DISCUSSION PAPER SERIES

DP15691
(v. 3)

Inflammatory Political Campaigns and Racial Bias in Policing

Pauline Grosjean, Federico Masera and Hasin Yousaf

ECONOMIC HISTORY

PUBLIC ECONOMICS

CEPR

ISSN 0265-8003

Inflammatory Political Campaigns and Racial Bias in Policing

Pauline Grosjean, Federico Masera and Hasin Yousaf

Discussion Paper DP15691
First Published 21 January 2021
This Revision 28 February 2022

Centre for Economic Policy Research
33 Great Sutton Street, London EC1V 0DX, UK
Tel: +44 (0)20 7183 8801
www.cepr.org

Inflammatory Political Campaigns and Racial Bias in Policing

Abstract

Can political rallies affect the behavior of law enforcement officers towards racial minorities? Using data from 35 million traffic stops, we show that the probability that a stopped driver is Black increases by 5.74% after a Trump rally during his 2015-2016 campaign. The effect is immediate, specific to Black drivers, lasts for up to 60 days after the rally, and is not justified by changes in driver behavior. The effects are significantly larger among police officers who were initially more stringent towards Black compared to White drivers, in areas that score higher on present-day measures of racial resentment, those that experienced more racial violence during the Jim Crow era, and in former slave-holding counties. Mentions of racial issues in Trump speeches, whether explicit or implicit, exacerbate the effect of a Trump rally among officers who were initially more stringent towards Black drivers.

JEL Classification: D72, J15, K42

Keywords: Police stops, political campaign, Racial Bias

Pauline Grosjean - pauline.a.grosjean@gmail.com
University of New South Wales (UNSW) and CEPR

Federico Masera - f.masera@unsw.edu.au
University of New South Wales (UNSW)

Hasin Yousaf - h.yousaf@unsw.edu.au
University of New South Wales (UNSW)

Acknowledgements
This paper supersedes an earlier draft circulated under the title “Whistle the Racist Dogs: Political Campaigns and Police Stops”. We are grateful to Sam Bazzi, Sascha Becker, Eli Berman, Julia Cagé, Federico Curci, Pedro Dal Bó, Gianmarco Daniele, Stefano Fiorin, Bob Gibbons, Gabriele Gratton, Richard Holden, Remi Jedwab, Marco Le Moglie, Gabriel Lenz, Leslie Martin, Conrad Miller, Andrea Prat, Nancy Qian, Aakaash Rao, Michele Rosenberg, Paul Seabright, Sarah Walker as well as other participants at presentations at the 2020 NBER Political Economy meeting, the 2020 Australian Political Economy Network meeting, George Washington University, LMU Munich and UC San Diego for helpful comments and suggestions. We thank Ben Enke and Daniel Thompson for generously sharing data with us. Elif Bahar, Jack Buckley, Jonathan Nathan, Ian Hoefer Marti, and Lehan Zhang provided outstanding research assistance. Pauline Grosjean acknowledges financial support from the Australian Research Council (grant FT190100298). This project received Ethics Approval from UNSW (HC200471). All errors remain our own.

Inflammatory Political Campaigns and Racial Bias in Policing

Pauline Grosjean Federico Masera Hasin Yousaf*

February 25, 2022

Abstract

Can political rallies affect the behavior of law enforcement officers towards racial minorities? Using data from 35 million traffic stops, we show that the probability that a stopped driver is Black increases by 5.74% after a Trump rally during his 2015-2016 campaign. The effect is immediate, specific to Black drivers, lasts for up to 60 days after the rally, and is not justified by changes in driver behavior. The effects are significantly larger among police officers who were initially more stringent towards Black compared to White drivers, in areas that score higher on present-day measures of racial resentment, those that experienced more racial violence during the Jim Crow era, and in former slave-holding counties. Mentions of racial issues in Trump speeches, whether explicit or implicit, exacerbate the effect of a Trump rally among officers who were initially more stringent towards Black drivers.

Keywords: Police stops, political campaign, racial bias.
JEL Codes: D72, J15, K42.

*Grosjean: School of Economics, University of New South Wales and CEPR. Masera and Yousaf: School of Economics, University of New South Wales. *p.grosjean@unsw.edu.au*; *f.masera@unsw.edu.au*; *h.yousaf@unsw.edu.au*. This paper supersedes an earlier draft circulated under the title "Whistle the Racist Dogs: Political Campaigns and Police Stops". We are grateful to Sam Bazzi, Sascha Becker, Eli Berman, Julia Cagé, Federico Curci, Pedro Dal Bó, Gianmarco Daniele, Stefano Fiorin, Bob Gibbons, Gabriele Gratton, Richard Holden, Remi Jedwab, Marco Le Moglie, Gabriel Lenz, Leslie Martin, Conrad Miller, Andrea Prat, Nancy Qian, Aakaash Rao, Michele Rosenberg, Paul Seabright, Sarah Walker as well as other participants at presentations at the 2020 NBER Political Economy meeting, the 2020 Australian Political Economy Network meeting, George Washington University, LMU Munich and UC San Diego for helpful comments and suggestions. We thank Ben Enke and Daniel Thompson for generously sharing data with us. Elif Bahar, Jack Buckley, Jonathan Nathan, Ian Hoefer Marti, and Lehan Zhang provided outstanding research assistance. Pauline Grosjean acknowledges financial support from the Australian Research Council (grant FT190100298). This project received Ethics Approval from UNSW (HC200471). All errors remain our own.

1. Introduction

Identity politics has played an increasing role in most advanced democracies in recent years (Bonomi, Gennaioli and Tabellini, 2021). This has been accompanied by a change in public discourse, with politicians increasingly appealing to communal values, emotional cues, and racial triggers (Enke, 2020; Becker, 2021; Gennaro and Ash, 2021) that have aggravated racial prejudice among some ordinary citizens (Müller and Schwarz, 2019; Bursztyn, Egorov and Fiorin, 2020; Newman et al., 2020). Has this change in public discourse also affected the treatment of different racial groups by the state?

In the US, Trump's first political campaign embodied the break in public discourse brought about by identity politics (see, e.g., Enke (2020)). In this paper, we explore how his 2015-2016 campaign affected the behavior of law enforcement towards racial minorities. Police behavior and alleged racially-motivated brutality have come to symbolize racial bias and discrimination against African Americans, especially since the Black Lives Matter movement began in 2013.[1] We focus here on the most frequent type of citizen-police interaction: traffic stops. 20 million people a year, or 8.6% of US residents aged 16 and over, are pulled over for a traffic stop (Davis, Whyde and Langton, 2018), providing the kind of contact that can lead to violent and potentially lethal escalation (Streeter, 2019).[2]

We use data on nearly 35 million traffic stops carried out by the police between 2015 and 2017, including in 141 counties where Trump held a campaign rally as a candidate for the Republican nomination or the presidency (Figure 1). To measure racially-directed police behavior, we rely on the racial classification of the motorist stopped (following, e.g., Knowles, Persico and Todd (2001); Anwar and Fang (2006); Antonovics and Knight (2009); Anbarci and Lee (2014); Goncalves and Mello (2021)).

We estimate the effect of a Trump rally occurring in a county on the probability that a stopped driver is Black (hereafter, the probability of a Black stop) using event-study and generalized difference-in-differences (DID) methodologies at the police stop level, controlling for county and day fixed effects as well as county-specific time trends. Our main outcome is defined as a variable taking value 100 if a stopped driver is Black and 0 if non-Black. Thus, our baseline specification compares the probability of a Black vs.

[1] Racial bias and discrimination in police behavior have been studied in several papers, including but not limited to Antonovics and Knight (2009); Anbarci and Lee (2014); Anwar and Fang (2006); Coviello and Persico (2015); Feigenberg and Miller (2022); Fryer (2019); Goncalves and Mello (2021); Grogger and Ridgeway (2006); Horrace and Rohlin (2016); Knowles, Persico and Todd (2001). Prejudice against African Americans is undoubtedly not limited to police behavior but pervades the entire justice system and manifests in bail decisions (Arnold, Dobbie and Yang, 2018), sentencing (Depew, Eren and Mocan, 2017), parole decisions (Anwar and Fang, 2015), and capital punishment (Alesina and La Ferrara, 2014).

[2] Up to a quarter of recent shootings of civilians by police have followed a traffic stop (NPR, January 25, 2021). Several recent high-profile police killings of Black civilians followed a traffic stop, including the shootings of Daunte Wright on April 11, 2021, and Philando Castile on July 6, 2016. In its investigation, The New York Times (October 31, 2021) found that between 2016 and 2021, police killed over 400 drivers or passengers who were not yielding a gun or a knife nor were under pursuit for a violent crime.

non-Black stop, in the days before and after a rally, in counties with and without a Trump rally.

We find evidence that Trump rallies increased the probability of a Black stop. Our estimate suggests that this probability increases by 1.07 percentage points on average in the 30 days following a rally, a 5.74% increase. The effect is immediate and lasts for up to 60 days.

To address the concern that the timing of rallies may be correlated with changes in the probability of a Black stop, we confirm in the event-study that there are no differences in pre-trends of the outcome before a Trump rally. We also show that there are no differences in levels of the outcome just before a rally between counties that are about to be treated and others. Recent econometric literature on staggered DID shows that two-way fixed effects estimate a weighted average of each treatment effect where the weights may be negative. We first follow the recommended diagnostics by de Chaisemartin and D'Haultfoeuille (2020) and show that none of the weights is negative for our specification. We then follow the estimation procedure proposed by Sun and Abraham (2021) and find similar results to our baseline estimate. We discuss how our results are insulated from the recent criticism of event-study designs by Borusyak, Jaravel and Spiess (2021), and show that our event-study and DID results are robust to the inclusion or exclusion of county-specific time trends.

We establish that the results are specific to Black drivers in several ways. We show in a triple difference design at the county-day-race level that Trump rallies result in an increase in the share and the number of Black stops with respect to any other race or ethnicity. Additionally, we estimate a generalized DID at the county-day level that shows an increase in the overall number of stops of Black drivers after a rally. Trump rallies are associated with a 5.6% increase in the number of Black stops relative to Whites and a 5.4% increase in the overall number of Black stops. By contrast, there are no treatment effects of Trump rallies on the share or the number of stops of any group other than Black drivers with respect to one another. There are also no effects on the absolute number of stops of other groups. The event-study counterparts of these specifications show no differences in pre-trends in any outcome, for any group.

The effects on the probability of a Black stop are also specific to Trump rallies. We show this using a triple differences specification that compares changes in police behavior after rallies by Trump vs. rallies by either the Democratic contender to the presidency, Hillary Clinton, or the other leading Republican opponent, Ted Cruz. We also show that there are limited geographic or social spillover effects of a Trump rally beyond the county where it occurred, suggesting that the county is the appropriate level of analysis.

We then analyze whether the change in the probability of a Black stop after a Trump rally is due to a change in police or driver behavior. Using stop-level information on collisions and speed radars as well as additional evidence from crash and fatality data, we

find no evidence for a change in the racial composition of drivers or in driver behavior. This suggests that the effect of Trump rallies is due to a change in law enforcement behavior.

The change in policing itself could stem from changes in behavior at different levels of the hierarchy. It may come from individual officers changing their behavior, law enforcement agencies changing patrolling decisions, or local politicians imposing changes on the police. To account for potential changes in the timing of patrols, we include fixed effects for each hour of the day in each county. Our results are robust and unchanged in magnitude. The results are also robust when we include local enforcement agency fixed effects, which account for potential changes in the shares of stops coming from different agencies within a county; or officer-level fixed effects, which account for potential shifts in the composition of the police force on duty within an agency. Although state troopers may be expected to be less influenced by pressures from the local political administration compared with police departments, we do not find any statistically significant difference in the effect of Trump rallies on state troopers compared with police.

What can explain the effect that Trump rallies have on the behavior of law enforcement towards Black drivers? Recent literature shows how Trump's campaign, election, and social media activism have unraveled social norms around the acceptability of discrimination and xenophobia against the groups that were the targets of his openly xenophobic rhetoric, mostly Central American migrants and Muslims. The effect is also found to be heterogeneous across individuals, with the most prejudiced becoming more likely to express their views (Edwards and Rushin, 2019; Müller and Schwarz, 2019; Bursztyn, Egorov and Fiorin, 2020; Newman et al., 2020).

Explicit racist political rhetoric against Black people, although standard in political speech in the United States well into the early twentieth century, has been limited in the post-Civil rights US context (Mendelberg, 2001; Banks and Hicks, 2019). To avoid the potential backlash associated with an openly racist rhetoric, politicians can instead use coded language that does not directly refer to the targeted racial group but is understood differently by different audiences as a function of their underlying prejudice (Lohrey, 2006; Fear, 2007; Goodin, 2008; Haney-Lopez, 2014). Such "dog-whistles" can consist, for example, of euphemisms understood by some as references to a racial group ("urban", "thug") or references to topics that can trigger some negative racial stereotypes. For example, talking about crime and sexual violence can activate stereotypes associating Blackness with crime or sexual violence (Eberhardt et al., 2004; Masera, Rosenberg and Walker, 2022). Trump's speeches include many such implicit references to racial issues.[3]

[3]Figure A1 in the Appendix shows that Trump includes a few more explicit mentions of racial issues compared to other Republican Presidential candidates in previous elections. However, the number of such explicit references is still limited, as also highlighted by Hopkins (2019). What really distinguishes Trump from all other candidates is how frequently he talks about implicitly racially charged issues, particularly crime.

Overall, the literature on racial rhetoric predicts that the effect of Trump rallies should be larger for more racially biased individuals, and all the more so when speeches contain many references to racial issues.

To test this, we explore the heterogeneous effect of Trump rallies across law enforcement officers who differ in measures of individual racial bias, and across areas that differ in their present-day levels of racial resentment or in their racial history. We measure officer-level racial bias by their relative stringency, i.e. how severe the outcome decision is after a stop of a Black vs White driver, controlling for a wide range of stop-level fixed effects. Our results show that the effect of Trump rallies is 10% to 16% larger for officers whose measure of racial bias against Black drivers at baseline is one standard deviation higher than the mean. To measure local racial resentment, we use responses to survey questions included in the 2012 and 2014 Cooperative Congressional Election Surveys (Schaffner and Ansolabehere, 2015), averaged in the county where the police stop takes place. In addition, we use proxies of deep-seated racial animus inherited from the pre-Civil War era. We follow Acharya, Blackwell and Sen (2016) who show how the prevalence of slavery has shaped racial prejudice against Black people in the US until the present day. Specifically, we use the presence of slaves in 1860 and, to deal with the potential endogeneity of slavery, like Acharya, Blackwell and Sen (2016) and Masera and Rosenberg (2020), we use cotton suitability as an exogenous predictor of slavery. Last, we use lynchings and executions of Black people in the county as proxies for private and institutional racial violence in the Jim Crow era (Hines and Steelwater, 2012; Espy and Smykla, 2016). We find that Trump rallies have a significantly larger effect in counties that today score higher on racial resentment measures, those that had slaves in 1860 and whose agricultural endowments were more suitable to slavery, as well as those where more lynchings and executions of Black people occurred. In contrast with racial attitudes, other potential sources of heterogeneity such as average income, college education, Democrat vote share, or sheriff political affiliation play no role in mediating the effect of a Trump rally on police behavior. We similarly observe no differential effect across counties more or less affected by import competition with China (Autor, Dorn and Hanson, 2013).

To provide further evidence of the effect of references to racial issues in Trump's rhetoric, we rely on the content of the speeches during rallies. As predicted, we find that the effect of a Trump rally is larger on officers with higher estimated racial bias, the more references to racial issues the speech includes. The effects are significant for explicit as well as implicit references. We also find that terrorism-related rhetoric has positive and statistically significant effects. This may be explained by the fact that terrorism-related rhetoric strokes feelings of racial and cultural threat by making divisions between in-group and out-group more salient (McConnell and Rasul, 2021). By contrast, there is no aggravating effect of speech related to the economy, international trade, or political corruption.

Our findings contribute to an emerging literature, namely by Edwards and Rushin (2019), Müller and Schwarz (2019), Bursztyn, Egorov and Fiorin (2020), and Newman et al. (2020) that shows how Trump's campaign, election, and social media activism have unraveled social norms around discrimination and xenophobia. In contrast to this literature, we show how Trump's campaign has affected not ordinary citizens but law enforcement; and we find effects not against the groups targeted most explicitly by Trump's racially inflammatory rhetoric but against Black people. Our results highlight how political campaigns can lead to potential abuses of delegated authority and state violence against historically marginalized and stereotyped minorities.

Since the effect of Trump rallies is stronger among officers who score higher on our measure of racial bias and in areas characterized by a violent racial history, our results point to an intensification of racially-biased behavior by law enforcement. This may have further deepened the kind of political polarization documented by Abramowitz and Saunders (2008), Gentzkow (2016), Abramowitz (2018), Bonomi, Gennaioli and Tabellini (2021) and Bordalo and Yang (2020), particularly in the context of race (Tesler, 2016; Sides, Tesler and Vavreck, 2019). Since polarization was instrumental to Trump's rise to power (Abramowitz, 2018) and because anti-Black prejudice predicted voting intentions for Trump in 2016 to a greater extent than anti-Latino prejudice (Hopkins, 2019), the radicalization of prejudice that our results suggest may have further contributed to Trump's electoral success.

More generally, our results illustrate how politicians can influence prejudice and racially-directed behavior. This complements recent literature that explores the ways leaders legitimize political preferences and mobilize their followers (Lenz, 2012; Dippel and Heblich, 2021; Cagé et al., 2020), even to the point of prompting them to perpetrate acts that signify a brutal and profound rupture with pre-existing norms of social and political acceptability (Cagé et al., 2020).

Our results also illustrate the power of explicit as well as implicit rhetoric. Implicit rhetoric may appear innocuous to some of the electorate while effectively radicalizing others. As such, our results speak to two related strands of literature on hidden values, or "crypto-morality" (Greif and Tadelis, 2010), and on the dog-whistle effect. While a plethora of recent studies have documented the persistence of values and norms (see Nunn (2012), Alesina and Giuliano (2015), and Nunn (2020) for recent reviews), some values may remain hidden but still shape political preferences (Hutchings and Valentino, 2004; Mendelberg, 2008). The literature on racial priming argues that such negative racial predispositions can be activated either by racial cues (see Mendelberg (2008) for a meta-analysis) or by coded language and symbols: what the literature calls dog-whistles (Valentino, Hutchings and White, 2002; Haney-Lopez, 2014; Valentino, Neuner and Vandenbroek, 2018).[4] These findings are of significant policy relevance in the United States

[4]A related phenomenon is the activation of a collective memory of traumatic events. For example,

and beyond, where politicians increasingly use xenophobic and racist rhetoric, either explicitly or by using coded language that appeals to deep-seated stereotypes.[5]

The rest of the paper is organized as follows. The next section describes the data used in the analysis. Section 3 shows that the probability of a Black stop increases after a Trump rally, and Section 4 provides evidence that the effect is not due to a change in driver behavior but rather to a change in policing. In line with the prediction of the literature on the effects of racial rhetoric, we show in Section 5 that the effect is stronger for officers who score higher on our individual measure of racial bias and in areas with a stronger and deeper-seated anti-Black sentiment. We also provide evidence from speeches that shows how racial rhetoric, either explicit or implicit, amplifies the effect of Trump rallies on traffic stops of Black drivers. We discuss broader implications in the conclusion.

2. Data

In what follows, we describe the data sources used in the paper.

Police Stops: Our data on police traffic stops comes from Pierson et al. (2020), who have made the information publicly available on the Stanford Open Policing Project website (last accessed 30 July 2021). To construct a national database of traffic stops, public records requests were filed with all 50 state patrol agencies and over 100 municipal police departments. Altogether, the data comprises approximately 95 million stops from 21 state patrol agencies and 35 municipal police departments from 2011-2018. We focus on the sample of stops in the years 2015-2017, for which we have information on the date and driver's race. This gives us a total of $34,940,130$ stops in $1,474$ counties where 66% of the current US population live. Race is recorded as "Asian/Pacific Islander" (hereafter API), "Black", "Hispanic", or "White." Information on the final decision made by the officer for a stop is available for $23,870,631$ of these stops and has been coded by the Stanford Open Policing Project into four possible categories (ranked in order of increasing severity): warning, citation, summon, and arrest. For a more limited set of stops ($13,586,539$), information is also available on the reason why the driver was stopped.

Campaign Rallies: Data on the rallies held by the 2016 presidential candidates comes from the Democracy in Action website (Appleman, 2019), which documents presidential candidates' schedules from pre-campaign to presidential inauguration. We geo-

Fouka and Voth (2020) and Ochsner and Roesel (2019) show how historical resentment against former enemies (e.g., Germans in Greece, Turks in Austria) can resurface and affect voting. Such activation of memories or of biased predispositions are not necessarily intentionally manipulated by politicians and can have unanticipated consequences. For example, in the case of Greece, although anti-German sentiment benefited some political parties, it was spurred by economic rather than political events (Fouka and Voth, 2020). In our case, we do not claim that Trump necessarily intended to affect law enforcement against Black drivers.

[5]For example, in Europe, Frans Timmermans, the first Vice President of the European Commission, accused the Prime Minister of Hungary Viktor Orban of dog-whistling antisemitic views.

code every Trump rally for the 2015-2016 presidential campaign, which started on June 17, 2015 and ended on November 7, 2016. Altogether, 221 Trump campaign rallies (out of 324) in 141 counties overlap with traffic stop data (plotted in Figure 1). We also geo-code information on the campaigns of the 2016 Democratic presidential candidate, Hillary Clinton, the other main Republican contender in 2016, Ted Cruz, and Barack Obama in 2008.

County Characteristics: Data on county-level average income, racial composition, and college completion comes from the 2015 American Community Survey. Data on county-level import competition shock is from Autor, Dorn and Hanson (2013). Information on 2012 county-level vote shares for Obama comes from Leip (2016) and information on county sheriff's political affiliation is from Thompson et al. (2020).

Summary Statistics: Summary statistics are provided in Table A1. Black drivers are over-represented in stops by a factor of 1.7: they represent 11.09% of our sample population, but 18.71% of stops. 55.47% of stops are of White drivers, who represent 71.44% of the population, and 22.42% of Hispanic drivers, who represent 12.05% of the population.

In addition to the above main sources of information, we also exploit the following county-level sources to explore heterogeneous effects:

Racial Resentment: We derive our measure of racial resentment from the 2012 and 2014 Cooperative Congressional Election Surveys (Schaffner and Ansolabehere, 2015) (hereafter, CCES). We chose the 2012 and 2014 waves to obtain a measure of pre-existing racial resentment before the launch of the Trump campaign. Specifically, we use questions CC442a and CC422b, which ask respondents how much they agree, on a scale of one to five, with the following statements: "*The Irish, Italians, Jews and many other minorities overcame prejudice and worked their way up. Blacks should do the same without any special favors*" ("Racial Resentment A"); "*Generations of slavery and discrimination have created conditions that make it difficult for Blacks to work their way out of the lower class*" ("Racial Resentment B"). We calculate the share of White people in the county who somewhat or strongly agree with the first statement and the share of White people who somewhat or strongly disagree with the second statement. Higher values, therefore, indicate greater resentment.

Responses to survey questions about racial resentment could suffer from conformity or social desirability bias, which would bias our estimates towards zero.[6] To circumvent this limitation, we use several proxies of deep-seated racial animus. As argued by Acharya, Blackwell and Sen (2016, 2019), present-day racism in the US can be traced back to

[6]Recent literature has suggested that responses to the racial resentment questions may capture a broader form of resentment and reflect political conservatism in general (Carney and Enos, 2017). We address this issue by relying on other proxies of racial animus inherited from a violent racial history. We also show that direct measures of political preferences, such as Democrat vote share in the 2012 election, are unrelated to our outcomes of interest.

slavery, which we proxy by the presence of slaves in 1860.[7] To deal with the potential endogeneity of slavery to local cultural and political factors, we use cotton suitability as an exogenous predictor of slavery, following Acharya, Blackwell and Sen (2016) and Masera and Rosenberg (2020). We also use measures of private and institutional racial violence between the Civil War and World War II, which we proxy by the local number of lynchings (from Hines and Steelwater (2012)) and executions of Black people (from Espy and Smykla (2016)) at the county level.

We discuss ancillary sources of data in relevant sections and the data on Trump's speeches at rallies in Section 5.3.

3. Empirical Strategy and Results

In this section, we establish the effect of Trump rallies on police stops towards Black drivers. We show that the increase in the probability that a stopped driver is of a given race is specific to Black drivers, unique to Trump rallies, materializes immediately after a rally, and lasts for up to 60 days.

3.1 DID Specification

We conduct our analysis at the stop level, estimating whether a Trump campaign rally e leads to an increase in the probability that the driver stopped by the police in stop i in county c on date t is Black. We first define $D_{c,t}^{(a,b)}$ as a dummy variable equal to one if day t is within a and b days from any rally in county c. Formally, $D_{c,t}^{(a,b)} = Max(\mathbb{1}(a \leq d_{c,t,e} \leq b)_{e=1,\ldots,N_c})$, where $d_{c,t,e}$ is the distance (in days) of day t from Trump rally e in county c. $d_{c,t,e}$ is positive if day t is after the rally and negative if day t is before the rally. A given county can have more than one rally, and up to N_c rallies. Out of the 141 counties in our sample, 98 have exactly one Trump rally, while 18 have 3 or more rallies. In the case of multiple rallies in a county, $D_{c,t}^{(a,b)}$ is equal to one if the stop is made between a and b days from any rally.[8] With a slight abuse of notation, $D_{c,t}^{(-\infty,a)}$ is defined as a dummy variable equal to one if the distance of day t from any Trump rally in county c is less than a. Similarly, $D_{c,t}^{(a,\infty)}$ is a dummy variable equal to one if the distance of day t from any Trump rally in county c is more than a. Formally, $D_{c,t}^{(-\infty,a)} = Max(\mathbb{1}(d_{c,t,e} \leq a)_{e=1,\ldots,N_c})$ and $D_{c,t}^{(a,\infty)} = Max(\mathbb{1}(d_{c,t,e} \geq a)_{e=1,\ldots,N_c})$.

Our estimation equation is:

$$Black_{i,c,t} = \gamma D_{c,t}^{(-\infty,-k-1)} + \eta D_{c,t}^{(0,0)} + \beta D_{c,t}^{(1,k)} + \delta D_{c,t}^{(k+1,\infty)} + \alpha_c + \theta_t + \sigma_c \times t + u_{i,c,t} \quad (1)$$

[7] We use the Census of 1860, the last official record of the number of slaves prior to the abolition of slavery.

[8] In robustness, we check that our results hold for alternative ways to deal with multiple rallies in a county.

where $Black_{i,c,t}$ takes value one hundred if the driver pulled over by the police in stop i in county c on date t is Black and zero otherwise (probability of a Black stop). In other words, our specification compares the probability of a Black vs. non-Black stop, in the days before and after a rally, in counties with and without a Trump rally

One concern may be that counties where Trump held a rally differ systematically from other counties. For example, Trump might target counties as a function of their underlying racism or police behavior. Time-invariant county institutional or cultural characteristics, including political preferences, permanent police capacity, legislative differences, or geographic differences, are captured by county fixed effects α_c. Additionally, to account for county-specific time trends in the probability of a Black stop, we include county-specific linear time trends $\sigma_c \times t$. Day fixed effects θ_t account for daily fluctuations in the nature of the traffic stops; for instance, across different days of the week, holidays, or end of the month effects. We focus our analysis on police stops in the years 2015-2017.

A rally may disrupt the daily routine of police departments in several ways. On the one hand, the organization of a large-scale public event could mean that police officers are deployed near the venue of the rally and are not patrolling the roads as they usually do. On the other hand, the authorities may prefer to enhance security in their local area by increased patrolling. We control for such potential disruptions using an indicator that takes the value of one for county c on date t of the rally (i.e., for $D_{c,t}^{(0,0)}$).

Our main parameter of interest is β. The variable that captures the treatment is a dummy variable that takes the value of one for the k days following any Trump rally in that county and zero otherwise. We also control for $D_{c,t}^{(-\infty,-k-1)}$, which is equal to one for the days prior to the comparison window k, and $D_{c,t}^{(k+1,\infty)}$, which is equal to one for the days following our treatment period. Therefore, the omitted comparison time window consists of an identical window immediately prior to the rally.

In the classical potential outcome framework of a DID estimation, the identification assumption requires that after controlling for day and county fixed effects, the probability of a Black stop (relative to a non-Black stop) would not change in the k days after a Trump rally compared to k days before, in the absence of the rally. This assumption would be violated if Trump rallies were systematically timed to correspond with a change in the probability of a Black stop. We address this in three ways. First, the inclusion of county-specific time trends enables us to account for different linear dynamics in the probability of a Black stop across counties. Second, we show in Table A2 that there is no statistically significant difference in the probability of a Black stop just before a rally between counties that are about to be treated and other counties. We show this for alternative windows of 5 to 30 calendar days (in increments of 5 days) before the rally in order to capture reasonable time frames for the scheduling of rallies. This absence of systematic differences shows that the timing of Trump rallies is not driven by short-run changes in the probability of a Black stop and alleviates potential concerns about reverse

causality. Third, we use an event-study analysis to test for the presence of pre-trend differences in the probability of a Black stop across counties before a Trump rally, as well as for differences in the dynamics of the outcome after the end of the treatment.

A potential threat to correct inference on the treatment effect consists of the serial correlation of the error term u_{ict} within a county over time or across counties on a particular date. We consequently adjust standard errors for two-way clustering at the county and day level. In Section 3.3, we check that our results are not subject to potential issues with the two-way fixed effects estimators highlighted by Sun and Abraham (2021), de Chaisemartin and D'Haultfoeuille (2020), and Borusyak, Jaravel and Spiess (2021).

We guide the selection of the treatment window k with an event-study methodology. We then estimate Equation 1 with the time window k that corresponds to the peak of the effect in the event-study results.

3.2 Event-study Specification

We conduct an event-study analysis, which offers several advantages. First, it allows us to check for trends in our dependent variable before a Trump rally and after our treatment window. Second, it enables us to estimate precisely when the effect of a Trump rally materializes. Third, we can study how the effect changes, across each time period, rather than averaging over the whole window k as in the generalized DID. The event-study specification is as follows:

$$\begin{aligned} Black_{i,c,t} = \gamma D_{c,t}^{(-\infty,-106)} + \sum_{\tau=-105(15)-30} \beta_\tau D_{c,t}^{(\tau,\tau+14)} + \beta_0 D_{c,t}^{(0,0)} \\ + \sum_{\tau=1(15)91} \beta_\tau D_{c,t}^{(\tau,\tau+14)} + \delta D_{c,t}^{(106,\infty)} + \alpha_c + \theta_t + \sigma_c \times t + u_{i,c,t} \end{aligned} \tag{2}$$

To smooth out noise in daily observations, we estimate parameters for a 15-day window. $D_{c,t}^{(\tau,\tau+14)}$ is equal to one for county-day observations that are between τ and $\tau + 14$ days from a Trump rally. $D_{c,t}^{(-\infty,-106)}$ is equal to one for county-day observations that are more than 105 days before a rally. $D_{c,t}^{(106,\infty)}$ is equal to one for county-day observations that are more than 105 days after a rally. In all specifications, we include county and day fixed effects as well as county-specific time trends. In the event study, the omitted time bin is $D_{c,t}^{(-15,-1)}$, i.e. 15 days prior to a Trump rally.

Borusyak, Jaravel and Spiess (2021) show that in some settings with staggered treatment where each unit is treated only once, econometric models with unit and time fixed effects are unable to identify a unit-specific linear trend. This happens in fully dynamic settings where the treatment effect persists even after the estimation sample ends, and in the absence of never-treated units. Our framework is different because, as we show, it is not fully dynamic: the treatment effect lasts for only 60 days. Furthermore, our estimation sample includes many days after the end of the treatment effect for treated

units as well as never-treated units. We still show the robustness of our estimates from Equation 2 to the exclusion of county-specific time trends $\sigma_c \times t$.

3.3 Main Results

3.3.1 Event-study Results Figure 2 shows the estimates of β_τ in Equation 2. Trump rallies result in a substantial and immediate spike in the probability of a Black stop following a rally. The probability of a Black stop increases by 1.04 p.p. in the first 15 days after a rally. It remains stable for 30 days after a rally and reaches its highest magnitude at that point (1.13). The effect then declines and fades away 60 days after a rally. The figure also shows the trend in the probability of a Black stop prior to a Trump rally. This trend is flat, with point estimates close to zero, and if anything, shows a slight and marginally statistically significant drop in the probability of a Black stop in the penultimate fortnight before a rally compared to the fortnight just before the rally. However, Figure A2 shows that this drop is not robust to the exclusion of county-specific time trends from the estimation of Equation 2. In all other regards, the event-study results are similar whether time trends are included or not: the effect of a Trump rally is immediate, peaks at 30 days after a rally, and fades away after 60 days. In addition to the absence of pre-treatment differences in trends, we show that the difference in trends between treated and control counties goes back to being close to zero after 60 days.

Overall, these results show that Trump did not specifically time his rallies in certain counties as a function of local police behavior towards Black drivers, and that his rallies had an immediate, large and statistically significant effect on the probability of a Black stop that lasts for two months after a rally. In what follows, we select 30 days after a Trump rally as the treatment window, which is when the effect is the largest.

3.3.2 DID results Column 1 of Table 1 displays the estimates of Equation 1 for a 30-day window around a Trump rally. We observe that the probability of a Black stop increases after a Trump rally. The estimate of β is positive and statistically significant at the 1% level. In terms of magnitude, we estimate a 1.07 p.p. increase in the probability of a Black stop during the first 30 days following a Trump campaign rally. Given that the pre-treatment average probability of a Black stop in the 30 days before a rally stands at 18.65%, this amounts to a 5.74% increase in the probability of a Black stop after a Trump rally.

In Column 2 of Table 1, we show that, consistent with the event-study results, the effect declines over time and is no longer statistically significant after 60 days.

3.3.3 Robustness We show in Column 3 of Table 1 that our results are robust to excluding county-specific time trends. They are also robust to including county-specific quadratic time trends (Column 4). In Column 5, for the treated counties, we restrict

the estimation sample to 30 days around a Trump rally and we estimate a simple DID specification in which the estimation equation no longer includes $D_{c,t}^{(-\infty,-31)}$, $D_{c,t}^{(31,\infty)}$ or county-specific trends. The results are robust.

Day fixed effects in Equation 1 are estimated using police behavior in all counties. Day-by-day law enforcement patterns may be very different in counties that do not hold Trump rallies. We show that such misspecification concern is not an issue in our setting: the results of estimating Equation 1 in the subsample of the 141 counties treated with at least one Trump rally are similar to the results obtained in the full sample. These results are displayed in Column 6 of Table 1.

Recent econometric literature on staggered DID highlights potential issues with the two-way fixed effect estimator used here. One of the main insights of this work is that the estimated parameter is a weighted average of each treatment (in our context, each rally) where the weights may be negative. We consequently follow the recommended diagnostics by de Chaisemartin and D'Haultfoeuille (2020). We compute the weights associated with each treatment both at the police stop level and at the county-day level to address the possibility that treatment effect heterogeneity may operate at the police stop or at the county-day level. Figure A3 shows that there is little variation in the weights and that none is negative in our preferred specification with $k = 30$ days after a rally. However, our setting does not perfectly match the situation studied by de Chaisemartin and D'Haultfoeuille (2020). First, the treatment only lasts for up to 60 days. Second, a county may be treated multiple times. Third, we bin days together to estimate the average effect of a Trump rally in the first k days (with $k = 30$ in our preferred specification). In a context more similar to our own, Sun and Abraham (2021) propose estimating the treatment effect for each event and then averaging the event-specific treatment effects out. Figure A4 displays the distribution of the estimated DID parameters for each county. Following their technique, we combine these estimates (with equal weights) and find that the probability of a Black stop increases by 1.05 p.p., which is very close to our baseline estimate of 1.07.

To ensure that Equation 1 is capturing the effect of Trump rallies rather than artifacts generated by the specification, we show the results of permutation inference (based on $1,000$ replications) in which we randomly assign Trump rallies across county-days. We restrict rally reassignment to occur between the Trump campaign's actual start and end dates. The results in Figure A5 show that our effect size is well outside the range of estimated effects from these placebo treatments.

Our results are robust to alternative ways to deal with multiple rallies. First, we redefine the treatment as the sum over multiple rallies within the treatment window. Second, we duplicate observations in the case of multiple rallies so that each rally gets its own panel. Results in Columns 1 and 2 of Table A3 show that estimated effects are close in magnitude to our main estimate and still statistically significant.

3.4 Triple Difference Results

Our results so far show that the probability that a stop is of a Black driver increases after a Trump rally, relative to the probability that a stop is of a non-Black driver. In this section, we estimate triple difference specifications to show the more specific effects of Trump rallies on the probability and number of traffic stops for each group or ethnicity relative to another.[9]

In this specification, an observation is at the race by county by day level. The dependent variable is the share (or, alternatively, number) of stops of Black, Hispanic, and API drivers, with the share (or number) of stops of White drivers as the excluded category. The specification includes the dummy variables capturing the timing of Trump rallies, interacted with race or ethnicity indicators. The specification includes day-by-race and county-by-race fixed effects as well as county-by-race linear trends. We estimate:

$$\begin{aligned} Y_{r,c,t} = & \sum_{j=B,H,API} \mathbb{1}(j=r) \times \left(\gamma_j D_{c,t}^{(-\infty,-31)} + \eta_j D_{c,t}^{(0,0)} + \beta_j D_{c,t}^{(1,30)} + \delta_j D_{c,t}^{(31,\infty)} \right) \\ & + \alpha_{c,r} + \theta_{t,r} + \sigma_{c,r} \times t + u_{r,c,t}, \end{aligned} \tag{3}$$

where $Y_{r,c,t}$ is either the share or the number of stops of drivers of race or ethnicity r (for Blacks, Hispanics, or Asian or Pacific Islanders) in county c on date t. $\mathbb{1}(j = r)$ takes value one for shares or numbers of stops of drivers of race or ethnicity r. For the number of stops, we additionally control for the total number of stops. We use the the inverse hyperbolic sine (IHS) transformation of the number of stops in order to taper the influence of observations with a very large number of stops and to deal with the fact that although all county-day observations contain at least one stop, some may not contain any stop of a particular group or ethnicity. The parameters of interest are β_B, β_H, and β_{API}, which capture the change in the share (or number) of stops for each group in the 30 days after a Trump rally with respect to the excluded category, Whites.

3.4.1 Relative probabilities of Black stops and other minority stops The estimates of Equation 3 for the share of stops are shown in the top panel of Column 1 of Table 2. They show that only the share of Black stops with respect to Whites increases after a Trump rally. The estimates for the change in the shares of Hispanic or API stops with respect to Whites after a Trump rally are small and statistically insignificant. In the bottom panel of Column 1, we use these estimates to compute the relative change in the share of Black stops with respect to any other group or ethnicity. We find that the share of Black stops also increases relative to Hispanics, and relative to API. In terms of magnitude, our results indicate that the probability of a Black stop increases by 1.06

[9] We thank an anonymous referee for this suggestion.

p.p. compared to a White stop, 1.45 p.p compared to a Hispanic stop, and 1.11 p.p with respect to an API stop.[10]

We also estimate the event-study counterpart of this triple difference specification in order to study the dynamics of the effect of Trump rallies. To do so, we estimate Equation 3 with similar time windows as in Equation 2. In Figure A6 in the Appendix, we use the estimates from the event-study regression to show, in separate panels, the effect on Black, Hispanic, or API drivers with respect to Whites. Figure A7 shows the results without linear trends. Panels A of Figures A6 and A7 show that the probability of a Black vs White stop immediately increases after a Trump rally and the effect is positive and statistically significant effects for 60 days after a rally. Panels B and C show that in contrast, the probabilities of a Hispanic vs. White stop, or API vs. White stop do not change after a Trump rally.

The event-study counterpart of the triple difference specification also shows that there is no change in any of these relative probabilities before a rally. Such a lack of pre-trends suggests that the timing and location of Trump rallies are not only uncorrelated with changes in the relative probability that a stopped driver is Black with respect to White, but also uncorrelated with changes in unobservable characteristics that could affect the relative probability that a driver is Hispanic or API with respect to White. For example, given Trump's openly xenophobic rhetoric against Central American migrants, one specific concern could have been that rallies were timed with changes in the probability of a Hispanic stop, or that their announcement would generate turmoil that would also affect the probability of a Hispanic stop. The evidence in Panels B of Figure A6 and Figure A7 is inconsistent with this scenario.

3.4.2 Number of Black stops and other minority stops The estimates of Equation 3 for the number of stops are shown in the top panel of Column 2 of Table 2. The results are similar to those obtained for the shares. The number of stops of Black drivers increases with respect to any other group or ethnicity. They increase by 5.6% with respect to Whites, 4.7% with respect to Hispanics, and 6.4% with respect to API. For any comparison of two groups or ethnicities not involving Blacks, we observe no statistically significant change in their relative number of stops.

In Figure A8 and Figure A9 in the Appendix, we show the results of the event-study counterparts of this triple difference, with and without time trends. Similarly to the case with the shares of stops, we observe no pre-trends in any of the outcomes, an immediate

[10] An alternative way of estimating the change in the relative probability of stop of a group relative to another is to estimate several DID specifications in split samples, where we restrict the estimation sample to two groups. In Table A4 in the Appendix, we show the results of these estimations. We observe a statistically significant change in the stop probability of Black drivers with respect to each other group; and no statistically significant change in the stop probability of any pair that does not include Black drivers.

increase only when comparing Black to White drivers, and no change in the relative number of stops of any other minority with respect to White drivers.

We also examine the effect of Trump rallies on the absolute number of stops by race or ethnicity. To do so, we estimate Equation 1 at the county-day level with the IHS transformation of the number of stops of each group or ethnicity as the dependent variable. Column 1 of Table A5 shows that the number of Black stops increases by 5.4%. This effect is statistically significant at the 1% level and it is 7 to 18 times as large in magnitude as the (statistically insignificant) change in the number of stops of drivers of any other group or ethnicity (Columns 2 to 4 of Table A5). The event-study counterparts of these specifications, estimated with or without county time trends, show that there are no pre-trends in the number of stops of any group or ethnicity prior to a rally and that the number of Black stops increases immediately after a rally, while the numbers of stops of White, Hispanic, or API drivers are unchanged (Figure A10 and Figure A11 in the Appendix). The pre-rally average total number of stops in counties with a Trump rally is 154 per day, including 29 stops of Black drivers. Trump rallies therefore resulted in 1.57 additional stops of Black drivers per day per county. This implies that the 221 rallies in our sample led to 10,409 additional stops of Black drivers in the 30 days after a rally ($1.57 * 221 * 30$).

The policing literature has documented cases of deliberate misreporting of minority motorists as Whites (Luh, 2019). Changes in such behavior cannot explain our results because we observe an increase in the share and the number of Black stops relative to any other group, not only White drivers, and no change in the shares or numbers of stops of any other minority.

Taken together, these results show that there were no pre-treatment differences in trends in the share or number of stops of any race or ethnicity, and that rallies only affect stop probabilities and overall stops of Black drivers. We now show that the effect is specific to Trump rallies.

3.4.3 Other political campaigns In order to identify more specifically the effect of a Trump rally rather than any political rally, we augment our main specification to account for the influence of other major contenders to the Republican nomination or the presidency during the 2015-2016 campaign, Ted Cruz and Hillary Clinton. To do so, we estimate Equation 1 using the windows defined for Trump rallies and additional windows specified in the same way but for any rally by Trump, Clinton, or Cruz. The estimates are displayed in Column 1 of Table A6. Political rallies in themselves have no effect on the probability of a Black stop. The estimate associated with the dummy variable taking value one in the 30 days after *any* rally is small (0.017) and statistically insignificant. Only rallies by Trump have a positive and significant (at the 1% level) effect on the probability of a Black stop. The overall effect of Trump rallies in this specification

(displayed in the bottom panel of Table A6) is close to the main estimate in Column 1 of Table 1.

The difference between Trump and either Clinton or Cruz is that Trump was a successful presidential candidate. To show that this cannot explain our results, we show in Table A6 that the rallies by another successful presidential candidate during his first campaign, Barack Obama, had no statistically significant effect on the probability of a Black stop.

4. Change in Policing vs. Change in Driver Behavior

Changes in the probability of a Black stop could stem from changes in police or driver behavior. In this section, we first show, based on stop-level information as well as crash and fatality data, that Trump rallies do not affect driver behavior. We then provide evidence that our results are driven by changes in policing. Last, we discuss various ways a Trump rally may have influenced officers.

4.1 Driver Behavior

If changes in driver behavior justified the increase in the probability of a Black stop, this should be reflected in the number of road crashes and fatalities. We measure crashes and fatalities from the Fatality Analysis Reporting System (FARS), a national traffic crash database and census of fatal injuries in motor vehicle crashes. Additionally, we should observe an increase in stops due to Black drivers being at fault. Many justifications for stops are either only observable *after* the stop has taken place (e.g., driving without a license) or are vague and likely to involve some degree of discretion from the officer (e.g., "moving violation", "improper change of lane or course", or even DUI checks). To overcome this issue, we retain stops following a collision and speeding stops triggered by a speed radar in order to capture stops that reflect driver behavior rather than potentially discretionary officer behavior. Overall, if Black drivers were driving more or more recklessly, we should observe an increase in crashes or fatalities of Black drivers, or in the probability of a Black stop due to collision or speeding.

We aggregate FARS data for 2015-2017 at the county-day level. The data includes information on 67,796 traffic crashes, including 34,980 fatalities in 60,386 county-days. The reason for the fatality (e.g., due to driving violation), as well as race or ethnicity of the driver is only recorded in the case of a fatality. Almost all county-day observations contain a crash, but many (44.76%) contain no fatality. To deal with this, we take the IHS transformation of crashes or fatalities and we estimate a specification similar to Equation 1 at the county-day level using the IHS of the total number of crashes, fatalities, or fatalities by race or ethnicity as alternative dependent variables. Results are displayed in Table 3. We observe no statistically significant change in overall traffic crashes, overall

fatalities, or fatalities due to driving violation after Trump rallies (Columns 1 to 3 of Panel A), and no significant change in road fatalities of Black drivers specifically (Column 4 of Panel A). Examining reasons for traffic stops of Black drivers in Columns 1 and 5 of Panel B, we observe no change in the probabilities that the stop of a Black driver is due to collision or triggering a speed radar.

Even if we observe no change in any of these measures of driving behavior, there could be a change in Black drivers' behavior that is not captured by these outcomes. For example, Trump rallies may generate resentment in the Black population leading to other changes in driving behavior. One measure of such resentment may be provided by the occurrence of Black Lives Matter protests. However, in Table A7, we rely on newly collected data on Black Lives Matter protests in 2015-2016[11] to show that these protests do not systematically follow Trump rallies. Overall, the evidence provided here overwhelmingly suggests that the change in the probability of a Black stop after a Trump rally is unlikely to be driven by changes in Black driver behavior.

Another possibility is that the increase in the probability of a Black stop could be driven by a combination of a change in police behavior and the behavior of non-Black drivers. For example, it could be the case that the police became more stringent towards all drivers, but only non-Black drivers drove less, or committed fewer driving offenses after a Trump rally. For this to explain our results, however, it should be the case that the change in driving behavior of non-Black drivers exactly compensates the increased stringency of the police, since we have established that the number of stops of any group other than Black is unchanged.

If non-Black drivers were driving less or more safely, we should observe fewer traffic crashes and fatalities of non-Black drivers. This is not the case, as shown in Column 5 in Panel A of Table 3. Columns 6 and 7 show specifically that there is no change in fatalities of White or Hispanic drivers. The FARS dataset also records fatalities of Mexican drivers. During his campaign, Trump specifically targeted Mexicans, raising potential concerns that his remarks may have driven behavioral changes among Mexican drivers specifically, which police-level data is not able to pick up. We provide evidence inconsistent with such an effect in Column 8: there is no statistically significant change in the number of fatalities of Mexican drivers.

As an additional piece of evidence that non-Black driving patterns are unchanged, Columns 2 and 6 in Panel B of Table 3 show that the probabilities of stop of non-Black drivers due to collision or speed radar are unchanged. The same holds when examining White or Hispanic drivers separately (Columns 3 and 4, and 7 and 8 in Panel B of Table 3). Overall, these results suggest that changes in driving behavior or in the racial composition of drivers cannot explain our results. The effect must therefore be due to a

[11] We collected data on Black Lives Matter events over the sample period from Elephrame (accessed on March 21, 2021).

change in policing.

4.2 Patrolling Decisions

The probability of a Black stop may change because law enforcement agencies or individual officers change patrolling decisions after a Trump rally. Changes in patrolling decisions could be driven by changes in the time of day at which patrols are deployed, where patrols are deployed, and which police officers are sent patrolling. These changes could reflect individual officers' decisions, agency-level decisions, or they could be imposed by local political pressure.[12]

To examine the potential role played by local political dynamics, we note that the duties and operations of state troopers are more insulated from local politicians' injunctions compared with police departments. In Column 1 of Table 4, we include an indicator variable that captures stops by police departments and its interaction with the POST-Trump variable. Our estimates show that stop patterns of Black drivers do not differ statistically across state troopers and police departments, suggesting a limited role played by top-down local political imperatives.

To account for potential differences in the timing of patrols, we include fixed effects for each hour of the day in each county (information on the precise timing of the stop is missing for $17,978,820$ stops). The results, displayed in Column 2 of Table 4 are robust and similar in magnitude to our main estimate. We show in Column 3 of Table 4 that our results are also robust to including local enforcement agency fixed effects that account for potential changes in the shares of stops coming from different agencies within a county. The results are still statistically significant at the 1% level but smaller in magnitude, suggesting that part of the effect is due to a differential increase in overall enforcement by agencies that disproportionately stop black drivers.

To examine whether the effects are due to decisions about which police officer within a given agency patrols (i.e., compositional change in deployed police after a rally), we add officer-level fixed effects to Equation 1. This singles out the effect of a Trump rally on the change in the probability of a Black stop for a given officer. Since individual officer's identifiers are only available for a subset of stops, the estimation sample drops by 51.80%.[13] The result related to the effect of Trump rallies is robust (Column 4 of Table 4), suggesting that the effect of Trump rallies cannot be solely explained by compositional changes in the force on duty.

Including agency or officer fixed effects reduces the estimated effect of Trump rallies

[12]Local mayors may influence their police departments because of a change in their own or their electorate's political preferences. County sheriffs that are elected may be equally sensitive to local electoral preferences. Snyder and Yousaf (2020) show that Trump's rallies increased his support among the electorate, intentions to vote, and individual campaign contributions in his favor.

[13]The point estimate associated with POST-Trump in our main specification in the comparable sample (i.e., for which we are able to include officer fixed effects) is 1.04.

on the probability of a Black stop. This suggests that part of the effect of Trump rallies is channeled through changes in the composition of stops between agencies within a county as well as through changes in the composition of officers who are patrolling. However, even including officer (or agency) fixed effects, the estimated effect remains large and statistically significant, suggesting that Trump rallies also affect the individual behavior of law enforcement officers. Overall, this evidence shows that the effect of Trump rallies operates through various levels of the hierarchy. One policy implication is that improvements in selection or training of individual police officers may not suffice to shield law enforcement from the influence of political campaigns.

4.2.1 Spillover Effects Our results show that Trump rallies generated a change in the behavior of law enforcement officers that is specifically targeted towards Black drivers. There are several channels through which Trump rallies can affect officers and their hierarchy. They may attend rallies, either as audience members or to fulfill their professional duties.[14] They may also be exposed indirectly through media coverage (Snyder and Yousaf, 2020), either traditional or social media, or through peers.

Our baseline specification assumes that counties that host a Trump rally are treated more intensely than other counties. We now provide additional analyses where treatment intensity instead depends on the physical or social distance to a Trump rally.

To measure treatment intensity using physical distance to a rally, we geolocate the exact location of each rally. For stops by state troopers, we use the distance between the rally location and the centroid of the county where the stop happens. For stops by a city police department, we use the distance between the rally location and the centroid of the city.[15] The assumption is that police stops that happen close to a Trump rally in the days after a rally are more intensely treated, compared with stops that happen the same day but further away. We estimate an equation similar to Equation 1 where a police stop is considered to be treated in the 30 days after a rally if the physical distance to the rally is less than l. Figure A12 in the Appendix depicts the results of a series of regressions in which $l = 10, 20, ..., 150$ km. We observe that the effect decreases with l and is no longer significant for l higher than 50.

Geographic distance may not be able to capture the full extent of the possible spillovers from Trump rallies. The influence of Trump rallies may extend across county borders also because people share information and news with their family, friends, and peers. We capture the extent of such potential social interaction with the measure of social connect-

[14]These rallies are large events, which gather an average of almost 5,000 people. The presence of police officers as attendees has been widely covered in the press (e.g., The Washington Post on 03/21/2016 or The Independent on 05/29/2016).

[15]The only consistent geographic information on a stop is the county in which the stop happened. In the case of city police departments, since the stop has to happen within the city limit, we can more precisely allocate a stop to a geographic area.

edness between counties developed by Bailey et al. (2018) based on Facebook friendship data. This index corresponds to the relative frequency of Facebook friendship links between every county-pair. While more proximate counties are generally more connected, this index also proxies the influence of other factors, such as migration patterns. For example, New York county is 20 times more connected to San Francisco county compared to an equidistant county: Kern, California.

We consider that a given county is treated more intensively by the social interaction effect of Trump rallies the more rallies have occurred in the previous 30 days and the more socially connected this county is to the counties where the rallies happened. Thus, we measure the social spillover effect by a weighted sum of the number of rallies in the previous 30 days, where the weights correspond to the index of connectedness between the county where the stop happens and the counties where the rallies have occurred. We call this weighted index the Trump social spillover index. We estimate Equation 1 in which we add the Trump social spillover index as a regressor, standardized to have mean zero and standard deviation of one. Therefore, we can measure both the effect of rallies that happen in a given county and the spillovers that spread through the kind of social interactions that are reflected by friendship links on Facebook. The results, displayed in Table A8 in the Appendix, show that the coefficient associated with the Trump social spillover index is positive and significant. This effect adds to the effect of hosting a rally in the county, which is still statistically significant and unchanged in magnitude. The results show that the influence of a rally that occurs in the county is much larger in magnitude than the social spillover effect. One standard deviation increase in the Trump social spillover index is associated with a statistically significant but small (0.06%) increase in the probability of a Black stop.

Overall, these results suggest that Trump rallies may have spillover effects beyond the county where the rally happens, but these are orders of magnitude smaller than the direct effect. An ancillary implication of these results is that given such limited spillovers, the county is the appropriate level of analysis in our study.

5. Mechanisms

In this section, we explore why Trump rallies generate a change in the behavior of law enforcement towards Black drivers. Previous literature has shown that openly xenophobic rhetoric can lead to an increase in the acceptability and expression of discrimination and xenophobia against the targets of that rhetoric, and that the effects are stronger for individuals who are already prejudiced (Yanagizawa-Drott, 2014; Adena et al., 2015; Bursztyn, Egorov and Fiorin, 2020; Romarri, 2020; Ang, 2021; Bracco et al., 2022).

Explicit racist rhetoric against Black people was standard in political speech in the United States well into the early twentieth century (Mendelberg, 2001; Becker, 2021).

This changed as racial equality supplanted racial inequality as the dominant accepted social norm in the United States in the post-Civil Rights era (Mendelberg, 2001; Newman et al., 2020).[16] In this new context, explicit racist rhetoric could backfire and politicians instead resorted to implicit communication (see Mendelberg (2008) for a meta-analysis and Thompson and Busby (2021) for a recent contribution), relying for example on coded language, references, or symbols – also called "dog-whistles" (Valentino, Hutchings and White, 2002; Haney-Lopez, 2014; Valentino, Neuner and Vandenbroek, 2018). These dog-whistles either exploit common knowledge between the principal and part of the audience or they harness stereotypes that are only held by part of the audience. We focus on an easily measurable aspect of dog-whistles: words. For example, in the modern US context, the word "thug" is, for some, an implicit reference to Black people. It can also trigger negative stereotypes associating Black people with violence and crime (Haney-Lopez, 2014; Eberhardt et al., 2004; Smiley and Fakunle, 2016).

Inflammatory rhetoric may impact the expression of prejudice through several channels. First, inflammatory speech may trigger negative emotions or increase the salience of issues stereotypically associated with racial groups. Second, it may convey information that changes beliefs about racial groups. Third, in the context of political rallies, attended by many people, inflammatory rhetoric may legitimize racially biased positions by changing perceptions about the acceptability of these positions (Bursztyn, Egorov and Fiorin, 2020). Implicit rhetoric operates through the same channels, but only on individuals who respond to implicit cues.

A common prediction of the literatures on both explicit and implicit rhetoric is that individuals who are already more prejudiced or hold stronger stereotypes should react more. In what follows, we test the hypothesis that Trump rallies should have a more substantial effect among law enforcement when pre-existing levels of bias are higher. We then provide evidence that the use of words that could be associated with racial issues, even implicitly, accentuates the effect of Trump rallies on the behavior of law enforcement towards Black drivers.

5.1 Heterogeneity Across Police Officers

To test for the prediction that Trump rallies should affect racially biased officers more, we estimate Equation 1 including a control for a proxy of the pre-treatment racial bias of the officer performing the stop, and an interaction between this proxy and the post-treatment window of 30 days.

Officer racial bias is unobservable. The literature on racial bias in policing aims at inferring officer racial bias by observing the race of citizens in citizen-police interactions.[17]

[16] We understand dominant social norm here as an "informal standard of social behavior accepted by most members of the culture and that guides and constrains behavior" (Mendelberg, 2001).

[17] See footnote 1 and Lang and Kahn-Lang Spitzer (2020) for a recent review.

In the context of traffic stops, the main difficulty is that the probability of a Black stop is not only affected by officer bias but also by potential heterogeneity in the context of traffic stops across officers. In order to overcome this difficulty, one approach is to use a rich set of controls in order to capture potential differences in the context of a stop, as in Ba et al. (2021). We follow this approach.[18]

Our data includes information on the day and hour of the stop, but information on the stop location is provided consistently only at the county level. Including fixed effects for these observable characteristics may not be enough to account for potential heterogeneity in the context of stops. There could remain substantial within-county heterogeneity in the probability of encountering a driver of a certain race. To reduce potential residual heterogeneity in stop patterns, we also take into account the outcome of a stop. The idea behind our estimation of officer-level bias is that conditional on a stop, a more racially biased officer is more likely to be lenient towards a White driver (e.g., by only giving a warning) than towards a Black driver.[19]

We first estimate the likelihood of a stop resulting in a warning (vs. more severe outcomes: citations, summons, or arrests), controlling for the most detailed set of fixed effects available in our data: county, day, and hour. We proxy officer-level bias by a measure of relative stringency towards Black vs. White drivers, which consists of the difference in the average estimated residuals for that officer when stopping a White driver with respect to a Black driver. A given officer scores high in our measure if they are more likely to give only a warning to a White vs. Black driver, compared to that difference in likelihood for the average stop in the same county, on the same day, and at the same hour. We compute a similar measure of bias for arrests (vs. less severe outcomes), based on the residuals of the likelihood of a stop resulting in an arrest. A given officer scores high in this second measure if they are more likely to arrest a Black vs. White driver (conditional on the set of fixed effects).

We estimate officer-level bias towards Black drivers based on pre-treatment data for officers in counties with a Trump rally, and on all data for officers in counties that did not host any Trump rally. On average, we observe $1,114$ stop decisions per officer that we use to estimate officer-level racial bias. The two measures of racial bias are highly correlated with one another (correlation coefficient: 0.96) but uncorrelated with the total number of Black stops by the officer (0.022 and 0.015, respectively), suggesting that they

[18]Studies aiming at measuring racial bias in other policing and judicial decisions face a similar issue. To circumvent it, some studies have used the "hit rate test", where discrimination is identified by comparing the outcome across races of a specific policing decision after the stop decision, such as vehicle search (Knowles, Persico and Todd, 2001; Antonovics and Knight, 2009). The theoretical idea behind this test is that under no discrimination, at the margin, the probability of finding contraband should be equalized across races. Our dataset records the outcome of a search in only 0.9% of observations, rendering the implementation of this method impossible.

[19]This idea is similar to the test developed in Goncalves and Mello (2021) that compares how officers manipulate reported speed after a stop across races. Unfortunately, we do not have data on the reported speed that would allow us to replicate their method.

adequately capture more than mere exposure to Black drivers.[20]

Results reported in Columns 1 and 3 of Table 4 show that the effect of Trump rallies is 10% to 16% larger for police officers whose estimated racial bias is one standard deviation above the mean. These differences are statistically significant at the 1% level and robust to the inclusion of officer-level fixed effects (Columns 2 and 4).

Consistent with the implications of the literature on the effect of racial rhetoric, these results show that Trump rallies have a much larger effect on the behavior of law enforcement officers who score higher on measures of racial bias. Our measures of officer-level bias, and thus our analysis of the heterogeneous effects of Trump rallies, are based on comparing officers within the same county. However, a large literature documents heterogeneity in racial attitudes across the US, which is partly inherited from differences in the prevalence of slavery (Acharya, Blackwell and Sen, 2019). We show the importance of this second source of heterogeneity in the next subsection.

5.2 Heterogeneity Across Counties

Our focus is now on racial attitudes at the county level. Our measures come from county-average responses to the two racial resentment questions included in the 2012 and 2014 CCES (Schaffner and Ansolabehere, 2015) described in Section 2, as well as proxies of deep-seated racial animus inherited from the pre-Civil War era (presence of slaves in 1860 and cotton suitability as an exogenous predictor of slavery) and measures of historical racial violence between the Civil War and the mid-20th century (number of lynchings and executions). To deal with the fact that some counties have a large number of lynchings and executions while others have none, we use the IHS transformation of the number of lynchings or executions. The idea behind this heterogeneity analysis is that officers' racial attitudes may reflect present-day average county-level racial attitudes as measured in surveys or the racial history of the county in which they work.

We estimate the heterogeneous effect of Trump rallies on police behavior across counties by including in the estimation of Equation 1 an interaction term between $D_{c,t}^{(1,30)}$ and pre-determined county characteristics.[21] All continuous variables measuring county characteristics are normalized to have a mean of zero and a standard deviation of one. We also control for linear trends based on these pre-determined county characteristics. Therefore, the interaction term captures differential changes from the underlying trend of the probability of a Black stop as a function of pre-determined county characteristics.

Results are presented in Columns 1 to 6 of Table 5 (Panel A). The effect of Trump

[20]The measures are based on differences between officers in the same county, implying that they should be uncorrelated with county-average characteristics. For example, both measures are uncorrelated with the proportion of Black people residing in the county (correlation coefficients of 0.004 and 0.015 respectively).

[21]As these characteristics are pre-determined, their main effects are absorbed in the county fixed effect.

rallies is larger in areas with stronger racial resentment as measured in present-day surveys as well as those that experienced a more violent racial history. In terms of magnitude, the effect of Trump rallies on the probability of a Black stop is between 80% (Column 1) and 67% (Column 2) larger in counties that are one standard deviation above mean racial resentment with respect to counties with mean racial resentment, as measured in the CCES survey, depending on which measure we use. It is more than twice as large in former slaveholding counties and 69% higher in counties whose soil conditions are one standard deviation above mean cotton suitability (Columns 3 and 4). As for proxies for racial violence during the Jim Crow era, we observe that the effect of Trump rallies on the probability of a Black stop is 35% higher in counties with one standard deviation above the mean of the IHS transformation of the number of lynchings (Column 5), and 78% higher in counties with one standard deviation above the mean of the IHS transformation of the mean number of executions of Blacks (Column 6).

We also check that our results are specific to racial resentment and a violent racial history rather than due to other county characteristics. Panel B of Table 5 shows that other potential sources of heterogeneity, such as those stemming from political partisanship, either measured by the vote share for Obama in the 2012 Presidential elections (Column 1) or by sheriff political affiliation (Column 2), differences in income (Column 3), or in average education (Column 4) do not play a role. Similarly, the trade shock with China, which has been shown to influence voting (Autor et al., 2017, 2020) does not mediate the impact of Trump rallies on the behavior of law enforcement towards Black drivers, regardless of whether we use the actual or instrumented measure of trade shock (Columns 5 and 6).[22]

So far, we have established that Trump rallies affect racially-directed behavior among law enforcement. The effect is stronger among officers who initially score higher on our measures of racial bias against Black drivers and in areas that express greater racial resentment today, former slaveholding counties, and those that experienced a more violent racial history during the Jim Crow era. These results are consistent with the prediction that rallies should radicalize individuals who already hold more prejudiced or stereotyped racial views. We now provide direct evidence on the mediating role of racial rhetoric in Trump's speeches.

5.3 Trump's Rally Speeches and Police Stops

In this section, we examine how the words spoken in Trump speeches mediate the effect of a rally on police behavior.

[22] A potential concern is that our measures of racial resentment are correlated with the share of Black people in the county. To alleviate this concern, we replicate the results in Table 5 controlling for time trends interacted with the share of Black people and obtain similar results. Results are shown in Table A9.

To perform our analysis, we obtain the content of speeches at campaign rallies from several sources. We gather data on 190 speeches overall from the American Presidency Project (Peters and Woolley, 2020),[23] Enke (2020) and the Trump Campaign Corpus Project[24].

We consider explicit mentions of racial issues ("race", "racial" or "racist", "Black", "African", mentioned on average 2.75 times per speech) as well as words that could be interpreted by some as references to racial issues or that could trigger negative stereotypes associated with Black people. Based on previous work, namely by Haney-Lopez (2014); Smiley and Fakunle (2016); Hopkins (2019); Becker (2021); Schutten et al. (2021), we select "drug", "crime" or "criminal", "gun", "prison", "rape", "riot", "thug", and "urban" as implicit references. On average, Trump mentions these words 8.29 times per speech, as shown in Figure A1. The Figure shows that Trump stands out from other candidates in the use of words that relate to racial issues. The difference is particularly pronounced when it comes to implicit mentions, notably crime-related issues.[25]

In order to show that the mechanism specifically operates through mentions of racial issues, rather than speech length or other issues unrelated to race that are also frequently mentioned in Trump's rallies, we include in the analysis other words that constitute common topics in his speeches. We focus on five other common topics: (i) job loss and manufacturing decline (busines, job, manufactur, tax) (mentioned 25.34 times per speech, on average), (ii) Clinton (Hilary, Clinton, email, lock) (23.01 times per speech), (iii) trade (China, trade, NAFTA) (15.57 times per speech), (iv) terrorism (ISIS, Syria, Iraq, terror, Afghanistan, Islam) (11.06 times per speech), and (v) media and political corruption (rig, media, CNN, Washington, corrupt, swamp) (8.61 times per speech).

We estimate the heterogeneous effect of Trump rally speeches on officers who differ in the measures of officer racial bias with the following specification:

$$\begin{aligned} Black_{i,j,c,t} &= \alpha_c + \theta_t + \gamma D_{c,t}^{(-\infty,-31)} + \eta D_{c,t}^{(0,0)} + \beta_1 D_{c,t}^{(1,30)} + \delta D_{c,t}^{(31,\infty)} + \eta \widehat{\lambda}_j \\ &+ \beta_2 D_{c,t}^{(1,30)} * \widehat{\lambda}_j + \beta_3 D_{c,t}^{(1,30)} * Words_{i,c,t} + \beta_4 D_{c,t}^{(1,30)} * \widehat{\lambda}_j * Words_{i,c,t} + \alpha_c \times t + u_{i,c,t} \end{aligned} \tag{4}$$

$\widehat{\lambda}_j$ is estimated officer bias, proxied by the relative stringency of each officer towards Black versus White drivers. $Words_{i,c,t}$ is the count of the words of interest in the speech for which $D_{c,t}^{(1,30)} = 1$. If there is only one speech in the county, we refer to that speech. If there is more than one, we refer to the closest speech. $Words_{i,c,t}$ as well as estimated

[23] https://www.presidency.ucsb.edu/documents/app-categories/elections-and-transitions/campaign-documents, accessed 10 June 2020.

[24] https://github.com/unendin/Trump_Campaign_Corpus, accessed 16 February 2021.

[25] Our approach here is descriptive. We are not imputing any intent in Trump's choice of words to specifically affect law enforcement, or in his use of implicit mentions as intentional references to Black people.

racial bias $\widehat{\lambda}_j$ are normalized so that they have mean zero and standard deviation of one for the average stop.

To estimate this equation, we need information on the driver's race, officer id, and rally speech. There are 95 speeches corresponding to rallies for which we have police stop data. The resulting estimating sample consists of $14,747,451$ traffic stops.

In this specification, β_1 captures the effect of a Trump rally that mentions the average number of words of interest $Words_{i,c,t}$, for an officer with mean estimated racial bias. $\beta_1+\beta_2$ captures the effect of a Trump rally that mentions the average number of words of interest, for officers with estimated racial bias that is one standard deviation above the mean. $\beta_1+\beta_3$ captures the effect of a Trump rally with one standard deviation above the mean of $Words_{i,c,t}$, for officers with mean estimated racial bias. $\beta_1+\beta_2+\beta_3+\beta_4$ captures the effect of a Trump rally with one standard deviation above the mean of $Words_{i,c,t}$ for officers whose estimated racial bias is one standard deviation above the mean.

Racially biased officers may react differently from non racially biased officers to Trump's speeches for two reasons. First, they may react to any Trump rally differently, which is captured by β_2. Second, they may also react more to specific, potentially triggering words, as captured by our main parameter of interest β_4. The prediction that we can derive from the literature on racial rhetoric is that $\beta_4 > 0$ for inflammatory words. For non-inflammatory words, we should not observe a systematic sign pattern for β_4.

One potential concern is that Trump may tailor his speeches to specific audiences. Unobserved county heterogeneity in average receptiveness of an audience to a specific message is absorbed in county fixed effects. Moreover, we actually observe little variation in speech content as a function of observable county characteristics (see Table A10 in the Appendix).

Estimation results of Equation 4 are displayed in Table 7. Panel A includes the results based on our first measure of officer racial bias (relative stringency towards Black vs. White drivers based on warnings vs. more severe outcomes) and Panel B for the second (relative stringency towards Black vs. White drivers based on arrests vs. less severe outcomes). The results show that the probability of a Black stop by an officer with average relative stringency does not increase after a speech (i.e., the coefficient associated with β_1 is not statistically significant). By contrast, the probability of a Black stop by an officer whose relative stringency is one standard deviation above the mean consistently increases after a speech (i.e., β_2 is consistently positive and statistically significant). The effect is even more pronounced when the speech contains more words that relate to racial issues (i.e., β_4 is positive and statistically significant in Column 1). Columns 2 and 3 show that such an effect holds both when the speech contains more explicit references to racial issues and when it contains more implicit references to racial issues.

The result that explicit racial mentions intensify the effect of rallies only on officers who score high on our measures of racial bias is consistent with racial priming theory,

which predicts that explicit racial appeals would be rejected by the majority (Mendelberg, 2008). Implicit racial appeals are also effective on this subset of officers, who are likely to understand and react to racial cues. One aspect that is perhaps surprising is that they react to implicit references even though they were often spoken in the context of Central American migration. On average, speeches that do not mention Mexico or Mexicans contain words that can be perceived as implicit references to racial issues 63.89% of the time. For speeches that mention Mexico or Mexicans, this proportion rises to 94.84%. This is exemplified in the statement during Trump's presidential announcement speech on June 16, 2015, when he remarked: "When Mexico sends its people, they're not sending their best [...] They're sending people that have lots of problems, and they're bringing those problems with them. They're bringing drugs. They're bringing crime. They're rapists." Therefore, our results suggest that even in other contexts not directly related to race, some words may trigger negative stereotypes and generate a biased response towards Black drivers. This is consistent with recent evidence that both anti-Latino and anti-immigrants appeals trigger feelings of racial threat among Whites (Hopkins, 2010) and that less common issue-group pairings (e.g., Latino and crime) are less recognized as being racist and, by this virtue, less likely to be rejected compared with more commonly used issue-group pairing (e.g., Black and crime) (Reny, Valenzuela and Collingwood, 2020).

Columns 4 and 5 show that although the probability of a Black stop increases for relatively more stringent officers after a rally, there is no additional effect of rally speeches that include more references to trade or, based on our first measure of bias, to Clinton.

The results in Column 6 suggest that mentions of terrorism-related words also amplify the effect of Trump rallies on relatively more stringent officers. An explanation for this result is that mentions of terrorism also trigger feelings of racial and cultural threat (McConnell and Rasul, 2021).

For job loss and manufacturing decline (Column 7) and media and political corruption (Column 8), β_4 is negative and statistically significant. This suggests that these two topics are the least triggering for relatively stringent officers: speeches with one standard deviation above the mean count of these words have a lower effect than speeches with mean word count.[26]

Overall, the takeaway is that Trump rallies have a bigger effect on the behavior of law enforcement officers who are already most biased against Black people, and particularly when the speeches include more words that refer explicitly to Black people, that merely evoke negative stereotypes associated by some to Black people, or that trigger feelings of racial or cultural threat.

[26]The nil or negative results obtained for other common topics in Trump's speeches additionally show that the positive and significant coefficients β_4 estimated for mentions of racial issues or terrorism reflect the effect of specific words rather than the length of the speeches.

6. Conclusion

This paper provides evidence that Trump's first campaign in 2015-2016 affected the behavior of law enforcement officers towards Black people. The probability of a Black stop increased in a county by 5.7% and the number of Black stops by 5.4% after a Trump rally. The effect was immediate and lasted for about two months after a rally. The evidence is consistent with a change in the behavior of law enforcement, including at the individual officer level, which is not justified by changes in driver behavior. Furthermore, we show that the effect was stronger among officers who were more stringent towards Black drivers to start with, and in counties where present-day racial resentment as measured in opinion surveys is higher, in former slaveholding counties, and in counties that experienced more lynchings and executions of Black people during the Jim Crow era. By contrast, income, average education level, democratic vote share, or exposure to import competition with China played no mediating role in the effect of Trump rallies on law enforcement. These results suggest that rallies radicalized law enforcement officers who were already relatively more racially biased, or who worked in areas characterized by higher levels of racial animus and historical racial violence.

The paper provides some evidence on the mechanisms through which Trump rallies affect law enforcement behavior towards Black drivers. Analyzing the content of speeches at Trump rallies, we show that explicit mentions of racial issues trigger a stronger reaction among officers who were relatively more stringent towards Black drivers at baseline. Trump mentioned racial issues more often than other Republican candidates, and he particularly stood out in the frequency of his references to crime, a particularly racially charged topic. We argue that such implicit references could be interpreted as racial appeals by some part of the audience who hold negative racial stereotypes associating crime and Blackness, even when Trump made them in the context of other racial groups. Accordingly, we observe that they generate similar behavioral responses as explicit references to racial issues.

The evidence speaks directly to the consequences of racial rhetoric, whether explicit or implicit, on the use of delegated authority by the bureaucracy and its deleterious consequences on the social fabric. Traffic stops constitute the majority of police-citizen interactions in the US and disproportionately target Black drivers. More than 20 million people are pulled over by the police every year in the US (Davis, Whyde and Langton, 2018), providing the kind of contact that can lead to violent and potentially lethal escalation. Fatal encounters between police and minority civilians have been shown to undermine cooperation and engagement with the police (Ang et al., 2021) and educational achievement (Ang, 2021). Although the object of less media and research attention, the over-enforcement of minor infractions and the kind of unjustified stops by the police that we document in this paper reflect a daily and generalized expression of discrimination

against minorities, which also has potentially dire consequences.[27] Ensuing feelings of discrimination and under-representation undermine minorities' faith in the police and public institutions at large.[28] This produces a vicious circle, whereby daily and unjustified harassment reduces trust in the police and can lead citizens to shy away from relying on law enforcement and to instead seek other, informal, and possibly violent means of protection and retributive justice (Giffords Law Center, 2020). Moreover, unjustified police repression can translate into voter suppression when disenchanted citizens extend their lack of trust in the police to other public institutions. Historical discrimination and violence against Black people is still associated with lower voter registration by Black voters today (Williams, 2020) and police presence at polling places reduces Black turnout (Niven, 2021). Estimating the more general impact of policing on voting behavior is a crucial direction for future research.

Recently, policy efforts have concentrated on training police officers to reduce biased behavior. In light of our results that the effect of Trump rallies permeates several levels of the police hierarchy, individual training may not be enough to insulate law enforcement from the influence of political rhetoric.

Our findings have relevance beyond the context of Trump's 2015-2016 campaign. In the United States, the use of racial rhetoric in politics is on the rise (Becker, 2021) and many politicians have adopted rhetoric and campaigning styles very close to Trump's. Beyond the United States, a similar style of political rhetoric has also seen a resurgence in the last few years. Our study illustrates the negative consequences for historically stereotyped and marginalized communities, who are the explicit or implicit target of that rhetoric.

[27]Related to this, Manski and Nagin (2017) underlines the negative consequences of confrontational policing, including traffic stops. Durlauf (2005) discusses equity considerations in the context of racial profiling and Durlauf (2006) concludes that any benefits from profiling have not been identified, while the harm to those who are innocent and stopped is high.

[28]Robert Wilkins described the experience of being pulled over for a traffic stop in testimony in the United States Senate (Wilkins v. Maryland): "So there we were. Standing outside the car in the rain, lined up along the road, with police lights flashing, officers standing guard, and a German Shepard jumping on top of, underneath, and sniffing every inch of our vehicle. We were criminal suspects; yet we were just trying to use the interstate highway to travel from our homes to a funeral. It is hard to describe the frustration and pain you feel when people presume you to be guilty for no good reason and you know that you are innocent." (cited in Gross and Barnes (2002), p.746).

References

Abramowitz, Alan I. 2018. *The Great Alignment: Race, Party Transformation, and the Rise of Donald Trump.* Yale University Press.

Abramowitz, Alan I., and Kyle L. Saunders. 2008. "Is Polarization a Myth?" *The Journal of Politics*, 70(2): 542–555.

Acharya, Avidit, Matthew Blackwell, and Maya Sen. 2016. "The Political Legacy of American Slavery." *The Journal of Politics*, 78(3): 621–641.

Acharya, Avidit, Matthew Blackwell, and Maya Sen. 2019. *Deep Roots: How Slavery Still Shapes Southern Politics.* Princeton University Press.

Adena, Maja, Ruben Enikolopov, Maria Petrova, Veronica Santarosa, and Ekaterina Zhuravskaya. 2015. "Radio and the Rise of Nazis in Prewar Germany." *Quarterly Journal of Economics*, 130(4): 1885–1939.

Alesina, Alberto, and Eliana La Ferrara. 2014. "A Test of Racial Bias in Capital Sentencing." *American Economic Review*, 104(11): 3397–3433.

Alesina, Alberto, and Paola Giuliano. 2015. "Culture and Institutions." *Journal of Economic Literature*, 53(4): 898–944.

Anbarci, Nejat, and Jungmin Lee. 2014. "Detecting racial bias in speed discounting: Evidence from speeding tickets in Boston." *International Review of Law and Economics*, 38: 11–24.

Ang, Desmond. 2021. "The effects of police violence on inner-city students." *The Quarterly Journal of Economics*, 136(1): 115–168.

Ang, Desmond, and John Tebes. 2021. "Civic Responses to Police Violence." Harvard Business School mimeo.

Ang, Desmond, Panka Bencsik, Jesse Bruhn, and Ellora Derenoncourt. 2021. "Police Violence Reduces Civilian Cooperation and Engagement with Law Enforcement." SSRN Working Paper 3920493.

Antonovics, Kate, and Brian G. Knight. 2009. "A new look at racial profiling: Evidence from the Boston Police Department." *The Review of Economics and Statistics*, 91(1): 163–177.

Anwar, Shamena, and Hanming Fang. 2006. "An Alternative Test of Racial Prejudice in Motor Vehicle Searches: Theory and Evidence." *American Economic Review*, 96(1): 127–151.

Anwar, Shamena, and Hanming Fang. 2015. "Testing for Racial Prejudice in the Parole Board Release Process: Theory and Evidence." *The Journal of Legal Studies*, 44(1): 1–37.

Appleman, Eric M. 2019. "Democracy in Action." *http://www.p2016.org/*.

Arnold, David, Will Dobbie, and Crystal S Yang. 2018. "Racial Bias in Bail Decisions*." *The Quarterly Journal of Economics*, 133(4): 1885–1932.

Autor, David, David Dorn, Gordon Hanson, and Kaveh Majlesi. 2020. "Importing Political Polarization? The Electoral Consequences of Rising Trade Exposure." *American Economic Review*, 110(10): 3139–83.

Autor, David H., David Dorn, and Gordon H. Hanson. 2013. "The China Syndrome: Local Labor Market Effects of Import Competition in the United States." *American Economic Review*, 103(6): 2121–68.

Autor, David H., David Dorn, Gordon H. Hanson, and Kaveh Majlesi. 2017. "A Note on the Effect of Rising Trade Exposure on the 2016 Presidential Election." MIT Mimeo.

Ba, Bocar A., Dean Knox, Jonathan Mummolo, and Roman Rivera. 2021. "The role of officer race and gender in police-civilian interactions in Chicago." *Science*, 371(6530): 696–702.

Bailey, Michael, Rachel Cao, Theresa Kuchler, Johannes Stroebel, and Arlene Wong. 2018. "Social connectedness: Measurement, determinants, and effects." *Journal of Economic Perspectives*, 32(3): 259–80.

Banks, Antoine J, and Heather M Hicks. 2019. "The effectiveness of a racialized counterstrategy." *American Journal of Political Science*, 63(2): 305–322.

Becker, Chris. 2021. "The Rise of Jim Crow Rhetoric in Republican Economic Speeches."

Bonomi, Giampaolo, Nicola Gennaioli, and Guido Tabellini. 2021. "Identity, Beliefs, and Political Conflict*." *The Quarterly Journal of Economics*, 136(4): 2371–2411.

Bordalo, Pedro, Marco Tabellini, and David Y. Yang. 2020. "Stereotypes and Politics." Harvard mimeo.

Borusyak, Kirill, Xavier Jaravel, and Jann Spiess. 2021. "Revisiting event study designs: Robust and efficient estimation." *arXiv preprint arXiv:2108.12419*.

Bracco, Emanuele, Maria De Paola, Colin Green, and Vincenzo Scoppa. 2022. "The Spillover of Anti-Immigration Politics to the Schoolyard." *Labour Economics*, 102141.

Bursztyn, Leonardo, Georgy Egorov, and Stefano Fiorin. 2020. "From Extreme to Mainstream: The Erosion of Social Norms." *American Economic Review*, 110(11): 3522–48.

Cagé, Julia, Anna Dagorret, Pauline Grosjean, and Saumitra Jha. 2020. "Heroes and Villains: The Effects of Combat Leadership on Autocratic Values and Nazi Collaboration in France." Stanford University Mimeo.

Carney, Riley K, and Ryan D Enos. 2017. "Conservatism and fairness in contemporary politics: Unpacking the psychological underpinnings of modern racism."

Coviello, Decio, and Nicola Persico. 2015. "An Economic Analysis of Black-White Disparities in the New York Police Department's Stop-and-Frisk Program." *The Journal of Legal Studies*, 44(2): 315–360.

Davis, Elizabeth, Anthony Whyde, and Lynn Langton. 2018. "Contacts Between Police and the Public 2015." U.S. Department of Justice Bureau of Justice Statistics Special Report NCJ 251145. Accessed: 2020-07-07.

de Chaisemartin, Clément, and Xavier D'Haultfoeuille. 2020. "Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects." *American Economic Review*, 110(9): 2964–96.

Depew, Briggs, Ozkan Eren, and Naci Mocan. 2017. "Judges, Juveniles, and In-Group Bias." *Journal of Law and Economics*, 60(2): 209–239.

Dippel, Christian, and Stephan Heblich. 2021. "Leadership in Social Movements: Evidence from the "Forty-Eighters" in the Civil War." *American Economic Review*, 111(2): 472–505.

Durlauf, Steven N. 2005. "Racial Profiling as a Public Policy Question: Efficiency, Equity, and Ambiguity." *American Economic Review*, 95(2): 132–136.

Durlauf, Steven N. 2006. "Assessing Racial Profiling." *The Economic Journal*, 116(515): F402–F426.

Eberhardt, Jennifer L, Phillip Atiba Goff, Valerie J Purdie, and Paul G Davies. 2004. "Seeing black: race, crime, and visual processing." *Journal of personality and social psychology*, 87(6): 876.

Edwards, Griffin Sims, and Stephen Rushin. 2019. "The effect of President Trump's election on hate crimes." *Available at SSRN 3102652*.

Enke, Benjamin. 2020. "Moral Values and Voting." *Journal of Political Economy*, 128(10): 3679–3729.

Espy, M. Watt, and John Orti Smykla. 2016. "Executions in the United States, 1608-2002: The ESPY File." *https://www.icpsr.umich.edu/web/NACJD/studies/8451*.

Fear, Josh. 2007. "Under the Radar: Dog-whistle Politics in Australia." 96, The Australia Institute Discussion Papers.

Feigenberg, Benjamin, and Conrad Miller. 2022. "Would eliminating racial disparities in motor vehicle searches have efficiency costs?" *The Quarterly Journal of Economics*, 137(1): 49–113.

Fouka, Vasiliki, and Hans-Joachim Voth. 2020. "Collective Remembrance and Private Choice: German-Greek Conflict and Consumer Behavior in Times of Crisis." Stanford University Mimeo.

Fryer, Roland. 2019. "An Empirical Analysis of Racial Differences in Police Use of Force." *Journal of Political Economy*, 127(3): 1210–1261.

Gennaro, Gloria, and Elliott Ash. 2021. "Emotion and Reason in Political Language." *The Economic Journal*.

Gentzkow, Matthew. 2016. "Polarization in 2016." Stanford mimeo.

Giffords Law Center. 2020. "In Pursuit of Peace: Building Police-Community Trust to Break the Cycle of Violence." Giffords Law Center to Prevent Gun Violence.

Goncalves, Felipe, and Steven Mello. 2021. "A Few Bad Apples? Racial Bias in Policing." *American Economic Review*, 111(5): 1406–41.

Goodin, Robert E. 2008. *Innovating Democracy: Democratic Theory and Practice after the Deliberative Turn.* Oxford University Press (Reprint ed.).

Greif, Avner, and Steve Tadelis. 2010. "A theory of moral persistence: Crypto-morality and political legitimacy." *Journal of Comparative Economics*, 38(3): 229–244.

Grogger, Jeffrey, and Greg Ridgeway. 2006. "Testing for Racial Profiling in Traffic Stops From Behind a Veil of Darkness." *Journal of the American Statistical Association*, 101(475): 878–887.

Gross, Samuel R, and Katherine Y Barnes. 2002. "Road work: Racial profiling and drug interdiction on the highway." *Michigan Law Review*, 101(3): 651–754.

Haney-Lopez, Ian. 2014. *Dog Whistle Politics: How Coded Racial Appeals Have Reinvented Racism and Wrecked the Middle Class. Oxford University Press.* Oxford University Press.

Hines, Elizabeth, and Eliza Steelwater. 2012. "Project HAL: Historical American Lynching Data Collection Project." *http://people.uncw.edu/hinese/HAL/*.

Hopkins, Daniel G. 2019. "The Activation of Prejudice and Presidential Voting: Panel Evidence from the 2016 U.S. Election." *Political Behavior*.

Hopkins, Daniel J. 2010. "Politicized places: Explaining where and when immigrants provoke local opposition." *American political science review*, 104(1): 40–60.

Horrace, William C., and Shawn M. Rohlin. 2016. "How Dark Is Dark? Bright Lights, Big City, Racial Profiling." *The Review of Economics and Statistics*, 98(2): 226–232.

Hutchings, Vincent L., and Nicholas A. Valentino. 2004. "The centrality of race in American politics." *Annual Review of Political Science*, 7(1): 383–408.

Knowles, John, Nicola Persico, and Petra Todd. 2001. "Racial Bias in Motor Vehicle Searches: Theory and Evidence." *Journal of Political Economy*, 109(1): 203–229.

Lang, Kevin, and Ariella Kahn-Lang Spitzer. 2020. "Race discrimination: An economic perspective." *Journal of Economic Perspectives*, 34(2): 68–89.

Leip, David. 2016. "David Leip's Atlas of U.S. Elections." *http://uselectionatlas.org/*, Accessed: 2020-07-07.

Lenz, Gabriel. 2012. *How Voters Respond to Politicians Policies and Performance.* University of Chicago Press.

Lohrey, Amanda. 2006. *Voting for Jesus: Christianity and Politics in Australia.* Melbourne: Black Inc.

Luh, Elizabeth. 2019. "Not so Black and White: Uncovering Racial Bias from Systematically Misreported Trooper Reports." Houston University mimeo.

Manski, Charles F., and Daniel S. Nagin. 2017. "Assessing benefits, costs, and disparate racial impacts of confrontational proactive policing." *Proceedings of the National Academy of Sciences*, 114(35): 9308–9313.

Masera, Federico, and Michele Rosenberg. 2020. "Shaping Culture, Ideology and Institutions: Economic Incentives and Slavery in the US South." UNSW mimeo.

Masera, Federico, Michele Rosenberg, and Sarah Walker. 2022. "The Power of Narratives: Anti-Black Attitudes and Violence in the US South." *Available at SSRN 4009956.*

McConnell, Brendon, and Imran Rasul. 2021. "Contagious animosity in the field: Evidence from the Federal Criminal Justice System." *Journal of Labor Economics*, 39(3): 739–785.

Mendelberg, Tali. 2001. *The Race Card: Campaign Strategy, Implicit Messages, and the Norm of Equality.* Princeton University Press.

Mendelberg, Tali. 2008. "Racial Priming Revived." *Perspectives on Politics*, 6(1): 109–123.

Müller, Karsten, and Carlo Schwarz. 2019. "From Hashtag to Hate Crime: Twitter and Anti-Minority Sentiment." SSRN Working Papers Working Papers 3149103.

Newman, Benjamin, Jennifer L Merolla, Sono Shah, Danielle Casarez Lemi, Loren Collingwood, and S Karthick Ramakrishnan. 2020. "The Trump Effect: An Experimental Investigation of the Emboldening Effect of Racially Inflammatory Elite Communication." *British Journal of Political Science*, 1–22.

Niven, David. 2021. "Policing Polling Places in the United States: The Negative Effect of Police Presence on African American Turnout in an Alabama Election." *Democracy and Security*, 1–14.

Nunn, Nathan. 2012. "Culture and the Historical Process." *Economic History of Developing Regions*, 27(S1): 108–126.

Nunn, Nathan. 2020. "The historical roots of economic development." *Science*, 367(6485).

Ochsner, Christian, and F Roesel. 2019. "Mobilizing history." CERGE University Mimeo.

Peters, Gerhard, and John T. Woolley. 2020. "The American Presidency Project."

Pierson, Emma, Camelia Simoiu, Jan Overgoor, Sam Corbett-Davies, Daniel Jenson, Amy Shoemaker, Vignesh Ramachandran, Phoebe Barghouty, Cheryl Phillips, Ravi Shroff, et al. 2020. "A large-scale analysis of racial disparities in police stops across the United States." *Nature human behaviour*, 1–10.

Reny, Tyler T, Ali A Valenzuela, and Loren Collingwood. 2020. “”No, you’re playing the race card”: Testing the effects of anti-black, anti-Latino, and anti-immigrant appeals in the post-Obama era.” *Political Psychology*, 41(2): 283–302.

Romarri, Alessio. 2020. “Do far-right mayors increase the probability of hate crimes? Evidence from Italy.” SSRN Working Paper 3506811.

Schaffner, Brian, and Stephen Ansolabehere. 2015. “CCES Common Content, 2014.”

Schutten, Nathaniel M, Justin T Pickett, Alexander L Burton, Cheryl Lero Jonson, Francis T Cullen, and Velmer S Burton Jr. 2021. “Are guns the new dog whistle? Gun control, racial resentment, and vote choice.” *Criminology.*

Sides, J., M. Tesler, and L. Vavreck. 2019. *Identity Crisis: The 2016 Presidential Campaign and the Battle for the Meaning of America.* Princeton University Press.

Smiley, CalvinJohn, and David Fakunle. 2016. “From ”brute” to ”thug:” The demonization and criminalization of unarmed Black male victims in America.” *Journal of human behavior in the social environment*, 26(3-4): 350–366.

Snyder, James M., and Hasin Yousaf. 2020. “, Making Rallies Great Again: The Effects of Presidential Campaign Rallies on Voter Behavior, 2008-2016.” NBER Working paper No. w28043.

Streeter, Shea. 2019. “Lethal force in black and white: Assessing racial disparities in the circumstances of police killings.” *The Journal of Politics*, 81(3): 1124–1132.

Sun, Liyang, and Sarah Abraham. 2021. “Estimating dynamic treatment effects in event studies with heterogeneous treatment effects.” *Journal of Econometrics*, 225(2): 175–199. Themed Issue: Treatment Effect 1.

Tesler, Michael. 2016. *Post-racial or Most-racial?* University of Chicago Press.

Thompson, Andrew Ifedapo, and Ethan C Busby. 2021. “Defending the Dog Whistle: The Role of Justifications in Racial Messaging.” *Political Behavior*, 1–22.

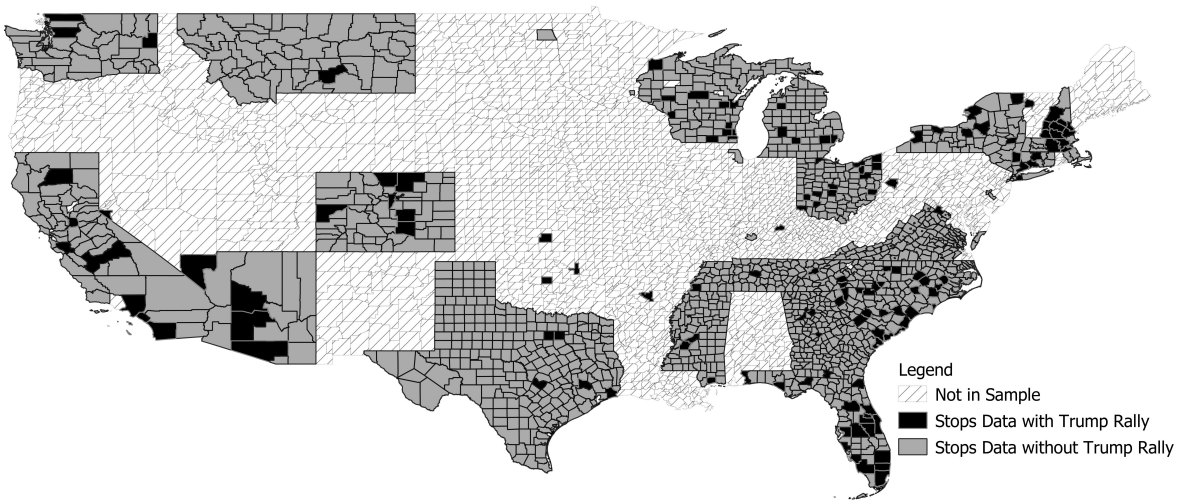
Thompson, Daniel M, et al. 2020. “How partisan is local law enforcement? evidence from sheriff cooperation with immigration authorities.” *American Political Science Review*, 114(1): 222–236.

Valentino, Nicholas A., Fabian G. Neuner, and L. Matthew Vandenbroek. 2018. “The Changing Norms of Racial Political Rhetoric and the End of Racial Priming.” *The Journal of Politics*, 80(3): 757–771.

Valentino, Nicholas A., Vincent L. Hutchings, and Ismail K. White. 2002. "Cues That Matter: How Political Ads Prime Racial Attitudes during Campaigns." *The American Political Science Review*, 96(1): 75–90.

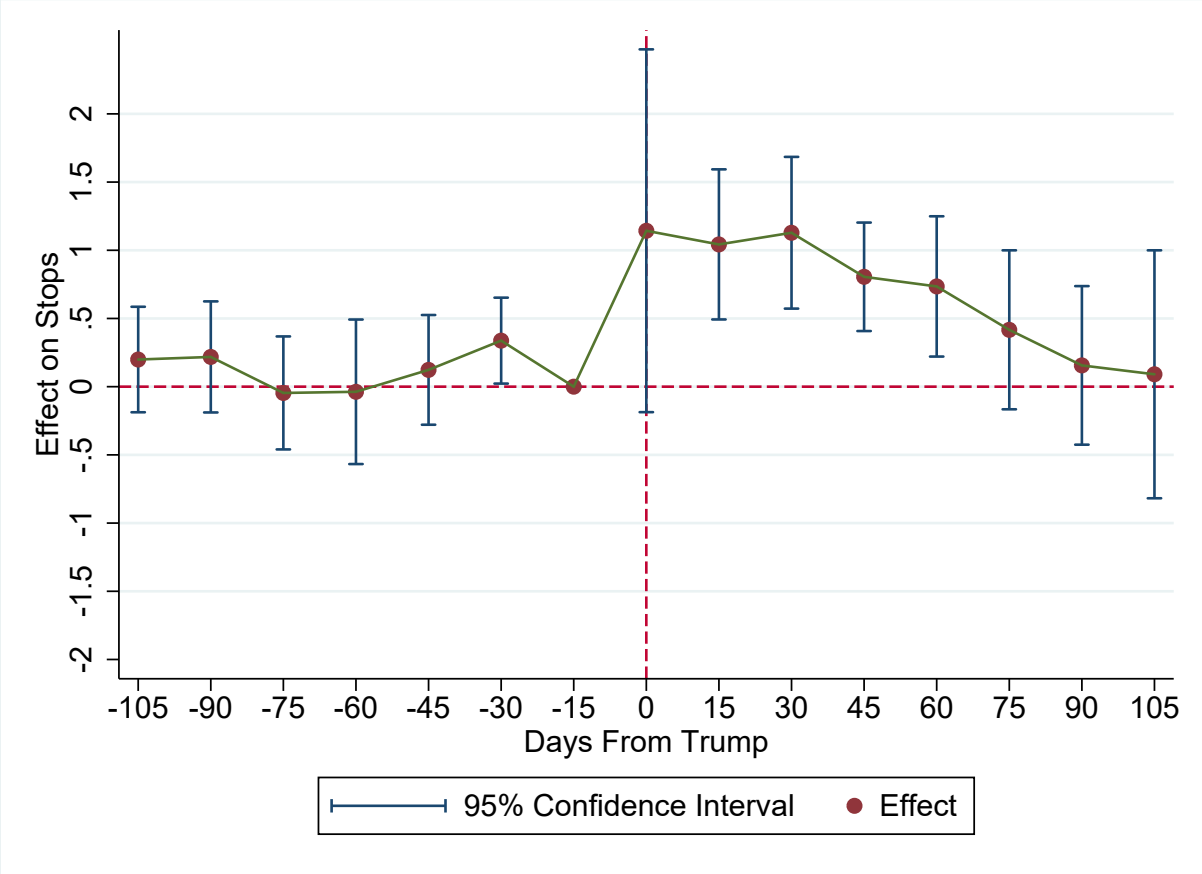
Williams, Jhacova. 2020. "Historical Lynchings and the Contemporary Voting Behavior of Blacks." *American Economic Journal: Applied Economics*, Forthcoming.

Yanagizawa-Drott, David. 2014. " Propaganda and Conflict: Evidence from the Rwandan Genocide." *The Quarterly Journal of Economics*, 129(4): 1947–1994.



Notes: This map shows the $1,474$ counties for which we have police stop data and the 141 counties among those where Trump held at least one rally during the 2015-2016 campaign.

Figure 1: Counties with Campaign Events and Police Stops



Notes: The figure plots OLS coefficients with 95% confidence intervals (vertical lines). The plotted coefficients are the β_τ coefficients associated with each 15 day window, described in Equation 2. Standard errors are corrected for two-way clustering at the county and day level.

Figure 2: Impact of Trump Rallies on the Probability of a Black Stop: Event-study Results

Table 1: Impact of Trump Rallies on the Probability of a Black Stop

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
			100 x $\mathbb{1}$(Black Stop)			
POST-Trump (1-30 days)	1.071***	1.068***	0.798***	0.825***	0.735**	0.990***
	(0.267)	(0.259)	(0.226)	(0.233)	(0.313)	(0.286)
POST-Trump (31-60 days)		0.727***				
		(0.212)				
POST-Trump (61-90 days)		0.300				
		(0.307)				
Observations	34,940,130	34,940,130	34,940,130	34,940,130	30,667,822	11,923,036
R-squared	0.174	0.174	0.173	0.174	0.192	0.096
Sample/Estimation	Baseline	Additional Windows	No Linear Trends	Quadratic Trends	Simple DiD	Counties with Trump Rallies
Mean Dep. Var	18.71	18.71	18.71	18.71	18.41	20.50
SD Dep. Var	39.00	39.00	39.00	39.00	38.76	40.37

Notes: The unit of observation is a police stop. The dependent variable is equal to 100 if the stopped driver is Black and zero otherwise. Column 1 shows the results of OLS estimation of Equation 1 with $k = 30$ and POST-Trump (1-30 days) equal to $D_{c,t}^{(1,30)}$. In Column 2, we augment that regression with two additional regressors: POST-Trump (31-60 days) equal to $D_{c,t}^{(31,60)}$ and POST-Trump (61-90 days) equal to $D_{c,t}^{(61,90)}$. Columns 3 shows the OLS estimation of Equation 1 with $k = 30$ without county-specific linear trends, and Column 4 shows the OLS estimation of Equation 1 with $k = 30$ with county-specific quadratic time trends. In Column 5, we restrict the estimation sample to 30 days around a Trump rally in treated counties and all observations in untreated counties. We estimate a simple difference-in-differences specification in which the estimation equation does not include $D_{c,t}^{(-\infty,-31)}$, $D_{c,t}^{(31,\infty)}$ or county-specific trends. Column 6 shows the OLS estimation of Equation 1 with $k = 30$ in the sample of 141 counties that hosted at least one Trump rally. Standard errors are adjusted for two-way clustering at the county and day level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: Triple Difference Results: Probability and Number of Stops by Race or Ethnicity

VARIABLES	(1) 100 x $\mathbb{1}$(Stop of Each Race)	(2) Number of Stops of Each Race
POST-Trump * Blacks	1.064***	0.056***
β_1	(0.264)	(0.019)
POST-Trump * Hispanics	-0.391	0.010
β_2	(0.447)	(0.019)
POST-Trump * APIs	-0.042	-0.007
β_3	(0.091)	(0.030)
Effect on Blacks Vs Whites	1.064***	0.056***
β_1	(0.264)	(0.019)
Effect on Blacks Vs Hispanics	1.455**	0.047**
$\beta_1 - \beta_2$	(0.577)	(0.022)
Effect on Blacks Vs APIs	1.106***	0.064**
$\beta_1 - \beta_3$	(0.294)	(0.034)
Effect on Hispanics Vs Whites	-0.391	0.010
β_2	(0.447)	(0.019)
Effect on APIs Vs Whites	-0.042	-0.007
β_3	(0.091)	(0.030)
Effect on APIs Vs Hispanics	0.349	-0.017
$\beta_3 - \beta_2$	(0.510)	(0.042)
Observations	2,513,226	2,513,226
R-squared	0.926	0.955
Mean Dep. Var	10.52	1.01
SD Dep. Var	19.09	1.34

Notes: This table shows the estimation results of Equation 3. The dependent variable in Column 1 is the share of stops of Black, Hispanic, and API drivers, with the share of stops of White drivers as the excluded category. The dependent variable in Column 2 is the inverse hyperbolic sine (IHS) of the number of stops of Black, Hispanic, and API drivers, with the IHS of the number of stops of White drivers as the excluded category. POST-Trump is equal to $D_{c,t}^{(1,30)}$. The specification includes day-by-race and county-by-race fixed effects as well as county-by-race linear trends. The specification in Column 2 also controls for the IHS of the total number of stops in a county-day. Observations are weighted by the number of stops. Standard errors are adjusted for two-way clustering at the county and day level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Driver Behavior

Panel A	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Fatal Incidents		Fatality Involving Driver:				
	Total Incidents	Total	Due to Violation	Black	Non-Black	White	Hispanic	Mexican
Post-TRUMP	0.694	-0.057	0.157	0.885	-1.192	-1.420	1.318	1.043
	(1.872)	(1.701)	(0.506)	(0.901)	(1.555)	(1.567)	(1.176)	(1.104)
Observations	60,386	60,386	60,386	60,386	60,386	60,386	60,386	60,386
R-squared	0.389	0.074	0.062	0.115	0.090	0.109	0.169	0.137
Mean Dep. Var	94.45	50.13	1.68	7.14	43.20	31.93	7.90	3.97
SD Dep. Var	20.27	46.08	12.07	24.21	45.86	43.13	25.66	18.53

Panel B	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Stop due to Collision Involving Driver:				Stop due to Radar Triggered Involving Driver:			
	Black	Non-Black	White	Hispanic	Black	Non-Black	White	Hispanic
Post-TRUMP	0.041	0.167	0.088	0.076	0.006	-0.024	-0.020	-0.005
	(0.048)	(0.295)	(0.155)	(0.149)	(0.032)	(0.056)	(0.056)	(0.004)
Observations	12,353,303	12,353,303	12,353,303	12,353,303	12,353,303	12,353,303	12,353,303	12,353,303
R-squared	0.017	0.062	0.031	0.035	0.052	0.084	0.080	0.005
Mean Dep. Var	0.22	1.80	0.93	0.69	0.12	0.21	0.20	0.01
SD Dep. Var	4.67	13.29	9.59	8.27	3.41	4.60	4.48	1.05

Notes: Panel A shows the OLS estimation of Equation 1 with $k = 30$ at the county-day level, where the dependent variable consists of the IHS of the number of: crashes (Column 1), road fatalities (Column 2), fatalities due to a driving violation (Column 3) and, in turn, fatalities of Black, non-Black, White, Hispanic, or Mexican drivers (Columns 4 to 8). Observations are weighted by the number of incidents. Data is from the Fatality Analysis Reporting System (FARS) for 2015-2017. The data includes information on 67,796 traffic crashes, including 34,980 fatalities in 60,386 county-days.
Panel B shows the OLS estimation of Equation 1 with $k = 30$ at the stop level, where the dependent variable takes value 100 if the stop is due to a collision and involves a Black driver (Column 1), a non-Black driver (Column 2), a White driver (Column 3), a Hispanic driver (Column 4). Columns 5 to 8 show the results of similar specifications in which the dependent variable instead consists of stops due to triggering a speed radar.
POST-Trump is equal to $D_{c,t}^{(1,30)}$. Standard errors are two-way clustered at the county and at the day level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Police Behavior

VARIABLES	(1)	(2)	(3)	(4)
		100 x $\mathbb{1}$(Black Stop)		
POST-Trump	0.947*	1.010**	0.522***	0.478***
β_1	(0.522)	(0.395)	(0.173)	(0.142)
POST-Trump * Police Dept	-0.215			
β_2	(1.418)			
Effect on Stops by Police Dept	0.731			
$\beta_1 + \beta_2$	(1.034)			
Observations	34,940,130	24,465,105	34,940,130	18,098,852
R-squared	0.124	0.193	0.179	0.172
Additional FE/ Sample	State PD	Hour of Stop FE	Agency FE	Officer FE
Mean Dep. Var	13.82	19.43	18.71	19.41
SD Dep. Var	34.51	39.55	39.00	39.55

Notes: The unit of observation is a police stop. The dependent variable is equal to 100 if the stopped driver is Black and zero otherwise. The table shows the results of OLS estimation of Equation 1 with $k = 30$ and POST-Trump equal to $D_{c,t}^{(1,30)}$. We augment this specification to include an indicator variable for whether the stop is made by a police department (rather than a state trooper) as well as its interaction with POST-Trump in Column 1. In Columns 2 to 4, alternatively we include: hour of the day fixed effects, agency fixed effects, and individual officer fixed effects, respectively. Standard errors are two-way clustered at the county and at the day level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Role of Estimated Officer Bias in the Effect of Trump Rallies on the Probability of a Black Stop

VARIABLES	(1)	(2)	(3)	(4)
	100 x $\mathbb{1}$(Black Stop)			
POST-Trump	0.304*	0.401***	0.302*	0.398***
	(0.167)	(0.133)	(0.167)	(0.132)
POST-Trump * Estimated Bias	0.049***	0.043***	0.049***	0.038***
	(0.018)	(0.009)	(0.005)	(0.005)
Observations	20,999,340	20,999,340	20,999,340	20,999,340
R-squared	0.133	0.164	0.133	0.164
Officer FE	NO	YES	NO	YES
Bias Measure	Method 1	Method 1	Method 2	Method 2
Mean Dep. Var	16.41	16.41	16.41	16.41
SD Dep. Var	37.03	37.03	37.03	37.03

Notes: The unit of observation is a police stop. The dependent variable is equal to 100 if the stopped driver is Black and zero otherwise. The table shows the results of OLS estimation of Equation 1 with $k = 30$ and POST-Trump equal to $D_{c,t}^{(1,30)}$, augmented with officer's estimated racial bias against Black drivers and its interaction with POST-Trump. The two alternative measures of bias are based on: officer using warnings vs. other outcomes (Method 1), and officer using arrests vs. other outcomes (Method 2). Both measures are standardized with mean zero and standard deviation of one. Columns 2 and 4 include, additionally, individual officer fixed effects. Standard errors are two-way clustered at the county and at the day level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Role of Local Characteristics in the Effect of Trump Rallies on the Probability of a Black Stop

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
			100 x 𝟙(Black Stop)			
POST-Trump	0.921***	0.877***	0.599***	1.010***	0.783***	0.695***
	(0.254)	(0.251)	(0.206)	(0.281)	(0.224)	(0.157)
POST-Trump * X	0.737**	0.591***	1.307*	0.699**	0.272**	0.545*
	(0.328)	(0.223)	(0.716)	(0.321)	(0.133)	(0.326)
Observations	33,726,046	33,726,046	34,940,130	34,939,551	34,940,130	34,940,130
R-squared	0.171	0.171	0.173	0.173	0.173	0.173
X=	Racial Resentment A	Racial Resentment B	Any Slaves	Cotton	Lynchings	Executions
Mean Dep. Var	18.92	18.92	18.71	18.71	18.71	18.71
SD Dep. Var	39.17	39.17	39.00	39.00	39.00	39.00

Panel B	(1)	(2)	(3)	(4)	(5)	(6)
			100 x 𝟙(Black Stop)			
POST-Trump	0.827***	1.358	0.842***	0.710***	0.769***	0.801***
	(0.227)	(0.971)	(0.233)	(0.207)	(0.216)	(0.220)
POST-Trump * X	0.098	-0.950	-0.263	0.156	0.039	-0.070
	(0.234)	(1.002)	(0.196)	(0.297)	(0.231)	(0.191)
Observations	34,940,130	13,087,291	34,939,910	34,940,130	34,813,677	34,813,677
R-squared	0.173	0.202	0.173	0.173	0.173	0.173
X=	DEM Share 2012	Sheriff REP	Income	College	China Shock	China Shock IV
Mean Dep. Var	18.71	24.02	18.71	18.71	18.72	18.72
SD Dep. Var	39.00	42.72	39.00	39.00	39.01	39.01

Notes: The unit of observation is a police stop. The dependent variable is equal to 100 if the stopped driver is Black and zero otherwise. The table shows the results of OLS estimation of Equation 1 with $k = 30$ and POST-Trump equal to $D_{c,t}^{(1,30)}$, augmented with an interaction term between POST-Trump and the following predetermined county characteristics: two measures of racial resentment from the 2012 and 2014 CCES (Schaffner and Ansolabehere, 2015), presence of slaves in 1860, soil suitability for growing cotton, IHS of the number of lynchings of Black people, or of executions of Black people during the Jim Crow era (Panel A); vote share of Democratic presidential candidate (Barrack Obama) in the 2012 presidential election, dummy variable equal to one if the county sheriff is Republican, median household income, share of college graduates, the China import competition shock and its instrument from Autor, Dorn and Hanson (2013) (Panel B). All continuous variables measuring county characteristics are normalized to have a mean of zero and standard deviation of one by subtracting the value of each county from the mean value and dividing the result by the standard deviation. Instead of county-specific linear trends, we include linear trends based on the pre-determined county characteristic. Standard errors are two-way clustered at the county and at the day level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

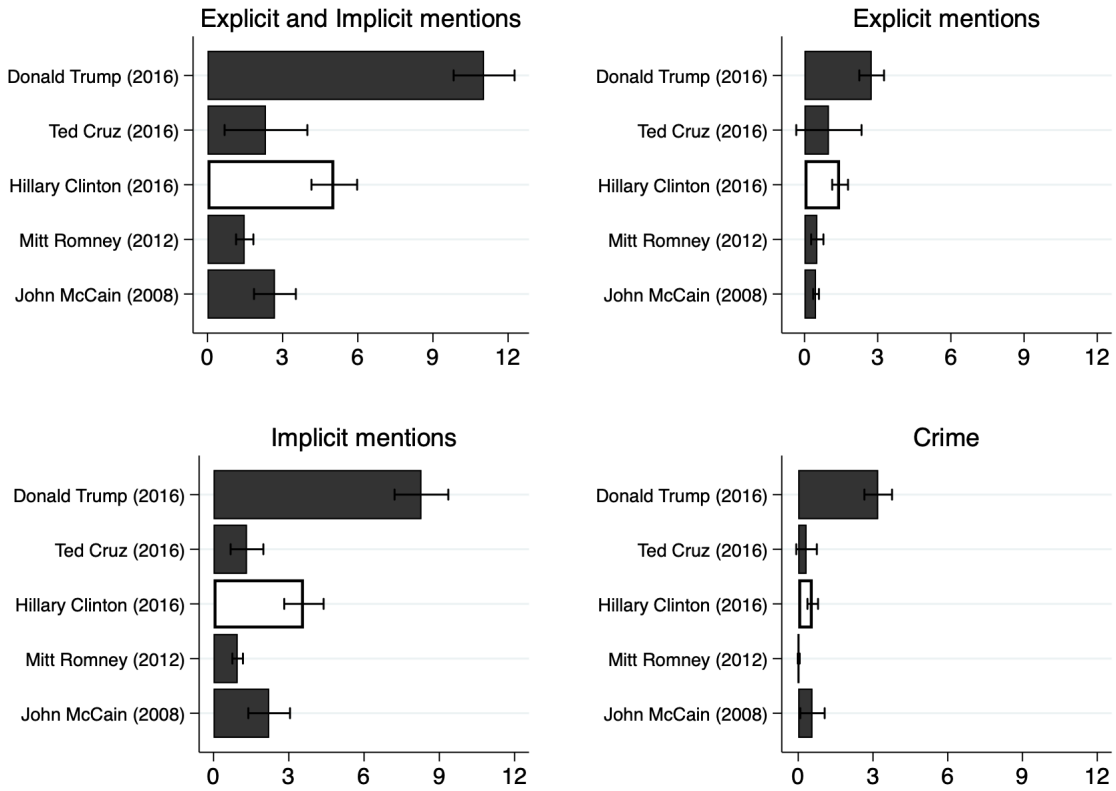
Table 7: The Effect of Trump's Rally Speeches on Police Stops

Panel A: Officer Bias 1	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				100 x 𝟙(Black Stop)				
POST-Trump (β_1)	0.323	0.308	0.323	0.224	0.289	0.314	0.316	0.306
	(0.288)	(0.277)	(0.296)	(0.279)	(0.230)	(0.235)	(0.267)	(0.274)
POST-Trump * Officer Bias (β_2)	0.045**	0.096***	0.041**	0.049**	0.051***	0.068***	0.035**	0.047***
	(0.018)	(0.019)	(0.018)	(0.019)	(0.018)	(0.017)	(0.018)	(0.018)
POST-Trump * Words (β_3)	-0.035	-0.078	-0.024	0.375	-0.097	0.001	-0.031	-0.201
	(0.101)	(0.216)	(0.103)	(0.236)	(0.354)	(0.369)	(0.243)	(0.289)
POST-Trump * Words * Officer Bias (β_4)	0.013***	0.100***	0.012***	0.009	0.004	0.094**	-0.107***	-0.062***
	(0.002)	(0.019)	(0.002)	(0.008)	(0.008)	(0.048)	(0.019)	(0.006)
Observations	14,747,451	14,747,451	14,747,451	14,747,451	14,747,451	14,747,451	14,747,451	14,747,451
R-squared	0.143	0.143	0.143	0.143	0.143	0.143	0.143	0.143

Panel B: Officer Bias 2	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				100 x 𝟙(Black Stop)				
POST-Trump (β_1)	0.325	0.310	0.324	0.227	0.293	0.316	0.319	0.309
	(0.288)	(0.277)	(0.296)	(0.279)	(0.230)	(0.235)	(0.266)	(0.274)
POST-Trump * Officer Bias (β_2)	0.050***	0.114***	0.046***	0.055***	0.060***	0.075***	0.044***	0.056***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.007)	(0.007)	(0.008)	(0.008)
POST-Trump * Words (β_3)	-0.031	-0.077	-0.021	0.373	-0.089	-0.002	-0.028	-0.198
	(0.102)	(0.216)	(0.104)	(0.236)	(0.356)	(0.370)	(0.241)	(0.290)
POST-Trump * Words * Officer Bias (β_4)	0.016***	0.125***	0.014***	0.001	0.019**	0.089***	-0.100***	-0.054***
	(0.002)	(0.007)	(0.002)	(0.006)	(0.009)	(0.009)	(0.008)	(0.007)
Observations	14,747,451	14,747,451	14,747,451	14,747,451	14,747,451	14,747,451	14,747,451	14,747,451
R-squared	0.143	0.143	0.143	0.143	0.143	0.143	0.143	0.143
Words	EXPL &IMPL	EXPL	IMPL	TRADE	CLINTON	TERROR	JOB	CORRUPTION
Mean Dep. Var	18.52	18.52	18.52	18.52	18.52	18.52	18.52	18.52
SD Dep. Var	38.84	38.84	38.84	38.84	38.84	38.84	38.84	38.84

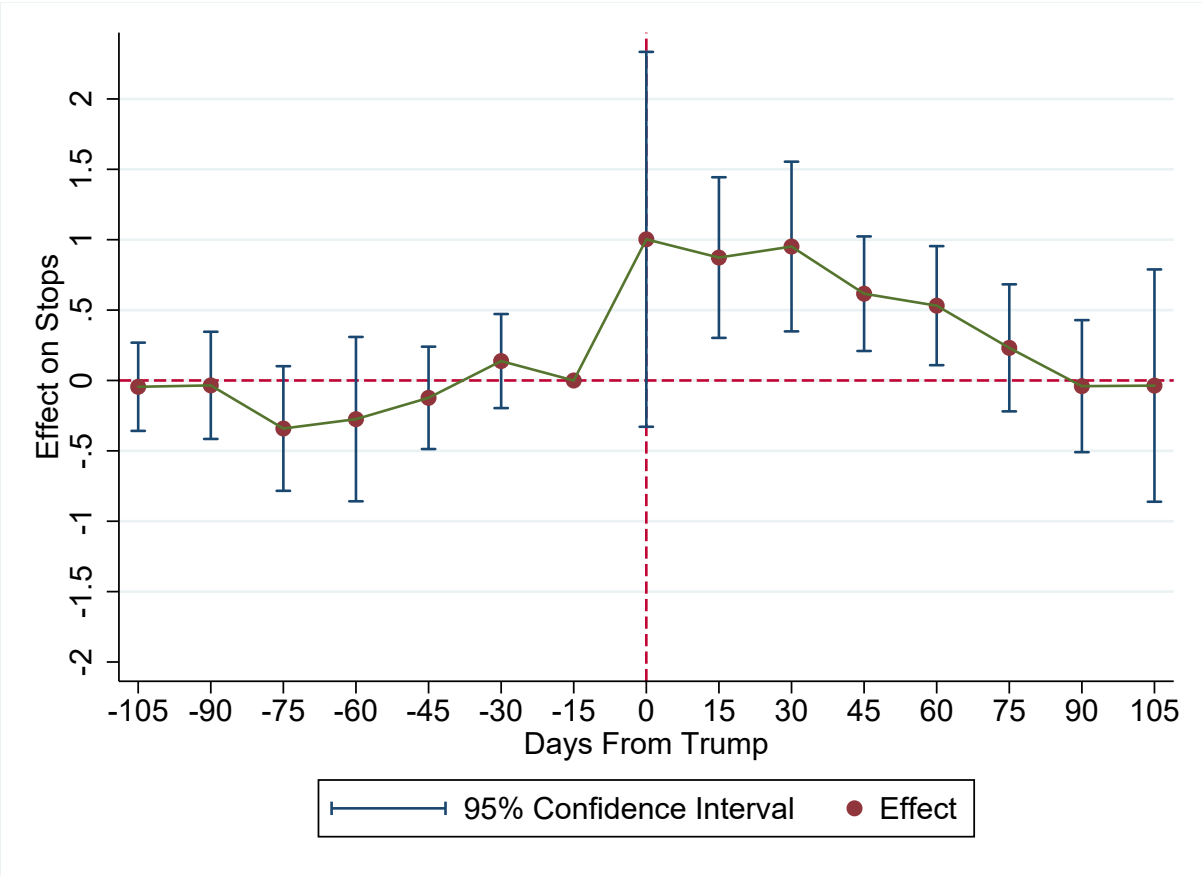
Notes: The Table shows the results of OLS estimation of Equation 4. The two alternative measures of officer bias are based on: officer using warnings vs. other outcomes (Panel A), and officer using arrests vs. other outcomes (Panel B). Both measures are standardized with mean zero and standard deviation of one. Stemmed words that constitute the "EXPL" (Explicit) category: African, Black, race, racial, racist. Stemmed words that constitute the "IMPL" (Implicit) category: drug, crime, crimin, rape, gun, prison, riot, thug, urban. In Col. 1, $Words_{i,c,t}$ is the count of the words in the Explicit or Implicit category. In Col. 2 (resp. 3), $Words_{i,c,t}$ is the count of the words in the Explicit (resp. Implicit) category. In the remaining Columns, $Words_{i,c,t}$ is the count of the following stemmed words: China, trade, NAFTA (Col. 4), Hilary, Clinton, email, lock (Col. 5), ISIS, Syria, Iraq, terrorist, Afghanistan, Islam (Col. 6), busi, job, manufactur, tax (Col. 7), and rig, media, CNN, Washington, corrupt, swamp (Col. 8). All word counts are standardized to have mean zero and standard deviation of one. Standard errors are two-way clustered at the county and at the day level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Online Appendix
(NOT FOR PUBLICATION)



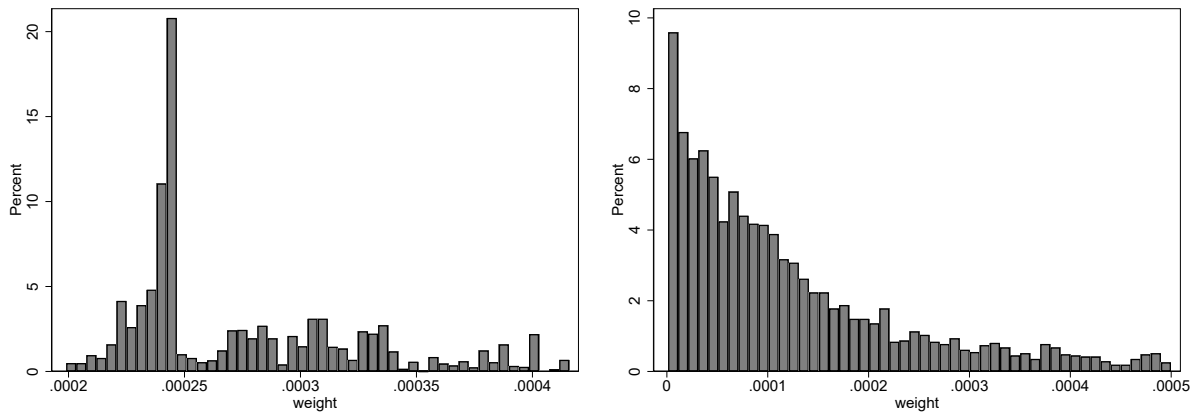
Notes: The graph displays the expected count and 95% confidence interval of stemmed words in each candidate's speeches. Stemmed words that constitute the "Explicit" category are: African, Black, race, racial, racist. Stemmed words that constitute the "Implicit" category are: drug, crime, crimin, rape, gun, prison, riot, thug, urban. Stemmed words that constitute the "Crime" category are: crime, crimin, rape. Data is from the text of campaign speeches obtained from Enke (2020) and The American Presidency Project (accessed 10 June 2020) (Peters and Woolley, 2020). For Trump speeches, we complement this data with data from the Trump Campaign Corpus Project (accessed 16 February 2021).

Figure A1: Relative Frequency of Words in Candidates' Speeches



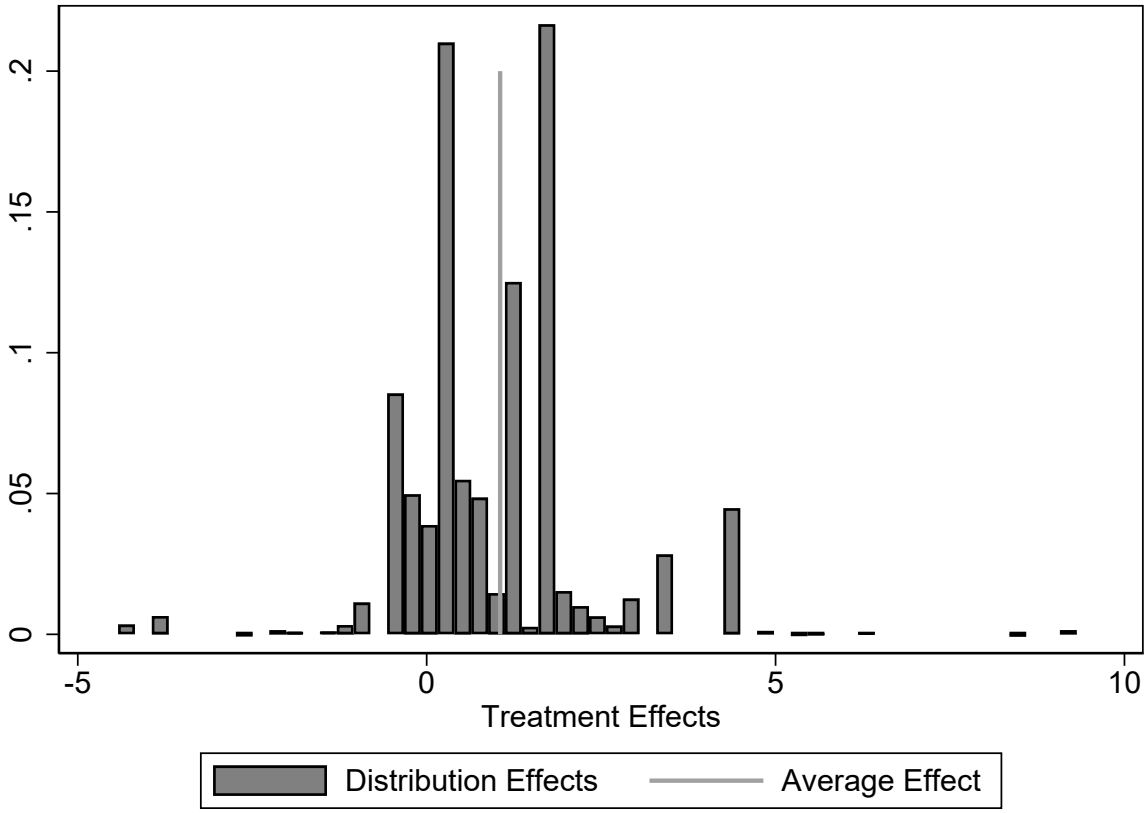
Notes: The figure plots OLS coefficients with 95% confidence intervals (vertical lines). The plotted coefficients are the β_τ coefficients associated with each 15 day window, described in Equation 2, but without county-specific linear trends. Standard errors are corrected for two-way clustering at the county and day level.

Figure A2: Impact of Trump Rallies on the Probability of a Black Stop: Event-study Results Without County-specific Linear Trends



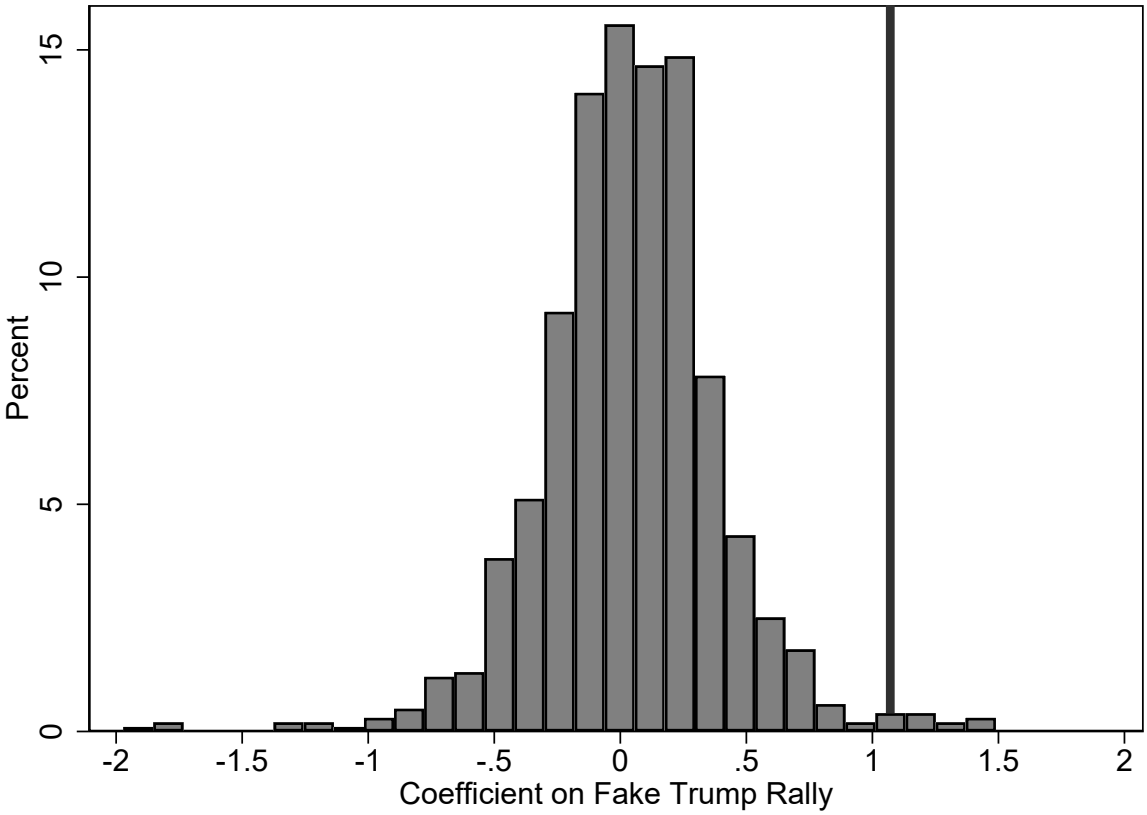
Notes: As shown by de Chaisemartin and D'Haultfoeuille (2020) the Difference-in-Differences estimator can be expressed as a weighted average of each individual treatment effect. Panel A shows the distribution of those weights associated with each county, and Panel B shows the distribution of those weights associated with each police stop.

Figure A3: Distribution of Weights of the Difference-in-Differences Estimator



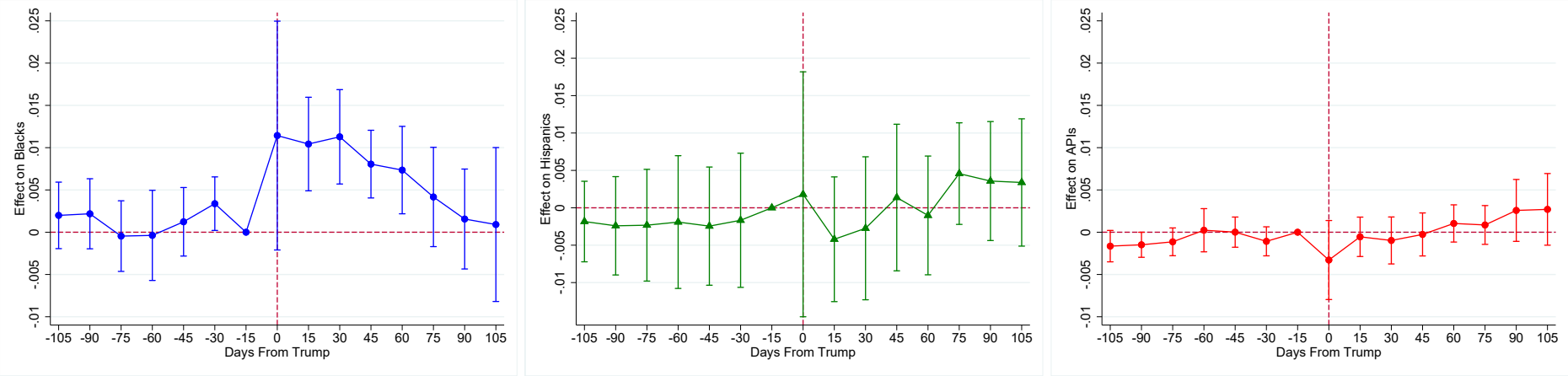
Notes: This figure shows the distribution of the estimated Difference-in-Differences estimator for each county. Each estimated effect is weighted by the number of stops in that county. The vertical bar is the average of these estimated coefficients.

Figure A4: Distribution of the Difference-in-Differences Effects



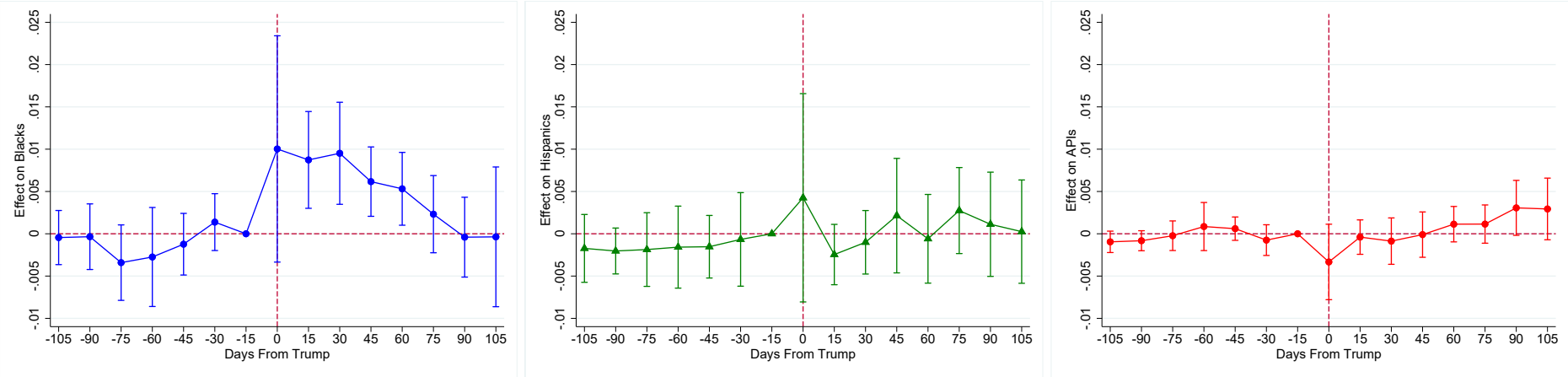
Notes: This figure shows the distribution of β from Equation 1 where instead of using the real distribution of Trump rallies, we randomly reallocate the same number of rallies during the campaign across county-days. The results of this permutation inference with placebo treatments are based on $1,000$ replications. The vertical bar indicates the coefficient obtained from the actual distribution of Trump rallies.

Figure A5: Permutation Inference



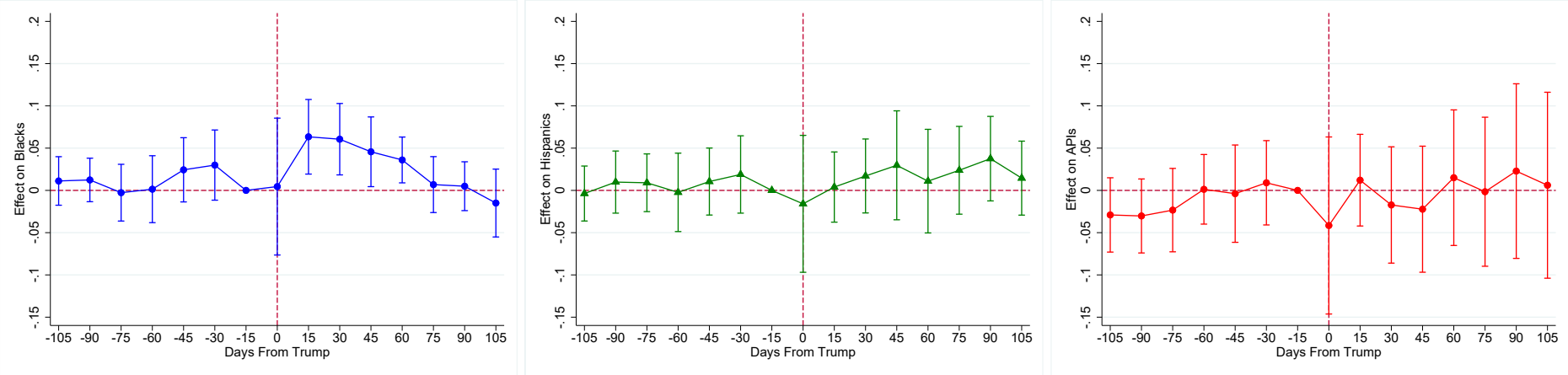
Notes: The figure plots the estimation results of the event-study counterpart of the triple difference specification corresponding to Equation 3 and displayed in Column 1 of Table 2. Panel A shows the dynamic of the effect of a Trump rally on the probability of a Black vs. White stop. Panel B shows the dynamic of the effect of a Trump rally on the probability of a Hispanic vs. White stop. Panel C shows the dynamic of the effect of a Trump rally on the probability of an API vs. White stop. The figure plots marginal effects with 95% confidence intervals (vertical lines). Standard errors are corrected for two-way clustering at the county and day level.

Figure A6: Impact of Trump Rallies on the Relative Probability of Stop With Respect to Whites: Event-study Results



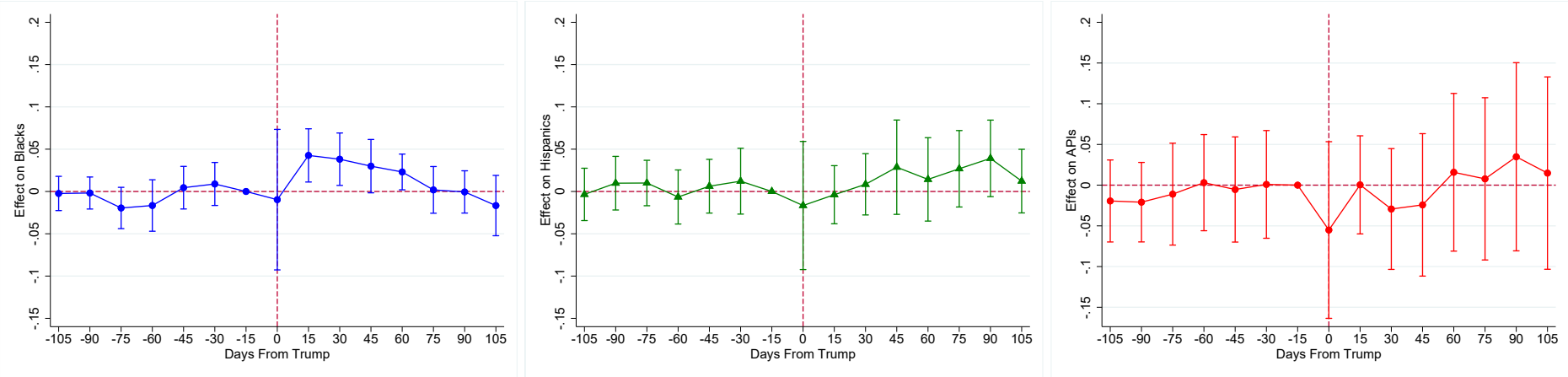
Notes: The figure plots the estimation results of the event-study counterpart of the triple difference specification corresponding to Equation 3 and displayed in Column 1 of Table 2, but without county-specific linear trends. Panel A shows the dynamic of the effect of a Trump rally on the probability of a Black vs. White stop. Panel B shows the dynamic of the effect of a Trump rally on the probability of a Hispanic vs. White stop. Panel C shows the dynamic of the effect of a Trump rally on the probability of an API vs. White stop. The figure plots marginal effects with 95% confidence intervals (vertical lines). Standard errors are corrected for two-way clustering at the county and day level.

Figure A7: Impact of Trump Rallies on the Relative Probability of Stop With Respect to Whites: Event-study Results Without County-specific Linear Trends



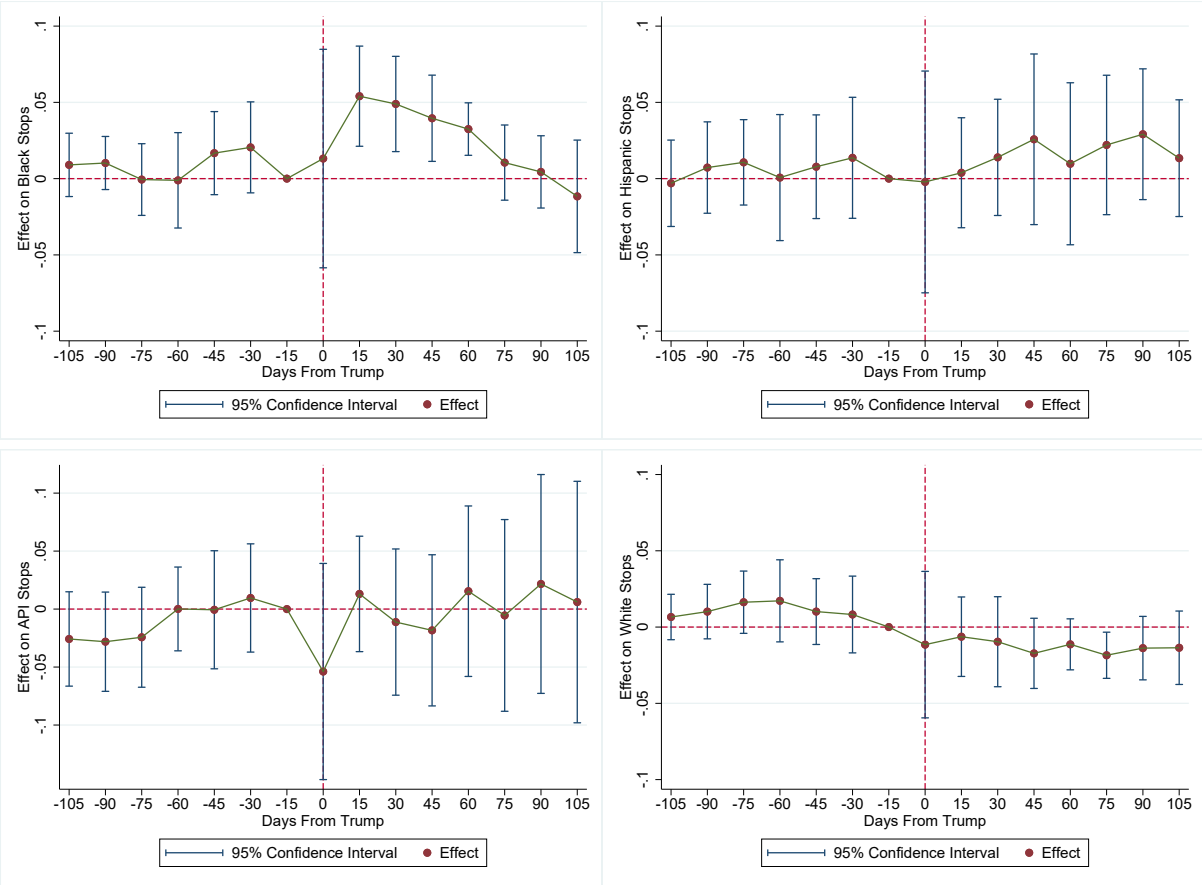
Notes: The figure plots the estimation results of the event-study counterpart of the triple difference specification corresponding to Equation 3 and displayed in Column 2 of Table 2. Panel A shows the dynamic of the effect of a Trump rally on the inverse hyperbolic sine (IHS) of the number of Black vs. White stops. Panel B shows the dynamic of the effect of a Trump rally on the IHS of the number of Hispanic vs. White stops. Panel C shows the dynamic of the effect of a Trump rally on the IHS of the number of API vs. White stops. The figure plots marginal effects with 95% confidence intervals (vertical lines). Standard errors are corrected for two-way clustering at the county and day level.

Figure A8: Impact of Trump Rallies on the Number of Stops With Respect to Whites: Event-study Results



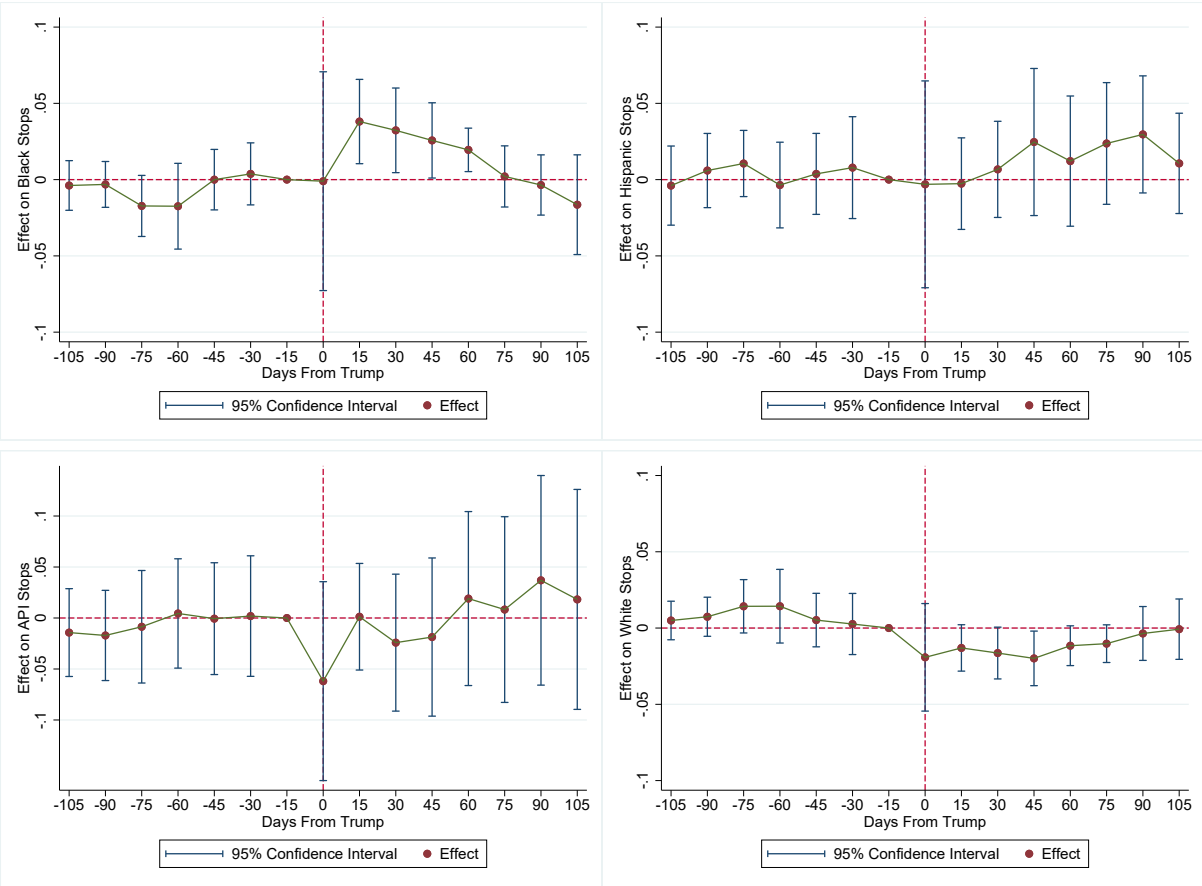
Notes: The figure plots the estimation results of the event-study counterpart of the triple difference specification corresponding to Equation 3 and displayed in Column 1 of Table 2, but without county-specific linear trends. Panel A shows the dynamic of the effect of a Trump rally on the IHS of the number of Black vs. White stops. Panel B shows the dynamic of the effect of a Trump rally on the IHS of the number of Hispanic vs. White stops. Panel C shows the dynamic of the effect of a Trump rally on the IHS of the number of API vs. White stops. The figure plots marginal effects with 95% confidence intervals (vertical lines). Standard errors are corrected for two-way clustering at the county and day level.

Figure A9: Impact of Trump Rallies on the Number of Stops With Respect to Whites: Event-study Results Without County-specific Linear Trends



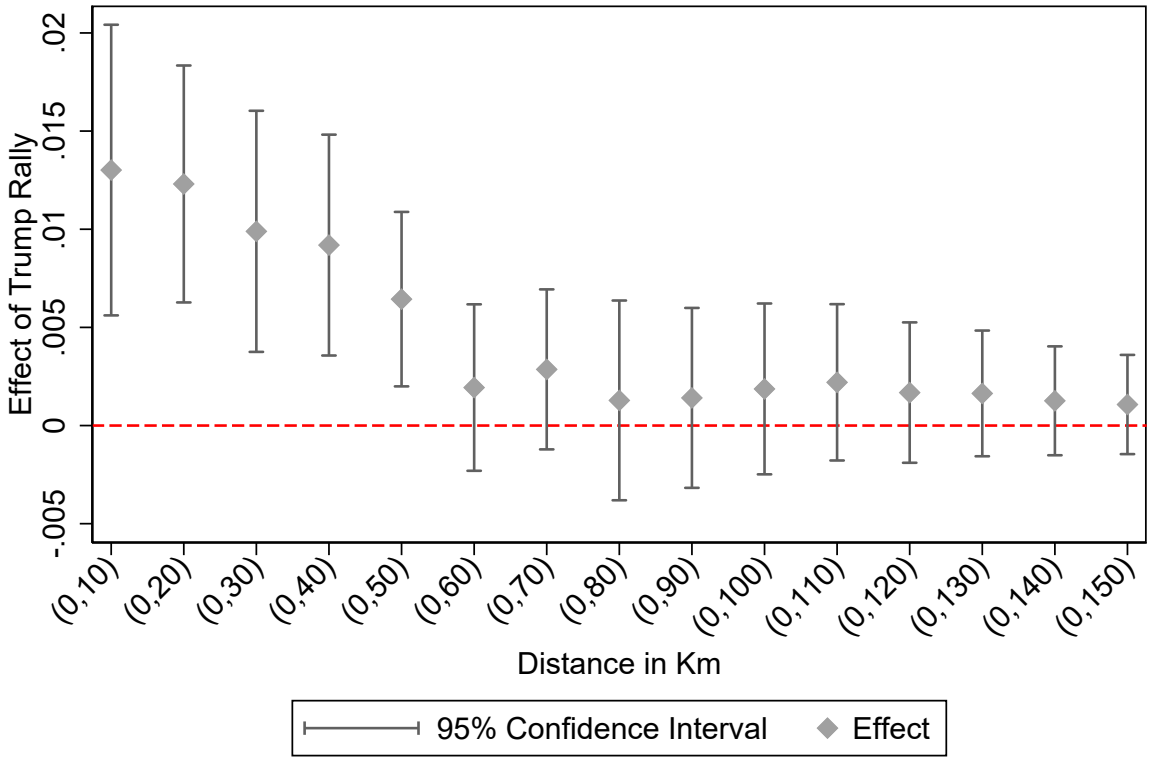
Notes: The figure plots the estimation results of the event-study counterpart of estimation of Equation 1 at the county-day level with the IHS transformation of the number of stops of each group or ethnicity as the dependent variable. Panel A shows the dynamic of the effect of a Trump rally on the IHS of the number of Black stops. Panel B shows the dynamic of the effect of a Trump rally on the IHS of the number of Hispanic stops. Panel C shows the dynamic of the effect of a Trump rally on the IHS of the number of API stops. Panel D shows the dynamic of the effect of a Trump rally on the IHS of the number of White stops. The figure plots marginal effects with 95% confidence intervals (vertical lines). Standard errors are corrected for two-way clustering at the county and day level.

Figure A10: Impact of Trump Rallies on the Number of Stops: Event-study Results



Notes: The figure plots the estimation results of the event-study counterpart of estimation of Equation 1 at the county-day level with the IHS transformation of the number of stops of each group or ethnicity as the dependent variable, without county-specific linear trends. Panel A shows the dynamic of the effect of a Trump rally on the IHS of the number of Black stops. Panel B shows the dynamic of the effect of a Trump rally on the IHS of the number of Hispanic stops. Panel C shows the dynamic of the effect of a Trump rally on the IHS of the number of API stops. Panel D shows the dynamic of the effect of a Trump rally on the IHS of the number of White stops. The figure plots marginal effects with 95% confidence intervals (vertical lines). Standard errors are corrected for two-way clustering at the county and day level.

Figure A11: Impact of Trump Rallies on the Number of Stops: Event-study Results Without County-specific Linear Trends



Notes: The figure plots the estimation results of various specifications similar to Equation 1 where a police stop is considered to be treated in the 30 days after a rally if the physical distance to the rally is less than l. For stops by state troopers, physical distance is measured as the distance between the rally location and the centroid of the county where the stop happens. For stops by a city police department, physical distance is measured as the distance between the rally location and the centroid of the city. Each coefficient (and associated 95% confidence interval) is from a different regression in which $l = 10, 20, ..., 150$ km. Standard errors are corrected for two-way clustering at the county and day level.

Figure A12: Geographic Spillovers

Table A1: Summary Statistics

Variable	N	Mean	SD	Min	Max
POST-Trump	34,940,130	0.02	0.13	0	1
100 x $\mathbb{1}$(Black Stop)	34,940,130	18.71	39.00	0	100
100 x $\mathbb{1}$(Hispanic Stop)	34,940,130	22.42	41.71	0	100
100 x $\mathbb{1}$(Asian or Pacific Islander (API) Stop)	34,940,130	3.40	18.12	0	100
100 x $\mathbb{1}$(White Stop)	34,940,130	55.47	49.70	0	100

Notes: The Table shows some summary statistics. The data is constructed from Stanford Open Policing Project (last accessed 30 July 2021) and POST-Trump is constructed from Appleman (2019). The unit of observation is a police stop. POST-Trump is equal to one in the 30 days window after a Trump rally and zero otherwise ($D_{c,t}^{(1,30)}$). 100 x $\mathbb{1}$(X Stop) is equal to one hundred if the stopped driver is of race X and zero otherwise, where X={Black, Hispanic, API, White}.

Table A2: Differences in the Probability of a Black Stop Before Treatment

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES			100 x $\mathbb{1}$(Black Stop)			
PRE-Trump	0.404	0.640	0.508	0.293	0.153	0.126
	(2.857)	(2.809)	(2.804)	(2.818)	(2.811)	(2.815)
Observations	10,937,832	15,094,964	17,162,490	18,512,490	19,339,821	19,615,099
R-squared	0.001	0.001	0.001	0.001	0.001	0.001
Window	5 days	10 days	15 days	20 days	25 days	30 days
Mean Dep. Var	18.53	18.51	18.55	18.58	18.61	18.60
SD Dep. Var	38.85	38.84	38.87	38.90	38.92	38.91

Notes: This table shows OLS regression results where the dependent variable is equal to 100 if the stopped driver is Black and zero otherwise. The estimating sample consists of all the stops that happen during the 5, 10, 15, 20, 25, or 30 calendar days (as indicated) before a Trump rally. PRE-Trump is a dummy variable that takes value one in the 5, 10, 15, 20, 25, or 30 days before a rally and value zero in the same calendar days in other counties where these intervals of days are not followed by a rally. That is, PRE-Trump is equal to $D_{c,t}^{(-k,-1)}$ for $k = 5, 10, 30$. All estimations include day fixed effects. Standard errors are two-way clustered at the county and day level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Impact of Trump Rallies on the Probability of a Black Stop: Alternative Methods to Deal with Multiple Rallies

VARIABLES	(1)	(2)
	100 x $\mathbb{1}$(Black Stop)	
POST-Trump	1.171***	1.296*
	(0.296)	(0.710)
Observations	34,940,130	44,364,243
R-squared	0.174	0.174
Sample	Sum Event	Event Panel
Mean Dep. Var	18.71	19.34
SD Dep. Var	39.00	39.50

Notes: The unit of observation is a police stop. The dependent variable is equal to 100 if the driver who is stopped is Black and zero otherwise. Column 1 shows the results of OLS estimation of Equation 1 with $k = 30$ where POST-Trump is equal to $Sum(\mathbb{1}(1 \leq d_{c,t,e} \leq 30)_{e=1,\ldots,N_c})$. Column 2 shows the results of OLS estimation of Equation 1 with $k = 30$ for which we duplicate observations within a county as many times as there are rallies, so that each event gets a full panel. In Column 2, observations are weighted by the inverse of the number of duplicates. Standard errors are two-way clustered at the county and day level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Impact of Trump Rallies on the Relative Probability that a Stopped Driver is of a Race or Ethnicity Relative to Another: Split Samples

VARIABLES	(1) Blacks V Whites	(2) Blacks V Hispanics	(3) Blacks V APIs	(4) Hispanics V Whites	(5) Hispanics V APIs	(6) APIs V Whites
POST-Trump	1.182***	1.008***	0.840**	0.226	0.655	-0.075
	(0.310)	(0.341)	(0.392)	(0.248)	(0.951)	(0.126)
Observations	25,919,098	14,369,988	7,724,282	27,215,848	9,021,032	20,570,142
R-squared	0.872	0.915	0.740	0.905	0.613	0.762
Mean Dep. Var	25.22	45.49	84.62	28.78	86.83	5.77
SD Dep. Var	20.86	32.66	19.55	26.11	16.25	7.40

Notes: The unit of observation is a police stop. The table shows the results of OLS estimation of Equation 1 with $k = 30$ and POST-Trump (1-30 days) equal to $D_{c,t}^{(1,30)}$. In Column 1, the estimation sample is restricted to traffic stops of Black or White drivers. The dependent variable is equal to 100 if the stopped driver is Black and zero otherwise. In Column 2, the estimation sample is restricted to traffic stops of Black or Hispanic drivers. The dependent variable is equal to 100 if the stopped driver is Black and zero otherwise. In Column 3, the estimation sample is restricted to traffic stops of Black or API drivers. The dependent variable is equal to 100 if the stopped driver is Black and zero otherwise. In Column 4, the estimation sample is restricted to traffic stops of Hispanic or White drivers. The dependent variable is equal to 100 if the stopped driver is Hispanic and zero otherwise. In Column 5, the estimation sample is restricted to traffic stops of Hispanic or API drivers. The dependent variable is equal to 100 if the stopped driver is Hispanic and zero otherwise. In Column 6, the estimation sample is restricted to traffic stops of API or White drivers. The dependent variable is equal to 100 if the stopped driver is API and zero otherwise. Standard errors are adjusted for two-way clustering at the county and day level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Impact of Trump Rallies on the Number of Stops by Race or Ethnicity

	(1)	(2)	(3)	(4)
	Number of Stops			
VARIABLES	Black	Hispanic	White	API
POST-Trump	0.054***	0.008	-0.005	-0.003
	(0.015)	(0.018)	(0.015)	(0.031)
Observations	837,378	837,378	837,378	837,378
R-squared	0.959	0.962	0.985	0.921
Mean Dep. Var	1.30	1.30	2.88	0.44
SD Dep. Var	1.38	1.48	1.40	0.88

Notes: This table shows the OLS estimation of Equation 1 with $k = 30$ at the county-day level. Observations are weighted by the number of stops. The dependent variables are the IHS transformation of the number of stops of drivers of a specific race or ethnicity, as indicated. All specifications control for the IHS of the total number of stops in a county-day. Standard errors are adjusted for two-way clustering at the county and day level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: The Differential Effect of Trump and Other Political Rallies on the Probability of a Black Stop

VARIABLES	(1)	(2)
	100 x $\mathbb{1}$(Black Stop)	
POST-Any Rally	0.017	
	(0.285)	
POST-Trump	1.009***	
	(0.423)	
POST-Obama		-0.505
		(0.700)
Observations	34,940,130	23,756,789
R-squared	0.174	0.154
Mean Dep. Var	18.71	18.82
SD Dep. Var	39.00	39.09
Trump Rally Effect	1.026***	
	(0.269)	

Notes: The unit of observation is a police stop. The dependent variable is equal to 100 if the stopped driver is Black and zero otherwise. Column 1 shows the results of OLS estimation of Equation 1 with $k = 30$ and POST-Trump equal to $D_{c,t}^{(1,30)}$, augmented with additional windows specified in the same way as the windows defined for Trump rallies but for any rally by Trump, Clinton, or Cruz (POST-Any Rally). Column 2 shows shows the results of OLS estimation of Equation 1 with $k = 30$ and POST-Obama equal to $D_{c,t}^{(1,30)}$ defined for Obama rallies during the 2011-2012 nomination and presidential election campaign, and police stops for 2011-2012. Standard errors are two-way clustered at the county and day level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Impact of Trump Rallies on BLM Protests

VARIABLES	(1) 100 * 𝟙(BLM Protest)
POST-Trump	-0.247
	(0.463)
Observations	34,940,130
R-squared	0.092
Mean Dep. Var	0.70
SD Dep. Var	8.31

Notes: Column 1 shows the OLS estimation of Equation 1 with $k = 30$ at the county-day level, where the dependent variable is an indicator variable equal to if there is a Black Lives Matter protest and zero otherwise. All estimates are weighted by total number of stops. Standard errors are two-way clustered at the county and day level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data on Black Lives Matter events over the sample period is from Elephrame (accessed on March 21, 2021).

Table A8: Social Spillover Effects of Trump Rallies

VARIABLES	(1) 100 x $\mathbb{1}$(Black Stop)
POST-Trump	1.058***
	(0.266)
Social spillover index	0.055***
	(0.020)
Observations	34,939,551
R-squared	0.174
Mean Dep. Var	18.71
SD Dep. Var	39.00

Notes: The unit of observation is a police stop. The dependent variable is equal to 100 if the stopped driver is Black and zero otherwise. The Table shows the results of OLS estimation of Equation 1 with $k = 30$ and POST-Trump equal to $D_{c,t}^{(1,30)}$, augmented with the Trump social spillover index, standardized to have mean zero and standard deviation of one. This index is the weighted sum of the number of rallies in the previous 30 days, where the weights correspond to the index of connectedness provided by Bailey et al. (2018) between the county where the stop happens and the counties where the rallies occurred in the previous 30 days. Standard errors are two-way clustered at the county and day level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A9: Role of Local Characteristics in the Effect of Trump Rallies on the Probability of a Black Stop Controlling for a Linear Time Trend Interacted with the Share of Black People in the County

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
			100 x 𝟙(Black Stop)			
POST-Trump	1.268***	1.175***	0.701***	1.326***	0.839***	0.429**
	(0.348)	(0.297)	(0.231)	(0.380)	(0.220)	(0.172)
POST-Trump * X	1.066**	0.867***	0.747	1.042**	0.359**	0.293*
	(0.477)	(0.326)	(0.638)	(0.508)	(0.177)	(0.170)
Observations	33,726,046	33,726,046	34,940,130	34,939,551	34,940,130	34,940,130
R-squared	0.171	0.171	0.173	0.173	0.173	0.173
X=	Racial Resentment A	Racial Resentment B	Any Slaves	Cotton	Lynchings	Executions
Mean Dep. Var	18.92	18.92	18.71	18.71	18.71	18.71
SD Dep. Var	39.17	39.17	39.00	39.00	39.00	39.00

Panel B	(1)	(2)	(3)	(4)	(5)	(6)
			100 x 𝟙(Black Stop)			
POST-Trump	0.750***	0.720*	0.839***	0.617**	0.769***	0.808***
	(0.264)	(0.376)	(0.232)	(0.307)	(0.216)	(0.222)
POST-Trump * X	0.110	-0.162	-0.259	0.154	0.047	-0.082
	(0.221)	(0.480)	(0.192)	(0.286)	(0.286)	(0.213)
Observations	34,940,130	13,087,291	34,939,910	34,940,130	34,813,677	34,813,677
R-squared	0.173	0.202	0.173	0.173	0.173	0.173
X=	DEM Share 2012	Sheriff REP	Income	College	China Shock	China Shock IV
Mean Dep. Var	18.71	24.02	18.71	18.71	18.72	18.72
SD Dep. Var	39.00	42.72	39.00	39.00	39.01	39.01

Notes: The unit of observation is a police stop. The dependent variable is equal to 100 if the stopped driver is Black and zero otherwise. The table shows the results of OLS estimation of Equation 1 with $k = 30$ and POST-Trump equal to $D_{c,t}^{(1,30)}$, augmented with an interaction term between POST-Trump and the following predetermined county characteristics: two measures of racial resentment from the 2012 and 2014 CCES (Schaffner and Ansolabehere, 2015), presence of slaves in 1860, soil suitability for growing cotton, IHS of the number of lynchings of Blacks, or of executions of Blacks during the Jim Crow era (Panel A); vote share of Obama in 2012 presidential election, identifier if the county sheriff is Republican, median household income, share of college graduates, the China import competition shock and its instrument from Autor, Dorn and Hanson (2013)(Panel B). All continuous variables measuring county characteristics are normalized to have a mean of zero and standard deviation of one by subtracting the value of each county from the mean value and dividing the result by the standard deviation. All estimations include county fixed effects, day fixed effects, predetermined county characteristics specific linear trends and a linear trend based on the share of Black people in the county. Standard errors are two-way clustered at the county and at the day level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A10: Correlations Between Trumps' Rally Speech and County Covariates

VARIABLES	(1) EXPL	(2) IMPL	(3) TRADE	(4) CLINTON	(5) TERROR	(6) JOB	(7) CORRUPTION
Racial Resentment A	0.201	-0.376	-0.171	-0.034	0.281	-0.266	0.021
	(0.301)	(0.379)	(0.353)	(0.478)	(0.346)	(0.388)	(0.334)
Racial Resentment B	-0.009	0.454	0.622*	-0.215	0.595	0.765**	0.337
	(0.316)	(0.354)	(0.321)	(0.428)	(0.361)	(0.355)	(0.319)
Any Slaves	-0.276	-0.495*	-0.019	-1.039***	-0.317	-0.364	-0.577*
	(0.352)	(0.254)	(0.287)	(0.344)	(0.330)	(0.325)	(0.313)
Cotton Suitability	-0.006	0.078	0.002	-0.046	-0.060	-0.043	-0.029
	(0.098)	(0.106)	(0.134)	(0.165)	(0.147)	(0.116)	(0.138)
Lynchings	0.075	0.029	0.026	-0.084	0.066	0.016	-0.075
	(0.056)	(0.069)	(0.066)	(0.099)	(0.076)	(0.073)	(0.077)
Executions	-0.161**	-0.109	-0.082	-0.006	-0.042	-0.091	-0.066
	(0.065)	(0.075)	(0.079)	(0.109)	(0.082)	(0.093)	(0.097)
County Population	-0.037	-0.028	0.064	-0.028	-0.059	0.040	0.039
	(0.042)	(0.053)	(0.062)	(0.074)	(0.064)	(0.065)	(0.071)
County DEM Share 2012	0.085	-0.064	0.292*	-0.110	0.170	0.243	0.208
	(0.160)	(0.163)	(0.159)	(0.231)	(0.198)	(0.187)	(0.167)
Republican Sheriff	-0.487**	0.148	0.401*	-0.303	0.152	-0.089	-0.012
	(0.197)	(0.222)	(0.234)	(0.321)	(0.284)	(0.278)	(0.240)
County HH Income	0.068	-0.124	0.173	0.098	0.019	0.209*	0.175
	(0.125)	(0.160)	(0.152)	(0.186)	(0.139)	(0.126)	(0.162)
County College Share	-0.054	0.079	-0.273*	0.075	-0.022	-0.220	0.035
	(0.145)	(0.185)	(0.160)	(0.226)	(0.168)	(0.167)	(0.179)
China Shock	0.077	0.008	0.049	0.237	0.181	0.230	0.100
	(0.155)	(0.138)	(0.171)	(0.198)	(0.163)	(0.189)	(0.156)
Observations	190	190	190	190	190	190	190
R-squared	0.128	0.094	0.107	0.136	0.101	0.117	0.095

Notes: An observation is a speech. The estimation sample consists of 190 Trump speeches in 126 counties. The table shows the correlations between bundles of words included in Trump's speeches and local characteristics of the county in which the speech happens. We consider the county characteristics used in the analysis of the effect of Trump speeches on police stops: racial resentment measure A, and B, presence of slaves in 1860, soil suitability for growing cotton, IHS of the number of executions of Blacks, IHS of the number of lynchings of Blacks, vote share of Democratic presidential candidate in 2012 elections, political affiliation of the county sheriff from Thompson et al. (2020), median household income, share of college graduates, China import competition shock, and its instrument from Autor, Dorn and Hanson (2013). All county characteristics are standardized with mean zero and standard deviation of one. Each column displays the results of an OLS regression of the bundle of words (as indicated in the Column header) on the list of county characteristics. Stemmed words that constitute the "Explicit" category are: African, Black, race, racial, racist. Stemmed words that constitute the "Implicit" category are: drug, crime, crimin, rape, gun, prison, riot, thug, urban. "Job" is the count of the following stemmed words: busi, job, manufactur, tax; "Clinton": email, lock; "Trade": China, trade, NAFTA, "Terror": ISIS, Syria, Iraq, terrorist, Afghanistan, Islam; and "Corruption": rig, media, CNN, Washington, corrupt, swamp. Robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.