaborated closely with dedicated program staff to develop a data collection system and an analytical framework. The staff's responsibilities extend to refining data collection methodologies, interpretation of analysis results, and recommending policy changes based on empirical evidence. The Advisory Board provides a platform for dialogue among varied stakeholders and, is mandated to monitor program adherence to high analytical standards, as well as to be responsive to community concerns and policing realities. This collective approach has the goal of fostering a transparent, accountable, and inclusive process that underpins the state's efforts to tackle racial disparities in policing effectively.

Central to the development of CTRP3 was the establishment of a statewide electronic data collection system, designed to gather detailed information on every traffic stop statewide by addressing the technical and logistical challenges associated with varying data reporting standards across police departments. A significant portion of the implementation phase was dedicated to training law enforcement personnel on the new reporting requirements and ensuring that data submitted were both comprehensive and accurate. The training emphasized the importance of detailed data capture, including the specifics of each stop, to facilitate detailed analysis of traffic enforcement practices. With the data collection infrastructure in place, the state conducted its inaugural analysis in 2014 and on an annual basis thereafter.[11]

CTRP3 follows a five-step system designed to identify disparities and to actively engage law enforcement agencies in developing and implementing targeted reforms. First, traffic stops are recorded throughout the year in a statewide data collection system. Second, the program staff employs various statistical methods to pinpoint police departments with significant racial disparities using a "preponderance of evidence" strategy, where multiple tests for disparity are used as a screening tool. Tests include descriptive analysis of stop disparities relative to benchmarks including the statewide average, estimated driving population, and resident-only stops. Statistical tests include a "veil of darkness" analysis which examines minority stops in daylight compared to darkness, when police are less able to visually observe a motorist's race before making a stop. Third, the Advisory Board is confidentially provided with a copy of the initial findings and a list of agencies potentially involved in disparate policing. We define this point as the beginning of treatment because immediately following the confidential meeting with the advisory board, program staff begin meeting with identified policing agencies. During these meetings, program staff provide law enforcement administrators with the results of an in-depth analyses of their crime and traffic stop data as well a set of preliminary recommendations for reform. Program staff continue to work intensively with the identified policing agencies to address concerns and implement the reforms. Fourth, the statewide analysis and findings are publicly released approximately six months to one year after the identified departments

[11] In 2023, a high-profile scandal revealed that many state police troopers had falsified traffic stop records from around 2015 onward. The primary motivation was to appear more productive and gain merit-based incentives like choice patrol assignments or specialized vehicles. Troopers exploited a loophole in the connection between the CTRP3 data collection system and their internal records management system. Our study, focused solely on municipal policing agencies, is unaffected by these fake tickets as the loophole was exclusive to the state police system. An independent audit confirmed that ticket falsification was limited to the state police. See: https://portal.ct.gov/-/media/office-of-the-governor/news/2024/20240201-finn-dixon-herling-report-on-csp.pdf

were first notified. Included in the report is a summary of the reforms taken (or not) by the policing agency in their efforts to mitigate the disparity. Lastly, program staff work with local leaders to facilitate community forums about the disparity and the reforms taken by the policing agency.

Key to our study are the intervention and reform phases of the program where identified departments receive notification about the evidence of disparate treatment and are suggested a series of reforms. Prior to the presentation to the board and the meetings with police departments, program staff prepare a detailed quantitative analysis identifying potential drivers of the disparity. These analyses have typically included identifying targeted geographic enforcement of high-minority neighborhoods, documenting patterns of pretextual traffic enforcement, and providing command staff with a list of potentially problematic high-disparity officers. While program staff often continue to work with identified agencies to address their concerns and implement reforms, the recommendations suggested in the initial meetings have typically involved reducing pretextual enforcement and hotspot policing as well as investigating and retraining officers with particularly high disparities. In short, the intervention is largely a presentation of data-driven insights and a discussion about enforcement practices likely to be driving observed disparities.

In our analysis, we estimate an intent-to-treat effect of being identified and subjected to the intervention, noting that a very small number of agencies chose to reject the suggested reforms. While the Advisory Board has the power to recommend reducing funding for non-participating agencies, the board has never taken this step, and the entire process is largely voluntary. However, a more salient concern is the threat of being publicly named as an identified department that is unwilling to address disparate treatment. For the very small number of agencies that initially resisted reform efforts in the early years of the program, there were numerous negative media stories that often brought about leadership changes. A few of the noncomplying agencies appear more than once in the annual report as having racial/ethnic disparities. We estimate intent to treat effects using the date of the first time each agency was identified and offered treatment.

About a period of approximately six months to one year after initial notifications, the Advisory Board publishes a public statewide traffic stop analysis, including a list of the agencies identified as having a disparity.[12] Key to agencies' participation in the program is that the report also includes a summary of positive actions taken by these police agencies to reduce their disparities. Following the release of the report, project

[12] The exact timing of the release of the public-facing report varied over time as the program evolved.

staff also work with policing administrators and local community leaders to hold public forums in affected communities. These forums are intended encourage dialogue and planning about how to continue addressing the issues found. The goal of the process is to craft effective and sustainable solutions by building strategic partnerships between policing administrators, policymakers, advocacy groups, and stakeholders.

Data and Descriptive Statistics

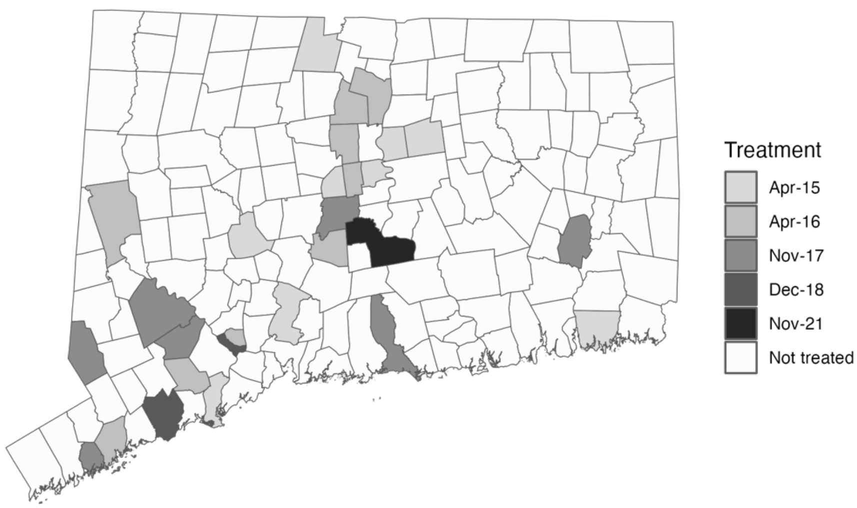
All police departments in the state of Connecticut are required to report all traffic stops starting in October 2013, identifying the time and location of the stop, the reason for the stop, whether a citation or warning was issued, motorist demographics, and whether a vehicle search or arrest took place. We obtained a database covering all traffic stops recorded by all police departments in the state . This analysis focuses on 2.55 million traffic stops made by 94 municipal police departments from October, 2013 to December, 2021. Table 1 presents descriptive statistics for the entirety of the approximately seven year period with column 1 presenting means for the 29 towns that were ever identified as having high rates of disparities in traffic stops[13], and column 2 presenting descriptive statistics for all other town police departments in the state. As shown below, treated towns generally make a higher volume of traffic stops, stop more minority motorists, and have higher rates of pretextual traffic stops.

When the racial profiling project began in 2013, a principal components analysis of town demographic attributes was run for all towns with independent police departments in the state. For each town with its own police department, five control towns were selected based on being the five towns that scored closest to the subject town on the primary principle component.[14] For each report year and identified town police department with high disparities, we create a sample with all stops 12 months before and 12 months after the report by departments in both the identified town and all the control towns that at the time of the report had not been identified itself as a

[13] Treated towns and their intervention dates include: Groton, Apr-15; Granby, Apr-15; Waterbury, Apr-15; Wethersfield, Apr-15; Hamden, Apr-15; Manchester, Apr-15; New Britain, Apr-15; Stratford, Apr-15; East Hartford, Apr-15; Ansonia, Apr-16; Bloomfield, Apr-16; Meriden, Apr-16; New Milford, Apr-16; Newington, Apr-16; Norwalk, Apr-16; Trumbull, Apr-16; West Hartford, Apr-16; Windsor, Apr-16; Berlin, Nov-17; Monroe, Nov-17; Newtown, Nov-17; Norwich, Nov-17; Ridgefield, Nov-17 Darien, Nov-17; Madison, Nov-17; Derby, Dec-18; Fairfield, Dec-18; and Middletown, Nov-21.
[14] The demographic attributes are drawn from the 5 year moving averages of the American Community Survey.

town department with high disparities.[15] Figure 1 provides a map of the treated towns disaggregated by the wave of treatment. Appendix Table A.3 provides a list of the treated and control towns based on this criteria and we provide additional empirical results in the appendix where we use all other towns as controls.

Figure 1: Treated and Untreated Departments by Wave of Treatment



Notes: Treated towns are shaded according to the treatment wave. Treated towns that are treated in multiple waves are shaded according to the month in which initial findings were released.

In columns 3 and 4 of Table 1,We present the same set of descriptive statistics for our stacked panel where untreated and later treated departments are repeated as controls and where we restrict the time period to twelve months before/after each groups' respective treatment event. The data are pooled across all ever-treated towns and their control towns for that town's year of treatment, and then are collapsed to the month by treatment year by treatment group by town by violation type by race/ethnicity of the motorist, where treatment group captures includes both a treated town and its control towns. Column 3 presents the descriptive statistics for the sample of stops in all treated towns, and column 4 presents the statistics for stops in all control towns within this stacked sample. The patterns in the stacked panel are similar to that

[15] We look 12 months after the provision of the report to the state because the report is publicly released 12 months after that date and we observe a one-month reactionary drop in traffic enforcement across all control towns when the report is publicly released.

in the entirety of the data where treated towns generally make a higher volume of traffic stops, stop more minority motorists, and have higher rates of pretextual traffic stops.

Table 1: Descriptive Statistics of Traffic Stops

Sample	1[Ever Treated]		1[Never Treated]		1[Treatment]		1[Control]	
Time Period	10/2013 – 12/2021				+/- 12 Months of Group Treatment			
Sample	Unique Agency by Month Obs.				Stacked Panel			
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Total Stops	374.74	217.03	246.08	208.05	368.88	222.53	277.42	220.36
Stops (Any Min.)	137.42	105.77	56.30	53.12	156.91	122.98	66.15	59.51
Stops (Black/AA)	64.71	58.86	24.37	23.85	77.35	69.39	28.66	26.44
Stops (Hisp/Lat)	57.39	49.73	24.88	25.76	63.88	59.03	29.21	28.64
Stops (Pretext)	20.43	42.46	17.51	29.73	19.03	33.90	18.85	33.98
Stops (Moving)	189.96	117.83	128.65	107.37	187.86	112.02	142.42	112.99
Stops (Equip.)	108.98	109.08	68.45	81.89	105.16	111.66	80.57	94.51
Stops (Admin)	41.05	32.25	27.51	33.41	42.14	35.61	29.83	33.34
Stops (Warning)	19.65	14.11	13.04	9.11	19.00	13.09	14.39	10.97
Stops (Cites)	42.08	28.69	32.11	28.11	45.03	30.11	33.80	27.22
Stops (Search)	90.78	88.20	50.94	66.70	90.75	88.97	60.45	73.80
Stops (Arrests)	90.55	69.45	51.54	56.75	89.10	69.69	60.44	61.82
N=	1337		2940		640		2585	

We use the empirical distribution of search/arrest and warning rates per statutory violation as a starting point classifying violations that have a search/arrest or warning rate above the global mean for all stops. Given the skewed distribution of warnings and search/arrests by violation type, this strategy identifies a set of violations types that depart substantially from common moving violations. We also hand curate our list of pretextual stops using the text description of violations. We exclude stops that are generally considered to be moving violations and include additional equipment violations that are widely considered to be driven by pretextual enforcement, e.g. tinted windows, turning without signaling, color or intensity of lights, windshield obstruction, snow/ice removal, additional classes of lighting violations The full list of citations we label as pretextual is included in the Appendix Table A.2. This list also identifies all by hand reclassifications. The final list very closely resembles the list identified by the California Racial and Identity Profiling Act.

In cases where an arrest or successful search occurred, the reason for the stop is often coded as a criminal statute that was violated by later actions that lead to an arrest or the results of the successful search, a statute violation that could not have been observed prior to the stop. We classify stops as (1) unsuccessful likely pretextual stops which were flagged using the criteria in the previous paragraph, but where the outcome of the traffic stop did not yield a successful search or an arrest; (2) stops that would be considered successful from a pre-textual perspective where the statute was flagged using the same criteria as the first classification, but yielded an successful search or arrest, or the stop reason listed was a criminal violation that could not have been observed prior to making a traffic stop[16]; and (3) successful potentially pretextual inquiries leading to a stop where we include administrative violations that require the officer to make a decision to run a license plate before making a decision to stop a vehicle. Row five, Stops (Pretext), contains all stops in these three categories.

We also assemble monthly data on traffic crashes from the State of Connecticut and crime and law enforcement data from the Uniform Crime Reports (UCR). The crash data identifies the number of crashes each month in each town in the state of Connecticut overall and by cause. Notably, we exclude crashes made on limited access highways since these roads are primarily patrolled by the Connecticut State Police. The UCR data identifies both total crimes and cleared crimes by police department and by type of crime.

Empirical Design

The dataset described above is constructed following the "stacked event study" design discussed in Deshpande and Li (2019) and Goodman-Bacon (2021). We create a series of datasets, or treatment groups, that consist of a treated town police department and comparison departments that have never or not yet been treated Then, by including separate fixed effect vectors for each treatment group, we can avoid concerns about bias from staggered roll-out because all comparisons are between treated and the associated not yet treated control observations. Specifically, we estimate models for the number of traffic stops (or crime clearances and accidents) Y_{gitv} in treatment group g, town police department i, month t, and violation v. We center on the month of treatment and estimate a linear regression

$$Y_{git} = \alpha_{gtv} + \gamma_{giv} + \sum_{\tau} \delta_{\tau}\left(1[treated]_{gi} * D_{gt}^{\tau}\right) + \eta_{gitv} \tag{1}$$

[16] Some examples include reasons for stop listed as drug possession, possession of stolen property, uninsured motorist, resisting arrest, or other violations unlikely to be observable prior to making a traffic stop.

where $1[treated]_{gi}$ is an indicator for whether town i is the treated town within group g, D_{gt}^{τ} is an indicator for whether τ equals the centered month t of the stop data, α_{gtv} are group by month by violation and γ_{giv} are group by town police department by violation fixed effects. We focus on estimating the impact of the program on the volume of traffic stops (or crimes and accidents) in levels. However, we note that we are robust to estimating impacts on the outcome in natural log and the inverse hyperbolic sine.

In our main estimates examining impacts across all traffic stops, we highlight the fact that we interact group-by-month and group-by-town police department fixed effects with sixteen violation categories. These categories, identified based on statutes by program staff at CTRP3, are provided in the publicly available data. In most other estimates, we don't include this interaction and instead condition the sample on a subset of violations, e.g. moving violations, equipment violations, or pretextual violations. In our main estimates of the effect of the intervention on overall traffic stops, we include this more granular set of fixed effects because much of the variation in traffic enforcement (seasonal and agency) is due to voluntary participation in federally funded enforcement campaigns sponsored by the National Highway Traffic Safety Authority. While collapsing violations and excluding these interacted fixed effects yield estimates of similar magnitude for the effect of the program on total stop volume, the precision is significantly reduced without these key controls.

This fixed effects structure assures that a separate difference-in-differences model is estimated for each treatment group and violation type, and the parameters δ_{τ} capture the average treatment effect across the group by type specific event studies. Traditionally, one would cluster at the level of the cross-sectional fixed effects in a difference-in-differences model, i.e. group by town police department by violation type. However, the same individual town police department can be a control department for multiple treated departments and so we cluster at the police department by violation type level. We also estimate models separately by violation type or separately for pretextual stops or by post-stop violations in which case we estimate models conditional on group by month fixed effects α_{gt} and group by town police department fixed effects γ_{gi} and cluster at the police department level.

As noted above, our primary set of estimates are based on using "peer" towns that were developed at the beginning of the CTRP3 project. In the appendix, we include estimates obtained from a sample that includes all town police departments as controls but where following Abadie (2005) we apply inverse propensity scores weights (IPSW) to balance the treatment and control samples. We estimate the IPSW using 2013 5-year Census ACS demographics, economic variables, and pre-treatment traffic stop volume.

Results for Traffic Stops

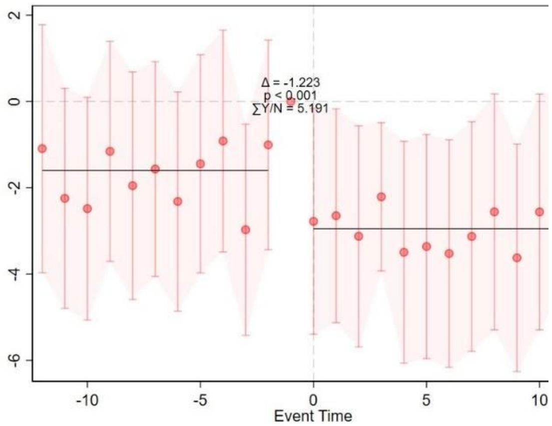
In Figure 2, we present estimates of the effect of the intervention on the total volume of traffic stops. For a composite grouping of all minority motorists (panel a), we estimate that the intervention resulted in a decline of -1.22 (23.56%) traffic stops made by an agency in a given month and each violation category. Collapsing across violation categories, this translates to an overall reduction of 19.52 stops per month per agency or 234.24 stops per agency in the year following treatment. For Black/AA (panel c) and Hispanic/Latino (panel d) motorists, we estimate relative declines of 19.72% and 18.93% respectively. In contrast for White Non-Hispanic (panel b) motorists, we estimate a statistically insignificant relative decline of only 1.15%. Appendix Figure A.1 reports a qualitatively similar set of estimates applying IPSW to all non-treated and later-treated units as controls. Table A.4-A.6 contain additional robustness checks with narrower bandwidths, including a control for Census demographics interacted with a linear time trend, and with a donut hole around the intervention month. In Table A.7, we estimate the intervention's impact separately for each wave of treatment.[17]

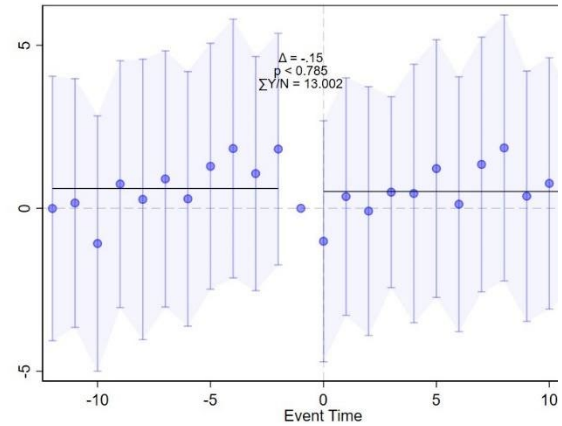
In Figure 3 panel a, we estimate the effect of the intervention on stop counts based on pretextual traffic stops. Across all potentially pretextual stops, we estimate a 40.0% decline for minority motorists or a reduction of approximately 17 traffic stops per month per police department following treatment.[18] We further disaggregate pretextual traffic stops into our three distinct categories: unsuccessful likely pretextual stops (panel b), successful likely pretextual stops (panel c), and potentially pretextual

[17] Contrary to expectations, the first wave did not show the largest impact, even though agencies with the most severe disparities were targeted in the first wave. These smaller effects may arise because agencies had not yet seen the public report and summaries. Since the intervention was mostly voluntary, first-wave agencies tended to be less cooperative with reforms. Agencies identified in later waves showed higher compliance due to witnessing the consequences of noncompliance. This pattern is consistent with a higher implicit compliance rate and larger intent to treat effect estimates in subsequent waves.

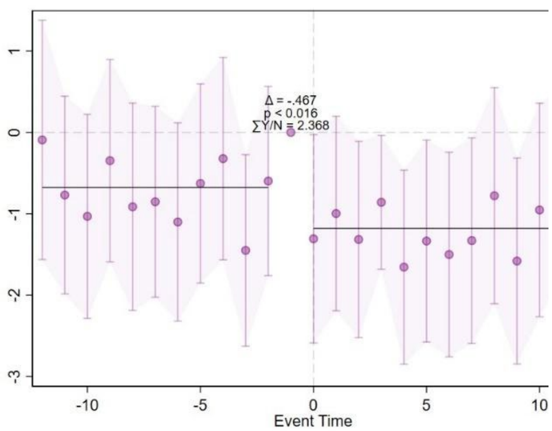
[18] A reduction of approximately 200 traffic stops is about 86% of the effect reported in Figure 2.

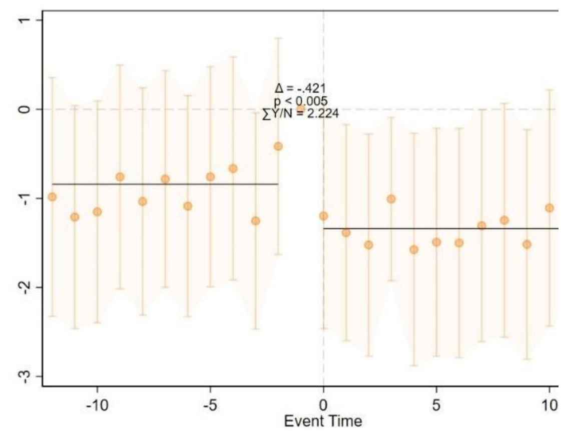
Figure 2: Impact of Intervention on Volume of Traffic Stops





(a) All Minority Motorists (b) White Non-Hispanic Motorists



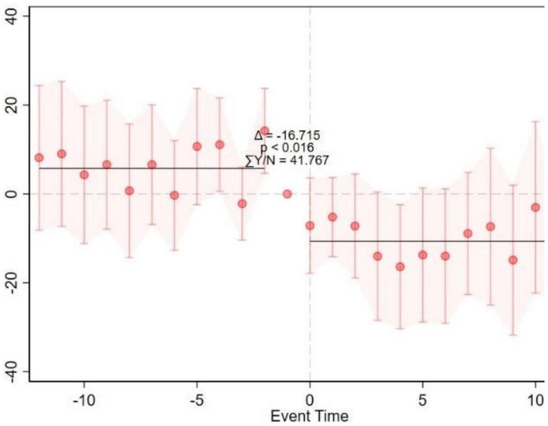


© Black/AA Motorists (d) Hispanic/Latino Motorists

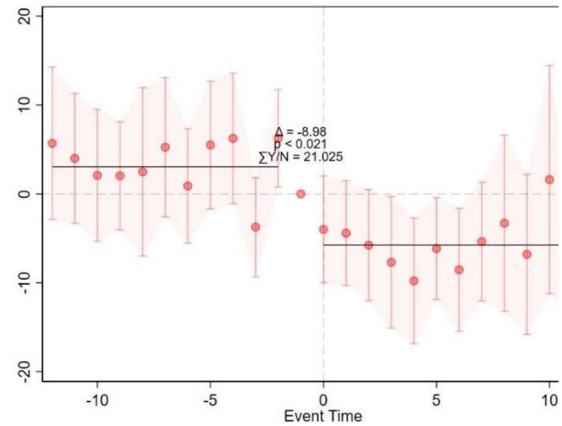
Notes: We plot a 95% confidence interval around the coefficient δ_τ using the point estimates and standard errors obtained from estimating Equation 1 on the volume of stops by race in the stacked panel. The stacked panel consisted of 51,600 group by agency by month by violation observations. The annotation Δ is the point estimate from a difference-in-differences estimator comparing periods -12 to -2 periods with 0 to +12 periods relative to the intervention in the treated relative to control agencies. Standard errors are clustered on agency by violation type.

inquiries (panel d). Across these three categories of pretextual stops, we estimate relative effects of 42.71% (panel b), 37.51% (panel c), and 37.14% (panel d). In Appendix Figures A.2 and A.3, we examine pretextual stop effects by race and ethnicity and estimate comparable declines of 27-41% for pretextual stops of Black/AA motorists and 30-44% for Hispanic/Latino motorists. In Appendix Figure A.4, we estimate statistically imprecise declines of only 1-5% for White Non-Hispanic motorists. Appendix Figure A.5 reports a qualitatively similar set of estimates applying IPSW to all non-treated and later-treated units as controls. Appendix Figures A.6-8, report results for the statute groupings contained in the raw data which show the largest declines occurring in defective lighting and criminal violations.

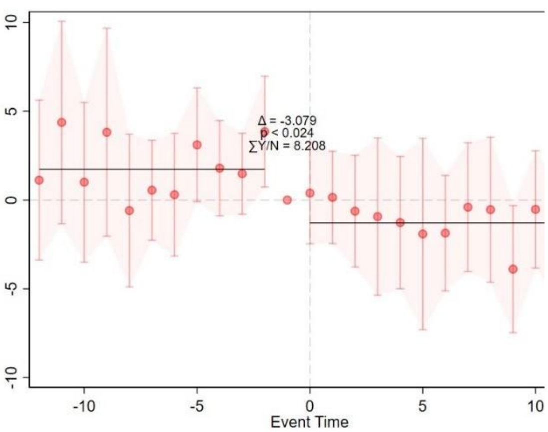
Figure 3: Impact of Intervention on Pretextual Traffic Stops for Minority Motorists



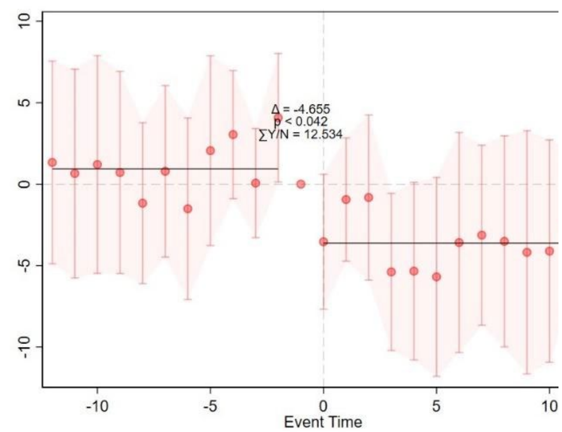
(a) Any Type of Pretextual Stops



(b) Unsuccessful Pretextual Stops



(c) Successful Pretextual Stops



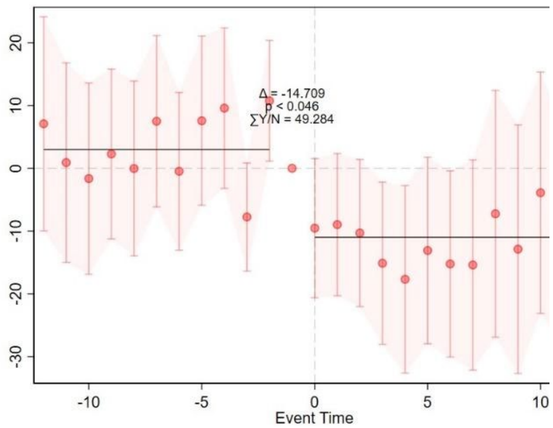
(d) Potential Successful Pretextual

Notes: We plot a 95% confidence interval around the coefficient δ_τ using the point estimates and standard errors obtained from estimating a variation of Equation 1 on the volume of stops by pretextual violation and race in the stacked panel. We include group by time α_{gt} and group by agency γ_{gi} fixed effects and collapse observations over violation such that the stacked panel consists of 3,255 group by agency by month observations. Standard errors are clustered on agency. The annotation Δ is the point estimate from a difference-in-differences estimator comparing periods -12 to -2 periods with 0 to +12 periods relative to the intervention in the treated relative to control agencies.

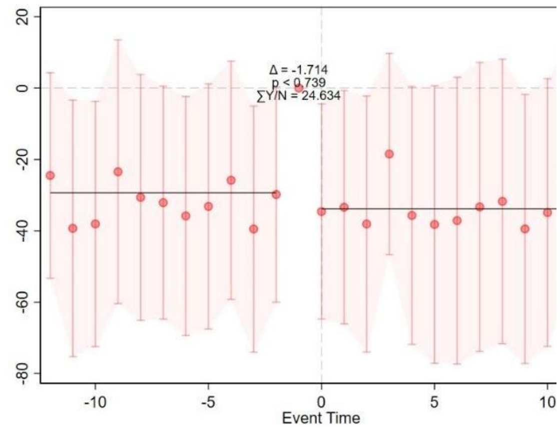
In Figure 4, we estimate the effect of the intervention on the volume of stops resulting in a specific set of dispositions. We estimate a relative decline of 29.85% of stops resulting in a warning (panel a) or a reduction of approximately 15 warnings per month per department following treatment. Similarly, we estimate a relative decline of

30.68% in traffic stops resulting in an arrest or a reduction of approximately 3 arrests per month per department. We estimate statistically imprecise and relatively small declines of 7% and 2.8% for citations and searches respectively. In Appendix Figure A.9 and A.10, we again estimate specific models and obtain comparable declines of 22-29% for arrests and warnings of Black/AA motorists and 22-34% for Hispanic/Latino motorists. In Appendix Figure A.10, we estimate declines of only 0-3% for arrests and warnings of White Non-Hispanic motorists. Appendix Figure A.12 and Table A.13 reports a qualitatively similar set of estimates applying IPSW to all non-treated and later-treated units as controls.

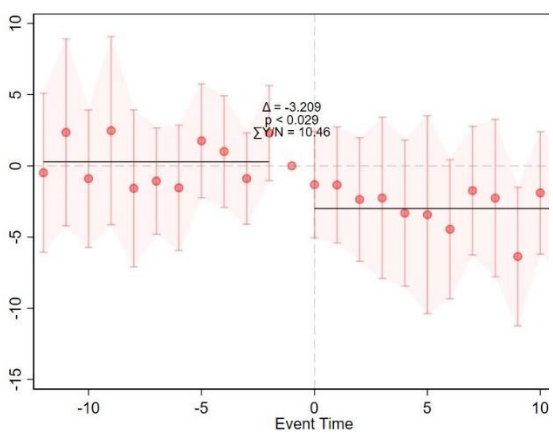
Figure 4: Impact of Intervention on Traffic Stop Outcomes for Minority Motorists



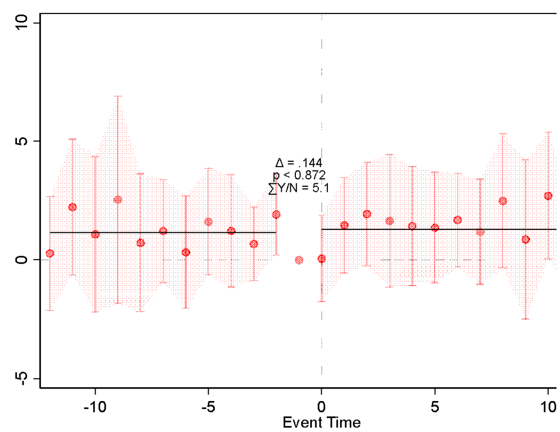
(a) Warnings



(b) Citations



(c) Arrest



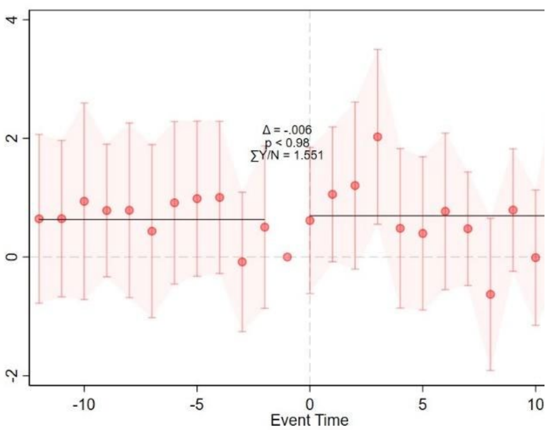
(d) Search

Notes: We plot a 95% confidence interval around the coefficient δ_τ using the point estimates and standard errors obtained from estimating a variation of Equation 1 on the volume of stop outcomes by race in the stacked panel. We include group by time α_{gt} and group by agency γ_{gi} fixed effects and collapse observations over violation such that the stacked panel consists of 3,255 group by agency by month observations. Standard errors are clustered on agency. The annotation Δ is the point estimate from a difference-in-differences estimator comparing periods -12 to -2 periods with 0 to +12 periods relative to the intervention in the treated relative to control agencies.

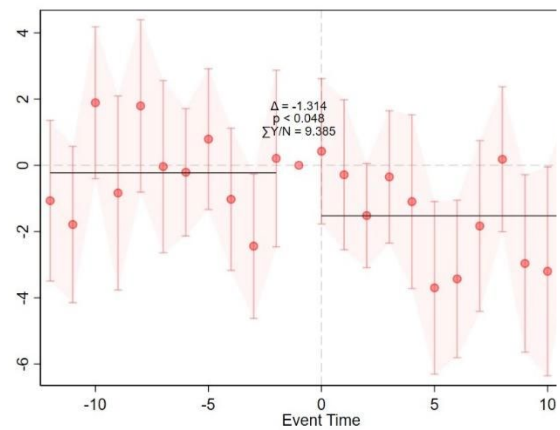
Community Level Outcomes

Pretextual police stops are often justified as a way to increase the effectiveness of law enforcement and deter criminal activity, while equipment violations, especially violations associated with defective lighting, are often justified as an effort to improve traffic safety. Therefore, one concern arising from reductions in likely pretextual stops and equipment violations more broadly is that this may lead to higher rates of crime, lower criminal case clearance rates, or higher motor vehicle crash rates.

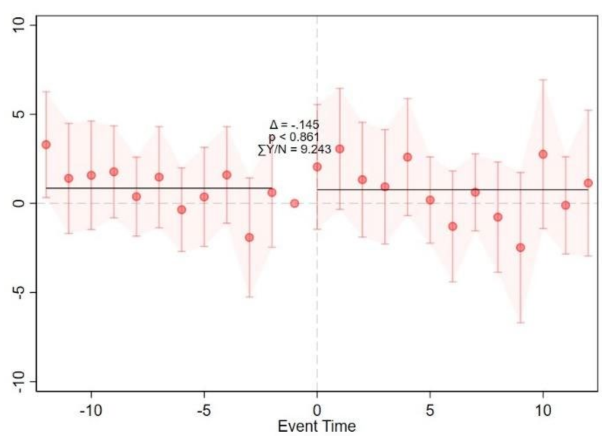
Figure 5: Impact of Intervention on Crimes Cleared by Arrest or Exceptional Means



(a) Part 1- Violent Crimes



(b) Part 1- Property Crimes



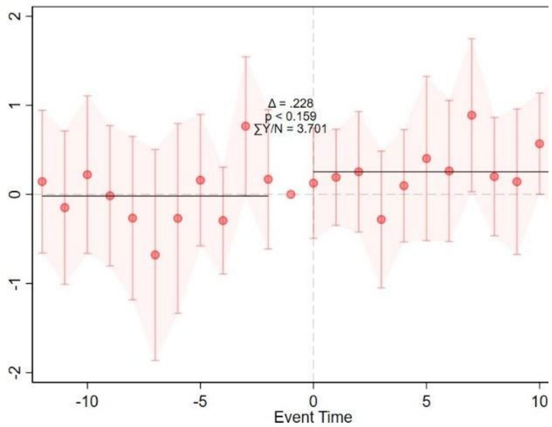
(c) Non-Part 1

Notes: We plot a 95% confidence interval around the coefficient δ_τ using the point estimates and standard errors obtained from estimating a variation of Equation 1 on the volume of cleared crimes in the stacked panel. We include group by time α_{gt} and group by agency γ_{gi} fixed effects such that the stacked panel consists of 3,255 group by agency by month observations. Standard errors are clustered on agency. The annotation Δ is the point estimate from a difference-in-differences estimator comparing periods -12 to -2 periods with 0 to +12 periods relative to the intervention in the treated relative to control agencies.

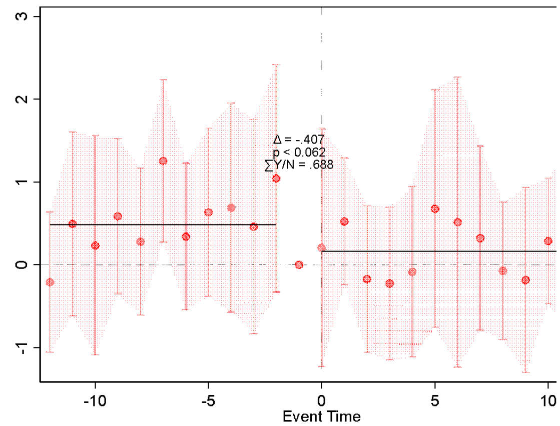
Figure 5 presents results for the number of cleared crime cases by month by town police department from the Uniform Crime Reports separately for violent crimes, property crimes and non-Part 1 crimes, primarily drug offenses. In these estimates, we estimate a variant of Equation 1 where the unit of observation is a town by month and where we exclude an irrelevant interaction with violation category. While treatment has no effect on violent crimes or non-Part 1 crimes, we do observe a decline in clearances for property crimes of about 1.3 cases per month per town or approximately a 14% decline. However, this decline is notably smaller in magnitude than the decline of 3 arrests from traffic stops. The smaller absolute decline in clearance, compared to the decline in arrests from stops, could be explained through three possible mechanisms: the decline in arrests from traffic stops was primarily among arrests that did not lead to charges, the decline in arrests was among cases that would have been solved anyway without the traffic stop, or the reduction in pretextual traffic stops allowed manpower to be diverted to other productive policing activities. Regardless of the reason, the smaller effects on clearances suggest a more limited decline in the effectiveness of policing, than suggested by the decline in the number of arrests arising from police stops.

Figure 6 presents the results for crashes both the total number per town per month (panel a), and separately by reason for the crash: equipment related (panel b), distracted driving (panel c), and speeding or other moving violations (panel d). In these estimates, we estimate a variant of Equation 1 where the unit of observation is a town by month by type of roadway, i.e. we interact the fixed effects with 20 major roadway categories rather than violation. We do not find any evidence of an increase in crashes in response to treatment. If anything, we find a modest but noisy decline in equipment related crashes which might be explained by refocusing enforcement on moving violations or distracted driving. Overall, these results suggest a negligible impact of the intervention on roadway safety.

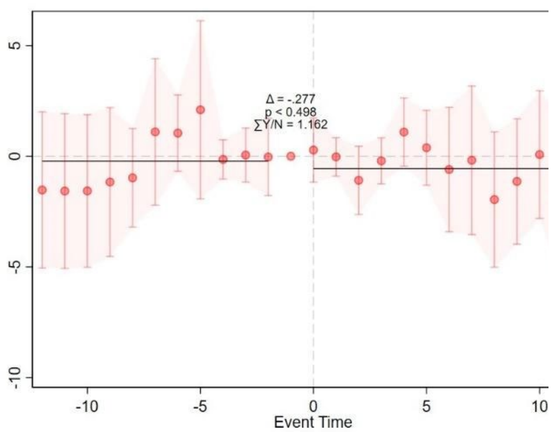
Figure 6: Impact of Intervention on Crashes



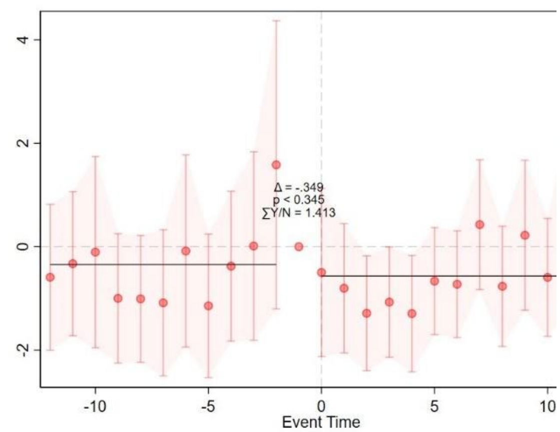
(a) Total Crashes



(b) Equipment Related



(c) Distracted Driving



(d) Speed/Moving Related

Notes: We plot a 95% confidence interval around the coefficient δ_τ using the point estimates and standard errors obtained from estimating a variation of Equation 1 on the volume of crashes in the stacked panel. We include group by time by road type α_{gtr} and group by agency by road type γ_{gir} fixed effects such that the stacked panel consists of 29,442 group by agency by month by road type observations. Note that crash data is only available starting in January of 2014. Standard errors are clustered on agency by road type. The annotation Δ is the point estimate from a difference-in-differences estimator comparing periods -12 to -2 periods with 0 to +12 periods relative to the intervention in the treated relative to control agencies.

Conclusions

In this study, we examine the impact of one of the first state-wide initiatives to reduce racial profiling in police stops, the Connecticut Racial Profiling Prohibition Project. This program has been in place for more than a decade and has been a model for other states and localities as they have been developing their own programs. Using a stacked difference-in-differences analysis, we find substantial reductions in stops of African-

American and Hispanic motorists on the order of 30% of the mean number of stops, with minimal changes in the number of stops of non-Hispanic whites.

The decline in stops is divided approximately equally among stops that might be reasonably classified as unsuccessful pretextual and stops and inquiries that might be considered successful based on a successful search, a criminal violation and/or an arrest. The unsuccessful pretextual stops decline by approximately 42%, and stops that resulted in warnings decline by 30%. At the same time, we also see declines of 37% in stops that might be categorized as successful stops. Further, we observe an associated decline in arrests of 31%.

In summary, the policy led to a decline of 7.5 stops of black motorists and 6.7 stops of Hispanic motorists by town by month reducing disparities across the state, but at a cost of 3 fewer successful pretextual stops and 3 fewer arrests. At the same time, when looking at clearance rates, the decline in the number of clearance rates by town by month was only about 1.3 property crimes suggesting that either many of these arrests did not actually lead to successful charges or there were offsetting activities that replace the majority of these lost arrests in terms case clearances. We also did not find any evidence of higher crash rates in treated towns as a result of the program.

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Appendix Tables

Table A.1: Relevant Intervention Dates for Agencies Identified in Annual Reports

Department Name	Analysis Year	Month Initial Findings Released	Findings published after several meetings
Groton	10/1/13 to 9/30/14	Apr-15	Apr-16
Granby	10/1/13 to 9/30/14	Apr-15	Apr-16
Waterbury	10/1/13 to 9/30/14	Apr-15	Apr-16
Wethersfield	10/1/13 to 9/30/14	Apr-15	Apr-16
Hamden	10/1/13 to 9/30/14	Apr-15	Apr-16
Manchester	10/1/13 to 9/30/14	Apr-15	Apr-16
New Britain	10/1/13 to 9/30/14	Apr-15	Apr-16
Stratford	10/1/13 to 9/30/14	Apr-15	Apr-16
East Hartford	10/1/13 to 9/30/14	Apr-15	Apr-16
Ansonia	10/1/14 to 9/30/15	Apr-16	Jul-17
Bloomfield	10/1/14 to 9/30/15	Apr-16	Jul-17
Meriden	10/1/14 to 9/30/15	Apr-16	Jul-17
New Milford	10/1/14 to 9/30/15	Apr-16	Jul-17
Newington	10/1/14 to 9/30/15	Apr-16	Jul-17
Norwalk	10/1/14 to 9/30/15	Apr-16	Jul-17
Trumbull	10/1/14 to 9/30/15	Apr-16	Jul-17
West Hartford	10/1/14 to 9/30/15	Apr-16	Jul-17
Windsor	10/1/14 to 9/30/15	Apr-16	Jul-17
Wethersfield	10/1/14 to 9/30/15	Apr-16	
New Britain	10/1/14 to 9/30/15	Apr-16	
Stratford	10/1/14 to 9/30/15	Apr-16	
Berlin	10/1/15 to 9/30/16	Nov-17	Oct-18
Monroe	10/1/15 to 9/30/16	Nov-17	Oct-18
Newtown	10/1/15 to 9/30/16	Nov-17	Oct-18
Norwich	10/1/15 to 9/30/16	Nov-17	Oct-18
Ridgefield	10/1/15 to 9/30/16	Nov-17	Oct-18
Darien	10/1/15 to 9/30/16	Nov-17	Oct-18
East Hartford	10/1/15 to 9/30/16	Nov-17	
Meriden	10/1/15 to 9/30/16	Nov-17	
Stratford	10/1/15 to 9/30/16	Nov-17	
Trumbull	10/1/15 to 9/30/16	Nov-17	
Wethersfield	10/1/15 to 9/30/16	Nov-17	
Ansonia	10/1/13 to 9/30/16	Nov-17	
Madison	10/1/13 to 9/30/16	Nov-17	Oct-18
Derby	1/1/17 to 12/31/17	Dec-18	Jun-19

Fairfield	1/1/17 to 12/31/17	Dec-18	Jun-19
Middletown	1/1/18 to 12/31/20	Nov-21	Nov-22

Notes: Departments are listed by the year in which the analysis of its stops data was conducted, the month initial findings were released, and the month in which findings were made public. Agencies with no public findings published date (Column 4) are those that previously had findings released but were identified in subsequent analysis for treatment. Agencies may be listed more than once if they were identified for treatment in subsequent analysis after their initial identification. In our analysis, we estimate intent to treat models using the first date they were offered the intervention.

Table A.2: Statute Codes Labeled as Pretextual Traffic Stops

Statute	Description	Stops	Empiric. Criteria	Hand Curated	Definite	Endogen	Potential	Any
14-96(C)	Lighted lamps and	56	1		1			1
53A-181	Breach of the	58	1			1		1
14-99GE	Definitions Tinted or	59	1		1			1
14-96(A)A	Lighted lamps and	61	1		1			1
14-18A2	Display of number	70	1				1	1
14-96F	Lighted lamps and	70	1		1			1
14-80(C)	Mechanical equipment	72	1		1			1
14-96(E)	Lighted lamps and	72	1		1			1
14-230(B)	Driving in right-hand	77	1		1			1
14-18A(1)	Display of number	78	1				1	1
53A-125B	Larceny in the	80	1			1		1
14-99G(E)*	Definitions Tinted or	82	1		1			1
14-80(D)	Mechanical equipment	84	1		1			1
14-99F	Definitions Tinted or	88	1		1			1
14-98A	Tires to be	101	1		1			1
14-18(E)*	Display of number	109	1				1	1
14-242A	Turns restricted Signals	118	1		1			1
14-243(B)	Starting or backing	121	1		1			1
14-80A	Mechanical equipment	124	1		1			1
14-80B	Mechanical equipment	128	1		1			1
14-243	Starting or backing	129	1		1			1
14-220(A)	Slow speed	143	1		1			1
14-242(A)	Turns restricted Signals	181	1		1			1
14-289D(B)	devices for	188	1		1			1
14-99G(E)	Definitions Tinted or	216	1		1			1
14-21B(C)	reflectorized	219	1				1	1
53A-110A	Infraction	223	1			1		1
14-237	Driving on divided	233	1		1			1
14-99F(B)	Definitions Tinted or	267	1		1			1
14-18C	Display of number	359	1				1	1
14-99F(A)	Definitions Tinted or	380	1		1			1
14-235	Vehicle not to	440	1		1			1
14-80(A)	Mechanical equipment	489	1		1			1
14-99	Definitions Tinted or	494	1		1			1

14-99G	Definitions Tinted or	570	1		1			1
14-80(B)	Mechanical equipment	577	1		1			1
14-99GG	Definitions Tinted or	586	1		1			1
14-230A	Driving in right-hand	938	1		1			1
14-101	N/A	1095	1			1		1
14-18	Display of number	1104	1				1	1
14-244	Signals	1322	1		1			1
14-230	Driving in right-hand	1708	1		1			1
14-18(C)	Display of number	1991	1				1	1
14-99G(G)	Definitions Tinted or	2240	1		1			1
14-230(A)	Driving in right-hand	2879	1		1			1
14-18A	Display of number	2937	1				1	1
14-164C(N)	emissions	3369	1				1	1
14-242	Turns restricted Signals	4894	1		1			1
14-18(A)	Display of number	8716	1				1	1
14-236	Multiple-lane highways	8899	1		1			1
14-163C	Motor carrier safety	59		1			1	1
14-96(A)	Head and rear	59		1	1			1
14-96D	Head and rear	59		1	1			1
14-96I	Head and rear	59		1	1			1
14-252	Parking so as	61		1	1			1
14-99GB	Definitions Tinted or	61		1	1			1
53A-181A	Creating a public	61		1		1		1
21A-279(A)	Penalty for illegal	64		1		1		1
14-242E	Turns restricted Signals	65		1	1			1
12-236G**	N/A	66		1			1	1
21A-279A	Limits for legal	72		1		1		1
14-17	Notice of change	75		1	1			1
14-96BA	Head and rear	75		1	1			1
14-164CN	General penalty	80		1		1		1
22A-250(A)	Forfeiture of vehicles	81		1		1		1
14-12(D)*	registration	83		1			1	1
14-36A(C)	operator's	91		1			1	1
14-253A	Special license plates	97		1			1	1
14-96E(A)	Head and rear	99		1	1			1
14-12A(F)	registration	100		1			1	1
38A-371	Mandatory security	104		1		1		1
14-96Q	Head and rear	110		1	1			1

14-36	operator's	115		1			1	1
14-35A	Restrictions on owner	118		1			1	1
14-147(A)	Improper use of	121		1			1	1
21A-279A1ST	Penalty for illegal	122		1		1		1
14-99F(C)	Windshield Obstruction	123		1	1			1
14-271(B)	Securing of loads	130		1	1			1
14-12(A)(2)	registration	134		1			1	1
14-96C(A)	Head and rear	137		1	1			1
14-243A	Starting or backing	148		1	1			1
14-36(A)**	operators	153		1			1	1
14-100A(C)(2)	Glass	155		1	1			1
14-100(A)(C)1	Glass	157		1	1			1
14-100A(C1)*	Glass	158		1	1			1
14-100AC	Glass	161		1	1			1
14219B	Limitation of municipal	162		1	1			1
14-100A(C)1	Glass	163		1	1			1
14-80	Mechanical equipment	164		1	1			1
14-298A	Operation of motor	181		1	1			1
1496	Head and rear	189		1	1			1
14-96R	Color of stop	196		1	1			1
14-147A	Theft or illegal	200		1			1	1
14-96C(C)	Head and rear	216		1	1			1
14-100A(C)	Glass	224		1	1			1
14-18(A)(2)	Display of number	224		1			1	1
14-252A(A)	Removal of ice	288		1	1			1
14-96B(A)	Head lamps	290		1	1			1
14-100A	Glass	300		1	1			1
14-96G	Colors of lamps	307		1	1			1
14-96P	Color of lights	350		1	1			1
14-100A(A)	Glass	358		1	1			1
14-213	License	382		1			1	1
14-215A	License	494		1			1	1
14-242(E)	Turns restricted Signals	507		1	1			1
14-243(A)	Starting or backing	512		1	1			1
14-147	N/A	520		1			1	1
14-13	Registration	521		1			1	1
14-36A	operators	552		1			1	1
14-147C	Improper use of	700		1			1	1
14-100	Glass	715		1	1			1
14-251	Parking vehicles	727		1	1			1
14-96U	Use of multiple-beam	775		1	1			1

14-234	no-passing	1068		1	1			1
14-215(A)	registration	1132		1			1	1
14-213B	N/A	1165		1			1	1
14-99G(B)	Definitions Tinted or	1229		1	1			1
14-296	General penalty	1266		1	1			1
14-36(A)	operator's	1402		1			1	1
14-96A	Lighted lamps and	1482		1	1			1
14-12	registration	1538		1			1	1
14-96AA	specifications	1709		1	1			1
14-240	Vehicles to be	1719		1	1			1
14-12A*	Registration of certain	1837		1			1	1
14-147(C)	improper use of	1946		1			1	1
14-219B	Limitation of municipal	2411		1	1			1
14-96	Lighted lamps	2692		1	1			1
14-215	registration	5307		1			1	1
14-12A	registration	5708		1			1	1
14-12(A)	registration	6021		1			1	1
14-298	Office of the	6540		1	1			1
14-96E	Lighted lamps and	7269		1	1			1
14-96B	Lighted lamps and	7484		1	1			1
14-96Y	Lighted lamps and	9818		1	1			1
14-96A(A)	Lighted lamps and	9828		1	1			1
14-96C	Lighted lamps and	12169		1	1			1
14-12(A)*	registration	24483		1			1	1
14-239	One-way streets	89	1	1				
N/A	N/A	109	1	1				
14-227	Operation while under	122	1	1				
14-303	Designation of one-way	807	1	1				

Notes: Statute code labels from traffic stops data from sample October, 2013 to December, 2021 are listed by the volume of associated stops in Column 3. In Column 4, we list statutes that meet our preliminary criteria for pretextual stops of having a warning rate and discretionary search or arrest rate above the sample global mean. In Column 5, we report statutes that we picked based on institutional knowledge and discretion for inclusion in the final pretextual categories. Statutes coded as both meets empirical criteria (Column 4) and hand curated (Column 5) are statutes that were removed from the list of pre-textual stops because they were clearly moving violations. In Column 6-10, we report statutes included in the three pretextual categories used in the empirical analysis as well as a composite inclusive of these categories.

Table A.3: Treatment and Control Departments in Stacked Panel with 2013 CTRP3 Peer Group Definitions

Department	Peer Group Towns				
Ansonia (4/2016)	Derby (12/2018)	Naugatuck	Stratford (4/2015)	Shelton	Berlin (4/2017)
Berlin (4/2017)	Shelton	Glastonbury	Naugatuck	Bristol	Plymouth
Bloomfield (4/2016)	Windsor (4/2016)	Suffield	Cromwell	Enfield	Hamden (4/2015)
Darien (4/2017)	Westport	Weston	Ridgefield (4/2017)	New Canaan	Trumbull (4/2016)
Derby (12/2018)	Farmington	Berlin (4/2017)	Newington (4/2016)	Cromwell	Orange
East Hartford (4/2015)	Glastonbury	Woodbridge	South Windsor (4/2016)	North Haven	Middlebury
East Windsor (4/2016)	Avon	Orange	Bethel	Clinton	Branford
Fairfield (12/2018)	Trumbull (4/2016)	West Hartford (4/2016)	Enfield	North Haven	Westport
Granby (4/2015)	Berlin (4/2017)	Naugatuck	Monroe (4/2017)	Windsor Locks	Avon
Groton (4/2015)	Enfield	Cheshire	Madison (4/2017)	Suffield	Naugatuck
Hamden (4/2015)	Middletown (11/2021)	Plymouth	Wallingford	Shelton	Fairfield (12/2018)
Madison (4/2017)	Middlebury	Branford	Guilford	Shelton	Plainfield
Manchester (4/2015)	Milford	Farmington	Cromwell	Newington (4/2016)	Trumbull (4/2016)
Meriden (4/2016)	Portland	Trumbull (4/2016)	Wallingford	North Haven	Simsbury
Monroe (4/2017)	Canton	Wallingford	Avon	Trumbull (4/2016)	Redding
New Britain (4/2015)	Waterbury (4/2015)	Plainville	Plymouth	Naugatuck	Bethel
New Milford (4/2016)	Newtown (4/2017)	Redding	Granby (4/2015)	Bethel	Monroe (4/2017)
Newington (4/2016)	North Haven	Trumbull (4/2016)	Thomaston	Milford	Plainville
Newtown (4/2017)	Monroe (4/2017)	Bethel	Redding	Avon	Canton
Norwalk (4/2016)	Wallingford	Stratford (4/2015)	Monroe (4/2017)	Trumbull (4/2016)	Shelton
Norwich (4/2017)	Brookfield	Bethel	Old Saybrook	Plainfield	Waterford
Ridgefield (4/2017)	Shelton	Berlin (4/2017)	Redding	Guilford	Glastonbury
South Windsor (4/2016)	Woodbridge	Glastonbury	Cheshire	Trumbull (4/2016)	Berlin (4/2017)
Stratford (4/2015)	Wallingford	Naugatuck	Trumbull (4/2016)	North Haven	Shelton
Trumbull (4/2016)	North Haven	Avon	Shelton	Naugatuck	Thomaston
Waterbury (4/2015)	New Britain (4/2015)	Plymouth	Plainville	Guilford	Farmington
West Hartford (4/2016)	Trumbull (4/2016)	Naugatuck	Newington (4/2016)	Berlin (4/2017)	Fairfield (12/2018)
Wethersfield (4/2015)	East Haven	Portland	Shelton	Stratford (4/2015)	Trumbull (4/2016)
Windsor (4/2016)	Naugatuck	Suffield	Bloomfield (4/2016)	Berlin (4/2017)	Trumbull (4/2016)

Notes: Treated departments are listed (Column 1) alongside control peer group departments (Columns 2-6) selected by CTRP3 staff prior to treatment. Treatment waves are in parentheses. Peer group towns shaded in grey denote later-treated departments that are only included in the control before the date of their own treatment or, if they were treated earlier than the focal town, not at all.

Table A.4: Impact of Intervention on Volume of Traffic Stops, Robustness for Various Bandwidths

	(1) All Minority	(2) White Non-Hispanic	(3) Black/AA	(4) Hispanic/Latino
1[Treat] * 1[Post]	-1.223***	-0.150	-0.467**	-0.421***
	(0.373)	(0.549)	(0.194)	(0.148)
BW =	12	12	12	12
N =	49424	49424	49424	49424
Y Mean =	5.191	13.00	2.368	2.224
1[Treat] * 1[Post]	-1.215***	-0.518	-0.376**	-0.495***
	(0.378)	(0.556)	(0.189)	(0.164)
BW =	8	8	8	8
N =	34304	34304	34304	34304
Y Mean =	5.091	12.75	2.320	2.190
1[Treat] * 1[Post]	-1.272***	-1.052**	-0.449**	-0.544***
	(0.368)	(0.535)	(0.192)	(0.169)
BW =	6	6	6	6
N =	25728	25728	25728	25728
Y Mean =	5.066	12.53	2.310	2.176
Group x Month x Violation FE	x	x	x	x
Group x Agency x Violation FE	x	x	x	x

Notes: We estimate a difference-in-differences estimator comparing periods t-n to -2 periods with 0 to t+n periods where n is the relevant bandwidth relative to the intervention in the treated relative to control agencies on the volume of stops by race in the stacked panel. The stacked panel consisted of 51,600 group by agency by month by violation observations. Standard errors are clustered on agency by violation type.

Table A.5: Impact of Intervention on Volume of Traffic Stops, Robustness to Dropping One Month Before and After the Intervention

	(1) All Minority	(2) White Non-Hispanic	(3) Black/AA	(4) Hispanic/Latino
1[Treat] * 1[Post]	-1.286***	0.0306	-0.488**	-0.434***
	(0.406)	(0.587)	(0.212)	(0.162)
BW =	12	12	12	12
N =	45136	45136	45136	45136
Y Mean =	5.199	13.05	2.374	2.226
1[Treat] * 1[Post]	-1.301***	-0.252	-0.405**	-0.516***
	(0.407)	(0.603)	(0.204)	(0.176)
BW =	8	8	8	8
N =	30016	30016	30016	30016
Y Mean =	5.090	12.78	2.321	2.186
1[Treat] * 1[Post]	-1.413***	-0.821	-0.513**	-0.586***
	(0.401)	(0.612)	(0.211)	(0.185)
BW =	6	6	6	6
N =	21440	21440	21440	21440
Y Mean =	5.058	12.55	2.310	2.169
Group x Month x Violation FE	x	x	x	x
Group x Agency x Violation FE	x	x	x	x

Notes: We estimate a difference-in-differences estimator comparing periods t-n to -2 periods with 0 to t+n periods where n is the relevant bandwidth relative to the intervention in the treated relative to control agencies on the volume of stops by race in the stacked panel. The stacked panel consisted of 51,600 group by agency by month by violation observations. Standard errors are clustered on agency by violation type.

Table A.6: Impact of Intervention on Volume of Traffic Stops, Robustness with Linear Trends Interacted with Department Census Demographics

	(1) All Minority	(2) White Non-Hispanic	(3) Black/AA	(4) Hispanic/Latino
1[Treat] * 1[Post]	-1.330***	0.0632	-0.517***	-0.490***
	(0.353)	(0.612)	(0.180)	(0.136)
BW =	12	12	12	12
N =	49424	49424	49424	49424
Y Mean =	5.191	13.00	2.368	2.224
1[Treat] * 1[Post]	-1.154***	-0.348	-0.395**	-0.434***
	(0.344)	(0.641)	(0.175)	(0.135)
BW =	8	8	8	8
N =	34304	34304	34304	34304
Y Mean =	5.091	12.75	2.320	2.190
1[Treat] * 1[Post]	-1.138***	-1.011	-0.402**	-0.445***
	(0.325)	(0.644)	(0.177)	(0.136)
BW =	6	6	6	6
N =	25728	25728	25728	25728
Y Mean =	5.066	12.53	2.310	2.176
Group x Month x Violation FE	x	x	x	x
Group x Agency x Violation FE	x	x	x	x

Notes: We estimate a difference-in-differences estimator comparing periods t-n to -2 periods with 0 to t+n periods where n is the relevant bandwidth relative to the intervention in the treated relative to control agencies on the volume of stops by race in the stacked panel. The stacked panel consisted of 51,600 group by agency by month by violation observations. Standard errors are clustered on agency by violation type.

Table A.7: Impact of Intervention on Volume of Traffic Stops by Treatment Wave

	(1) All Minority	(2) White non-Hispanic	(3) Black/AA	(4) Hispanic/Latino
1[Treat] * 1[Post] * 1[2015 Wave]	-0.492	1.445	0.156	-0.185
	(0.972)	(1.074)	(0.475)	(0.396)
1[Treat] * 1[Post] * 1[2016 Wave]	-1.968***	0.265	-0.933***	-0.632***
	(0.550)	(0.865)	(0.317)	(0.210)
1[Treat] * 1[Post] * 1[2017 Wave]	-1.283***	-2.742***	-0.617***	-0.513***
	(0 .405)	(0.888)	(0.215)	(0.156)
BW =	12	12	12	12
N =	45584	45584	45584	45584
Y Mean =	5.138	12.927	2.325	2.222
Group x Month x Violation FE	x	x	x	x
Group x Agency x Violation FE	x	x	x	x

Notes: We estimate the intervention's impact separately for each wave of treatment, excluding the 2018 and 2021 waves due to limited data (only three towns treated). We estimate a difference-in-differences estimator comparing periods t-n to -2 periods with 0 to t+n periods where n is the relevant bandwidth relative to the intervention in the treated relative to control agencies on the volume of stops by race in the stacked panel. The stacked panel consisted of 51,600 group by agency by month by violation observations. Standard errors are clustered on agency by violation type.

Table A.8: Impact of Intervention on Pretextual Traffic Stops, Robustness for Various Bandwidths

	(1) All Pretextual	(2) Unsuccessful Pretextual	(3) Successful Pretextual	(4) Potential Successful Pretextual
1[Treat] * 1[Post]	-8.980**	-3.079**	-4.655**	-16.72**
	(3.787)	(1.335)	(2.244)	(6.723)
BW =	12	12	12	12
N =	3089	3089	3089	3089
Y Mean =	21.02	8.208	12.53	41.77
1[Treat] * 1[Post]	-9.287**	-2.267*	-4.578**	-16.13**
	(3.758)	(1.309)	(2.222)	(6.694)
BW =	8	8	8	8
N =	2144	2144	2144	2144
Y Mean =	20.78	8.106	12.34	41.23
1[Treat] * 1[Post]	-9.531***	-2.966*	-5.127**	-17.62***
	(3.307)	(1.532)	(2.337)	(6.530)
BW =	6	6	6	6
N =	1608	1608	1608	1608
Y Mean =	20.71	8.124	12.36	41.20
Group x Month FE	x	x	x	x
Group x Agency FE	x	x	x	x

Notes: We estimate a difference-in-differences estimator comparing periods t-n to -2 periods with 0 to t+n periods where n is the relevant bandwidth relative to the intervention in the treated relative to control agencies on the volume of stops by race in the stacked panel. We include group by time α_{gt} and group by agency γ_{gi} fixed effects and collapse observations over violation such that the stacked panel consists of 3,255 group by agency by month observations. Standard errors are clustered on agency.

Table A.9: Impact of Intervention on Pretextual Traffic Stops, Robustness to Dropping One Month Before and After the Intervention

	(1) All Pretextual	(2) Unsuccessful Pretextual	(3) Successful Pretextual	(4) Potential Successful Pretextual
1[Treat] * 1[Post]	-9.301**	-3.397**	-5.004**	-17.70**
	(4.010)	(1.482)	(2.462)	(7.272)
BW =	12	12	12	12
N =	2821	2821	2821	2821
Y Mean =	21.09	8.193	12.45	41.73
1[Treat] * 1[Post]	-9.883**	-2.608*	-5.050**	-17.54**
	(3.958)	(1.500)	(2.519)	(7.318)
BW =	8	8	8	8
N =	1876	1876	1876	1876
Y Mean =	20.85	8.070	12.19	41.11
1[Treat] * 1[Post]	-10.57***	-3.482*	-5.809**	-19.86***
	(3.397)	(1.787)	(2.721)	(7.214)
BW =	6	6	6	6
N =	1340	1340	1340	1340
Y Mean =	20.79	8.077	12.15	41.01
Group x Month FE	x	x	x	x
Group x Agency FE	x	x	x	x

Notes: We estimate a difference-in-differences estimator comparing periods t-n to -2 periods with 0 to t+n periods where n is the relevant bandwidth relative to the intervention in the treated relative to control agencies on the volume of stops by race in the stacked panel. We include group by time α_{gt} and group by agency γ_{gi} fixed effects and collapse observations over violation such that the stacked panel consists of 3,255 group by agency by month observations. Standard errors are clustered on agency.

Table A.10: Impact of Intervention on Pretextual Traffic Stops, Robustness with Linear Trends Interacted with Department Census Demographics

	(1) All Pretextual	(2) Unsuccessful Pretextual	(3) Successful Pretextual	(4) Potential Successful Pretextual
1[Treat] * 1[Post]	-10.15***	-3.663***	-4.969**	-18.78***
	(3.684)	(1.261)	(2.343)	(6.814)
BW =	12	12	12	12
N =	3089	3089	3089	3089
Y Mean =	21.02	8.208	12.53	41.77
1[Treat] * 1[Post]	-8.536**	-2.652**	-4.163*	-15.35**
	(3.519)	(1.161)	(2.235)	(6.320)
BW =	8	8	8	8
N =	2144	2144	2144	2144
Y Mean =	20.78	8.106	12.34	41.23
1[Treat] * 1[Post]	-7.835**	-2.522**	-4.152*	-14.51***
	(2.951)	(1.081)	(2.136)	(5.397)
BW =	6	6	6	6
N =	1608	1608	1608	1608
Y Mean =	20.71	8.124	12.36	41.20
Group x Month FE	x	x	x	x
Group x Agency FE	x	x	x	x

Notes: We estimate a difference-in-differences estimator comparing periods t-n to -2 periods with 0 to t+n periods where n is the relevant bandwidth relative to the intervention in the treated relative to control agencies on the volume of stops by race in the stacked panel. We include group by time α_{gt} and group by agency γ_{gi} fixed effects and collapse observations over violation such that the stacked panel consists of 3,255 group by agency by month observations. We also include interactions between five principal components obtained from Census data and a linear time trend. Standard errors are clustered on agency.

Table A.11: Impact of Intervention on Traffic Stop Outcomes, Robustness for Various Bandwidths

	(1) Warnings	(2) Citations	(3) Arrests	(4) Searches
1[Treat] * 1[Post]	-14.71**	-1.714	-3.209**	0.144
	(7.236)	(5.112)	(1.434)	(0.887)
BW =	12	12	12	12
N =	3089	3089	3089	3089
Y Mean =	49.28	24.63	10.46	5.100
1[Treat] * 1[Post]	-16.08**	-0.912	-2.477*	0.375
	(6.917)	(5.038)	(1.413)	(0.757)
BW =	8	8	8	8
N =	2144	2144	2144	2144
Y Mean =	48.80	23.69	10.28	5.023
1[Treat] * 1[Post]	-16.69**	-0.704	-3.137*	0.205
	(6.689)	(4.327)	(1.620)	(0.791)
BW =	6	6	6	6
N =	1608	1608	1608	1608
Y Mean =	48.63	23.44	10.25	4.993
Group x Month FE	x	x	x	x
Group x Agency FE	x	x	x	x

Notes: We estimate a difference-in-differences estimator comparing periods t-n to -2 periods with 0 to t+n periods where n is the relevant bandwidth relative to the intervention in the treated relative to control agencies on the volume of stops by race in the stacked panel. include group by time α_{gt} and group by agency γ_{gi} fixed effects and collapse observations over violation such that the stacked panel consists of 3,255 group by agency by month observations. Standard errors are clustered on agency.

Table A.12: Impact of Intervention on Traffic Stop Outcomes, Robustness to Dropping One Month Before and After the Intervention

	(1) Warnings	(2) Citations	(3) Arrests	(4) Searches
1[Treat] * 1[Post]	-15.27*	-1.728	-3.548**	0.268
	(7.763)	(5.340)	(1.600)	(0.991)
BW =	12	12	12	12
N =	2821	2821	2821	2821
Y Mean =	49.38	24.68	10.46	5.111
1[Treat] * 1[Post]	-17.10**	-0.771	-2.851*	0.571
	(7.438)	(5.243)	(1.614)	(0.862)
BW =	8	8	8	8
N =	1876	1876	1876	1876
Y Mean =	48.87	23.62	10.24	5.028
1[Treat] * 1[Post]	-18.35**	-0.594	-3.715*	0.431
	(7.129)	(4.454)	(1.872)	(0.902)
BW =	6	6	6	6
N =	1340	1340	1340	1340
Y Mean =	48.69	23.29	10.20	4.995
Group x Month FE	x	x	x	x
Group x Agency FE	x	x	x	x

Notes: We estimate a difference-in-differences estimator comparing periods t-n to -2 periods with 0 to t+n periods where n is the relevant bandwidth relative to the intervention in the treated relative to control agencies on the volume of stops by race in the stacked panel. We include group by time α_{gt} and group by agency γ_{gi} fixed effects and collapse observations over violation such that the stacked panel consists of 3,255 group by agency by month observations. Standard errors are clustered on agency.

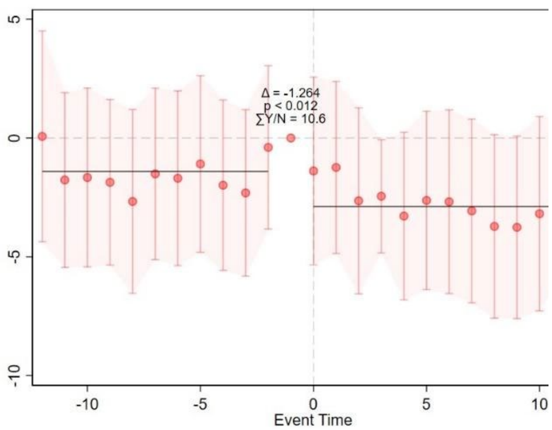
Table A.13: Impact of Intervention on Traffic Stop Outcomes, Robustness with Linear Trends Interacted with Department Census Demographics

	(1) Warnings	(2) Citations	(3) Arrests	(4) Searches
1[Treat] * 1[Post]	-17.61**	-0.320	-3.602**	-0.247
	(7.180)	(4.754)	(1.363)	(0.687)
BW =	12	12	12	12
N =	3089	3089	3089	3089
Y Mean =	49.28	24.63	10.46	5.100
1[Treat] * 1[Post]	-16.39**	0.440	-2.703**	-0.403
	(6.581)	(4.317)	(1.263)	(0.642)
BW =	8	8	8	8
N =	2144	2144	2144	2144
Y Mean =	48.80	23.69	10.28	5.023
1[Treat] * 1[Post]	-15.05**	-0.544	-2.714**	-0.311
	(5.792)	(3.579)	(1.198)	(0.644)
BW =	6	6	6	6
N =	1608	1608	1608	1608
Y Mean =	48.63	23.44	10.25	4.993
Group x Month FE	x	x	x	x
Group x Agency FE	x	x	x	x

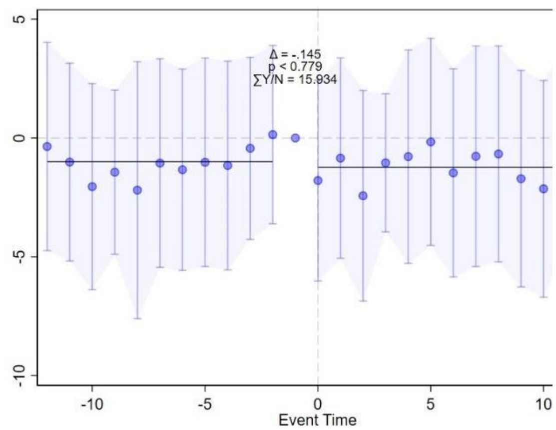
Notes: We estimate a difference-in-differences estimator comparing periods t-n to -2 periods with 0 to t+n periods where n is the relevant bandwidth relative to the intervention in the treated relative to control agencies on the volume of stops by race in the stacked panel. We include group by time α_{gt} and group by agency γ_{gi} fixed effects and collapse observations over violation such that the stacked panel consists of 3,255 group by agency by month observations. We also include interactions between five principal components obtained from Census data and a linear time trend. Standard errors are clustered on agency.

Appendix Figures

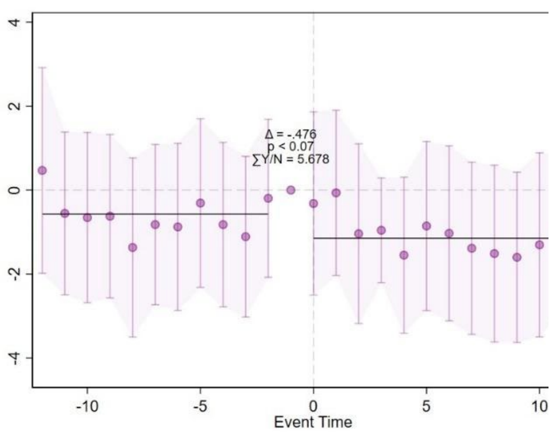
Appendix Figure A.1: Impact of Intervention on Volume of Traffic Stops, All Controls with Inverse Propensity Score Weights



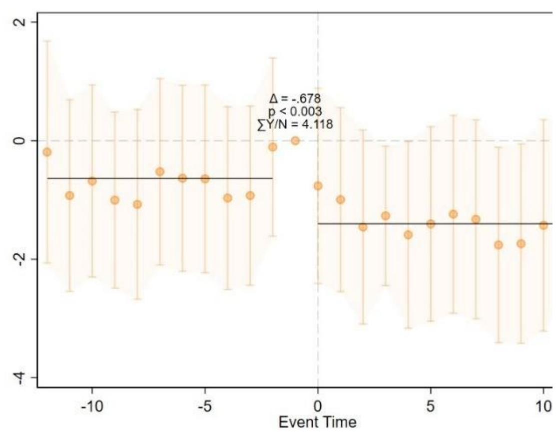
(a) All Minority Motorists



(b) White Non-Hispanic Motorists



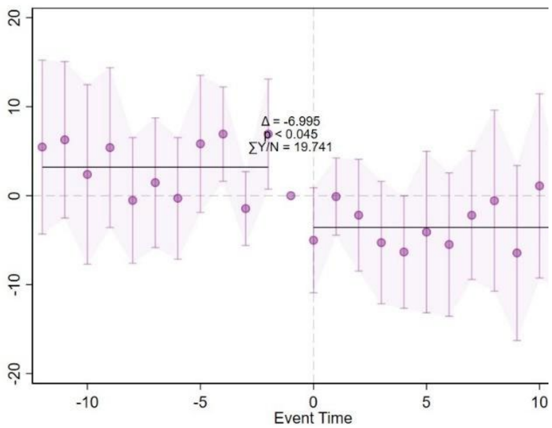
(c) Black/AA Motorists



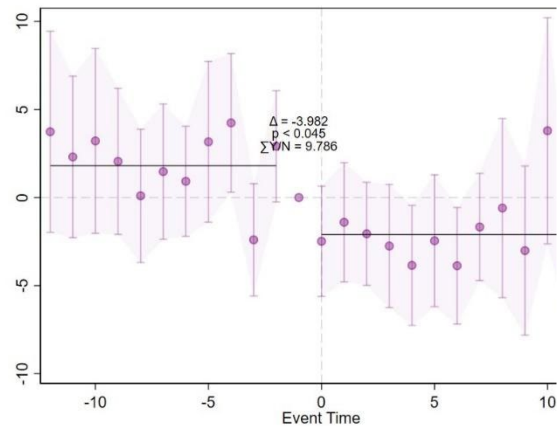
(d) Hispanic/Latino Motorists

Notes: We plot a 95% confidence interval around the coefficient δ_τ using the point estimates and standard errors obtained from estimating Equation 1 on the volume of stops by race in the stacked panel. The stacked panel consisted of 797,120 group by agency by month by violation observations. The annotation Δ is the point estimate from a difference-in-differences estimator comparing periods -12 to -2 periods with 0 to +12 periods relative to the intervention in the treated relative to control agencies. All standard errors are clustered on town by violation category. Controls consist of all non-treated and not-yet-treated agencies in Connecticut as opposed to the CTRP3 selected peer departments. Inverse propensity scores are estimated with Logit as the link function and five variables derived from running a principal components analysis on Census data, total stops from 10/2013 to 3/2015. We run the principal component analysis using the following Census variables: median income, share of commuters by car, population, percent of population Black/AA, percent of population Hispanic/Latino, percent of employment in arts and entertainment, percent of employment Black/AA, percent of employment Hispanic/Latino, total stops, total stops per population.

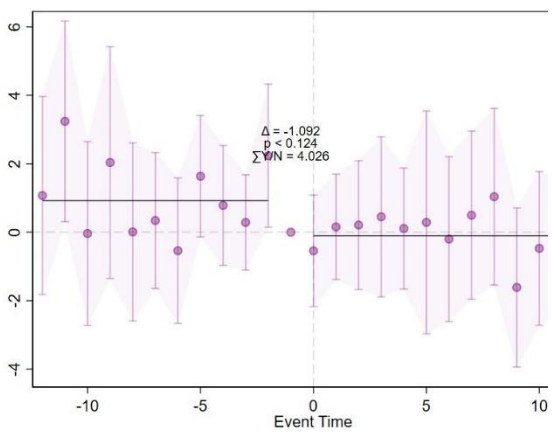
Appendix Figure A.2: Impact of Intervention on Pretextual Traffic Stops for Black/AA Traffic Stops



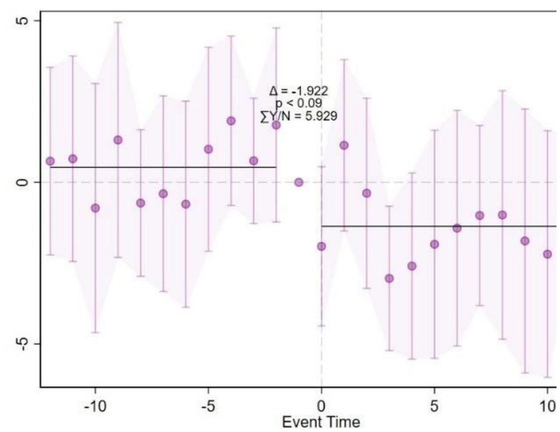
(a) Any Type of Pretextual Stops



(b) Unsuccessful Pretextual Stops



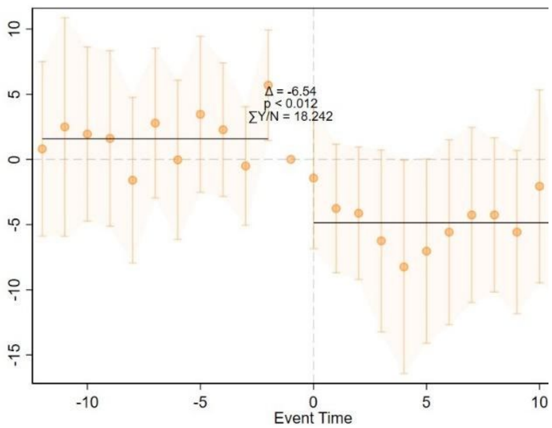
(c) Successful Pretextual Stops



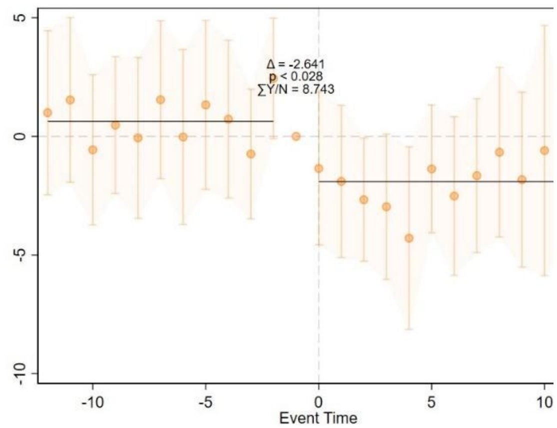
(d) Potential Successful Pretextual

Notes: We plot a 95% confidence interval around the coefficient δ_τ using the point estimates and standard errors obtained from estimating a variation of Equation 1 on the volume of stops by pretextual violation and race in the stacked panel. We include group by time α_{gt} and group by agency γ_{gi} fixed effects and collapse observations over violation such that the stacked panel consists of 3,255 group by agency by month observations. The annotation Δ is the point estimate from a difference-in-differences estimator comparing periods -12 to -2 periods with 0 to +12 periods relative to the intervention in the treated relative to control agencies. All standard errors are clustered on town. The annotation Δ is the point estimate from a difference-in-differences estimator comparing periods -12 to -2 periods with 0 to +12 periods relative to the intervention in the treated relative to control agencies.

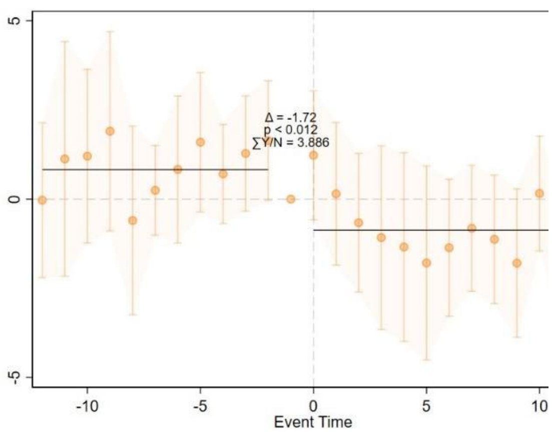
Appendix Figure A.3: Impact of Intervention on Pretextual Traffic Stops for Hispanic/Latino Traffic Stops



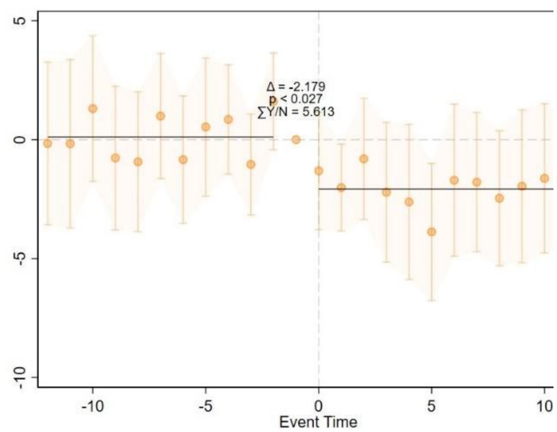
(a) Any Type of Pretextual Stops



(b) Unsuccessful Pretextual Stops



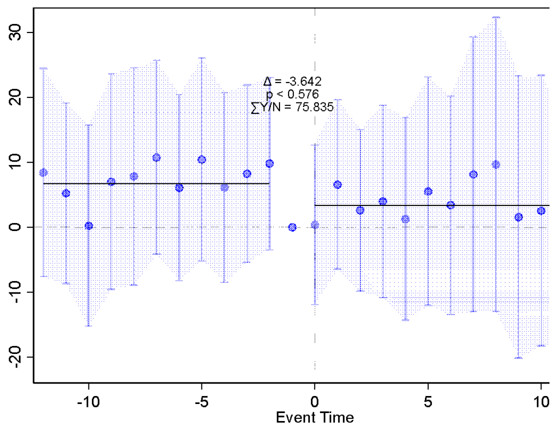
(c) Successful Pretextual Stops



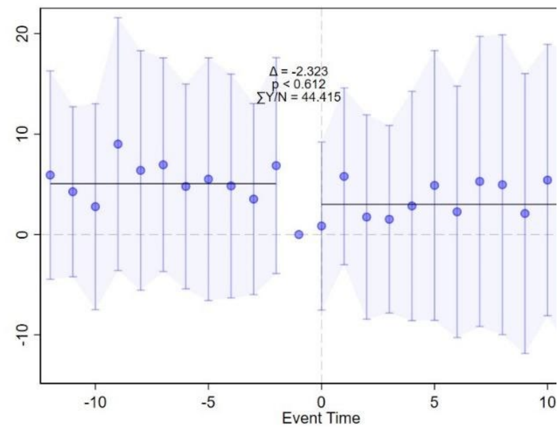
(d) Potential Successful Pretextual

Notes: We plot a 95% confidence interval around the coefficient δ_τ using the point estimates and standard errors obtained from estimating a variation of Equation 1 on the volume of stops by pretextual violation and race in the stacked panel. We include group by time α_{gt} and group by agency γ_{gi} fixed effects and collapse observations over violation such that the stacked panel consists of 3,255 group by agency by month observations. The annotation Δ is the point estimate from a difference-in-differences estimator comparing periods -12 to -2 periods with 0 to +12 periods relative to the intervention in the treated relative to control agencies. All standard errors are clustered on town. The annotation Δ is the point estimate from a difference-in-differences estimator comparing periods -12 to -2 periods with 0 to +12 periods relative to the intervention in the treated relative to control agencies.

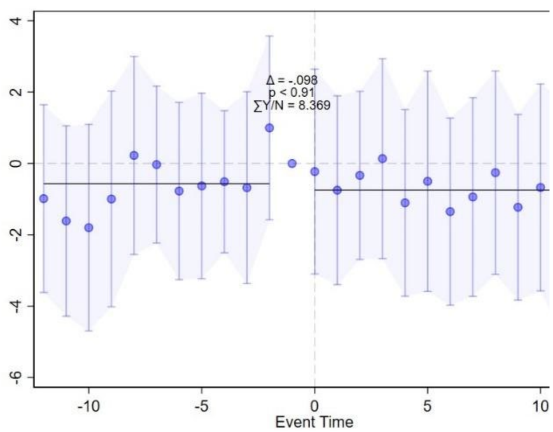
Appendix Figure A.4: Impact of Intervention on Pretextual Traffic Stops for White Non-Hispanic Traffic Stops



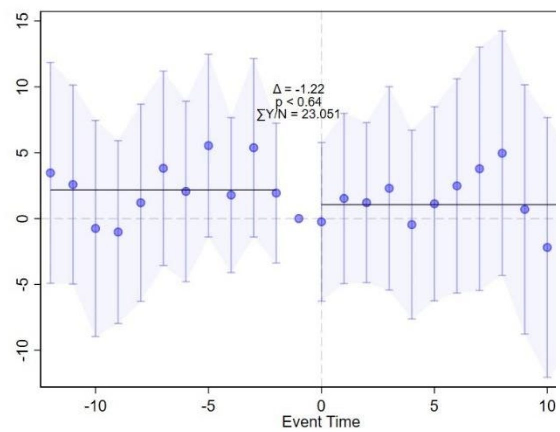
(a) Any Type of Pretextual Stops



(b) Unsuccessful Pretextual Stops



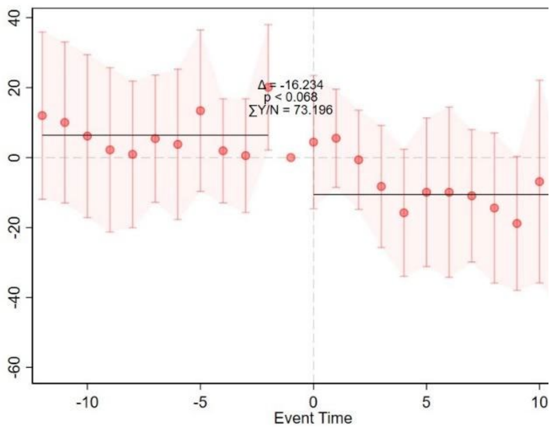
(c) Successful Pretextual Stops



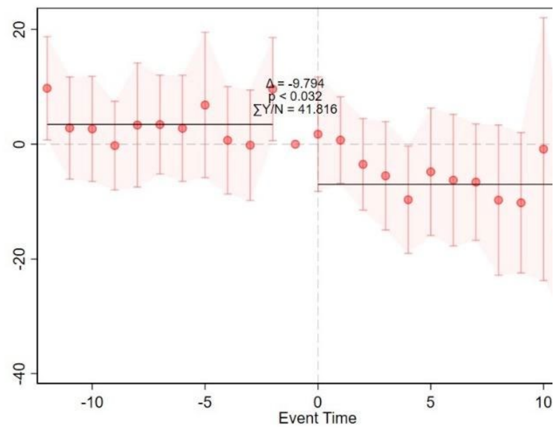
(d) Potential Successful Pretextual

Notes: We plot a 95% confidence interval around the coefficient δ_τ using the point estimates and standard errors obtained from estimating a variation of Equation 1 on the volume of stops by pretextual violation and race in the stacked panel. We include group by time α_{gt} and group by agency γ_{gi} fixed effects and collapse observations over violation such that the stacked panel consists of 3,255 group by agency by month observations. The annotation Δ is the point estimate from a difference-in-differences estimator comparing periods -12 to -2 periods with 0 to +12 periods relative to the intervention in the treated relative to control agencies. All standard errors are clustered on town. The annotation Δ is the point estimate from a difference-in-differences estimator comparing periods -12 to -2 periods with 0 to +12 periods relative to the intervention in the treated relative to control agencies.

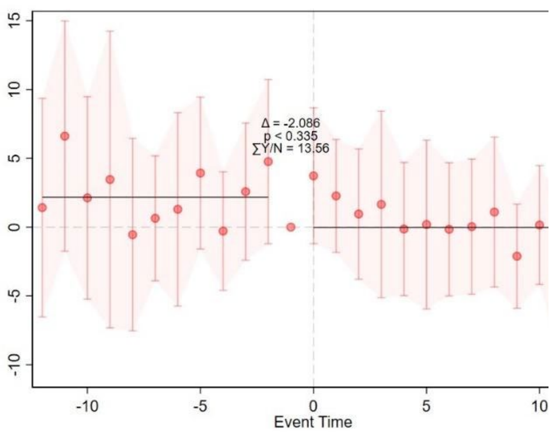
Appendix Figure A.5: Impact of Intervention on Pretextual Traffic Stops for All Minority Traffic Stops, All Control Units with Inverse Propensity Score Weights



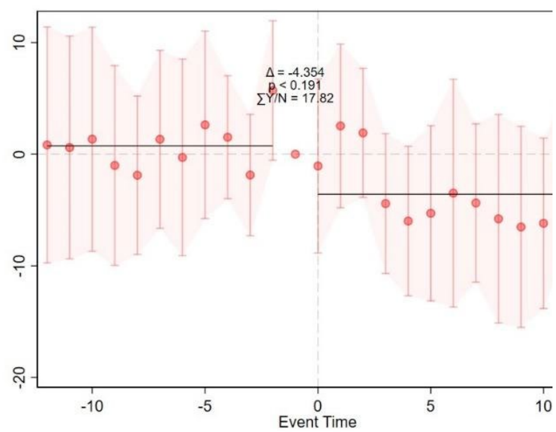
(a) Any Type of Pretextual Stops



(b) Unsuccessful Pretextual Stops



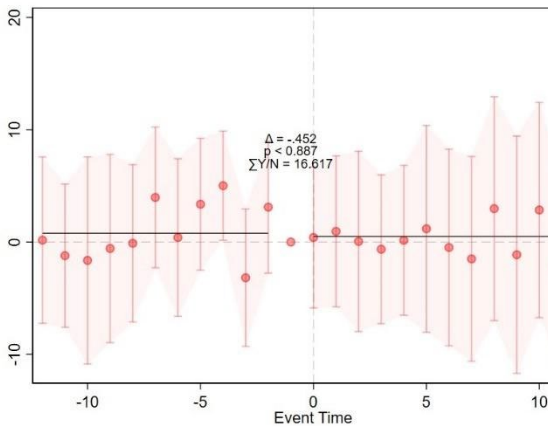
(c) Successful Pretextual Stops



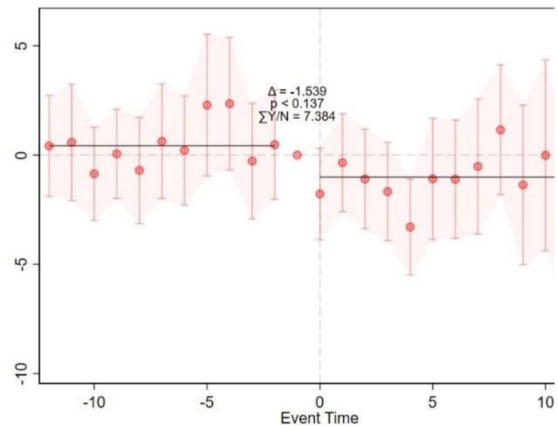
(d) Potential Successful Pretextual

Notes: We plot a 95% confidence interval around the coefficient δ_{τ} using the point estimates and standard errors obtained from estimating a variation of Equation 1 on the volume of stops by pretextual violation and race in the stacked panel. We include group by time α_{gt} and group by agency γ_{gi} fixed effects and collapse observations over violation such that the stacked panel consists of 3,255 group by agency by month observations. The annotation Δ is the point estimate from a difference-in-differences estimator comparing periods -12 to -2 periods with 0 to +12 periods relative to the intervention in the treated relative to control agencies. All standard errors are clustered on town. The annotation Δ is the point estimate from a difference-in-differences estimator comparing periods -12 to -2 periods with 0 to +12 periods relative to the intervention in the treated relative to control agencies. Controls consist of all non-treated and not-yet-treated agencies in Connecticut as opposed to the CTRP3 selected peer departments. Inverse propensity scores are estimated with Logit as the link function and five variables derived from running a principal components analysis on Census data, total stops from 10/2013 to 3/2015. We run the principal component analysis using the following Census variables: median income, share of commuters by car, population, percent of population Black/AA, percent of population Hispanic/Latino, percent of employment in arts and entertainment, percent of employment Black/AA, percent of employment Hispanic/Latino, total stops, total stops per population.

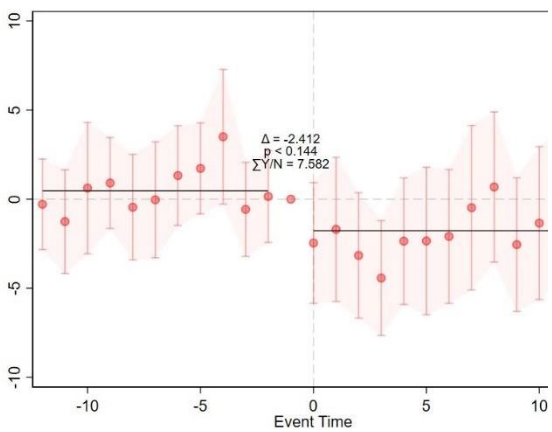
Appendix Figure A.6: Impact of Intervention on Moving Violations for Minority Motorists



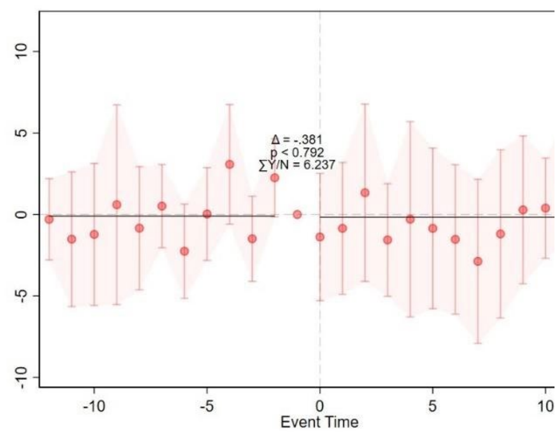
(a) Speeding



(b) Moving Violations (inc. STC)



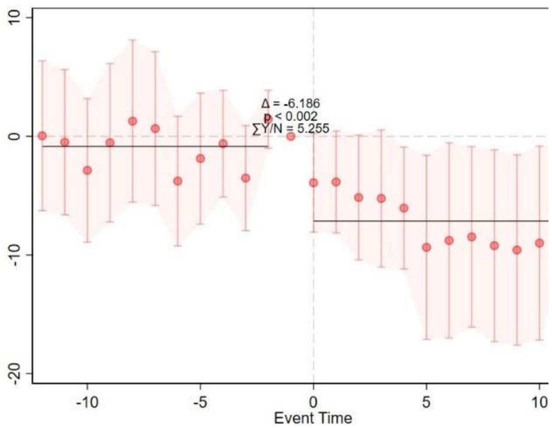
(c) Failure to Signal



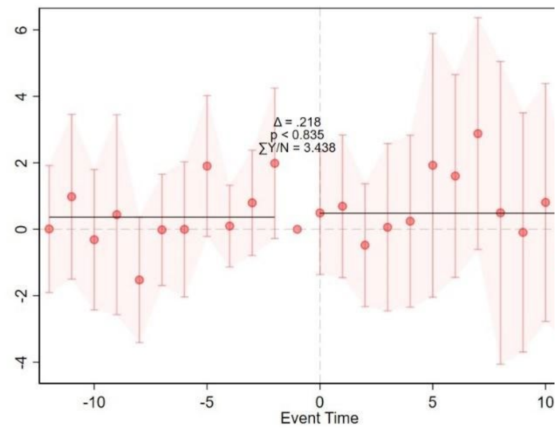
(d) Failure to Stop

Notes: We plot a 95% confidence interval around the coefficient δ_τ using the point estimates and standard errors obtained from estimating a variation of Equation 1 on the volume of stops by violation and race in the stacked panel. We include group by time α_{gt} and group by agency γ_{gi} fixed effects and collapse observations over violation such that the stacked panel consists of 3,255 group by agency by month observations. The annotation Δ is the point estimate from a difference-in-differences estimator comparing periods -12 to -2 periods with 0 to +12 periods relative to the intervention in the treated relative to control agencies. All standard errors are clustered on town. The annotation Δ is the point estimate from a difference-in-differences estimator comparing periods -12 to -2 periods with 0 to +12 periods relative to the intervention in the treated relative to control agencies.

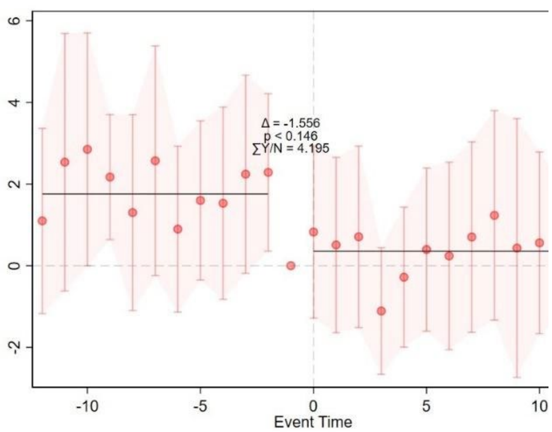
Appendix Figure A.7: Impact of Intervention on Administrative Violations for Minority Motorists



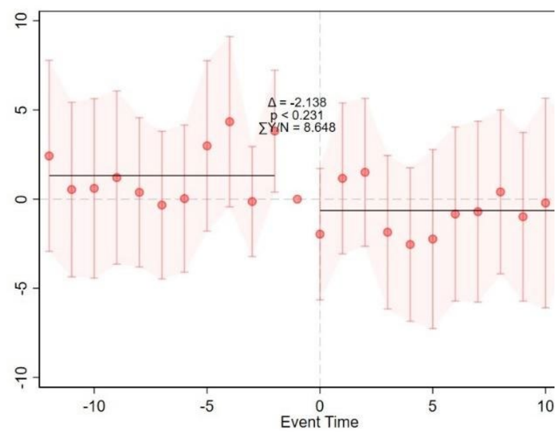
(a) Criminal Violations



(b) Administrative (inc. License)



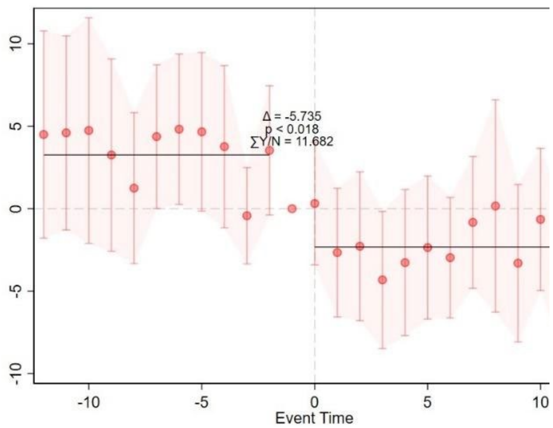
(c) Display of Plates



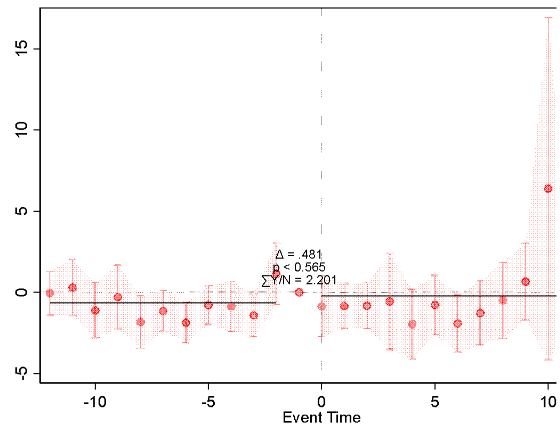
(d) Registration

Notes: We plot a 95% confidence interval around the coefficient δ_τ using the point estimates and standard errors obtained from estimating a variation of Equation 1 on the volume of stops by violation and race in the stacked panel. We include group by time α_{gt} and group by agency γ_{gi} fixed effects and collapse observations over violation such that the stacked panel consists of 3,255 group by agency by month observations. The annotation Δ is the point estimate from a difference-in-differences estimator comparing periods -12 to -2 periods with 0 to +12 periods relative to the intervention in the treated relative to control agencies. All standard errors are clustered on town. The annotation Δ is the point estimate from a difference-in-differences estimator comparing periods -12 to -2 periods with 0 to +12 periods relative to the intervention in the treated relative to control agencies.

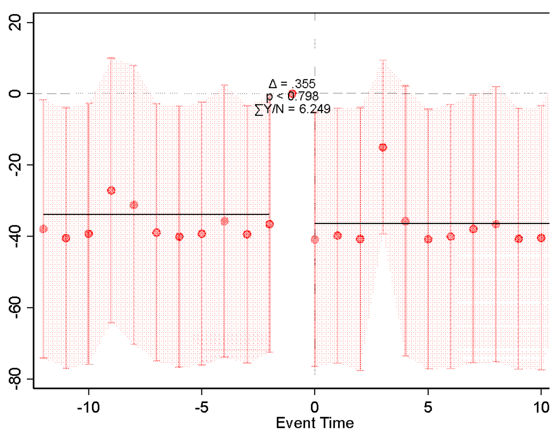
Appendix Figure A.8: Impact of Intervention on Equipment Violations for Minority Motorists



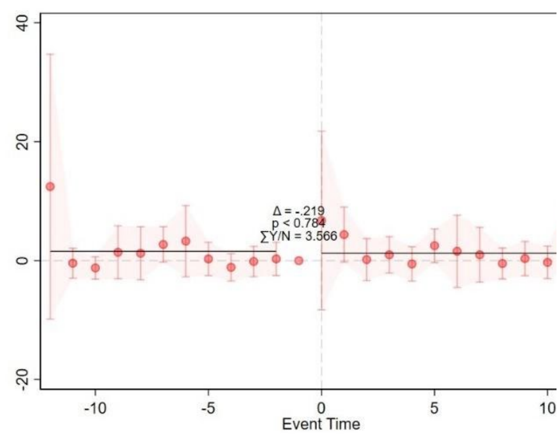
(a) Defective Lights



(b) Equipment (inc. Window Tint)



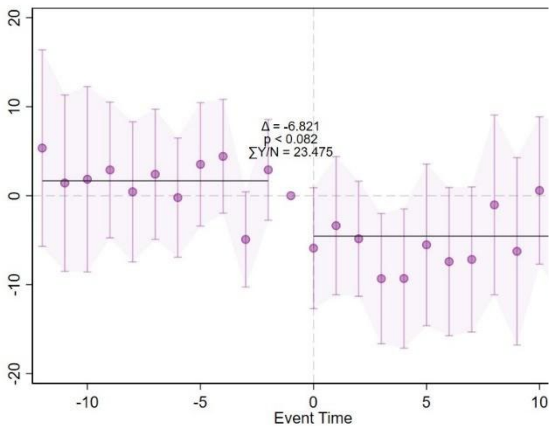
(c) Mobile Phone



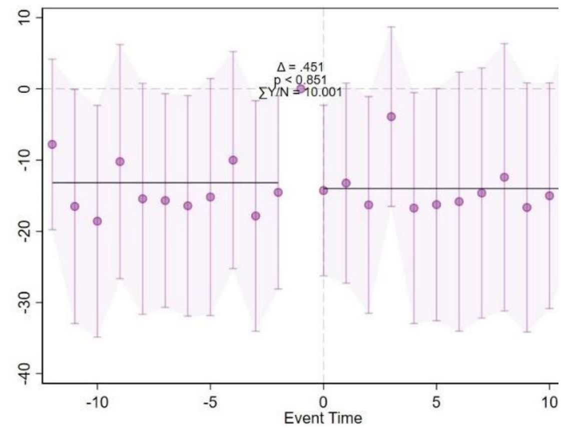
(d) Safety Restraints

Notes: We plot a 95% confidence interval around the coefficient δ_τ using the point estimates and standard errors obtained from estimating a variation of Equation 1 on the volume of stops by violation and race in the stacked panel. We include group by time α_{gt} and group by agency γ_{gi} fixed effects and collapse observations over violation such that the stacked panel consists of 3,255 group by agency by month observations. The annotation Δ is the point estimate from a difference-in-differences estimator comparing periods -12 to -2 periods with 0 to +12 periods relative to the intervention in the treated relative to control agencies. All standard errors are clustered on town. The annotation Δ is the point estimate from a difference-in-differences estimator comparing periods -12 to -2 periods with 0 to +12 periods relative to the intervention in the treated relative to control agencies.

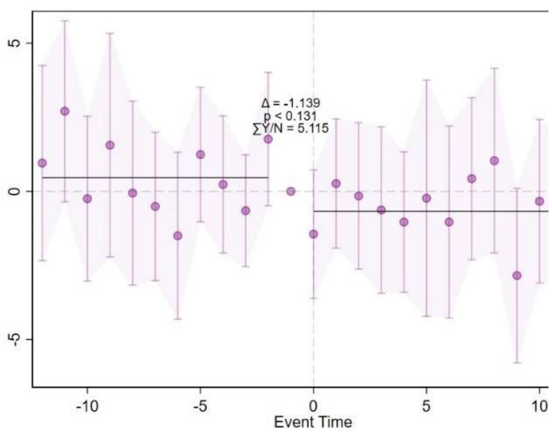
Figure A.9: Impact of Intervention on Traffic Stop Outcomes for Black/AA Motorists



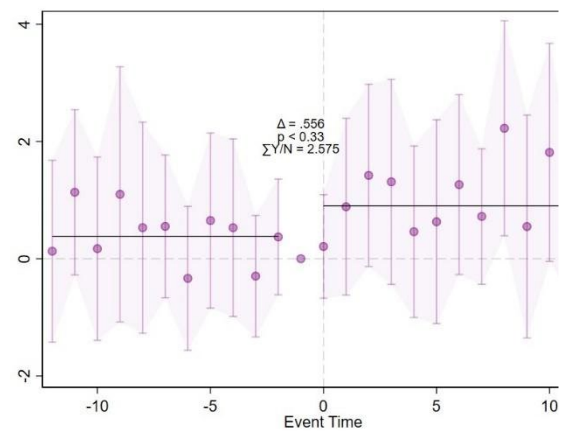
(a) Warnings



(b) Citations



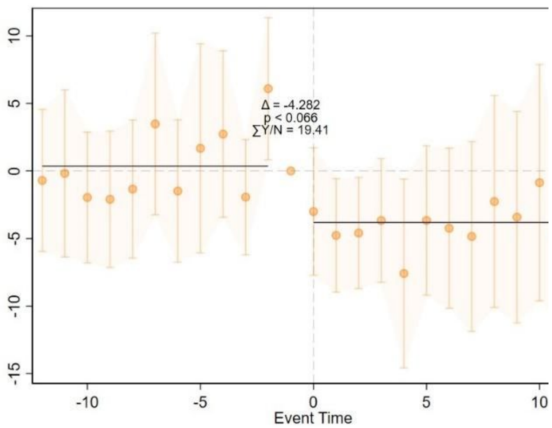
(c) Arrest



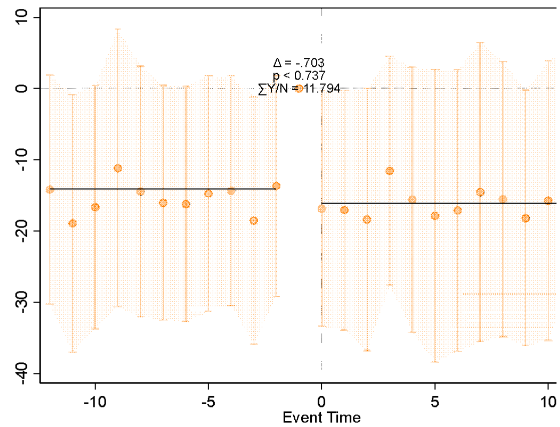
(d) Search

Notes: We plot a 95% confidence interval around the coefficient δ_τ using the point estimates and standard errors obtained from estimating a variation of Equation 1 on the volume of stop outcomes by race in the stacked panel. We include group by time α_{gt} and group by agency γ_{gi} fixed effects and collapse observations over violation such that the stacked panel consists of 3,255 group by agency by month observations. The annotation Δ is the point estimate from a difference-in-differences estimator comparing periods -12 to -2 periods with 0 to +12 periods relative to the intervention in the treated relative to control agencies. All standard errors are clustered on town. The annotation Δ is the point estimate from a difference-in-differences estimator comparing periods -12 to -2 periods with 0 to +12 periods relative to the intervention in the treated relative to control agencies.

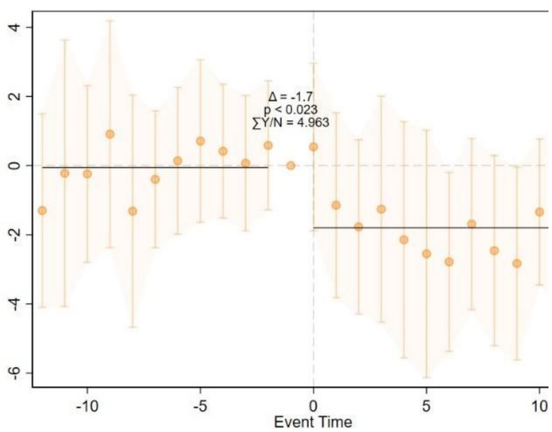
Figure A.10: Impact of Intervention on Traffic Stop Outcomes for Hispanic/Latino Motorists



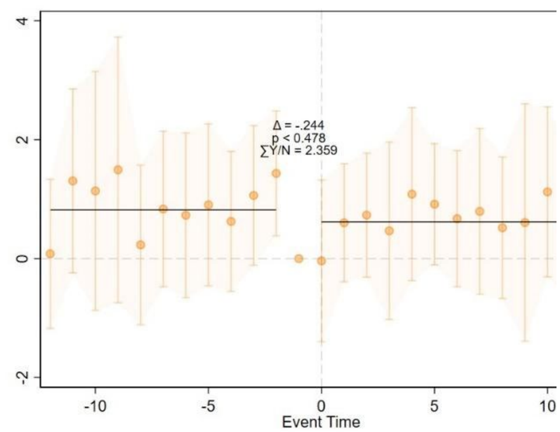
(a) Warnings



(b) Citations



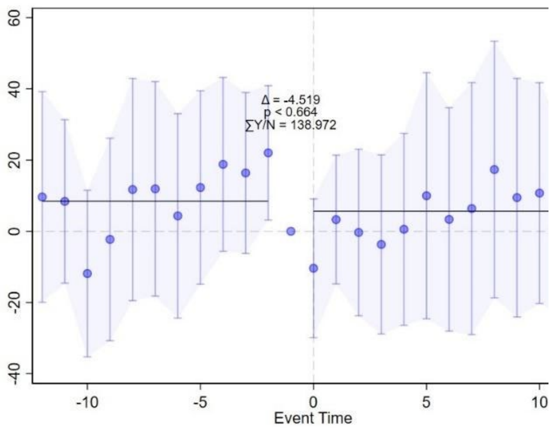
(c) Arrest



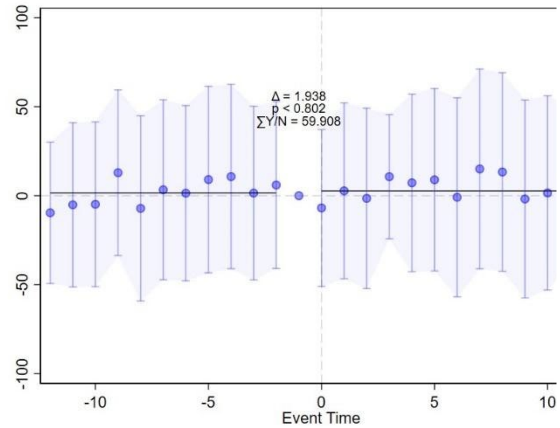
(d) Search

Notes: We plot a 95% confidence interval around the coefficient δ_τ using the point estimates and standard errors obtained from estimating a variation of Equation 1 on the volume of stop outcomes by race in the stacked panel. We include group by time α_{gt} and group by agency γ_{gi} fixed effects and collapse observations over violation such that the stacked panel consists of 3,255 group by agency by month observations. The annotation Δ is the point estimate from a difference-in-differences estimator comparing periods -12 to -2 periods with 0 to +12 periods relative to the intervention in the treated relative to control agencies. All standard errors are clustered on town. The annotation Δ is the point estimate from a difference-in-differences estimator comparing periods -12 to -2 periods with 0 to +12 periods relative to the intervention in the treated relative to control agencies.

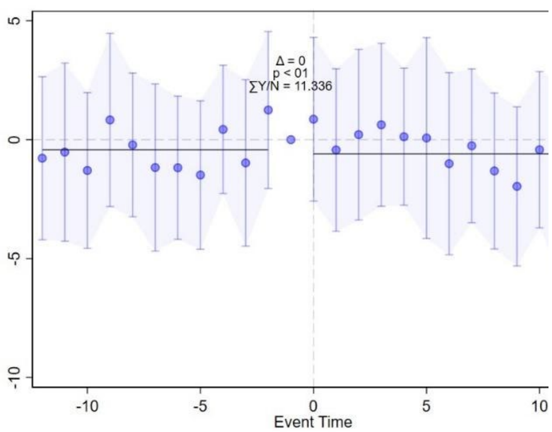
Figure A.11: Impact of Intervention on Traffic Stop Outcomes for White Non-Hispanic Motorists



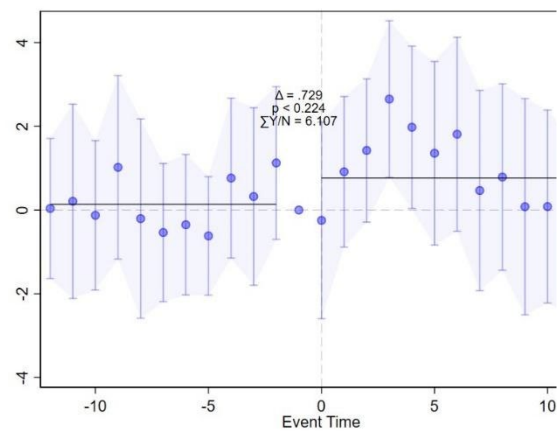
(a) Warnings



(b) Citations



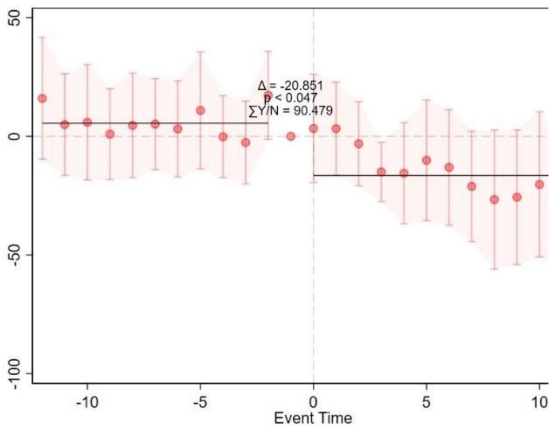
(c) Arrest



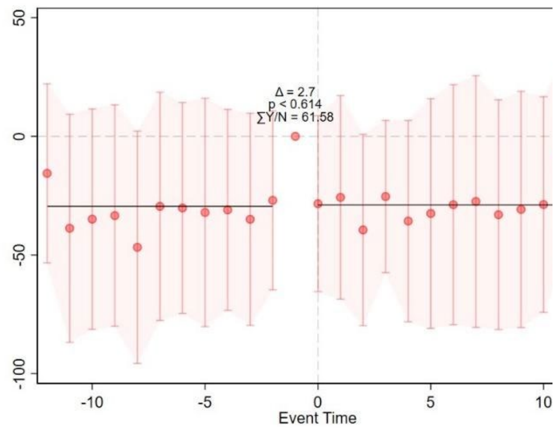
(d) Search

Notes: We plot a 95% confidence interval around the coefficient δ_τ using the point estimates and standard errors obtained from estimating a variation of Equation 1 on the volume of stop outcomes by race in the stacked panel. We include group by time α_{gt} and group by agency γ_{gi} fixed effects and collapse observations over violation such that the stacked panel consists of 3,255 group by agency by month observations. The annotation Δ is the point estimate from a difference-in-differences estimator comparing periods -12 to -2 periods with 0 to +12 periods relative to the intervention in the treated relative to control agencies. All standard errors are clustered on town. The annotation Δ is the point estimate from a difference-in-differences estimator comparing periods -12 to -2 periods with 0 to +12 periods relative to the intervention in the treated relative to control agencies.

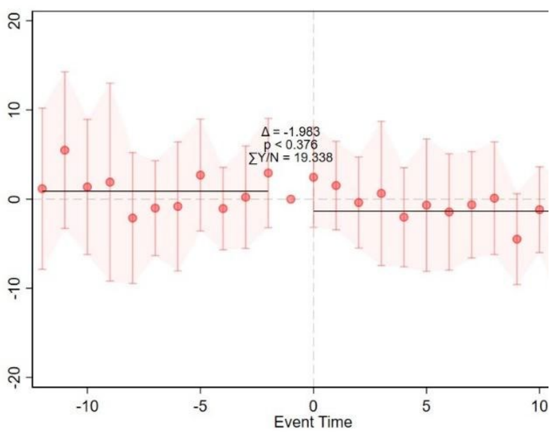
Figure A.12: Impact of Intervention on Traffic Stop Outcomes for Minority Motorists



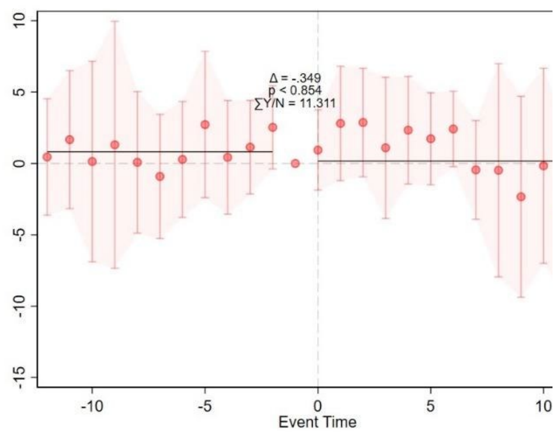
(a) Warnings



(b) Citations



(c) Arrest



(d) Search

Notes: We plot a 95% confidence interval around the coefficient δ_τ using the point estimates and standard errors obtained from estimating a variation of Equation 1 on the volume of stop outcomes by race in the stacked panel. We include group by time α_{gt} and group by agency γ_{gi} fixed effects and collapse observations over violation such that the stacked panel consists of 3,255 group by agency by month observations. The annotation Δ is the point estimate from a difference-in-differences estimator comparing periods -12 to -2 periods with 0 to +12 periods relative to the intervention in the treated relative to control agencies. All standard errors are clustered on town. The annotation Δ is the point estimate from a difference-in-differences estimator comparing periods -12 to -2 periods with 0 to +12 periods relative to the intervention in the treated relative to control agencies.