Whistle the Racist Dogs: Political Campaigns and Police Stops

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Abstract

Did Trump rallies aggravate anti-Black racism? Using data from nearly 12 million traffic stops, we show that the probability that a police officer stops a Black driver increases by 5.1% after a Trump rally during his 2015-2016 campaign. The effect is immediate, specific to Black drivers, lasts for up to 50 days after the rally, and is not due to changes in drivers' behavior. The effects are significantly larger among racially biased officers, in areas with more racist attitudes today, that experienced more racial violence during the Jim Crow era, or that relied more heavily on slavery. Results from a 2016 online experiment show that Trump's inflammatory campaign speech, although not explicitly mentioning Black people, specifically aggravated respondents' prejudice that Black people are violent. We find that the same words also increase the effect of a Trump rally among racially biased officers. We take this as evidence that although not explicitly anti-Black, Trump's campaign radicalized racial prejudice against Black people – through a phenomenon known as dog-whistling – and the expression of such prejudice in a critical and potentially violent dimension: police behavior.

Keywords: Police stops, political campaign, racial prejudice.

JEL Codes: D72, J15, K42.

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"This is a president who has used everything as a dog-whistle to try to generate racist hatred, racist division."- President Joe Biden, during the presidential election debate with Donald Trump, 30 September 2020.

1. Introduction

Identity politics has played an increasing role in most advanced democracies in recent years (Gennaioli and Tabellini, 2019). This has been accompanied by a change in public discourse, with politicians increasingly appealing to identitarian values and emotional triggers (Enke, 2020; Gennaro and Ash, 2021). Historically, explicitly inflammatory political rhetoric has spurred racial and ethnic violence (Adena et al., 2015; Yanagizawa-Drott, 2014). Consequently, today, what can be openly said against many historically victimized groups is restricted either by explicit laws or by social acceptability norms.[1]

During the 2015-2016 campaign for the US Presidency, Donald Trump, a candidate with openly xenophobic views against foreigners, incarnated the break in public discourse brought about by identity politics (see, e.g., Enke (2020)). Trump, however, never challenged social and political norms of racial equality, and his discourse during the 2015-2016 campaign was never explicitly anti-Black.[2] Yet, mounting racial tensions in the US since his election raise the question as to whether Trump's rhetoric also aggravated prejudice and discrimination against Black people.

In this paper, we explore how Trump's 2015-2016 political campaign affected the expression of racial prejudice and discrimination against Black people in one of its most fundamental and potentially violent dimensions: police behavior. Police behavior and alleged racially-motivated brutality have come to symbolize racial bias and discrimination

[1] The European Convention on Human Rights, article 10, protects freedom of speech but also acknowledges the State's right to limit this freedom in certain circumstances. One of the most prominent examples in France, where the Law of 1881, amended in 1972, prohibits hate speech intended to "provoke discrimination, hate, or violence towards a person or a group of people because of their origin or because they belong or do not belong to a certain ethnic group, nation, race, or religion". Since 1990, it is also illegal to deny crimes against humanity. Many other countries, including Australia, Austria, Belgium, Brazil, Canada, Cyprus, Denmark, England, Germany, India, Ireland, Israel, Italy, Sweden, and Switzerland, also have laws that restrict hate speech (Tsesis, 2009). In the US, in its Brandenburg v. Ohio ruling, the Supreme Court held that the government could not punish inflammatory speech unless it is "directed to inciting or producing imminent lawless action and is likely to incite or produce such action". Nevertheless, even if not reaching the threshold of legal prohibition, the use of explicitly racial speech is still constrained by the norms of racial equality and tolerance that have prevailed since the end of the Civil Rights era (Mendelberg, 2001).

[2] Figure A1 in Appendix shows that Trump does not frequently talk about race or Black people in his speeches, compared to other candidates in 2016 or other Republican Presidential candidates in previous elections. Hopkins (2019) makes a similar point. He also does not seem to differ from other candidates in how much he speaks about terrorism, business, corruption. However, he talks relatively more than other candidates about crime, migration, Mexico, and trade. Excerpts of Trump's speeches during his 2015-2016 rallies (see Appendix 3) show that when he explicitly talks about African Americans, he generally describes them as *victims* (of poverty and crime). By contrast, he systematically associates foreigners with crime *offenders* (including many references to "brutal drug cartels"), against whom it is necessary to build a "wall".

against African Americans, especially since the Black Lives Matter movement began in 2013.[3] We focus here on the most frequent type of citizen-police interaction: traffic stops. Every day, 55,000 people are pulled over for a traffic stop,[4] providing the kind of contact that can lead to violent and potentially lethal escalation (Streeter, 2019). Up to a quarter of recent shootings of civilians by police have followed a traffic stop.[5]

We use data on nearly 12 million traffic stops carried out by the police in the 142 counties where Trump held a campaign rally, either as a candidate for the Republican nomination or the presidency (Figure 1).[6] 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)).

To guide our empirical strategy, we outline a conceptual framework that models the choice of a police officer to perform a traffic stop as a function of the race of the driver and other observable characteristics of the traffic event. This model implies that the outcome of interest consists in the probability that a traffic stop involves a Black driver (hereafter, the probability of a Black stop). The model also illustrates the channels through which Trump rallies can affect this outcome. We estimate the effect of Trump campaign rallies on the probability of a Black stop using a generalized difference-in-differences methodology (DiD) at the police stop level, controlling for county and day fixed effects as well as county-specific time trends.[7]

We find evidence that Trump rallies increased the probability of a Black stop. Our baseline estimate suggests that this probability increases by 0.94 percentage points on average in the month following a rally, a 5.1% increase. The effect is immediate and lasts

[3] 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 (2020); 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).

[4] According to the Bureau of Justice Statistics, 8.6% of US residents aged 16 and over (more than 20 million people) were pulled over by the police for a traffic stop in 2015 (Davis, Whyde and Langton, 2018).

[5] 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 12, 2021, and Philando Castile on July 6, 2016.

[6] Typically, as described in Section 3, these rallies are large events, which gather an average of almost 5,000 people. Local police might be exposed to a rally in a given county in several ways: as security detail or as an attendee. 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). The rallies were also intensively covered in local media (Snyder and Yousaf, 2020).

[7] Recent econometric literature on staggered difference-in-differences 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 are negative for our specification. We then follow the estimation procedure proposed by Sun and Abraham (2018) and find similar results to our baseline DiD. We also show that our results are immune to the recent criticism of event-study designs by Borusyak and Jaravel (2017).

for up to 50 days. This outcome is robust to varying the observation window from 10 to 100 days around the event, as well as to including other flexible county-specific time trends. To address the concern that the timing of rallies may be correlated with changes in the probability of a Black stop, we first check with an event-study design 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. Additional results show that the effects are specific to Trump rallies. We observe no effect on police behavior of campaign events carried out by either the Democratic contender to the presidency, Hillary Clinton, or by the other leading Republican opponent, Ted Cruz.

Our conceptual framework highlights how the change in the probability of a Black stop after a Trump rally could be due to a change in individual officer racial bias, patrolling decisions, or driver behavior. We find no evidence for a change in driver behavior (either of Black or non-Black drivers). We also observe no change in the total number of traffic stops nor in the individual probabilities that a traffic stop is of a White driver, a Hispanic driver, or an Asian/Pacific Islander driver.[8]

The change in patrolling decisions itself could stem from changes at different levels of the hierarchy. It may come from individual officers changing their behavior, law enforcement agencies changing patrolling decisions, or from local politicians imposing changes on the police. We show that our effect is robust to controlling for officer-level fixed effects, implying that at least part of the effect is due to a change in individual officers' behavior rather than a shift in the composition of the police force on duty. The result is also robust to including fixed effects for each hour of the day by county, which account for a potential change in the timing of patrols. Last, our results hold when we restrict our sample to state troopers, which are less influenced by local politics. We conclude that change in patrolling decisions at the enforcement agency level cannot fully explain the effect of Trump rallies on the probability of a Black stop. Finally, we construct measures of racial bias at the individual police officer level, based on the difference in officers' ticketing decisions towards White vs. Black drivers, controlling for a detailed set of fixed effects that reduce heterogeneity in the context of a traffic stop. We show that this index of officer racial bias increases after a rally.

To rationalize our findings of an effect of Trump rallies on policing behavior and racial bias towards Black people even though Trump's speeches never explicitly targeted that group, we refer to a political science and law literature that shows how certain speech carries a hidden message only understood by a subgroup – a phenomenon known as the

[8]This also rules out that our main effect is due to a change in misreporting, with stops that were previously misreported as White now reported as Black. For this to be the case, we should observe a similarly sized decrease in the probability that a stopped driver is White, which is not the case. We should also observe a one-to-one substitution for each category of offense, which again is not the case.

"dog-whistle effect" (Lohrey, 2006; Fear, 2007; Goodin, 2008; Haney-Lopez, 2014).[9] Dog-whistling has been defined in various ways. Here, we consider a specific dimension of the definition: when coded language triggers strongly rooted stereotypes about groups perceived as threatening (Haney-Lopez, 2014).[10] Specifically, we explore whether Trump's campaign activated prejudice against Black people, namely the stereotype associating them with violence and crime (Eberhardt et al., 2004), and particularly amongst individuals with deep-seated bias against Black people. To do this, we proceed in three steps.[11]

First, consistent with the argument that dog-whistling harnesses deep-rooted stereotypes about the perceived threat of certain groups, we show that the effect of Trump campaigns on police behavior is larger in magnitude among officers who are more biased towards Black drivers and in areas with a stronger and deeper-seated anti-Black sentiment. The effect is three times as large for officers whose bias at baseline is one standard deviation higher than the mean. There is no effect of Trump rallies on officers with average bias. To measure the strength of local anti-Black views, we use county-average responses to racial resentment questions included in the 2012 and 2014 Cooperative Congressional Election Surveys (Schaffner and Ansolabehere, 2015). 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 shaped racial prejudice against Black people in the US and continues to do so to this day. Specifically, we use the number of slaves in the year 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 private and institutional racial violence measures in the Jim Crow era: the intensity of lynchings and

[9] The term dog-whistle was first coined in the context of Australian politics in the mid-1990s when the then leader of the conservative Liberal party John Howard was accused of pandering to racist views with coded language enabling him to maintain plausible deniability and avoid overtly racist wording (Lohrey, 2006; Fear, 2007; Goodin, 2008). Haney-Lopez (2014) describes dog-whistle techniques in American politics in detail.

[10] There is a large literature in political science and social psychology that studies the role of racial priming on voting preferences (see e.g. Mendelberg (2008); Hopkins (2019)). Dog-whistling can be understood as a form of *implicit* racial priming. It uses 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 against that racial group. It typically achieves this by exploiting common knowledge between the principal and part of the audience or by harnessing stereotypes that are only held by part of the audience. The theoretical persuasion literature shows how persuaders can do better in front of a heterogeneous audience by sending private signals (Krähmer, 2020; Zhu, 2017). However, when restricted to public communication, the principal will generally achieve a pooling, compromising action (Bar-Isaac and Deb, 2014). Dog-whistling can thus be interpreted as a case in which the principal achieves private communication through a public signal interpreted differently by different audiences.

[11] Recently, scholars have argued that Trump has moved beyond dog-whistling by breaking racial discourse norms and making explicit and direct racial appeals (see, e.g., Smith (2020)). While this paper does not debate the extent to which Trump's discourse on race is implicit or explicit, we do observe that explicitly negative language in the 2015-2016 campaign that we study in this paper has primarily targeted migrants rather than Black people. See Footnote 2.

executions of Black people at the county level (Hines and Steelwater, 2012; Espy and Smykla, 2016). We find that Trump's campaign rallies have a significantly larger effect in counties that today have more intense racial resentment, those that had more slaves in 1860 and whose agricultural endowments were more suitable to slavery, as well as those where the intensities of lynching and executions of Black people were higher.[12] In contrast to racial attitudes, other potential sources of heterogeneity such as average income, college education, racial fragmentation, average Democrat vote share, or sheriff political affiliation play no role in aggravating the effect of a Trump rally on police behavior. We similarly observe no differential effect either across counties more or less affected by import competition with China (Autor, Dorn and Hanson, 2013).

Second, we provide a direct test of a dog-whistle effect that operates beyond police behavior and concerns the population as a whole. To do so, we revisit the experiment conducted by Newman et al. (2020). This experiment took place during the 2016 Presidential campaign and presented respondents with Trump's and other candidates' campaign speeches on immigration as well as other topics. While the original paper by Newman et al. (2020) focused solely on the acceptance of discrimination against Latinos, the authors also collected data on prejudice against Black people. We use the latter, to the best of our knowledge for the first time, in this paper. Employing their sample and data, we show that respondents with above median (or above mean) pre-existing prejudice against Black people become even more prejudiced when exposed to Trump's anti-immigration rhetoric, specifically when he accused Mexican migrants of bringing drugs and crime and of being rapists.[13] No effect is observed for respondents who were not initially prejudiced before reading Trump's statement; nor for respondents who are exposed to speeches by other politicians, even on the same topic of immigration, but which do not contain dog-whistles. Moreover, the effect is specific to prejudice against Black people: no effect is observed for bias against other groups, even for respondents who were initially prejudiced against these groups. Consistent with the theoretical conceptualization of dog-whistling appealing particularly to the stereotype of a threatening and dangerous group, the effect here is also specific to a distinct dimension of prejudice: the belief that Black people are *violent*, as opposed to other dimensions of bias measured in the experiment, for example, the belief that they are lazy, or lack intelligence. We take these findings as evidence that Trump's rhetoric resonates, especially among individuals already prone to thinking that Black people are violent, and radicalizes these views even further.

Third, to provide further evidence of a dog-whistle effect, we utilize the content of the

[12] While the effect on the probability of a Black stop is higher, we do not observe that Trump holds more rallies in these counties.

[13] During his presidential announcement speech on June 16, 2015, Trump 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."

speeches by Trump during his rallies in conjunction with the police data and examine how the words included in the experimental inflammatory statement mediate the effect of a rally on police behavior. As predicted by dog-whistling theory, and consistent with what we find in the online sample of respondents, we find that the most prejudiced officers are particularly triggered by those words.[14] By contrast, there is no aggravating effect on prejudiced officers of speech related to the economy, international trade, political corruption, or Hillary Clinton.

Overall, our results show how Trump's rallies have fueled racist behavior and sentiment against Black people. Moreover, the experimental findings and analysis of speech content demonstrate that it is a *particular* dimension of prejudice that is affected – the stereotype that Black people are violent – thus suggesting more than a simple activation of indiscriminate racial bigotry. Our analysis cannot, however, fully untangle whether Trump's rhetoric aggravated prejudice or simply activated or normalized pre-existing prejudice. Regardless, we are able to highlight the direct and real consequences not only on views expressed by the population but also in terms of racially-directed behavior by the police. Moreover, since the effect is stronger in more racist areas and amongst most biased individuals, our results indicate a radicalization of prejudice against Black people, which also contributes to polarization on racial issues. These findings are of significant policy relevance in the United States and beyond, where politicians increasingly use xenophobic and racist rhetoric, either explicitly or using coded language that appeals to deep-seated stereotypes.[15] Arguably, dog-whistle politics can be a powerful political tool for politicians that want to engage with the more racist part of the electorate while maintaining a veneer of plausible deniability.

Our findings contribute to an emerging literature, namely by Bursztyn, Egorov and Fiorin (2020), Edwards and Rushin (2019), Müller and Schwarz (2019), and Newman et al. (2020), that shows how Trump's campaign, election, and social media activism have unraveled social norms around the acceptability of discrimination and xenophobia.[16] In contrast to this literature, we show how Trump's campaign has aggravated prejudice not against the groups targeted explicitly by Trump's racially inflammatory rhetoric but against Black people. Comparing a panel of survey respondents between 2012 and 2016, Hopkins (2019) find that anti-Black prejudice predicts voting intentions for Trump in 2016 to a greater extent than anti-Latino prejudice. This suggests that the radicalization

[14] Guided by the experiment, we use the words: Rape, Drug, Crime, Criminal, Mexico, Mexican, bring, send, problem as the triggering words.

[15] 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.

[16] Feinberg, Branton and Martinez-Ebers (2019) document a correlation at the county level between hosting a Trump's presidential rallies and the incidence of hate crimes. The authors do not account for the potential endogenous selection of counties in which a rally is held, nor for the influence of several potential omitted variables. Lilley and Wheaton (2019) show that this correlation is not robust when controlling for population size.

of prejudice which we document in this paper may have further contributed to Trump's electoral success. Another notable contribution of our work with respect to the existing literature is to document the effect of Trump's political campaign on the behavior of the police. In this respect, our results point to how political campaigns can lead to potential abuses of delegated authority and state violence against specific groups of citizens.

More generally, our results illustrate how politicians can radicalize prejudice and influence racially-directed behavior by triggering ingrained stereotypes. As such, our results speak to two related strands of literature on hidden values, or "crypto-morality," and on the dog-whistle effect. In fact, while a plethora of recent studies have documented the persistence of values and norms,[17] some values may remain hidden, a phenomenon described as "crypto-morality" by Greif and Tadelis (2010).[18] In the context of the US, racial inequality was the dominant accepted social norm into the early twentieth century,[19] until it was supplanted by a norm of racial equality in the post-Civil Rights era (Mendelberg, 2001; Newman et al., 2020). Yet, negative racial views did not simply vanish; they are hidden and continue to shape political preferences (Hutchings and Valentino, 2004; Mendelberg, 2008). Experimental and survey evidence suggests 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). Politicians can thus appeal to racial bias and activate racial resentment.[20] Our work, therefore, contributes to recent literature that explores the ways leaders legitimize political preferences and mobilize their followers (Lenz, 2012; Dippel and Heblich, 2018; 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).[21] By showing how dog-whistle politics radicalize already-prejudiced individuals, this paper also complements studies on political radicalization and polarization.[22]

[17] This literature is now too voluminous to cite comprehensively. See Nunn (2012), Alesina and Giuliano (2015), and Nunn (2020) for reviews.

[18] This highlights a limitation of studies that measure norms in surveys. A potential confound is that opinions revealed in surveys may be moderated by the social acceptability of expressing these views. Studies on voting behavior are similarly constrained by the supply of political parties. For example, Cantoni, Hagemeister and Westcott (2019) argue that the lack of supply of party platforms restricted the expression of populist right-wing views in Germany. By contrast, we are able to observe actual behavior by the police in this paper.

[19] We define 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)

[20] A related phenomenon is the activation of a collective memory of traumatic events. For example, Fouka and Voth (2020), and Ochsner and Roesel (2019) show how politicians can gain political advantage by stimulating historical resentment against former enemies (e.g., Germans in Greece, Turks in Austria).

[21] A related literature shows how traditional or social media, rather than leaders, can facilitate the coordination of xenophobic attacks (Bursztyn et al., 2019; Della Vigna et al., 2014; Yanagizawa-Drott, 2014).

[22] See, e.g., Abramowitz and Saunders (2008); Gentzkow (2016); Abramowitz (2018); Gennaioli and

Our paper is loosely related to a literature aiming at measuring racial bias in policing and judicial decisions.[23] Our conceptual framework shows that the probability of a Black stop is the appropriate outcome variable to capture the effects of Trump rallies, which may operate not only on officers' racial bias but also on institutional policing decisions.

The rest of the paper is organized as follows. The next section presents the conceptual background. Section 3 describes the data used in the analysis. Section 4 shows that the probability of a Black stop increases after a Trump rally, and Section 5 shows that the effect is not due to a change in driver behavior but to a change in policing. In line with dog-whistling theory, we show in Section 6 that the effect is stronger for more biased officers and in areas with a stronger and deeper-seated anti-Black sentiment. We provide experimental evidence on the effect of exposure to Trump's racially inflammatory speech on racial prejudice in the population in Section 7. We then show how such racially inflammatory speech also amplifies the effect of Trump rallies on traffic stops of Black drivers in section 8. In conclusion, we discuss broader implications.

2. Conceptual Background

To motivate our empirical strategy, we introduce a simple model of traffic stop decisions by a police officer. This model helps clarify what the outcome of interest is and how – through which channels – it is potentially affected by Trump rallies.

In our setting, an officer j observes driving event i and has to decide whether to stop the car. S denotes the stop decision. A driving event is characterized by the driver's race, $R_{i,j}$, and other observable characteristics $M_{i,j}$. The race of the driver can either be Black ($R_{i,j} = 1$) or non-Black ($R_{i,j} = 0$) and is drawn from a Bernoulli distribution f_j. The probability that $R_{i,j} = 1$ is p_j. We, therefore, allow for different officers to face a different racial composition of drivers, for example, if they are deployed in different neighborhoods. $M_{i,j}$ includes all the characteristics of the event that impact the utility that an officer derives from a stop, beyond race. For example, $M_{i,j}$ encompasses policing objectives such as the potential deterrence or incapacitation effect of the stop, career concerns of the officer, or the inconvenience of the stop, such as attending traffic court. $M_{i,j}$ is drawn from a distribution $g_{R,j}$ where R is the race of the driver of event i. We thus allow for drivers of different races to systematically drive differently. p_j and $g_{R,j}$ could depend on three factors: driver demographics and behavior, institutional patrolling decisions, and individual officers' patrolling decisions.

The officer derives the following utility from an event:

Tabellini (2019); Bordalo and Yang (2020) for recent contributions documenting polarization in the US. While many, in particular, Abramowitz (2018), argue that Trump's rise to power was the consequence of polarization, we focus instead on how his campaign further deepened divisions.

[23] See footnote 3, Lang and Kahn-Lang Spitzer (2020) for a recent review, and Arnold, Dobbie and Yang (2018), Goncalves and Mello (2021), and Ba et al. (2021) for recent contributions.

$$U_{i,j}(S_{i,j}, M_{i,j}, R_{i,j}) = \begin{cases} 0 & \text{if } S_{i,j} = 0 \\ M_{i,j} + \lambda_j R_{i,j} & \text{if } S_{i,j} = 1 \end{cases} \tag{1}$$

λ_j parametrizes the racial bias of the officer. An officer with a positive λ_j derives higher utility from stopping a Black driver compared to a non-Black driver, everything else constant. The optimal stopping strategy of officer j when faced with an event i ($S^*_{i,j}$) is therefore defined by:

$$S^*_{i,j}(M_{i,j}, R_{i,j}) = \begin{cases} 1 & \text{if } M_{i,j} \geqslant -\lambda_j R_{i,j} \\ 0 & \text{if } M_{i,j} < -\lambda_j R_{i,j} \end{cases} \tag{2}$$

This implies that an officer with higher λ_j will stop a Black driver for a larger set of observable characteristics $M_{i,j}$ than they would a non-Black driver.

One issue here is that we only observe $S_{i,j} = 1$ in the data. This is because we do not observe the universe of driving events, but only when a driver is stopped. We therefore focus on the implications of the optimal stopping strategy based only on stops that are made. In particular, we derive the probability that a stop made by officer j is of a Black driver as:

$$\begin{aligned} q_j &= \frac{Prob(f_j = 1)Prob(g_{1,j} \geq -\lambda_j)}{Prob(f_j = 1)Prob(g_{1,j} \geq -\lambda_j) + Prob(f_j = 0)Prob(g_{0,j} \geq 0)} \\ &= \frac{p_j[1 - G_{1,j}(-\lambda_j)]}{p_j[1 - G_{1,j}(-\lambda_j)] + (1 - p_j)[1 - G_{0,j}(0)]} \end{aligned} \tag{3}$$

where $G_{R,j}$ is the CDF of $g_{R,j}$.

The numerator of this expression is the probability that an event leads to a stop of a Black driver. It is the product of two components: the probability that the driver is Black and the probability that this event by a Black driver leads to a stop. The denominator is the probability that an event leads to a stop. It is the sum of the probability that an event leads to a stop of a Black driver and the probability that an event leads to a stop of a non-Black driver. From now on, we will refer to q_j as the probability of a Black stop.

Proposition 1. *The probability of a Black stop q_j increases in racial bias λ_j and in the probability that the driver is Black p_j. If $g_{1,j}$ first order dominates $g_{1,j'}$ then $q_j > q_{j'}$. If $g_{0,j'}$ first order dominates $g_{0,j}$ then $q_j > q_{j'}$.*

Proof: See Appendix 4.

According to the prediction of dog-whistling, we expect Trump campaign events to increase q_j through several possible channels. The first is through an increase in racial bias λ_j. The second is through changes in individual or institutional patrolling decisions. For example, q_j could increase because of an increase in p_j if officers patrol areas with more Black drivers, or because officers patrol areas where they are more likely to face an event in which Black drivers have a high $M_{i,j}$, or non-Black drivers a low $M_{i,j}$ (resulting in a change in either $G_{1,j}$ or $G_{0,j}$).

Trump rallies could also increase q_j through channels unrelated to dog-whistling. They could change who drives and how. For example, Black drivers could drive more, or non-Black drivers less, which would increase p_j. Another possibility is that Black drivers would change their driving behavior in a way that is more likely to lead to a stop (change in $G_{1,j}$), or non-Black drivers change their driving behavior in a way that is less likely to lead to a stop (change in $G_{0,j}$). In Section 4,we first establish that Trump rallies result in an increase in q_j. We then investigate in Section 5 the different channels behind this effect.

3. 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. To construct a national database of traffic stops, they filed public records requests with all 50 state patrol agencies and over 100 municipal police departments. More details on the data collection can be found in Pierson et al. (2020). 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 in counties where Trump held a rally during his 2015-2016 campaign. This gives us a total of $11,931,161$ stops for which we have information on the date of the stop and the driver's race. The race is recorded as "Asian/Pacific Islander", "Black", "Hispanic", or "White." Information on the final decision made by the officer for a stop is available for $7,521,505$ of these stops and has been coded by the Stanford Open Policing Project into four possible categories (in increasing severity): warning, citation, summons, and arrest. For a more limited set of stops ($5,387,948$), 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-code all Donald Trump's rallies for the 2015-2016 presidential campaign that started on June 17, 2015 and ended on November 7, 2016. Altogether, 224 Trump campaign rallies (out of 324) in 142 counties overlap with traffic stop data. Typically, Trump's rallies were large events, gathering an average of 4,774 people across 2015-2016 (with 4,930 people on average for nomination rallies and 4,619 for presidential rallies).[24] We also geo-code in-

[24]From Wikipedia (2016).

formation on the 2016 Democratic presidential candidate, Hillary Clinton, and the other main Republican contender, Ted Cruz.

County Characteristics: Data on ethnic fractionalization, average income, the share of Blacks, and average college completion comes from the 2015 American Community Survey. Data on county-level import competition shock is from Autor, Dorn and Hanson (2013), while the information on 2012 county-level vote shares for Obama comes from Leip (2016).

Summary Statistics: Summary statistics are provided in Table 1. Black people represent 11.22% of our sample population, but as much as 20.39% of stops. Black drivers are thus over-represented in stops by a factor of two. 51.50% of stops are of White drivers and 24.13% of Hispanic drivers. Black drivers represent 22.8% (6.87/30.18) of the warnings and 17.2% (0.81/4.70) of the arrests.

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 1 to 5, to 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 Whites who somewhat or strongly agree with the first statement and the share of Whites 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. To circumvent this limitation, we also 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 slavery, which we proxy by the number of slaves per capita in 1860.[25] 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 intensity of lynchings (from Hines and Steelwater (2012)) and of executions of Blacks (from Espy and Smykla (2016)) at the county level.

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

The experimental data is described in Section 7. The data on Trump's rallies speeches is described in Section 8.

4. Empirical Strategy and Results

We follow our conceptual background and Equation 3 to guide our empirical strategy and show the effect of Trump rallies on the outcome of interest: the probability of a Black stop.

4.1 Difference-in-Differences Analysis

4.1.1 Empirical 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 1 if day t is within a and b days from any event 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 event and negative if day t is before the event. A given county can have more than one event, and up to N_c events. Out of the 142 counties in our sample, 99 have exactly one Trump rally, while 23 have 3 or more rallies. With a slight abuse of notation, $D_{c,t}^{(-\infty,a)}$ is defined as a dummy variable equal to 1 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 1 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:

$$Black_{i,c,t} = \alpha_c + \theta_t + \gamma D_{c,t}^{(-\infty,-k-1)} + \eta D_{c,t}^{(0,0)} + \beta D_{c,t}^{(1,k)} + \delta D_{c,t}^{(k+1,\infty)} + \alpha_c \times t + u_{i,c,t}, \quad (4)$$

where $Black_{i,c,t}$ is a dummy that takes value one if the driver pulled over by the police in stop i in county c on date t is Black. In terms of our conceptual framework, we estimate changes in the average q_j in county c on date t after a Trump rally.

One concern may be that counties in which Trump held a rally differ systematically from other counties. For example, Trump or his campaign team might target counties as a function of their underlying racism or police behavior. Time-invariant county institutional or cultural characteristics, including racism, 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 $\alpha_c \times t$. To address potential remaining issues related to the systematic selection of counties with a campaign event, we rely only on the set of counties in which Trump has ever held a rally in the estimation of Equation

4 (although our results are robust to including never-treated counties). 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 of roads. We control for such disruptions using an indicator that takes the value of one for county c on date t of the day 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.

To address potential concerns about the selection of the treatment window, we adopt a flexible approach. Specifically, we estimate Equation 4 varying the time window k after a Trump rally by increments of 10 days, from 10 to 100 days. The omitted comparison time window consists of an identical window ($k = -10, ..., -100$, i.e., 10 to 100 days before the rally), immediately prior to the rally. We therefore 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. For counties with multiple events, we allocate each stop i in county c to each possible event in the county and define the windows around each event.

To define the dummy variables that capture the different windows, it is thus necessary to take the maximum of the indicator variables that capture the possible windows for each possible event. For example, if we consider a treatment window k of 30 days after a rally, the omitted comparison window is 30 days before the rally (for all possible rallies in the county), $D_{c,t}^{(-\infty,-31)}$ indicates more than 30 days before a rally and $D_{c,t}^{(31,\infty)}$ more than 30 days after the rally. β, therefore, captures the change within a county in the probability of a Black stop 30 days after any Trump rally compared to the 30 days prior.

In the potential outcome framework, our identification assumption requires that after controlling for day fixed effects and county-specific linear trends, the probability of a 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 an increase in the probability of a Black stop. We address this in four ways. First, we include county-specific time trends. Second, we show in Table A1 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, 10, or 30 calendar days before the rally in order to capture reasonable time frames for the scheduling of rallies. Third, in

Section 4.2, we present the findings of an event-study analysis that shows the absence of pre-trends in the probability of Black stops across counties. The event study results also show that the only period in which we pick up a treatment effect is immediately after a Trump rally and that the effect lasts up to 50 days. Last, results of permutation inference (based on $1,000$ replications) in which we randomly reassign Trump rallies within counties across days show that our effect size is well outside the range of estimated effects from these placebo treatments (see Figure A2).[26]

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 4.1.2, we check that our results are not subject to potential issues with the two-way fixed effects estimators highlighted by Sun and Abraham (2018), de Chaisemartin and d'Haultfoeuille (2020), and Borusyak and Jaravel (2017).

4.1.2 Results Table 2 shows the estimates of Equation 4 for increasing windows k around a Trump rally. We observe that the probability of a Black stop increases after a Trump rally. The effect is immediate, constant in magnitude for the first 30 days after a rally, and then slowly fades away. The effect remains statistically significant for up to 100 days after a rally, although at the 100-day window, it is only half in magnitude compared to the largest effects immediately after the rally, suggesting that the effect lasts for around 50 days. An event-study analysis will confirm that the effect is very stable in the first month after a rally, largest in magnitude for up to 30 days, then declines and is no longer significant 50 days after the rally. In what follows, we retain a 30-day window after a rally as the main focus of our analysis.

In Table A2, we verify that our results are robust to excluding county specific time trends or instead to including county-specific quadratic time trends (Columns 1 and 2). Borusyak and Jaravel (2017) 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. In these settings, the authors recommend dropping the linear trend or including never-treated observations. Our analysis is unlikely to suffer from this issue as the treatment effects last for only 50 days and our estimation sample includes many days after that. We nonetheless follow their recommendations and show in Table A2 that our results are robust to dropping the

[26] It is not unexpected that the distribution of the estimated placebo treatment effects has more extreme positive values compared with negative values in the presence of a dynamic treatment effect that fades over time and when the treatment effect is estimated using a sharp window, such as in our setting. If the placebo treatment date falls close to the actual treatment date, the estimated placebo effect will take an extreme positive value (close to the true treatment effect), which will not be compensated by similarly high negative treatment effects when the placebo treatment falls further away from the actual treatment.

linear trend (Column 1) and to including never-treated units (Column 3). We consider as never-treated units the counties where other major political candidates in the 2015-2016 campaign (Clinton or Cruz), but not Trump, held a rally. We do so in order to reduce the potential heterogeneity between treated and untreated counties. When we include those counties, adding about 5 million observations to the estimation sample, the estimated coefficient becomes larger in magnitude (from 0.94 to 1.01) and still statistically significant at the 1% level.

Recent econometric literature on staggered difference-in-differences highlights potential issues with the two-way fixed effect estimator employed 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). 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 situations studied by de Chaisemartin and d'Haultfoeuille (2020). First, the treatment only lasts for up to 50 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 (2018) 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 difference-in-differences parameters for each county. Following their technique, we combine these estimates (with equal weights) and find that the probability of a Black stop increases from 0.94 p.p. in our baseline to 0.98 p.p., suggesting that our baseline estimate is, if anything, underestimated.

We show that our results are robust to alternative measures of the dependent variable (logarithm or inverse hyperbolic sine of the share of Black stops in a county-day) in Columns 4 and 5 of Table A2). Last, in Section 2.3 of the Appendix and Table A3, we show that our results are robust in an alternative difference-in-differences specification, in which we fix the pre-treatment window to 100 days and only include observations k days after a rally, for $k = 10, ..., 100$.

In terms of magnitude, we estimate a 0.94 p.p. increase in the probability of a Black stop during the first 30 days following a Trump campaign rally. Given that in our estimation sample, the probability of a Black stop in the 30 days before a Trump rally is 18.65%, this estimate amounts to a 5.1% increase. The total number of stops in our sample in a 30-day window prior to any rally is 575,042. Thus, our analysis reveals that Trump's rallies led to 5,470 additional stops of Black people by the police in the month following the events. Note that this number is an underestimate since we only have information on 224 out of the roughly 320 campaign rallies and on a subset of days and of law enforcement agencies.

To estimate the overall effect of the 2015-2016 Trump campaign, notice that the police makes about 55,000 stops a day overall (Davis, Whyde and Langton, 2018). In our sample, the average rally is held in a county with a population of 785,338 or around 0.243% of the total US population. We can use this information to produce a back-of-the-envelope calculation of the overall effect of the Trump 2015-2016 campaign by making some simplifying assumptions. First, let us assume that our sample of rallies is representative of the overall population of Trump rallies. Second, let us assume that, on average, police stops are similarly distributed between counties that hold and do not hold Trump rallies. Given these assumptions, police in a county that holds a Trump rally perform 134 stops a day (0.243% of 55,000). This suggests that if we were able to observe all the stops in all the counties in which Trump ever held a rally, we would observe $1,286,400$ (calculated as 134 stops $\times$ 320 rallies $\times$ 30 days) stops in a 30-day window before any rally, and not $575,042$ as in our sample. Our estimates, therefore, imply that Trump's rallies led to 12,237 additional stops of Black drivers by the police in the 30 days following the rallies and 17,409 stops of Black drivers in the 50 days following the rallies.[27]

Our analysis thus far considers all the rallies held during the campaign, both those for the nomination and those for the presidency. Yet, some of the rallies held for the nomination occurred when Trump was still a marginal player. We may thus expect that the rallies held during his presidential campaign had bigger effects. Indeed, the increase in Trump's visibility and popularity, from when he became the Republican nominee and throughout his presidential campaign, may have had an emboldening effect. For example, the experiment by Bursztyn, Egorov and Fiorin (2020) reveals that signals of Trump's popularity make xenophobic respondents more likely to express their views against immigrants. That said, Enke (2020) shows that Trump's campaign became more moderate after he secured the Republican nomination, which would instead imply that the presidential rallies had a smaller effect.[28] In column 6 of Table A2, we differentiate between rallies held either before or during the presidential campaign. We find that consistent with the moderation of speech over the course of the campaign described by Enke (2020), the effects are significantly smaller for general election rallies.

A potential concern is that $Black_{i,c,t}$ may be mismeasured and that Trump rallies

[27]The effect in the 30 days after a Trump rally is computed as $5,470$ (additional stops of Black people in the month following a rally) $\times$ $1,286,400$ (the number of total stops we would observe in a 30-day window before any rally if we observed all rallies and all police agencies) $\div$ $575,042$ (the number of stops we actually observe in a 30-day window prior to any rally). We follow the same steps using coefficient estimates for a 50-day window to estimate the effect in the 50 days after a Trump rally.

[28]It is also possible that places visited during the nomination campaign were very different from places visited during the presidential campaign. Additional analysis in Appendix Table A4 suggests, however, that the counties in which Trump held campaign rallies for the Republican nomination did not statistically differ from counties in which he held rallies for the general election along a wide range of dimensions, including pre-trends in the number of police stops or the share of stops involving Black drivers.

systematically affects this mismeasurement. Recent literature shows that police may misclassify the race of stopped drivers. For example, Luh (2019) shows that minority drivers are sometimes misreported as Whites. Misclassification could affect the validity of our results if the misreporting of Blacks as Whites systematically decreases after a Trump rally. If this was the case, we should observe a decrease in the probability that a stopped driver is White of the same magnitude as the increase for Blacks. To check for this, we report in Column 5 of Table 4 estimates of Equation 4 where the outcome is a dummy variable equal to one if the stop is of a White driver. The coefficient associated with the post-Trump rally dummy is not statistically significant, suggesting that systematic changes in misclassification cannot explain our result.

To better illustrate the dynamics of the effect, we now turn to an event-study analysis.

4.2 Event-study Analysis

In this section, we perform an event-study analysis, which offers several advantages. First, it allows to check for the existence of 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 we did before. The event-study specification is as follows:

$$\begin{aligned} Black_{i,c,t} = \alpha_c + \theta_t + \gamma D_{c,t}^{(-\infty,-106)} + \sum_{\tau=-105(15)-30} \beta_\tau D_{c,t}^{(\tau,\tau+14)} \\ + \beta_0 D_{c,t}^{(0,0)} + \sum_{\tau=1(15)91} \beta_\tau D_{c,t}^{(\tau,\tau+14)} + \delta D_{c,t}^{(106,\infty)} + \alpha_c \times t + u_{i,c,t} \end{aligned} \tag{5}$$

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 this event study, the omitted time bin is $D_{c,t}^{(-15,-1)}$, which identifies the 15 days prior to a Trump rally.

Figure 2 shows the estimates of β_τ in Equation 5. We see that Trump rallies result in a substantial and immediate spike in Black traffic stops following a rally. The probability of a Black stop increases by 0.8 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. The effect is nevertheless transitory and has faded away by 60 days after a rally. The figure also shows the trend in the probability of a Black stop prior to a Trump rally, which is very stable and does not change significantly in either direction up to 105 days leading up to a rally. This result suggests that Trump did not specifically time his rallies in certain counties as a function of local police behavior towards Black drivers.

5. Establishing Change in Policing vs. Change in Driver Behavior

Equation 3 and our discussion in Section 2 highlight that the probability of a Black stop is a function of officer racial bias, patrolling decisions, and driver behavior. Changes in racial bias and patrolling decisions are consistent with dog-whistling, while changes in driver behavior are not. We first show that Trump rallies do not affect driver behavior. Second, we show that enforcement agencies' patrolling decisions cannot explain all the effect of Trump rallies on our outcome of interest. Last, we provide direct evidence that officer racial bias increases after Trump rallies.

5.1 Driver Behavior

If the increase in the probability of a Black stop was justified by changes in driver behavior, this should be reflected in the severity of the stop outcome. If Black drivers were driving more recklessly or were engaging in more criminal acts that would lead to a traffic stop, we should observe an increase in the probability of a stop resulting in a Black driver being arrested, an increase in fatalities of Black drivers, or in the probability of a stop of a Black driver due to a collision or to speeding. To capture stops that truly reflect drivers' behavior rather than officers' potentially discretionary behavior, we retain speeding stops triggered by a speed radar. This is to avoid instances in which speed is given as a justification but is not recorded on a machine and may have involved some degree of discretion by the police officer. Many other justifications for stops are either only observable after the stop has taken place (e.g., driving without a license) or are often vague and likely to involve some degree of discretion from the police officer (e.g. "moving violation", "improper change of lane or course", or even DUI checks). The other exceptions are driving with an expired tag or without a seatbelt, which generally also automatically trigger a stop. We also use these as proxies for driver rather than police behavior.

We do not observe any change in the probability of a Black stop resulting in an arrest (Columns 1 in Panel A of Table 3), nor in the number of Black traffic fatalities in the county (Column 3 of Table A5),[29] or in the probabilities that the stop of a Black driver is due to collision, triggering a radar, driving without a seatbelt, or tag violation, either taken individually or together (Columns 2 to 6 of Table 3). As Column 7 of Table 3

[29] Data on fatalities is only available at the county level. Data on motor accidents is not available at a more disaggregated level than the state on a monthly basis. We use the data on fatalities from the Centers for Disease Control (CDC) Wonder's Multiple Cause of Death, 1999-2018 dataset available on the CDC website. The data contains monthly information on the number of deaths in each county by leading 130 causes of deaths. We use the codes V01-V99 to measure the number of fatalities due to motor accidents. We obtain data on all traffic fatalities and on traffic fatalities of Black or African American drivers.

shows, Trump rallies instead only affect the probability of a Black stop for the set of other justifications, which are *not* perfectly observable or automatically triggered.

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 Column 1 of Table A5, which relies on newly collected data on Black Lives Matter protests in 2015-2016, we 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 potential explanation for our results lies in the change in driving behavior of non-Black drivers. If non-Black drivers were driving less or more safely, they would be stopped less, which would mechanically increase the probability of a Black stop, everything else being constant. However, if this was the case, and with the same logic as above, we should also observe fewer traffic fatalities of non-Black drivers, which is not the case (see Column 4 of Table A5). We should also observe a decrease in the probability of stop of non-Black drivers that are automatically triggered due to a collision, a speed radar being triggered, or tag or seatbelt violations. As shown in Panel B of Table 3, we observe no change in these outcomes. The only statistically significant effect is that the probability of stop of a non-Black driver due to other, discretionary stops goes down (Column 7 of Table 3), which is consistent with (in fact the mechanical consequence of) the increase in the probability of a discretionary stop of a Black driver. Moreover, in Column 1 of Table 4, we show that the total number of stops is unchanged.[30] Overall, we thus observe no change in driving patterns that could explain our results. The effect must therefore be due to a change in police behavior. We now shed direct evidence on this.

5.2 Patrolling Decisions

The probability of a Black stop may change because law enforcement agencies or individual officers change patrolling decisions after a Trump rally. At the agency level, these decisions could involve which police officer is sent patrolling, what time of day the patrols are deployed, and where patrols are deployed. One reason why these changes may occur after a Trump rally could come from local political pressures on law enforcement. 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

[30] To examine the effect of a Trump rally on the total number of stops, we aggregate our dataset at the county-day level.

to local electoral preferences.[31]

To examine whether the effects are due to decisions about which police officer should patrol (i.e., compositional change in deployed police after a rally), we add officer-level fixed effects to Equation 4. This singles out the effect of a Trump rally on the change in the probability of a Black stop for a given officer. Since information on individual officers identifiers is only available for a subset of stops, the estimation sample drops by 44.4%. The result related to the effect of Trump rallies is nonetheless robust (Column 2 of Table 4) although smaller in magnitude.[32] 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 $4,019,385$ stops). The results, displayed in Column 3 of Table 4 are robust and similar in magnitude. Information on the stop location is only provided consistently at the county level, thereby limiting our ability to provide additional analyses that account for potential changes in the location of patrols after a rally. Nevertheless, we show in Column 7 of Table A2 that our results are robust to including local enforcement agency fixed effects that account for potential changes in deployment across agencies within a county.

To rule out changes in traffic stop patterns that may be due to local politics (e.g., mayor or county sheriff), we restrict our attention to stops by state troopers, who are insulated from local political pressures. Our estimation sample consequently drops by slightly more than 61.5%, but results in Column 4 of Table 4 show that the effect of Trump rallies on Black stops remains statistically significant at the 1% level.

Overall, these results show that Trump rallies generated a change in individual police officer behavior towards Black drivers. We now further establish that this change is targeted explicitly towards Black drivers instead of minorities in general. To do so, we show the results of estimating Equation 4 for other minorities. Results are shown in Columns 5 to 7 of Table 4. We observe no statistically significant effects of Trump rallies on the probability of stop of Hispanic, Asian or White drivers. This is especially notable given that Trump's inflammatory rhetoric overtly targeted Hispanics (as well as Muslims). One explanation for the difference consists in the prevalence and the nature of racism towards Black people. Studies have established that the population in general and the police in particular hold stereotypes associating Blackness with violence and crime (Eberhardt et al., 2004). Moreover, racism against Black people has a long history and has pervaded law enforcement since its inception (Acharya, Blackwell and Sen, 2019; Muhammad, 2019). This suggests that consistent with the predictions of dog-whistling, deeply ingrained prejudice may mediate the effect of Trump rallies on police behavior. We examine this possibility in the next section. Before that, we provide direct evidence

[31] 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.

[32] The coefficient associated with POST-Trump in the comparable sample (i.e., for which we are able to include officer fixed effects) is 0.87.

that officer-level racial bias against Black drivers increases after a rally.

5.3 Officer Racial Bias

Officer racial bias, λ_j, is unobservable. To directly test for the prediction that Trump rallies increase λ_j, we could try and estimate λ_j before and after a Trump rally. The literature on racial bias in policing aims at inferring λ_j by observing the race of citizens in citizen-police interactions.[33] In the context of traffic stops, the main difficulty faced by this literature is that the probability of a Black stop is not only affected by λ_j but also, as we show in our conceptual framework, by the type of driver the officer encounters, both in terms of race and driving behavior (p_j and $G_{R,j}$ in our conceptual framework). In order to overcome this difficulty, one approach is to use a rich set of controls in order to capture potential differences in p_j and $G_{R,j}$, as in Ba et al. (2021). We follow this literature to estimate λ_j.[34]

Our data includes information on the day and hour of the stop, but information on the stop location is only provided 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 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.[35]

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. Our measure of officer-level bias 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 would score high in our estimated racial bias 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,

[33]See footnote 3 and Lang and Kahn-Lang Spitzer (2020) for a recent review.

[34]Other 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 of a specific policing decision after the stop decision, such as vehicle search (Knowles, Persico and Todd, 2001; Antonovics and Knight, 2009) across races. The theoretical idea behind this test is that under no discrimination, at the margin, the probability to find contraband should be equalized across races. Our dataset records the outcome of a search in only 1.1% of observations, rendering the implementation of this method impossible in our study. Another approach is to use discontinuities in observed ticketing decisions (Goncalves and Mello, 2021). This method requires precise information on the speed recorded by the office, which is not provided in our data.

[35]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.

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 of estimated racial bias if they are more likely to arrest a Black vs. a White driver (conditional on the set of fixed effects). Given that our treatment window of interest is 30 days, we compute these officer-level bias measures based on 30 days of stops after a Trump rally and 30 days before. This reduces potential heterogeneity over time in the bias of a given individual and maintains the same control group as in our main specification.

We estimate Equation 4 where the outcome of interest is a measure of the estimated racial bias of the officer performing the stop. Our data is now at the officer level, and the specification also includes officer-level fixed effects. We thus estimate the change in estimated racial bias within officer after a Trump rally. Because we only retain 30 days of stops on either side of a Trump rally, we only observe a limited number of stops per officer, on average 42 per 30-days time period. Our resulting estimates are thus somewhat imprecise, but Table A6 shows that, regardless of which measure we use, officer racial bias against Black drivers increases after a Trump rally. By contrast, we find small and insignificant treatment effects on similarly constructed measures of bias against Hispanic drivers.[36]

Given that we have less granular information on the circumstances of a stop compared to existing studies, such as Ba et al. (2021) or Goncalves and Mello (2021), we may be overestimating the bias of some officers (for example, those systematically patrolling neighborhoods where Black drivers drive more recklessly than White drivers) and underestimating the bias of others. This would be a concern if our objective was to ordinally rank officers or characterize individual characteristics related to officer bias, as in Goncalves and Mello (2021) or Ba et al. (2021). Our objective, instead, is to identify if officer bias *changes* after a Trump rally. In other words, given the high frequency of our data, the difference between our approach and existing studies is that we do not need to assume that we capture all the differences in the context of a stop across officers but that these do not change abruptly after a Trump rally, on average and for a given officer. Results in subsection 5.1 showing that driver behavior is unchanged after a Trump rally provide some confidence that any change in the estimated officer bias reflects an actual change in bias rather than in the circumstances of a stop.

6. Mechanisms

In this section, we explore why Trump rallies generate a change in police behavior towards Black drivers. First, we show in Section 6.1 that the effect is specific to Trump rallies

[36] The coefficients corresponding to Columns 1 and 2 of Table A6 for bias against Hispanics are 0.026 (p-value 0.42) and 0.043 (p-value 0.23).

and not simply due to the occurrence of any political rally. We do so by showing no effect of rallies by other Republican or Democrat candidates. What differentiates Trump from other candidates is his xenophobic rhetoric. However, his rhetoric targets Central American migrants and Muslims rather than Blacks. It may therefore be surprising that we only observe a change in police behavior towards Black drivers. A potential explanation offered by the literature on dog-whistling is that Trump's rallies and speeches activated negative stereotypes of the police towards Black people. Studies have established that police officers hold stereotypes associating Black people with violence and crime (Eberhardt et al., 2004), have a preference for discrimination against Blacks (Fryer, 2019; Goncalves and Mello, 2021), are more racially resentful, and are more likely to see Blacks as violent compared to non-police (LeCount, 2017; Ba et al., 2020). The dog-whistle theory would predict that the most prejudiced police officers are precisely those who should respond to the dog-whistle. We make the best possible use of our data in Sections 6.2 and 6.3 to show that our results are consistent with these theoretical predictions.

6.1 Political Campaigns Placebos

If the effect was simply driven by the fact that a political rally was held, rather than specifically a Trump rally, we should also observe changes in police behavior after rallies by other candidates. We consequently examine the impact on police behavior of rallies held by the other presidential candidate, Hillary Clinton, during her 2015-2016 campaign. We estimate Equation 4 using the same windows after each rally as in Table 2 but using Clinton's rallies instead of Trump's, in the sample of counties in which Clinton ever held a rally. Table 5 (Panel A) shows that Clinton rallies did not result in any systematic change in the probability of a Black stop. The estimated coefficients are sometimes positive, sometimes negative, and only marginally statistically significant (and negative) in one instance.

Clinton's rallies may not, however, be the correct comparison. The effect of Trump could be due to the fact that he was running for the Republican party, a party known for its conservative and tough-on-crime policies. If this were the case, we should observe similar changes in police behavior after rallies held by Ted Cruz, the other main contender for the Republican nomination in 2016. Cruz was closely trailing Trump during the Republican primaries and ran on a socially conservative, pro-gun, anti-crime, and pro-police platform.[37] Cruz, however, never engaged in the kind of inflammatory rhetoric that Trump adopted. We again estimate Equation 4 using the same windows as in Table 2

[37] Ted Cruz famously declared in 2015 that police were "feeling the assault from the President, from the top on down, as we see – whether it's in Ferguson or Baltimore, the response from senior officials, the President or the Attorney General, is to vilify law enforcement. That's wrong. It's fundamentally wrong. It's endangering all of our safety and security" (reported in the Houston Chronicle on Monday, August 31, 2015, https://www.chron.com/news/politics/tedcruz/article/Ted-Cruz-blames-Obama-for-death-of-Harris-County-6476309.php), accessed June 15, 2020.

after Cruz 2016 rallies, in the sample of counties in which Cruz held a rally. Table 5 (Panel B) shows that Cruz's rallies did not result in any change in the probability of a Black stop. The estimates range from -0.29 to 0.32 p.p. and are never statistically significant.

In Appendix Table A7, we run the regressions in the same respective samples of counties but add variables to account for whether Trump also held rallies in those counties. The results confirm that Clinton's and Cruz's rallies generate no increase in the probability of a Black stop, while Trump's rallies generate positive and statistically significant effects. These results address the potential concern that our failure to detect any effect of Cruz's rallies may be due to statistical power, since Cruz only ran in the primaries, or the concern that the null results for Cruz or Clinton may be due to a particular selection of counties. They also confirm that the effect on traffic stops is specific to Trump's rallies.

6.2 Heterogeneity Across Police Officers

To test for the prediction that dog-whistles should affect racially biased officers more, we estimate Equation 4 including a control for the pre-treatment racial bias of the officer performing the stop, and an interaction between this bias and the post-treatment window of 30 days. Here, we measure officer-level bias following the methods described in Section 5.3, but we no longer need to restrict our attention to 30 days before the stop. Instead, we can use all the pre-treatment available data. On average, we observe 385 stop decisions per officer. As usual, we present standard errors adjusted for clustering at the day and county levels. Officer bias is a generated regressor, which leads to potentially biased standard errors. To address this, we also present results with bootstrapped standard errors based on $1,000$ replications. Note that recent literature suggests that bootstrapped standard errors represent a conservative estimate (Hahn and Liao, 2019). Results reported in Columns 1 and 3 of Table 6 show that the effect of Trump rallies is three times as large for police officers whose estimated racial bias is one standard deviation above mean. In Columns 2 and 4, we show that these effects are qualitatively robust to the inclusion of officer-level fixed effects.

Consistent with the implications of dog-whistling, these results show that Trump rallies have a much larger effect on the behavior of police officers who are already amongst the most prejudiced against Black drivers. Our measures of bias are obtained from comparing officers within the same county. However, a large literature documents heterogeneity in racial bias across the US, which is partly inherited from differences in the prevalence of slavery (Acharya, Blackwell and Sen, 2019). As such, we may miss a large dimension of heterogeneity in the Trump effect across areas that vary in racial bias. We show the importance of this second source of heterogeneity in the next subsection.

6.3 Heterogeneity Across Counties

Our focus is now on racial resentment at the county level. We proxy racial resentment by county-average responses to the two racial resentment questions included in the 2012 and 2014 CCES (Schaffner and Ansolabehere, 2015) described in Section 3, as well as proxies of deep-seated racial animus inherited from the pre-Civil War era (number of slaves per capita 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 (lynchings and executions).

To estimate the heterogeneous effect of Trump rallies on police behavior across counties, we analyze the heterogeneous impact of Trump rallies by including in the estimation of Equation 4 an interaction term between $D_{c,t}^{(1,30)}$ and pre-determined county characteristics.[38] All variables measuring county characteristics are normalized to have a mean of 0 and standard deviation of 1. 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 7 (Panel A). Consistently across the different measures, we observe that the effect of Trump rallies is larger in areas with stronger and deeper-seated prejudice. The magnitude of the effect is large: the effect of Trump rallies on the probability of a Black stop is between 51% (Column 2) and 64% (Column 1) 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 (B or A). It is 36% higher in counties with one standard deviation above the mean share of slaves per capita in 1860 (Column 3), almost double in counties whose soil conditions are one standard deviation above mean cotton suitability (Column 4), 45% higher in counties with one standard deviation above the mean number of executions of Blacks (Column 5), and 39% higher in counties with one standard deviation above the mean number of lynchings (Column 6).

We also check that our results are specific to racial resentment rather than due to other county characteristics that could be correlated with our measures of resentment, such as ethnic fragmentalization. Panel B of Table 7 shows ethnic fragmentalization does not play a role (Column 1). Further, we show that other potential sources of heterogeneity, such as those stemming from political partisanship (as measured by the vote share for Obama in the 2012 Presidential elections in Column 2 of Table 7 or by sheriffs' political affiliation[39]),

[38] As these characteristics are pre-determined, they are absorbed in the county fixed effect.

[39] Using the data on political affiliation compiled in Thompson et al. (2020), we find that the coefficient associated with the interaction between POST-Trump and a dummy variable indicating that the sheriff is affiliated with the Democratic party is 0.0114 (standard error: 0.008), while the coefficient associated with the interaction between POST-Trump and a dummy variable indicating that the sheriff is affiliated with the Republican party is 0.0001 (standard error: 0.004) (the excluded category being non-elected

differences in income (Column 3 of Table 7), 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 influence the impact of Trump rallies on the expression of racial prejudice on the part of the police, regardless of whether we use the actual or instrumented measure of trade shocks (Columns 5 and 6). Another potential concern is that our measures of racial resentment are correlated with the share of Blacks in the county. Therefore, the differences in the effects of Trump rallies may be due to differences in the share of Blacks across these counties. To alleviate this concern, we replicate the results in Table 7 controlling for time trends interacted with the share of Blacks and obtain similar results. Results are shown in Table A8. The findings are unchanged.

So far, we have established that Trump rallies affect racially-directed behavior among the police. Consistent with dog-whistling predictions, the effect is only present among the most racist officers and in the most racist areas. While police behavior has a large impact on social welfare, one may wonder whether the effects are also present in the overall population. Given that the dog-whistle effect is expected to be stronger among more racist individuals, we may not be able to extrapolate our results to the broader population if police officers are a selected group. On the one hand, we might overestimate the effect of Trump rallies if police officers are positively selected among racially prejudiced individuals. On the other hand, we might underestimate the effect if police is negatively selected or if training and procedure prevent the expression of prejudice among police. In the next section, we use data from an online experiment to study the effect of Trump's speeches on racial prejudice in the population.

7. Experimental Study

In this section, we use the experimental study implemented by Newman et al. (2020) during the 2016 Trump campaign. The original paper studies the effect of Trump's campaign on the willingness to discriminate against Hispanics. To provide a direct test of the dog-whistling theory, we instead study the effect on prejudice against Black people.

To do so, we rely on experimental exposure to Trump's (in)famous racially inflammatory speech about "Mexico [...] sending people [...] bringing their problems, bringing drugs, bringing crime," and being "rapists".[40] We find that this treatment, and not other (non inflammatory) Trump's speeches or other candidates' speeches (including about immigration), radicalizes prejudice against Black people. Specifically, respondents who are initially already highly prejudiced become even further radicalized in their view that African Americans are *violent*. No effect is observed against other racial minorities nor

sheriffs).

[40] See footnote 13.

for other dimensions of prejudice.

Another advantage of this experiment is that we are able to illustrate a specific way through which dog-whistle works. Dog-whistling could operate through three main methods. First, the use of euphemisms. For example, in American politics, inner cities or welfare queens are implicit references that some individuals understand to describe Black people (Haney-Lopez, 2014). Second, the use of triggers. For example, references to drugs, crime, and rape trigger negative stereotypes that some individuals may hold about Black people. Third, through reputation. For example, the accumulation of the previous inflammatory, racist remarks may make part of the population interpret all speech through the lens of race. The experimental design and results highlight the role of triggers.

7.1 Data

The data comes from the study by Newman et al. (2020) and was provided by Collingwood (2020).[41] The authors conducted an online two-wave panel survey experiment using Amazon's Mechanical Turk platform. The experiment took place during the 2016 Trump campaign. Their investigation is thus perfectly well suited to validate, in a randomly selected sample of respondents, the results of our preceding analysis of police behavior during Trump's 2016 campaign. The study includes 1,287 adults in the first wave (conducted between March 19 and April 23, 2016) and 997 in the follow-up, which took place 3 days afterward. As is standard for M-Turk samples, the group is more left-leaning and more highly educated compared to a nationally representative sample, suggesting that the obtained estimates related to prejudice may provide a lower bound of the population treatment effect.

In addition to socio-demographic and political orientation information, the authors collected measures of racial prejudice against several groups (Blacks, Asians, Hispanics, and Whites) using a negative stereotype index from the 2008 American National Elections Studies (ANES). Prejudice was measured at baseline in Wave 1, thereby providing a measure of initial prejudice prior to the treatment. The authors then administered the treatment in Wave 2 and measured prejudice again. The published paper by Newman et al. (2020) focuses on how Trump's anti-Hispanic speech affects prejudice and discriminatory behavior against Hispanics. The authors only use the measure of initial prejudice against Hispanics as an independent variable in their analysis. Instead, we focus on prejudice against Blacks and use the measures of endline prejudice as the dependent variable to study the extent to which Trump's seemingly anti-Mexican rhetoric activated prejudice against Black people.

Their experimental treatment consisted of exposing respondents to political speeches that vary in their content (racially inflammatory or not) and protagonist (Jeb Bush,

[41] Replication data available at dataverse, accessed on 15 May 2020.

Hillary Clinton, or Donald Trump). Respondents were randomly assigned to one of six experimental conditions, which are reproduced in Figure A5 in the Appendix. In the control condition, respondents were neither exposed to Trump nor racially inflammatory speech. The primary treatment condition – "Trump Prejudice" – instead presented the respondents with the racially inflammatory remarks made by Trump against Mexican immigrants (the quote from Trump's announcement speech about Mexicans being criminals, drug dealers, and rapists quoted in footnote 13). This statement was – in all appearances – targeted against Mexican immigrants and also contained a strong reference to crime. Crucially, the study also differentiates prejudice along several dimensions: laziness, lack of intelligence, tendency for violence, or in the US illegally. For each dimension, respondents were asked to rate on a scale from 1 to 5 whether an adjective - either "lazy," "intelligent," "violent," or "here illegally" - describes the racial group. All the answers are rescaled so that 1 captures the lowest level of prejudice and 5 the highest. Given the connection of the statement to crime and to an outgroup that might activate fear, we specifically expect that exposure to the treatment activates the bias that African Americans are *violent*. This dimension of prejudice is also arguably the one that most influences how police perceive African Americans (LeCount, 2017). At baseline, the average opinion that African Americans are violent is 2.40 (s.d. 1.18), as opposed to 2.16 (s.d. 1.01) for Whites or 1.38 for Asians (s.d. 1.18).

To isolate the effect of racially inflammatory speech from the effect of simply mentioning immigration or the effect of Trump alone, the study includes two additional treatments. First, an "Immigration Prime" treatment featured a discussion of immigration by Jeb Bush as the Republican candidate. Second, the "Trump Prime" treatment showed Trump discussing campaign finance reform rather than immigration. The researchers also added two treatments measuring the extent to which other elite condoned or condemned Trump's racially inflammatory speech. While we have no prediction concerning how endorsement or condemnation by other political elite might impact the dog-whistle effect, we retain these last two treatment conditions in the analysis to preserve statistical power (the results are robust to excluding these treatments from the estimation sample, see Figures A7 to A9 in the Appendix).

7.2 Empirical Specification and Results

The study by Newman et al. (2020) focuses on attitudes towards Hispanics and uses as the dependent variable the acceptability of discriminatory behavior against this group, measured in a specific vignette experiment. Here, we use a different dependent variable not employed by the authors in their study. As we are interested in the expression of prejudice against African Americans, especially the bias that they are violent, we use as the dependent variable the endline measure of such prejudice, after the treatment.[42] We also control for initial prejudice, measured before the treatment, in all specifications.[43]

Dog-whistling resonates among the subgroups who hold the views that are being harnessed. We, therefore, hypothesize that the Trump Prejudice condition will only prompt bias among those who are already in the higher end of the distribution of prejudice in the population. To test this, we include an interaction between one's position in the prejudice distribution (e.g., above the median)[44] and the treatment condition. We also present the results of placebo specifications in which we use the same dimension of prejudice but against other racial groups and other dimensions of prejudice against Black people.

Newman et al. (2020) present the results of ordered logistic regression analysis. However, Ai and Norton (2003) highlight issues associated with estimating marginal effects associated with interaction terms in logit and probit models. For this reason, we use instead OLS estimation.

Our estimation equation is as follows:

$$PrejViol_{i2} = \alpha + \beta\, Treat_i{*}PrejViol_{i1} + \gamma PrejViol_{i1} + \theta\, Treat_i + \eta X_i + u_i, \tag{6}$$

$PrejViol_{it}$, for $t = \mathit{1}, \mathit{2}$, is the measure of prejudice that a racial group is violent declared by individual i at time t, where $t = \mathit{1}$ is the pre-treatment period and $t = \mathit{2}$ the post-treatment period. More precisely, $PrejViol_{i1}$ is a dummy variable that indicates whether the respondent's initial level of prejudice is above sample median. $Treat_i$ denotes the treatment condition for i. X_i is a vector of individual-level controls, all measured at baseline. We use the same set of individual controls as in Newman et al. (2020): age, education, gender, race (Black, Hispanic, Asian, or White), employment status, party identification, and politicization (i.e., whether the respondent pays attention to elections).

In Figure 3, we present the coefficients associated with each treatment condition, controlling for baseline prejudice and the full set of controls, but without including the interaction between treatment and baseline prejudice described in Equation 6. In other words, this represents the average effect of each treatment across all respondents in the sample. Regression results are presented in Appendix Table A9. None of the treatment conditions significantly affects the average prejudice against any racial group. As in Newman et al. (2020), we interpret these results as suggestive of the fact that equality

[42] The vignette adopted by Newman et al. (2020) to measure the acceptability of discriminatory behavior was only implemented for a Hispanic target of discrimination, not a Black one. By contrast, the endline prejudice was measured for all racial groups.

[43] Our regressions can thus be interpreted as difference-in-differences specifications, comparing within-subject differences in prejudice across randomly administered treatments.

[44] Running the specifications by differentiating respondents as above or below the mean gives identical results. In many cases, due to the categorical nature of the prejudice variable, the dummy variables for above/below median or above/below mean are identical.

and tolerance norms are too widely shared in the population[45] for us to detect an average effect of inflammatory speech.

Yet, among the most prejudiced individuals, the picture is strikingly different. In Figure 4, we display the coefficients associated with the interaction between each treatment condition and $PrejViol_{i1}$ in the estimation of Equation 6. The complete set of regression results are in Table A10 in the Appendix. The results show that respondents who are initially prejudiced that Blacks are violent become significantly further radicalized in their bias when exposed to Trump's racially inflammatory statement. The effect is positive for the Trump Prime treatment but only statistically significant for the Trump Prejudice condition. In other words, mere exposure to Trump does not significantly radicalize individuals in their prejudice that Blacks are violent; but being exposed to Trump's racially inflammatory rhetoric does. Furthermore, the result is only statistically significant for prejudice against Blacks: none of the interaction coefficients are significant for any other racial groups. In other words, Trump's racially inflammatory statements do not affect respondents' prejudice against Whites, Asians, or Hispanics; even among respondents who are particularly prejudiced against each of these groups. This finding is especially remarkable as Trump's statement is not – at least at a superficial level – directed against African Americans. The fact that we observe activation of prejudice against African Americans but not against Hispanics provides a direct validation of the dog-whistle effect. The fact that we observe an effect not merely of exposure to Trump but to Trump's inflammatory rhetoric highlights the role of triggers in generating the result. Since we only observe a significant increase in prejudice among the individuals who are already most prejudiced, we interpret our results as showing a radicalization of prejudice.

The magnitude of the effect is large. In the specification with the full set of individual controls, for respondents with above median baseline prejudice, exposure to Trump's inflammatory statement increases declared prejudice by 0.41 points, from a sample mean of endline prejudice of 2.28. This represents an 18% increase.

We provide further evidence that the effect is tied specifically to the activation of the prejudice that *Blacks* are *violent*. To this end, Figure 5 (full regression results in Table A11 in Appendix) presents the results of a similar analysis for the other dimensions of prejudice against Blacks measured in the survey: laziness, lack of intelligence, illegal presence in the US. We estimate a specification similar to Equation 6 but using measures of these different dimensions of prejudice at baseline and endline (instead of $PrejViol_{i1}$ and $PrejViol_{i2}$). None of the coefficients associated with the interaction terms between the treatment conditions and initial prejudice that Black people are lazy, lack intelligence, or are in the US illegally is statistically significant.[46] The effect of exposure to Trump's

[45]And perhaps especially in an M-turk sample of respondents that are more highly educated and more left-wing than the average population.

[46]We also verify that none of the treatment conditions has any significant effect on average.

racially inflammatory speech in aggravating the prejudice that Blacks lack intelligence is statistically significant at the 10 percent level, but not robust to the inclusion of individual controls (as displayed in Figure 5). Furthermore, in Figure A6 in the Appendix, we verify that the treatment does not make individuals with a high pre-existing prejudice that African Americans are *violent* more likely to express prejudice in these *other* dimensions. To conclude, we observe that exposure to Trump's racially inflammatory speech radicalizes prejudice against African Americans but is robust only for the prejudice that African Americans are violent.

Additional results displayed in Table 8 indicate that the effect is only significant, and larger in magnitude, for White respondents. For those in this group above the (sample) median prejudice, exposure to Trump's inflammatory statement increases declared prejudice by 0.55 points, a 24% increase with respect to the sample mean. By contrast, we observe no difference across education levels. The point estimate of the coefficient associated with the interaction of baseline prejudice and exposure to Trump's inflammatory statement is identical for college graduates (0.38) and those who did not complete a university education (0.37).[47]

Columns 7 to 9 of Table 8 show the effect of Trump's inflammatory speech along political lines. Although Republicans generally display higher prejudice associating Blacks with violence, far from all prejudiced individuals are Republican. Of the 20.90% of respondents in the sample who self-identify as Republican, 59.81% are above the sample median prejudice level, compared to 40.46% among the self-identified Democrats or Independents. Although this difference is statistically significant, its magnitude is far from a perfect sample split, where all Republicans would stand above-median prejudice and Democrats below. Moreover, Column 7 shows that Republicans are not more likely than Democrats or Independents to respond to the treatment. However, Columns 8 and 9 show that already-prejudiced Republicans increase their level of prejudice when exposed to Trump's racially inflammatory statements when other Republican elites condone such speech ("Trump Condone" treatment). This suggests that the attitude of the Republican leadership toward Trump may play an important role in potentially aggravating the effect of Trump's divisive speech on racial attitudes held by self-identified Republican voters. The magnitude of the effect of exposure to Trump's racially inflammatory speech among other political affiliations is less than half in magnitude but still statistically significant. In other words, all prejudiced individuals, regardless of their political affiliation, become radicalized in their prejudice that blacks are violent when exposed to Trump's inflammatory statements. For self-identified Republicans, the effect is larger in magnitude and influenced by the broader position of the Republican leadership.

[47]More generally, we do not observe a systematic difference in prejudice between college graduates and non-graduates: 42% of college graduates are above the sample median of prejudice, against 47% of non-graduates, a difference that is not statistically significant (P-value of two-sided difference in means: 0.18).

Overall, these results show that prejudiced individuals, especially when White and Republican, react to Trump's campaign inflammatory speech by becoming further radicalized in their prejudice in specifically one dimension: the belief that Blacks are violent.

An important question is whether the effect we observe is due to a simple increase in the willingness to express prejudice or an actual increase in prejudice. To an extent, exploiting other dimensions of anti-African American prejudice enables us to address this issue. Indeed, if the treatment simply liberated the expression of prejudice, respondents would be more likely to express any form of prejudice against African Americans. However, this is not the case. Trump's speech did not result in more expression of *any* kind of prejudice against Blacks, but the prejudice that Blacks are violent. This suggests that the effect may go beyond the liberation of racially prejudiced speech and reflect an increase in prejudice.

One explanation for our results is that talk of crime, rape, and drugs, or the evocation of a potentially threatening outgroup activates among the subgroup of most prejudiced Americans their long-standing and ingrained threatening stereotype: African Americans are responsible for violence and crime. This is all the more surprising that the outgroup evoked in the speech does not consist of African Americans but rather of foreign nationals (Mexicans). Simultaneously, this enables Trump to maintain deniability that his campaign is in any way anti-African American while also avoiding alienating those strongly attached to norms of equality and tolerance. Meanwhile, the most prejudiced voters understand these words differently – in a way that radicalizes their prejudice even further. Altogether, this evidence is consistent with the theoretical conceptualization of the dog-whistle effect.

8. Trump's Rally Speeches and Police Stops

Informed by the experimental findings, we go back to our police data and examine how the words included in the experimental inflammatory statement mediate the effect of a rally on police behavior when spoken at Trump's rallies. As predicted by dog-whistling theory, and corresponding to what we find in the online sample of respondents, we expect only the most prejudiced officers to be triggered. We rely on our measure of officer bias to examine the heterogeneous effect of speeches across officers.

To perform our analysis, we obtain the content of speeches at each campaign rally from several sources. We use the American Presidency Project (Peters and Woolley, 2020),[48] which we complement with data from the Trump Campaign Corpus Project.[49] We code how many times the potentially triggering (stem) words from the statement used in the

[48] https://www.presidency.ucsb.edu/documents/app-categories/elections-and-transitions/campaign-documents, accessed 10 June 2020. This dataset only contains data for about half of the campaign speeches we use in the analysis.

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

online experiment appear in a speech. We focus on Mexic, send, bring, crim, drug, rap.[50] In our estimating sample, these words are spoken 17.37 times per speech, on average.

We also include other bundles of words that are common topics in Trump's speeches. We do so in order to provide additional evidence that the mechanism operates through dog-whistling. We focus on five other common topics in Trump's speeches: (i) Clinton (Hilary, Clinton, email, lock) (mean of 7.8 words per speech), (ii) trade (China, trade, NAFTA) (13.6 words per speech), (iii) terrorism (ISIS, Syria, Iraq, terrorist, Afghanistan, Islam) (5.3 words per speech), (iv) job loss and manufacturing decline (busi(ness), job, manufactur, tax) (18.4 words per speech), (v) media and political corruption (rig, media, CNN, Washington, corrupt, swamp) (6.8 words per speech).

To estimate the heterogeneous effect of Trump rally speeches on more or less racially prejudiced officers, we estimate the following specification:

$$\begin{aligned} Black_{i,j,c,t} = \alpha_c + \theta_t + \gamma D_{c,t}^{(-\infty,-31)} + \eta D_{c,t}^{(0,0)} + \beta_1 D_{c,t}^{(1,30)} + \delta D_{c,t}^{(31,\infty)} + \eta \widehat{\lambda}_j \\ + \beta_2 D_{c,t}^{(1,30)} * \widehat{\lambda}_j + \beta_3 D_{c,t}^{(1,30)} * Words_{i,c,t} + \beta_4 D_{c,t}^{(1,30)} * \widehat{\lambda}_j * Words_{i,c,t} + \alpha_c \times t + u_{i,c,t} \end{aligned} \tag{7}$$

$Words_{i,c,t}$ 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 racial bias $\widehat{\lambda}_j$ are normalized so that they have mean 0 and standard deviation of 1 for the average stop.

To estimate this equation, we need information on the driver's race, officer id, and the speech of the rally. The resulting estimating sample consists of 74 counties for which we have data for 97 speeches and $3,737,766$ 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 officer with mean racial bias. $\beta_1+\beta_2$ capture the effect of a Trump rally that mentions the average number of words of interest, for officers with racial bias that is one standard deviation above mean. $\beta_1+\beta_3$ captures the effect of a Trump rally with one standard deviation above mean of $Words_{i,c,t}$, for officers with mean racial bias. $\beta_1+\beta_2+\beta_3+\beta_4$ captures the effect of a Trump rally with one standard deviation above mean of $Words_{i,c,t}$ for officers whose racial bias is one standard deviation above 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, in addition to this effect, they may react more

[50] We do not include "people" or "problems", which appear in the inflammatory statement but are common words that appear many times in Trump's speeches in other, unrelated, and non-inflammatory contexts.

to triggering words, as captured by our main parameter of interest β_4. The prediction that we can derive from dog-whistling theory is that $\beta_4 > 0$ for triggering words. For non-triggering words, we should not observe a systematic sign pattern for β_4.

To test the dog-whistling theory, we need to determine how to characterize triggering words. Potentially, many different words could be triggering. Informed by the experiment, we choose the words contained in the inflammatory experimental statement, which, we have shown, trigger racially prejudiced respondents. β_2 could be positive if there is an effect of Trump speeches that operate beyond dog-whistling or because there exist other triggering words. Otherwise, β_2 might be zero.

Estimation results of Equation 7 are displayed in Table 9. We present results with standard errors clustered at the county and day level, as well as bootstrapped standard errors to deal with potential issues arising from the fact that officer bias is a generated variable. Panel A includes the results based on our first measure of officer racial bias (warnings vs. more severe outcomes) and Panel B for the second (arrests vs. less severe outcomes). Column 1 shows that the probability of a Black stop by an officer with average bias does not increase after a speech. By contrast, the probability of stop by an officer whose bias is one standard deviation above mean increases after a speech, and even more so when the speech contains the triggering words included in the experiment studied in Section 7 (i.e., β_2 and β_4 are both positive and statistically significant). Columns 2 and 3 show that the probability of a Black stop increases for more racially biased officers after a rally, but there is no additional effect of rally speeches that include more references to Clinton or to trade. Depending on the measure of bias, the same is true for terrorism-related words (Column 4). For job loss and manufacturing decline (Column 5) and media and political corruption (Column 6), β_4 is negative and statistically significant. This suggests that these two topics are the least triggering for racially biased officers: speeches with one standard deviation above mean count of these words have a lower effect than speeches with mean word count. This could be explained, for example, if speeches that contain a high count of words related to job loss/manufacturing or media/political corruption contain fewer other words that could potentially trigger racially biased officers. The results in Columns 2 to 6 additionally show that the positive and significant coefficient β_4 estimated in Column 1 reflects the effect of the triggering words rather than the length of the speeches.

These results add two important pieces of evidence that concord with our experimental results and validate our hypothesized mechanism of the dog-whistle effect. First, they confirm that the effect of Trump's speeches on prejudice against Blacks is triggered by mentions of specific words that do not explicitly target Blacks but stroke fear, either by merely mentioning crime or by invoking a potentially threatening outgroup. Second, the effect hinges on pre-existing attitudes. In an online random sample of respondents as among police officers, more prejudiced individuals react more to triggering words.

9. Conclusion

In this paper, we show how Trump's political campaign radicalized racial prejudice against African Americans as well as its expression in a critical and potentially violent dimension: police behavior. Our estimates suggest that Trump's campaign events led to 5,470 additional traffic stops of Black drivers in our sample and more than 12,000 overall in the month following Trump rallies. These stops would not have occurred otherwise and were not justified by a serious offense or one that should automatically trigger a stop. Using data from an online survey experiment conducted during Trump's 2016 campaign, we confirm that the effect is specific to prejudice against Black people, particularly the bias that they are violent. Consistent with our interpretation that Trump's campaign activated racial stereotypes, the effect is stronger, or only present, among individuals that were initially more racist and in areas with a legacy of racial animus and violence. Overall, our results show how politicians can radicalize deeply ingrained stereotypes through language that, at least superficially, appears either innocuous or unrelated.

Traffic stops represent, by far, the majority of police-citizen interactions in the US. More than 20 million people are pulled over by the police every year in the US (Davis, Whyde and Langton, 2018), with Black drivers over-represented by a factor of two compared to their proportion in the population (our estimate in Section 3). Although fatal encounters between police officers and Black civilians capture greater media attention, the over-enforcement of minor infractions and the kind of unjustified stops by the police that we document in this paper highlight a daily and generalized expression of discrimination against minorities, with potentially dire consequences.[51] Ensuing feelings of discrimination and under-representation undermine minorities' faith in the police and public institutions at large.[52] This produces a vicious circle, whereby daily and unjustified harassment reduces trust in the police and leads citizens to shy away from relying on law enforcement and instead to 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 all public institutions. Recent research shows how historical discrimination and violence against Black people is associated with lower voter registra-

[51] To this regard, 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 then in a later paper Durlauf (2006) concludes that any benefits from profiling have not been identified, while the harm to those who are innocent and stopped is high.

[52] 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).

tion by Black voters (Williams, 2020) and how fatal police shootings undermine racial minorities' educational achievement (Ang, 2021) as well as civic engagement (Ang and Tebes, 2021). Estimating the impact of discriminatory police behavior on voting behavior is thus a crucial direction for future research.

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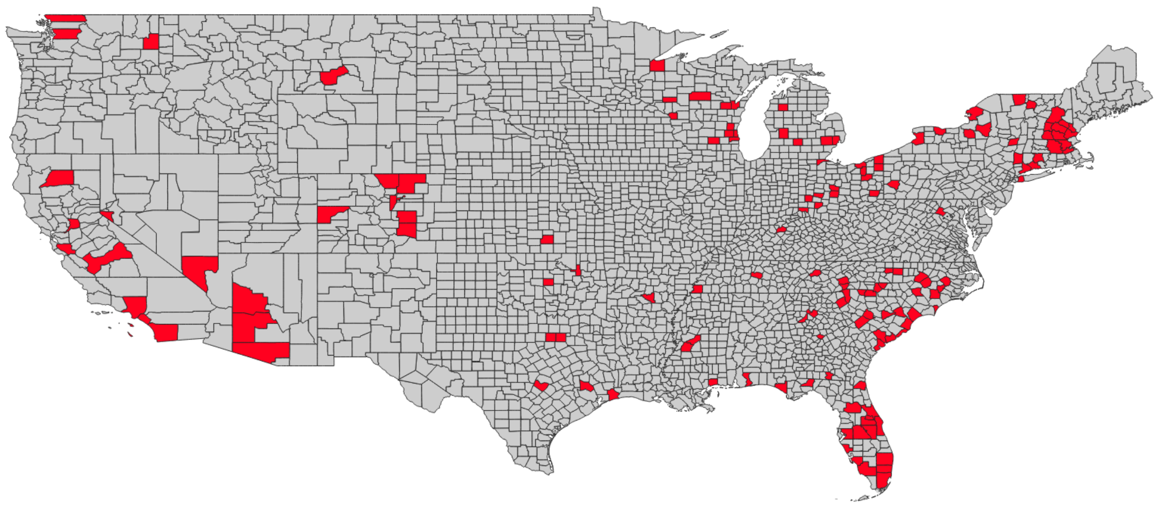
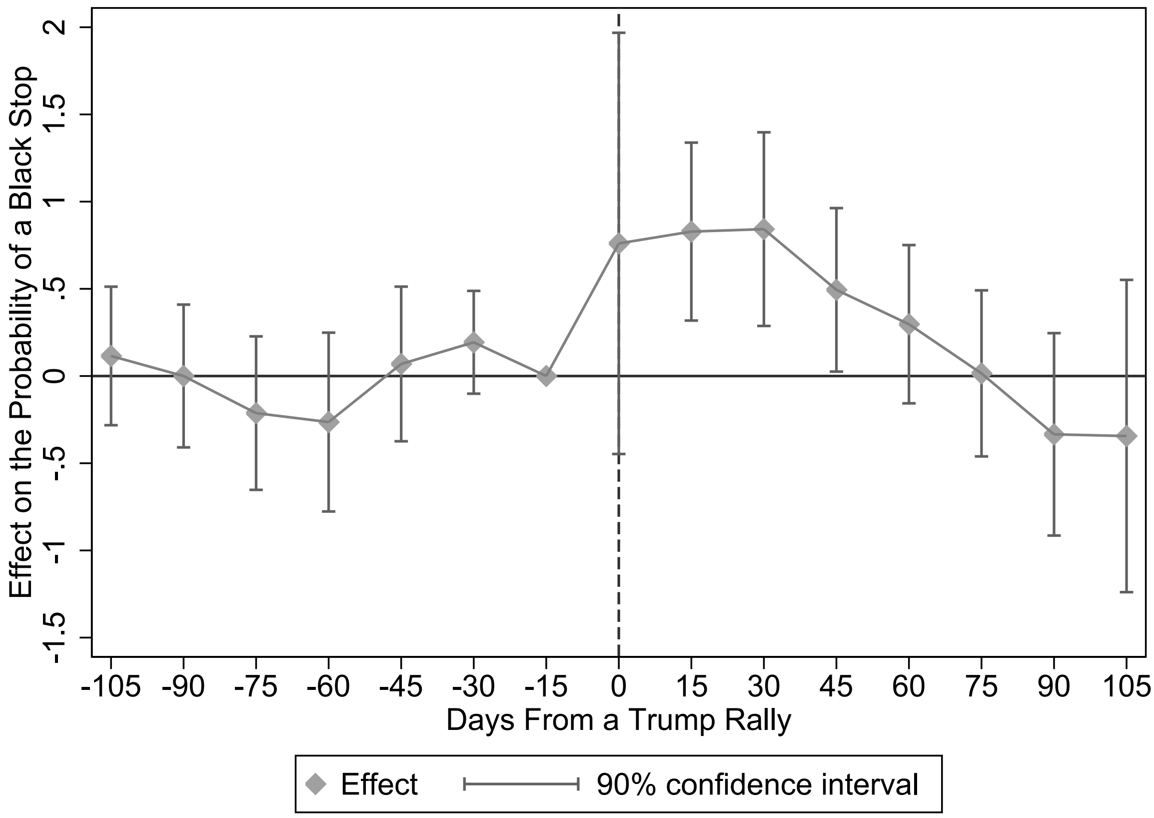


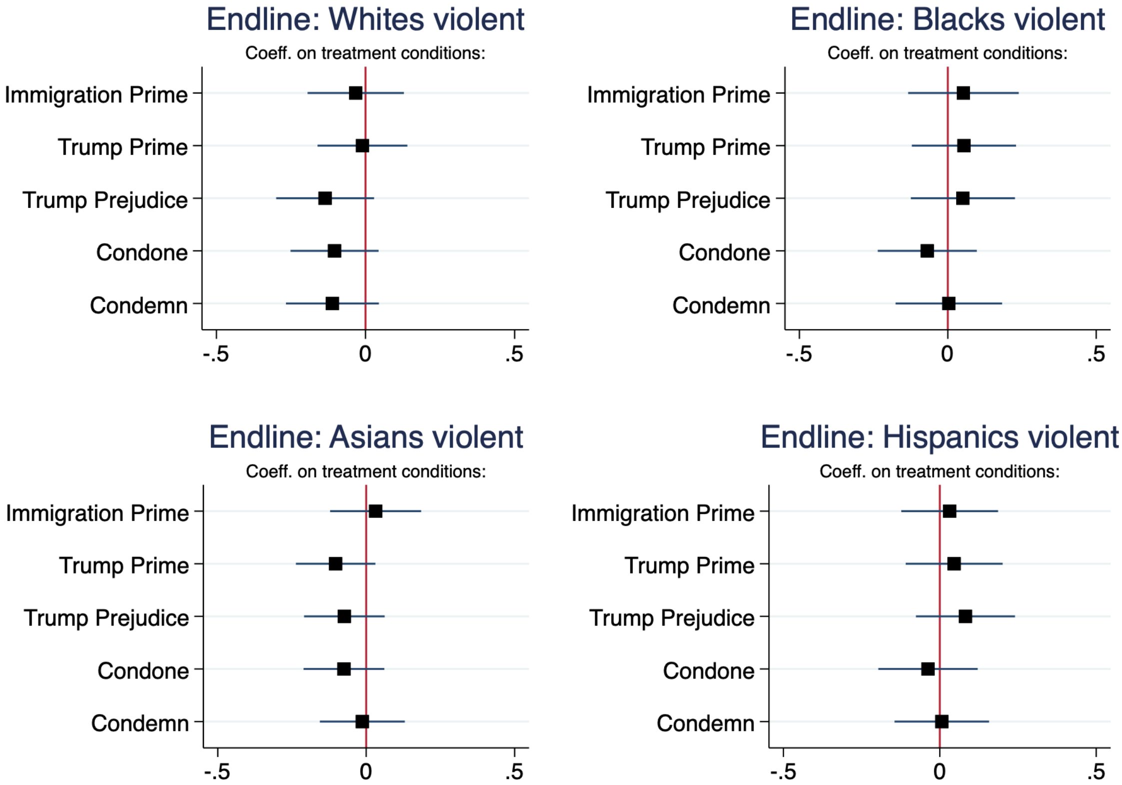
Notes: This map shows the 142 counties with 2015-2016 Trump's campaign events for which we have data on police stops.

Figure 1: Counties with campaign events and police stops



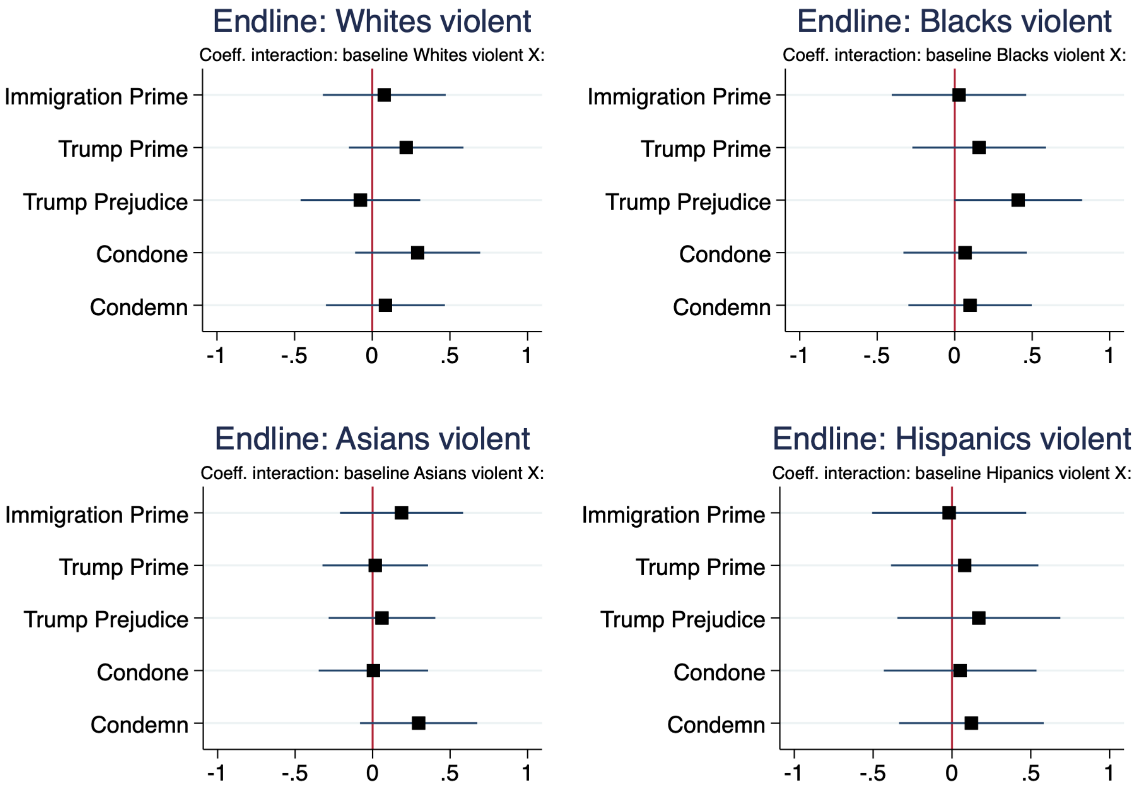
Notes: The figure plots OLS coefficients with 95% confidence intervals (vertical lines). The plotted coefficients are the β_τ coefficients associated with each 15 day window, described in Equation 5. Standard errors are corrected for two-way clustering at the county and day level.

Figure 2: Impact of Trump rallies on the probability of a Black stop: Event-study results



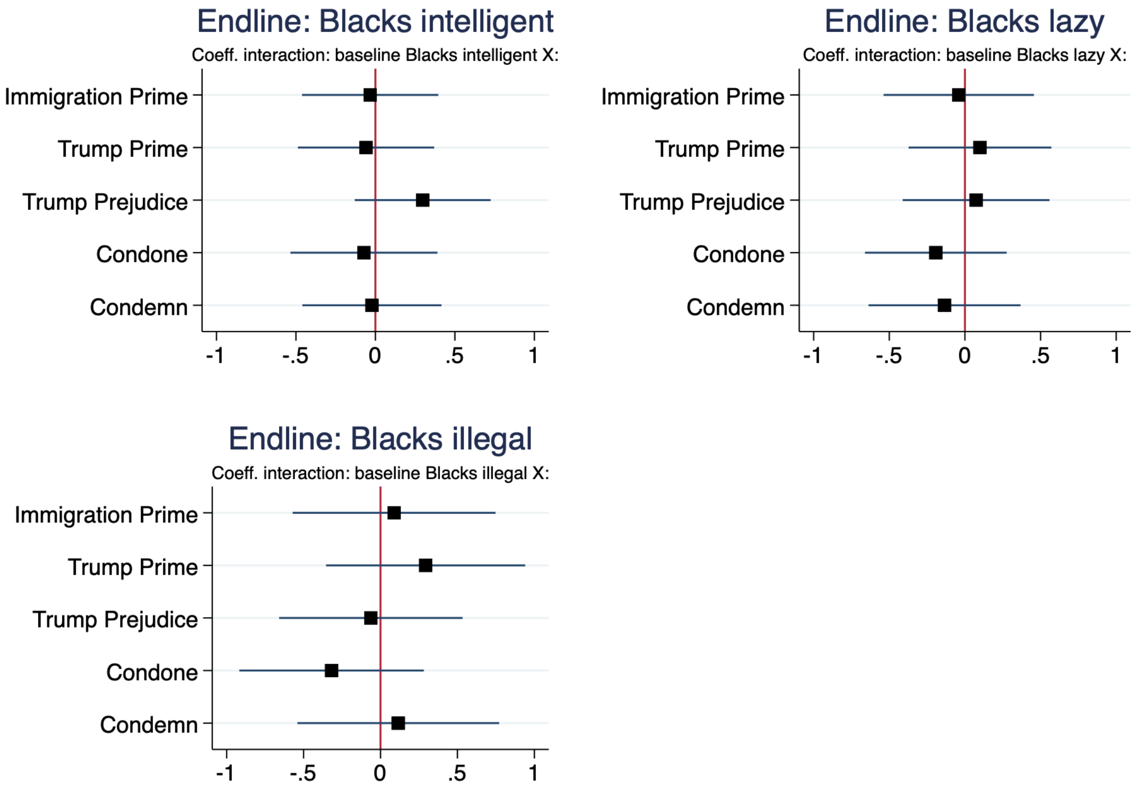
Notes: The figure plots OLS coefficients with 95% confidence intervals. The plotted coefficients are the coefficients associated with each treatment condition, as indicated on the vertical axes (see Table A5 for more detail on each treatment), controlling for whether the respondent is above sample median baseline prejudice and for the full set of controls, but without including the interaction effect described in Equation 6. Each panel is a separate regression in which the dependent variable is the endline prejudice that each racial group (as indicated in the header of each panel) is violent. Standard errors are corrected for heteroskedasticity. See Table A9 for the full regression results.
The Figure shows that the treatments do not affect prejudice against any racial group for the average respondent.

Figure 3: Violent Prejudice: Main effects of treatment conditions on endline prejudice



Notes: The figure plots OLS coefficients with 95% confidence intervals. The dependent variable is the endline prejudice that each racial group (as indicated in the header of each panel) is violent. The plotted coefficients are the coefficients associated with the interaction between each treatment condition (as indicated on the vertical axes) and an indicator variable for being above sample median prejudice that each racial group is violent. All regressions control for the indicator variable separately, for the main effect of the treatment condition and for the full set of controls described in Equation 6. Standard errors are corrected for heteroskedasticity. See Table A10 for regression results. We display results for the last two treatments ("Condone" and "Condemn") for completeness, although we do not have predictions for either of these treatments. See Figure A8 when these two treatments are excluded from the estimation sample.
The figure shows that Trump's inflammatory speech activates the prejudice that Black people are violent among prejudiced respondents. By contrast, it does not affect the prejudice that any other racial group (as indicated in the panel header) is violent, even among respondents who are initially prejudiced against these racial groups.

Figure 4: Trump's inflammatory speech activates the prejudice that Black people are violent among prejudiced respondents



Notes: See notes to Figures 4 as well as Table A11 for regression results. We focus here only on prejudice against Black people in dimensions other than violence, which are measured in the survey. We examine the prejudice that Black people lack intelligence, are lazy, or are in the US illegally. The Figure shows that none of the treatment conditions activates prejudice against Black people along these dimensions, even for respondents who are highly prejudiced to start with.

Figure 5: Trump's inflammatory speech does not activate other dimensions of anti-Black prejudice

Table 1: Summary Statistics

Variable	N	Mean	SD
Black	11,931,161	20.39	40.29
White	11,931,161	51.50	49.98
Hispanic	11,931,161	24.13	42.79
Warning	7,071,236	30.18	45.90
Arrest	7,071,236	4.70	21.16
Collision	5,387,948	7.22	25.88
Seat Belt	5,387,948	1.76	13.14
Radar Triggered	5,387,948	0.95	9.68
Expired Tag	5,387,948	0.60	7.70
Automatic Stops	5,387,948	10.52	30.68
Other Stops	5,387,948	89.48	30.68
Black-Warning	7,071,236	6.87	25.29
Black-Arrest	7,071,236	0.81	8.95
Black-Collision	5,387,948	0.77	8.73
Black-Seat Belt	5,387,948	0.54	7.36
Black-Radar Triggered	5,387,948	0.30	5.46
Black-Expired Tag	5,387,948	0.13	3.64
Black-Automatic Stop	5,387,948	1.74	13.09
Black-Other Stop	5,387,948	18.94	39.19
POST-Trump	11,931,161	0.05	0.21

Notes: The Table shows some summary statistics. The data for variables from Black to Black-Other Stops is constructed from Stanford Open Policing Project and POST-Trump is constructed from Appleman (2019). The unit of observation is a police stop. Black, White, and Hispanic are equal to one if the stopped driver is Black, White, and Hispanic, respectively, and zero otherwise. Warning and Arrest are equal to one if the stop resulted in a warning and arrest, respectively. Collision, Seat Belt, Radar Triggered, and Expired Tag are equal to one if the stop was due to vehicle collision, driver not wearing a seat belt, traffic control radar was triggered, and vehicle had an expired tag, respectively, and zero otherwise. Automatic Stop is equal to one if the driver was stopped due to either collision, not wearing a seat belt, radar was triggered or vehicle had expired tag, and zero otherwise. Other Stops is equal to one if the stop was not automatic, and zero otherwise. Black-Warning and Black-Arrest are are equal to one if the driver is Black and the stop resulted in a warning or an arrest, respectively. Black-Collision, Black-Seat Belt, Black-Radar Triggered, and Black-Expired Tag are equal to one if the driver is Black and the stop was due to vehicle collision, driver not wearing a seat belt, traffic control radar was triggered, and vehicle had an expired tag, respectively, and zero otherwise. Black-Automatic Stop and Black-Other Stop are defined analogously. POST-Trump is equal to one in the 30 days window after a Trump rally and zero otherwise ($D_{c,t}^{(1,30)}$).

Table 2: Impact of Trump rallies on the probability of a Black stop

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
VARIABLES	100 x $\mathbb{P}$(Black Stop)= 1									
POST-Trump	0.943***	0.982***	0.941***	0.857***	0.812***	0.733***	0.689***	0.642***	0.586***	0.548**
	(0.298)	(0.264)	(0.268)	(0.233)	(0.224)	(0.205)	(0.200)	(0.197)	(0.196)	(0.213)
Observations	11,931,161	11,931,161	11,931,161	11,931,161	11,931,161	11,931,161	11,931,161	11,931,161	11,931,161	11,931,161
R-squared	0.091	0.091	0.091	0.091	0.091	0.091	0.091	0.091	0.091	0.091
County FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Day FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
County Trend	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Window	10	20	30	40	50	60	70	80	90	100

Notes: The Table shows the results of OLS estimation of Equation 4. The unit of observation is a police stop. The dependent variable is a dummy equal to one if the driver who is stopped is Black and zero otherwise. The main independent variable is POST-Trump, which is equal to one for the days after a Trump rally in our treatment window, and is zero otherwise. We vary the size of this treatment window in increments of 10 days from 10 to 100 days in Column 1 to 10. That is, POST-Trump is equal to $D_{c,t}^{(1,10)}$, $D_{c,t}^{(1,20)}$, ... , and $D_{c,t}^{(1,100)}$ in Columns 1 to 10. All estimations include county and day fixed effects, and county linear time trends. 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)
	Stop of a Black driver lead/due to:						
	Arrest	Collision	Radar Triggered	No Seat Belt	Expired Tag	Automatic Stops	Other Stops
POST-Trump	-0.007	0.054	0.005	-0.025	-0.006*	0.028	0.655*
	(0.054)	(0.056)	(0.035)	(0.057)	(0.003)	(0.099)	(0.332)
Observations	7,071,236	5,387,948	5,387,948	5,387,948	5,387,948	5,387,948	5,387,948
R-squared	0.028	0.011	0.048	0.022	0.011	0.019	0.129

Panel B	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Stop of a Non-Black driver lead/due to:						
	Arrest	Collision	Radar Triggered	No Seat Belt	Expired Tag	Automatic Stops	Other Stops
POST-Trump	-0.307	-0.012	-0.019	-0.066	-0.010	-0.109	-0.574*
	(0.338)	(0.277)	(0.050)	(0.083)	(0.012)	(0.307)	(0.307)
Observations	7,071,236	5,387,948	5,387,948	5,387,948	5,387,948	5,387,948	5,387,948
R-squared	0.201	0.072	0.079	0.045	0.033	0.044	0.084

Notes: See notes to Table 2. POST-Trump is equal to $D_{c,t}^{(1,30)}$. Panel A studies the outcome and reason for a stop of a Black driver. Panel B studies the outcome and reason for a stop of a non-Black driver. Columns 1 shows the probability that a traffic stop leads to the arrest of the driver. Columns 2 to 5 show the probability that a stop of driver is due to: a collision, a radar trigger, seat belt violation, or an expired tag. Column 6 considers these four reasons for stop together (collision, radar, seat belt violation, and tag violation), and Column 7 considers all other reasons for stops. Column 1 uses the sample of stops for which information on the outcome of a stop is given ($7,071,236$ stops), and Columns 2 to 7 use the sample of stops for which information on the reason for a stop is given ($5,387,948$ stops). All estimations include county and day fixed effects, and county linear time trends. 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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	Tot. Stops	100 x $\mathbb{P}(.)$					
		Black Stop= 1			White Stop= 1	Hispanic Stop= 1	Asian Stop= 1
POST-Trump	0.016	0.292**	0.908**	0.468***	-0.376	-0.544	-0.029
	(0.031)	(0.141)	(0.354)	(0.154)	(0.321)	(0.347)	(0.078)
Observations	144,119	6,635,064	7,911,762	8,272,885	11,931,161	11,931,161	11,931,161
R-squared	0.953	0.154	0.098	0.107	0.166	0.188	0.028
Sample	County-Day	Stops	Stops	Stops	Stops	Stops	Stops
Additional FE	None	Officer-FE	Hour-County	State-PD	None	None	None

Notes: See notes to Table 2. POST-Trump is equal to $D_{c,t}^{(1,30)}$. Column 1 shows results of a specification at the county-day level using the total number of stops as the dependent variable. Columns 2 to 7 use stop-level data. The dependent variable is probability of a Black stop in Columns 2 to 4. Column 2 includes officer-level fixed effects. Column 3 includes hour of the day at which the stop was performed by county fixed effects. Column 4 restricts the estimation sample to stops by state troopers. In Columns 5 to 7, the dependent variables are: the probability of a White stop, Hispanic stop, and Asian stop, respectively. All estimations include county and day fixed effects, and county linear time trends. 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: Impact of Clinton and Cruz rallies on the probability of a Black stop

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A	100 x $\mathbb{P}$(Black Stop= 1)									
POST-Clinton	-0.787*	-0.373	-0.105	0.103	0.138	0.216	0.321	0.317	0.329	0.375
	(0.435)	(0.436)	(0.378)	(0.351)	(0.328)	(0.309)	(0.305)	(0.308)	(0.325)	(0.319)
Observations	11,687,120	11,687,120	11,687,120	11,687,120	11,687,120	11,687,120	11,687,120	11,687,120	11,687,120	11,687,120
R-squared	0.176	0.176	0.176	0.176	0.176	0.176	0.176	0.176	0.176	0.176
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel B	100 x $\mathbb{P}$(Black Stop= 1)									
POST-Cruz	0.133	-0.287	0.110	0.232	0.214	0.261	0.260	0.264	0.214	0.316
	(0.303)	(0.249)	(0.244)	(0.251)	(0.197)	(0.216)	(0.219)	(0.278)	(0.221)	(0.239)
Observations	4,713,907	4,713,907	4,713,907	4,713,907	4,713,907	4,713,907	4,713,907	4,713,907	4,713,907	4,713,907
R-squared	0.086	0.086	0.086	0.086	0.086	0.086	0.086	0.086	0.086	0.086
County FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Day FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
County Trend	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Window	10	20	30	40	50	60	70	80	90	100

Notes: See notes to Table 2. We replicate the analysis reported in Table 2 (estimation of Equation 4) for the rallies held by Hillary Clinton during the Democratic primary and the 2016 presidential campaign, or by Ted Cruz in 2015-2016 for the Republican nomination. The main independent variable is POST-Clinton (Panel A) or POST-Cruz (Panel B), which is equal to one for days after a Clinton rally (Panel A) and Cruz rally (Panel B) in our treatment window, and is zero otherwise. We vary the size of this treatment window in increments of 10 days from 10 to 100 days in Column 1 to 10. All estimations include county and day fixed effects, and county linear time trends. Standard errors are two-way clustered at the county and day level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Role of officer bias in the effect of Trump rallies on the probability of a Black stop

	(1)	(2)	(3)	(4)
VARIABLES	100 x $\mathbb{P}$(Black Stop= 1)			
POST-Trump	0.086	0.275*	0.088	0.276*
	(0.279)	(0.139)	(0.281)	(0.139)
	(0.099)	(0.100)	(0.105)	(0.106)
Officer Bias * POST-Trump	0.085***	0.096***	0.096***	0.076***
	(0.028)	(0.023)	(0.013)	(0.012)
	(0.048)	(0.048)	(0.045)	(0.045)
Observations	4,764,450	4,764,450	4,764,450	4,764,450
R-squared	0.095	0.144	0.095	0.144
County FE	YES	YES	YES	YES
Day FE	YES	YES	YES	YES
CountyXDay	YES	YES	YES	YES
Officer FE	NO	YES	NO	YES
Bias Measure	Method 1	Method 1	Method 2	Method 2

Notes: The Table shows the results of OLS estimation of Equation 4 with the addition of an interaction term between POST-Trump, which is equal to $D_{c,t}^{(1,30)}$, and the officer's racial bias against Black drivers (controlling for officer's racial bias). 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. All estimations include county and day fixed effects, and county linear time trends. Standard errors (shown in parenthesis) are constructed using two different methods: the first ones are two-way clustered at the county and day level, and the second ones are nonparametric bootstrap estimation of standard errors, with replacement, with 1,000 replications. The statistical significance depicted is based on clustered standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Role of local characteristics in the effect of Trump rallies on the probability of a Black stop

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	100 x $\mathbb{P}$(Black Stop= 1)					
	Panel A: Variables related to race					
POST-Trump	0.808***	0.766**	0.806***	0.787**	0.786***	0.823***
	(0.295)	(0.309)	(0.263)	(0.303)	(0.276)	(0.260)
POST-Trump * X	0.517***	0.389**	0.293*	0.602*	0.304*	0.368*
	(0.154)	(0.179)	(0.160)	(0.330)	(0.175)	(0.189)
Observations	11,931,161	11,931,161	11,931,161	11,931,161	11,931,161	11,931,161
R-squared	0.090	0.090	0.090	0.090	0.090	0.090
X=	Racial Resentment-A	Racial Resentment-B	Slaves p.c. 1860	Cotton Suitability	Executions	Lynchings
County FE	YES	YES	YES	YES	YES	YES
Daily FE	YES	YES	YES	YES	YES	YES
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel B: Other variables					
POST-Trump	0.896***	0.813***	0.821***	0.784***	0.760***	0.794***
	(0.307)	(0.298)	(0.290)	(0.255)	(0.255)	(0.259)
POST-Trump * X	0.227	0.140	0.118	0.094	-0.010	-0.081
	(0.172)	(0.193)	(0.172)	(0.188)	(0.182)	(0.160)
Observations	11,931,161	11,931,161	11,451,866	11,931,161	11,805,287	11,805,287
R-squared	0.090	0.090	0.086	0.090	0.090	0.090
X=	County Racial-HHI	County DEM share'12	County HH income	County College	China Shock	China Shock-IV
County FE	YES	YES	YES	YES	YES	YES
Daily FE	YES	YES	YES	YES	YES	YES

Notes: The Table shows the results of OLS estimation of Equation 4 with the addition of an interaction term between POST-Trump, which is equal to $D^{(1,30)}_{c,t}$, and the following predetermined county characteristics: two measures of racial resentment from the 2012 and 2014 CCES (Schaffner and Ansolabehere, 2015), slaves per capita in 1860, soil suitability for growing cotton, executions of Black people, lynchings of Black people (Panel A); racial HHI diversity index, vote share of Obama in 2012 presidential election, median household income, share of college graduates, the China import competition shock from Autor, Dorn and Hanson (2013), and the instrumental variable for the China import competition shock from Autor, Dorn and Hanson (2013). All 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 and predetermined county characteristics specific linear trends. Standard errors are two-way clustered at the county and at the day level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Experimental Results: Marginal effects of treatment conditions on endline prejudice that Black people are violent among subgroups of respondents

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent variable:	Endline prejudice African Americans are violent								
Sample	**Whole**	**Whites**	**Non-Whites**	**Whole**	**College Grads**	**Non-Grads**	**Whole**	**Republican**	**Indep. or Demo.**
Interaction between Column *subheader* and:	*White*	*>med prej.*	*>med prej.*	*College grad*	*>med prej.*	*>med prej.*	*Republican*	*>med prej.*	*>med prej.*
Immigration Prime	0.039	−0.071	0.391	−0.328	0.029	−0.058	0.088	0.368	−0.064
	(0.337)	(0.241)	(0.565)	(0.243)	(0.301)	(0.343)	(0.300)	(0.481)	(0.257)
Trump Prime	0.195	0.040	0.897	−0.079	0.324	−0.146	0.133	0.215	0.195
	(0.339)	(0.238)	(0.612)	(0.249)	(0.290)	(0.346)	(0.306)	(0.510)	(0.258)
Trump Prejudice	−0.030	0.547**	0.269	0.256	0.383	0.365	0.269	0.709	0.370
	(0.282)	(0.236)	(0.485)	(0.237)	(0.293)	(0.303)	(0.288)	(0.445)	(0.248)
Trump Condone	−0.018	0.062	0.289	−0.046	0.213	−0.158	0.016	0.879*	−0.128
	(0.285)	(0.224)	(0.497)	(0.227)	(0.279)	(0.297)	(0.311)	(0.470)	(0.223)
Trump Condemn	0.390	0.029	0.418	−0.105	−0.024	0.219	0.221	0.356	0.003
	(0.290)	(0.226)	(0.486)	(0.229)	(0.281)	(0.291)	(0.289)	(0.469)	(0.232)
White	−0.188	—	—	—	—	—	—	—	—
	(0.205)								
> median prejudice	—	1.115***	0.860**	—	1.084***	1.122***	—	0.807**	1.148***
		(0.168)	(0.371)		(0.211)	(0.215)		(0.315)	(0.174)
College Grad	—	—	—	0.263	—	—	—	—	—
				(0.197)					
Republican	—	—	—	—	—	—	0.093	—	—
							(0.236)		
Individual controls	YES	YES	YES	YES	YES	YES	YES	YES	YES
Main effect: treatment conditions	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	996	786	210	996	593	403	996	209	787
R^2	0.098	0.376	0.324	0.108	0.362	0.366	0.103	0.340	0.326

Notes: This table shows that exposure to Trump's inflammatory speech does not differently affect Whites and Non-Whites (Col. 1), college educated or not (Col. 4), or Republican or not (Col. 7). However, *prejudiced* Whites become even more prejudiced when exposed to the treatment (Col. 2). The effect of exposure to the treatment is not at all moderated by college education, even among prejudiced individuals (Col. 5 and 6). Prejudiced Republicans, and to a lesser extent prejudiced Independents or Democrats, also become even more prejudiced when exposed to the treatment, but the effects are not statistically significant (Col. 8 and 9). The table reports OLS estimation results with a constant. All regressions control for the main effects of each treatment condition. Individual controls are, as in all specifications: age, education, gender, race (African American, Hispanic, Asian, or White in Columns 4 to 9; just White or non-White in Column 1, omitted in Columns 2 and 3), employment status, party identification (omitted in Columns 7 to 9), and politicization (i.e. whether the respondent pays attention to elections). Standard errors corrected for heteroskedasticity are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

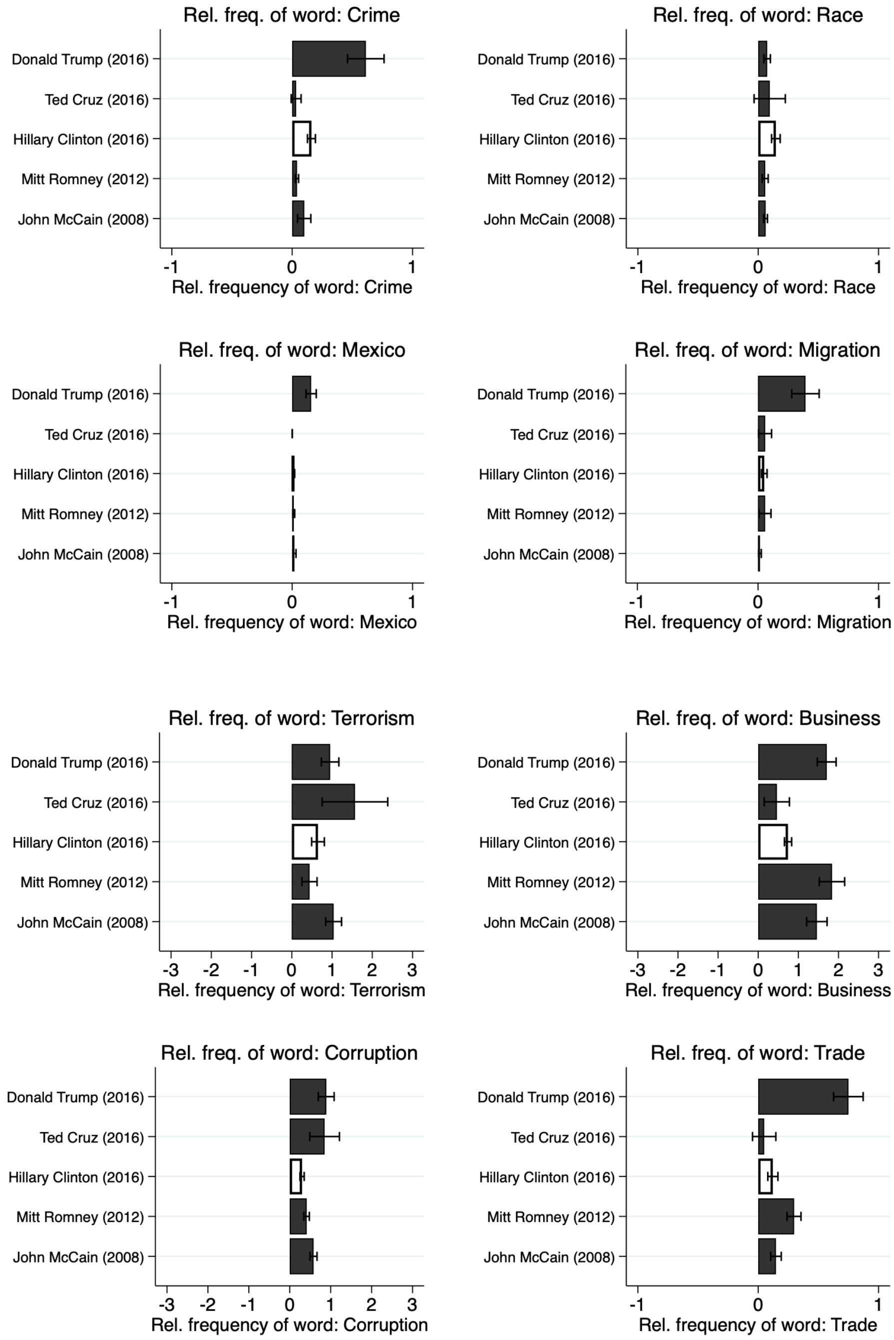
Table 9: Effect of triggering words on racially biased officers

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A			100 x $\mathbb{P}$(Black Stop= 1)			
POST-Trump	0.222	0.356	0.256	0.313	0.324	0.302
	(0.351)	(0.313)	(0.338)	(0.281)	(0.308)	(0.312)
	(0.125)	(0.127)	(0.127)	(0.125)	(0.125)	(0.125)
POST-Trump * Words	0.146	0.150	0.177	-0.012	-0.060	-0.317
	(0.166)	(0.492)	(0.305)	(0.364)	(0.234)	(0.345)
	(0.078)	(0.176)	(0.121)	(0.136)	(0.140)	(0.176)
POST-Trump * λ_j	0.098***	0.095***	0.096***	0.125***	0.057**	0.086***
	(0.028)	(0.028)	(0.028)	(0.040)	(0.028)	(0.028)
	(0.048)	(0.047)	(0.052)	(0.053)	(0.052)	(0.049)
POST-Trump * Words * λ_j	0.082***	-0.008	0.019	0.168	-0.249***	-0.121***
	(0.008)	(0.019)	(0.030)	(0.137)	(0.051)	(0.018)
	(0.045)	(0.066)	(0.092)	(0.122)	(0.109)	(0.062)
Observations	3,737,766	3,737,766	3,737,766	3,737,766	3,737,766	3,737,766
R-squared	0.109	0.109	0.109	0.109	0.109	0.109
	(1)	(2)	(3)	(4)	(5)	(6)
Panel B			100 x $\mathbb{P}$(Black Stop= 1)			
POST-Trump	0.224	0.361	0.259	0.317	0.328	0.305
	(0.352)	(0.314)	(0.339)	(0.289)	(0.308)	(0.312)
	(0.136)	(0.123)	(0.129)	(0.125)	(0.125)	(0.125)
POST-Trump * Words	0.146	0.155	0.175	-0.015	-0.059	-0.321
	(0.141)	(0.494)	(0.306)	(0.363)	(0.233)	(0.345)
	(0.074)	(0.174)	(0.117)	(0.139)	(0.141)	(0.176)
POST-Trump * λ_j	0.115***	0.113***	0.109***	0.135***	0.079***	0.108***
	(0.018)	(0.016)	(0.016)	(0.020)	(0.017)	(0.017)
	(0.046)	(0.047)	(0.049)	(0.049)	(0.050)	(0.047)
POST-Trump * Words * λ_j	0.103***	0.027	0.003	0.132***	-0.211***	-0.095***
	(0.014)	(0.020)	(0.016)	(0.039)	(0.018)	(0.017)
	(0.042)	(0.061)	(0.091)	(0.118)	(0.111)	(0.058)
Observations	3,737,766	3,737,766	3,737,766	3,737,766	3,737,766	3,737,766
R-squared	0.109	0.109	0.109	0.109	0.109	0.109
Mean	17.37	7.75	13.56	5.34	18.43	6.78
Words	DRUG	HILARI	CHINA	ISI	BUSI	RIG
	RAP	CLINTON	TRADE	IRAQ	JOB	CNN
	CRIM	EMAIL	NAFTA	SYRIA	TAX	SWAMP
	MEXIC	LOCK		TERRORIST	MANUFACTUR	MEDIA
	PROBLEM			AFGHANISTAN		CORRUPT
	BRING			ISLAM		WASHINGTON
	SEND					

Notes: The Table shows the results of OLS estimation of Equation 7. 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. The words used in each column are recorded in the row labelled "Words". All estimations include county fixed effects, day fixed effects, and county linear time trends. Standard errors (shown in parenthesis) are constructed using two different methods: the first ones are two-way clustered at the county and day level, and the second ones are nonparametric bootstrap estimation of standard errors, with replacement, with 1,000 replications. The statistical significance depicted is based on clustered standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

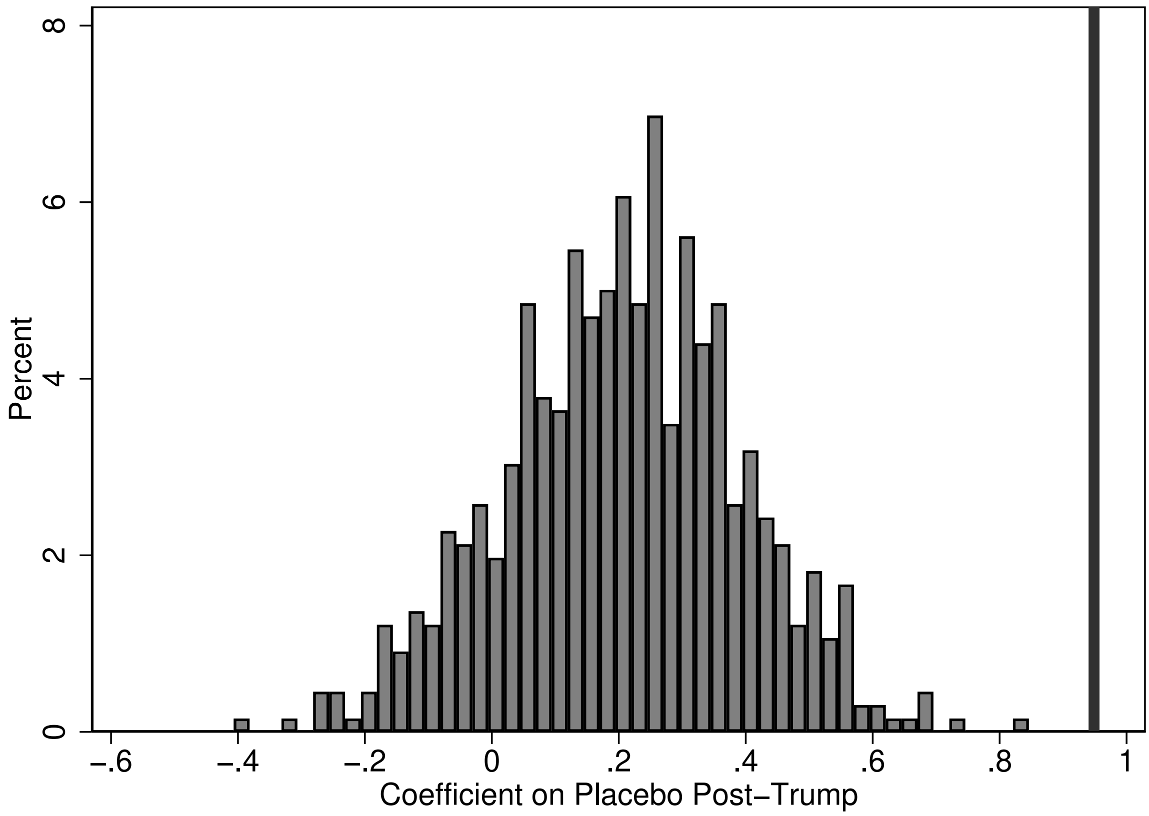
Online Appendix
(NOT FOR PUBLICATION)

1. Additional Figures



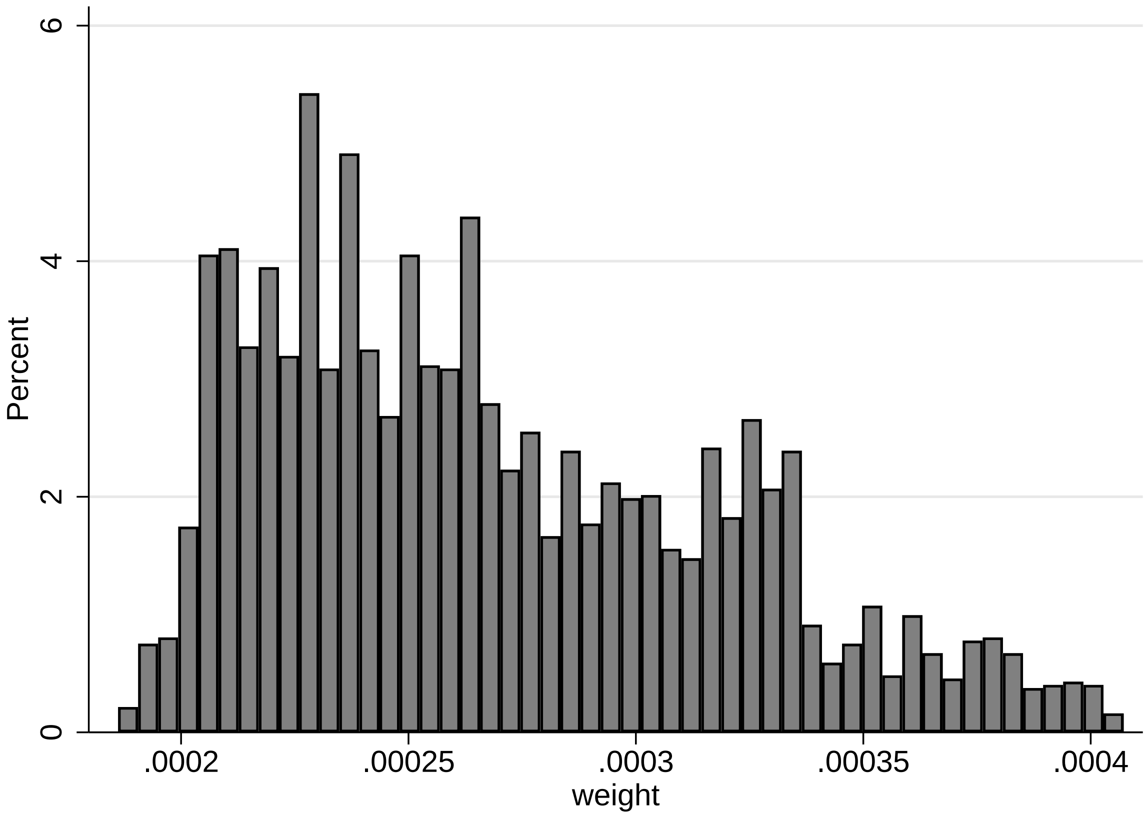
Notes: Relative frequency of each word, stem, or group of words in each candidate's campaign speeches. The graph displays the mean and 95% confidence interval of the proportion of each word, stem, or group of words over the total number of non-stop words in each candidate's speeches. Crime includes (dots indicate stemmed words): crim., gang, rape, rapist, drug, meth, crack, cocaine, heroin, opioid, pill. Race includes: race, racial, black. Mexico includes Mexic. Migration: immigrant, immigration, migration. Terrorism: Afghanistan, Iraq, Isis, Islam, jihad, Syria, terror.. Business: business, job, tax, manufacturing. Corruption: rig, CNN, swamp, media, corrupt., Washington. Trade: trade, China, NAFTA. Data is from the The American Presidency Project (accessed 10 June 2020) (Peters and Woolley, 2020) and (Enke, 2020).

Figure A1: Relative Frequency of Words in Candidates' Speeches



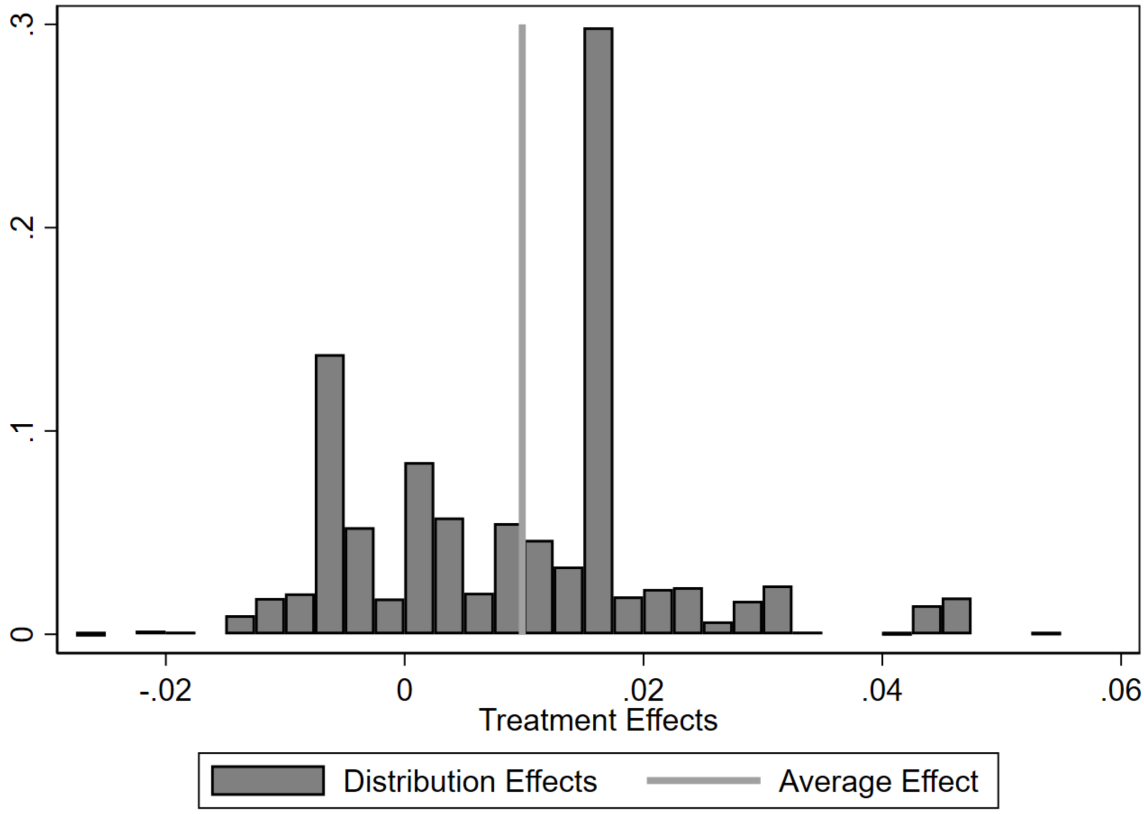
Notes: This figure shows the distribution of β from Equation 4 where instead of using the real distribution of Trump rallies, we randomly reallocate rallies within a county, across dates. The results of this permutation inference with placebo treatments are based on $1,000$ replications. The vertical bar indicates the coefficient obtained from the actual distribution of Trump rallies.

Figure A2: Permutation inference



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 county treatment effect. This figure shows the distribution of these weights.

Figure A3: Distribution Weights Difference-in-Differences Estimator



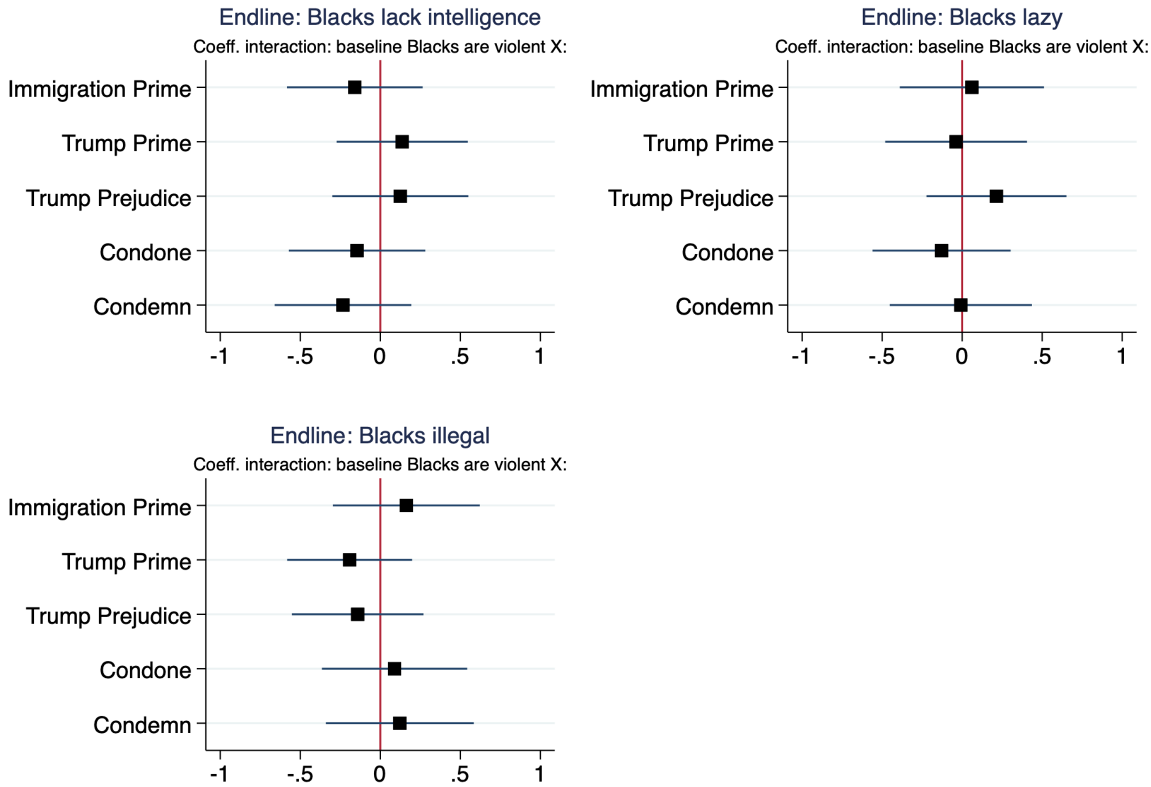
Notes: This figure shows the distribution of the estimated difference-in-differences estimator for each county. Each estimated effect is weighted by the number of stops in that county. The vertical bar is the average of these estimated coefficients.

Figure A4: Distribution Difference-in-Differences Estimator

	Name	Candidates	Political issue	Prejudiced speech	Other elite signal
1.	Control	Clinton/Bush	Campaign finance reform	None	None
2.	Immigration Prime	Clinton/Bush	Immigration reform	None	None
3.	Trump Prime	Clinton/Trump	Campaign finance reform	None	None
4.	Trump Prejudice	Clinton/Trump	Immigration reform	Yes – Trump	None
5.	Trump Condone	Clinton/Trump	Immigration reform	Yes – Trump	Bipartisan condone
6.	Trump Condemn	Clinton/Trump	Immigration reform	Yes – Trump	Bipartisan condemn

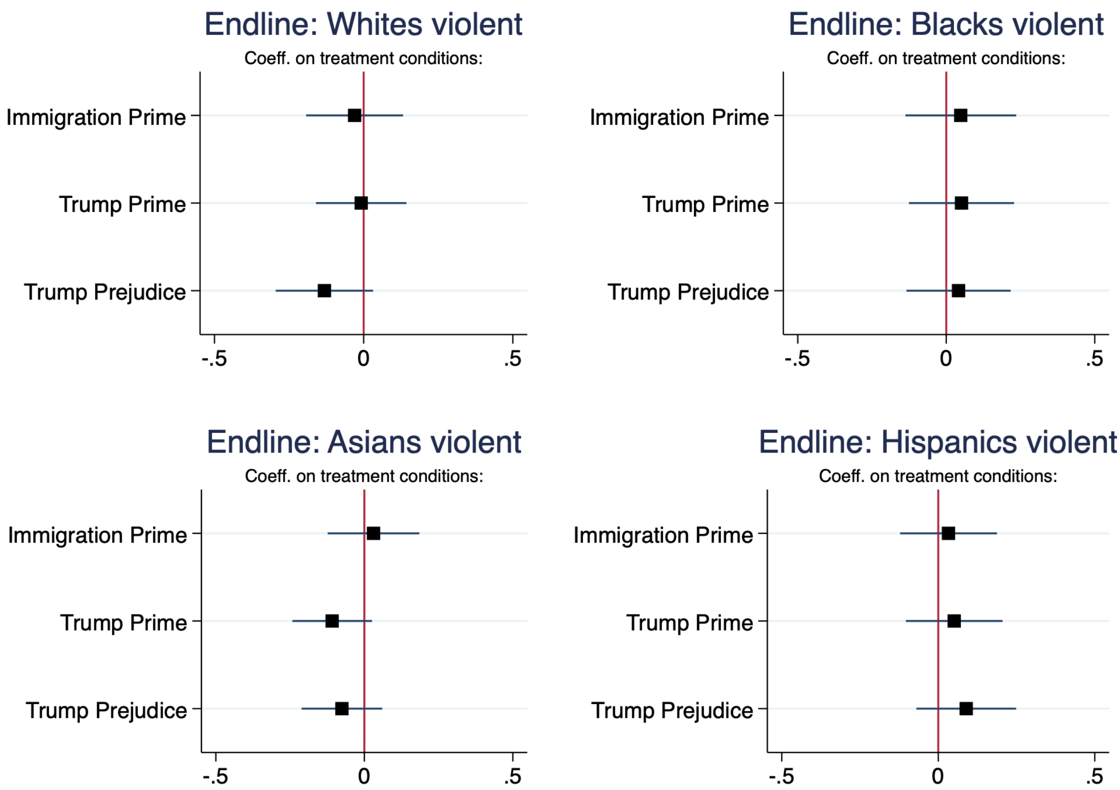
Notes: This figure is from (Newman et al., 2020) (Table 1 of the paper, p.8). The treatment consisted of randomly assigning a respondent to read one article about the 2016 presidential election. The articles were created by the authors, drawing on real election content. The table describes the content of the six different versions of the article respondents were asked to read in Wave 2 of the authors' online panel survey experiment.

Figure A5: Experimental Treatments



Notes: See notes to Figures 4 and 5. The only difference here is that we interact the treatment conditions with the dummy indicating above sample median prejudice that Black people are *violent* (as opposed to the same dimension of prejudice as the dependent variable). The Figure shows that none of the treatment conditions activates prejudice against Black people along these dimensions, even for respondents who are initially highly prejudiced that Blacks are violent.

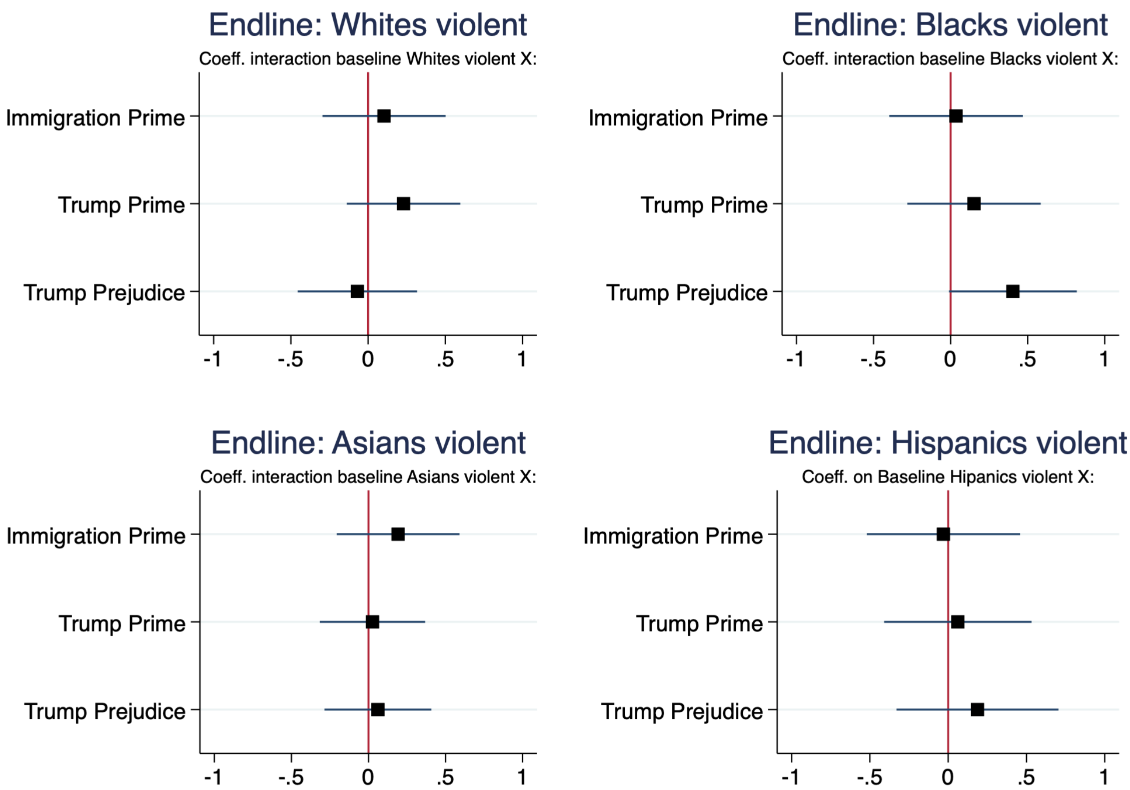
Figure A6: Trump's inflammatory speech does not activate other dimensions of anti-Black prejudice even among respondents who are initially highly prejudiced that Black people are violent.



Notes: See notes to Figure 3. The only difference is that we exclude here the 341 respondents in the "Trump Condone" and in the "Trump Condemn" conditions. The results commented in the notes to Figure 3 and in the paper carry through.

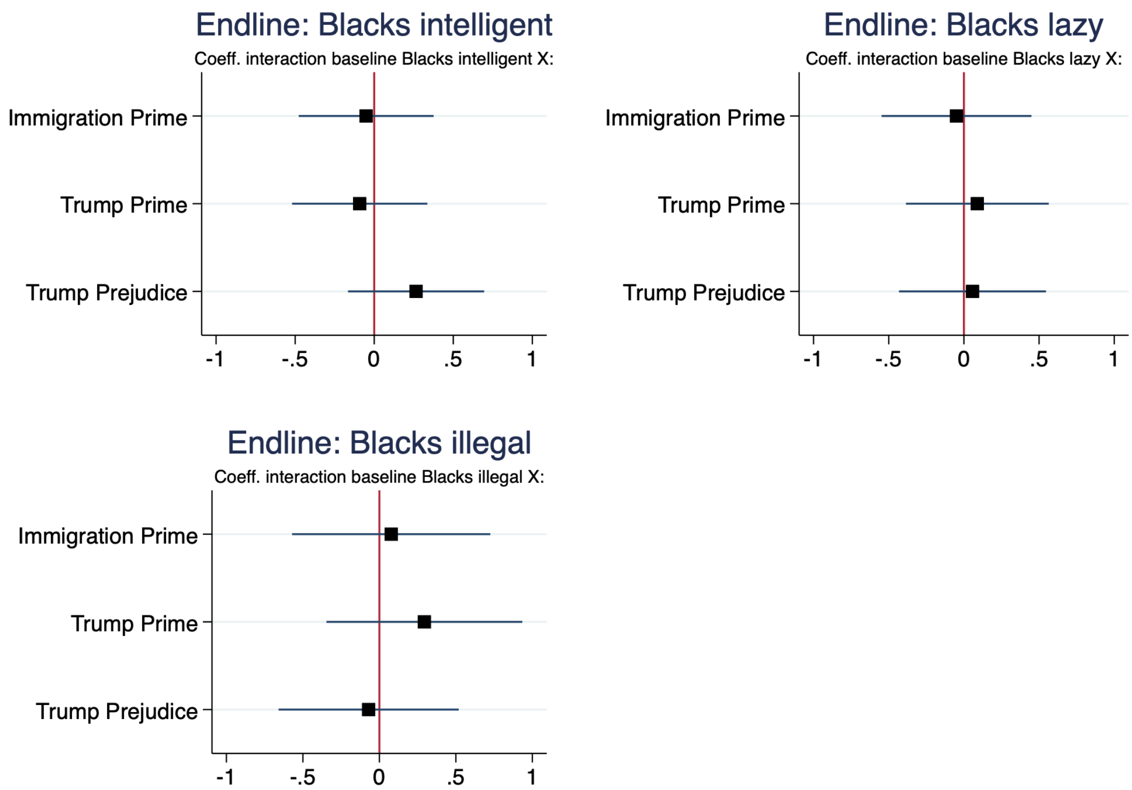
Figure A7: Violent Prejudice: Figure 3 without the last 2 treatment conditions.

The following figures replicate Figures 3 to 5 in the paper but exclude respondents in the "Trump Condone" and the "Trump Condemn" conditions. The sample size is reduced to 656 respondents. All the results in the paper carry through in this subsample.



Notes: See notes to Figure 4. The only difference is that we exclude here the 341 respondents in the "Trump Condone" and in the "Trump Condemn" conditions. The results commented in the notes to Figure 4 and in the paper carry through.

Figure A8: Trump's inflammatory speech activates the prejudice that Black people are violent among biased respondents: Figure 4 without the last 2 treatment conditions.



Notes: See notes to Figure 5. The only difference is that we exclude here the 341 respondents in the "Trump Condone" and in the "Trump Condemn" conditions. The results commented in the notes to Figure 5 and in the paper carry through.

Figure A9: Trump's inflammatory speech does not activate other dimensions of anti-Black prejudice: Figure 5 without the last 2 treatment conditions.

2. Additional Tables

2.1 Differences in levels before treatment

Table A1: Differences in the probability of a Black stop before treatment

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES			100 x $\mathbb{P}$(Black Stop)= 1			
PRE-Trump	-0.702	0.129	-0.493	0.301	-1.174	-0.035
	(1.688)	(0.252)	(1.672)	(0.243)	(1.762)	(0.157)
Observations	3,778,752	3,778,752	5,219,797	5,219,797	6,803,089	6,803,089
R-squared	0.003	0.091	0.002	0.091	0.002	0.092
County FE	NO	YES	NO	YES	YES	YES
Day FE	YES	YES	YES	YES	YES	YES
Window	5 days	5 days	10 days	10 days	30 days	30 days

Notes: This table shows OLS regression results where the dependent variable is the probability of a Black stop. The estimating sample consists of all the stops that happen during the 5, 10, or 30 calendar days (as indicated) before a Trump rally. PRE-Trump is a dummy variable that takes value one in the 5, 10, 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 $k = 5, 10, 30$. Standard errors are two-way clustered at the county and day level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

2.2 Robustness of Police Results

In this section, we present the robustness analysis for the results on traffic stops. First, we present estimation results of Equation 4 when we restrict the sample to Trump's presidential campaign rallies. We then check that our results are robust to the exclusion of time trends or the inclusion of county-specific quadratic time trends. Our results on Black stops are robust and increase in magnitude without a linear trend, or when a quadratic time trend is added.

Table A2: Impact of Trump rallies on the probability of a Black stop: Robustness

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES		100 x $\mathbb{P}$(Black Stop= 1)		Log(Black-share)	IHS(Black-share)	100 x $\mathbb{P}$(Black Stop= 1)	
POST-Trump	0.779***	0.801***	1.009***	4.356***	5.057***	1.182***	0.474***
	(0.253)	(0.238)	(0.317)	(1.043)	(1.384)	(0.280)	(0.176)
POST-Trump * GE-Rally						-0.798*	
						(0.456)	
Observations	11,931,161	11,931,161	16,914,533	11,931,161	11,931,161	11,931,161	11,931,161
R-squared	0.090	0.091	0.161	0.863	0.818	0.091	0.099
Robustness	W/O Linear Trend	Quadratic Trend	Full-Sample	Log	IHS	GE-Rally	LEA FE

Notes: See notes to Table 2. The table reports the results from Table 2 with POST-Trump, which is equal to $D_{c,t}^{(1,30)}$. Column 1 present estimation results without the inclusion of linear county-specific time trends. Column 2 estimates include quadratic county-specific time trends. In Column 3, we extend our data to include the sample of counties in which Clinton or Cruz (but not Trump) held rallies. In Columns 4 and 5, we use county-day observations weighted by number of stops. In Column 4, the dependent variable is 100* natural logarithm of Black stops as a share of total number of stops. In Column 5, the dependent variable is 100* inverse hyperbolic sine transformation of Black stops as a share of total number of stops: $100 * log(\text{Black-share} + (\text{Black-share}^2 + 1)^{1/2})$. In Column 6, we include an interaction between POST-Trump and a dummy variable indicating whether the stop took place after June 10, 2016. Column 7 estimates include local enforcement agency fixed effects.

2.3 Police stops: Simple Difference-in-Differences Analysis

In this section, we present an analysis that complements the results discussed in Section 4 of the paper. We adopt a slightly different approach, with a more classic simple difference-in-differences specification. This specification only includes the sample around the event and one parameter of interest. More precisely, we estimate:

$$Black_{ict} = \alpha_c + \theta_t + \eta \mathbb{1}(D_{cte} = 0)_{e=1,\ldots,N_c} + \beta Max(\mathbb{1}(0 < D_{cte} \leq k)_{e=1,\ldots,N_c}) + \alpha_c \times t + u_{ict}, \tag{8}$$

where $Black_{ict}$ is a dummy equal to one if the driver stopped is Black in county c on date t. $\mathbb{1}(0 < D_{cte} \leq k)_{e=1,\ldots,N_c}$ is equal to one for varying windows of days k after a Trump rally, and zero otherwise. In contrast with the main approach adopted in the paper, here we estimate Equation 8 for different sample periods around a Trump rally. We keep the pre-period sample as 100 days before a rally. We vary the treatment window in increments of 10 days, from 10 to 100 days of post-rally observations.

Column 1 of Table A3 shows that rallies by Trump lead to a 1.23 p.p. increase in the probability that a stopped driver is Black in the first ten days after the rally. The mean value (standard deviation) of the probability of a Black stop 100 days before a Trump rally in our sample is 0.20 (0.40). This means that the coefficient implies an increase equivalent to 6.15% of the mean value, or 0.03 standard deviations in the probability of a Black stop. The effect is robust to increasing the treatment window, but drops in magnitude, consistent with the main results in the paper that suggests that the effects wanes over time.

Table A3: DID estimates of impact of Trump rallies on the probability of a Black stop

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
VARIABLES					100 x $\mathbb{P}$(Black Stop)= 1					
POST-Trump	1.227***	1.056***	0.996***	0.920***	0.907**	0.848**	0.804**	0.769**	0.728**	0.697**
	(0.423)	(0.371)	(0.369)	(0.351)	(0.352)	(0.342)	(0.331)	(0.323)	(0.321)	(0.318)
Observations	7,391,468	7,500,560	7,612,534	7,712,750	7,801,329	7,902,247	7,993,613	8,086,209	8,179,223	8,270,126
R-squared	0.115	0.114	0.114	0.113	0.112	0.112	0.111	0.110	0.110	0.109
Sample	10 days	20 days	30 days	40 days	50 days	60 days	70 days	80 days	90 days	100 days
County FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Daily FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Notes: The Table shows OLS estimation of Equation 8. The unit of observation is a police stop. The dependent variable is the probability that a stopped driver is Black. The main independent variable is POST-Trump, which is equal to one for k days after a Trump rally, and is zero otherwise. Our sample consists of 100 days before each Trump rally and up to a 100 days after each Trump rally, in increments of 10 days, from 10 to 100 days, as indicated. All estimations include county and day fixed effects, and county linear time trends. Standard errors are two-way clustered at the county and day level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

2.4 Comparing Nomination vs Presidential Rallies

The magnitude of the effect of a Trump rally on the probability that a Black driver is stopped by the police is lower for general election rallies. We interpret this result in the paper as consistent with the moderation of Trump's speech after he secured the nomination (Enke, 2020). An alternative explanation for the difference in the magnitude of the results could be that the type of counties visited for the nomination campaign was very different from those visited for the presidential campaign. In what follows, we show that this is not the case along a wide range of dimensions that are relevant for our analysis, including pre-trends in police behavior, racial composition, education, recent economic shock, or underlying racial resentment.

In Table A4, we compare counties that held Trump rallies for the Republican nomination to counties that held Trump rallies for the presidency. Our analysis is at the county level. For police behavior, we use the number of stops and the share of Black drivers as a share of total stops prior to the first ever rally held by Trump in order to capture pre-trends. All other variables are measured at baseline, before 2015 (see Section 3 of the paper for a description of the data sources).

Table A4: Trump's presidential rallies versus nomination rallies

Variable	Presidential Rally	
Total Stops	-242.69	(200.38)
Black Stops	0.02	(0.02)
Racial Resentment-A	-0.02	(0.02)
Racial Resentment-B	-0.01	(0.02)
Slaves p.c. 1860	-0.03	(0.04)
Cotton Suitability	0.00	(0.00)
Executions	0.21	(0.17)
Lynchings	0.16	(0.16)
County Population	-13,992.30	(173,773.52)
County Blacks	0.18	(2.20)
County White	-2.41	(2.96)
County Hispanics	3.62*	(1.95)
County Racial HHI	-0.02	(0.03)
County DEM share'12	-0.42	(1.81)
County HH income	503.13	(934.81)
County College	0.01	(0.02)
China Shock	-0.07	(0.35)

Notes: The Table shows coefficients and standard errors (in parenthesis) obtained in an OLS regressions of various county characteristics on a dummy variable that takes value one if a presidency rally was held, as opposed to a nomination rally (excluded category). We define as a presidential rally any rally that took place after the investiture of Trump by the Republican Party. The observation is a county. Robust standard errors corrected for heteroskedasticity are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The Table shows that counties in which Trump held a presidency rally do not differ statistically from nomination rallies counties in terms of pre-trends in police behavior, defined both in terms of total stops and share of Black stops. The two types of counties are also statistically similar along a wide range of socio-economic characteristics.

2.5 Effect of rallies on Black drivers' behavior

In this section, we present an analysis that suggests that driving behavior by Black drivers did not change as a result of rallies by Trump. We collected data on Black Lives Matter events over the sample period from Elephrame (accessed on March21, 2021). We also use the data on fatalities from Centers for Disease Control (CDC) Wonder's Multiple Cause of Death, 1999-2018 dataset available on the CDC website. The data contains monthly information on number of deaths in each county by leading 130 causes of deaths. We use the codes V01-V99 to measure the number of fatalities due to motor accidents for each county in our sample from 2015 to 2017. We obtain data on all deaths as well as deaths of Black or African American people due to motor accidents.

Table A5 shows the impact of Trump rallies on Black Lives Matter protests (Column 1) and motor accidents fatalities (Columns 2 to 4). Trump rallies do not result in a higher number of Black Lives Matter protests in the county. Moreover, we do not observe any change in the number of Black traffic fatalities in the county after a Trump rally.

Table A5: Impact of Trump rallies on motor accidents

	(1)	(2)	(3)	(4)
VARIABLES	BLM Protest	ln(Deaths)	ln(Deaths-Blacks)	ln(Deaths-Non Blacks)
POST-Trump	-0.001	-0.061	-0.160	-0.071
	(0.004)	(0.083)	(0.120)	(0.077)
Observations	95,440	3,105	3,094	3,094
R-squared	0.211	0.908	0.533	0.906
County FE	YES	YES	YES	YES
Month FE		YES	YES	YES
Day FE	YES			
CountyXDay	YES			

Notes: This table shows the effect of Trump rallies on Black Lives Matter protests (Column 1) and motor accidents fatalities (Columns 2 to 4). The unit of observation is county-day in Column 1 and county-month in Columns 2 to 4. The dependent variable in Column 1 is an indicator variable equal to if there is a Black Lives Matter protest and zero otherwise. The dependent variables in Columns 2 to 4 are natural logarithm of total deaths in motor accidents, natural logarithm of deaths of Black or African American people in motor accidents, and natural logarithm of deaths of other races in motor accidents. The dependent variables in Columns 2 to 4 are constructed using Centers for Disease Control (CDC) Wonder's Multiple Cause of Death database. Regressions control for county, day and county linear trends in Column 1. In Columns 2 to 4, regressions control for county and month fixed effects. Standard errors are two-way clustered at the county and day level in Column 1 and two-way clustered at the county and at the month level in Columns 2 to 4. All estimates are weighted by total number of stops. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Effect of Trump rallies on officer bias

	(1)	(2)
VARIABLES	Officer Bias	
POST-Trump	0.052*	0.049*
	(0.027)	(0.029)
	(0.028)	(0.029)
Observations	5,810	5,806
R-squared	0.517	0.533
Officer FE	YES	YES
Bias Measure	Method 1	Method 2

Notes: The Table shows OLS estimation of Equation 4 with officer's racial bias as the dependent variable. Data is at the officer level. 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. All estimations include county and day fixed effects, and county linear time trends. Standard errors (shown in parenthesis) are constructed using two different methods: the first ones are two-way clustered at the county and day level, and the second ones are nonparametric bootstrap estimation of standard errors, with replacement, with 1,000 replications. The statistical significance depicted is based on clustered standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Impact of Trump rallies in the samples of Clinton and Cruz rallies on the probability of a Black stop

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
VARIABLES	100 x $\mathbb{P}$(Black Stop= 1)									
POST-Trump	1.403***	1.565***	1.439***	1.358***	1.305***	1.213***	1.122***	1.063***	0.969***	0.894***
	(0.329)	(0.328)	(0.347)	(0.308)	(0.291)	(0.286)	(0.300)	(0.299)	(0.302)	(0.331)
POST-Clinton	-0.837**	-0.524	-0.163	0.097	0.151	0.265	0.372	0.341	0.346	0.365
	(0.395)	(0.419)	(0.386)	(0.387)	(0.378)	(0.366)	(0.360)	(0.358)	(0.378)	(0.364)
Observations	11,038,749	11,038,749	11,038,749	11,038,749	11,038,749	11,038,749	11,038,749	11,038,749	11,038,749	11,038,749
R-squared	0.949	0.949	0.949	0.949	0.949	0.949	0.949	0.949	0.949	0.949

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel B	100 x $\mathbb{P}$(Black Stop= 1)									
POST-Trump	0.491	0.830**	0.950**	0.955**	0.983**	0.939**	0.933**	0.913**	0.900**	0.889**
	(0.407)	(0.405)	(0.448)	(0.419)	(0.408)	(0.394)	(0.400)	(0.411)	(0.400)	(0.408)
POST-Cruz	0.288	-0.155	0.270	0.405	0.342	0.407	0.409	0.408	0.331	0.436
	(0.469)	(0.324)	(0.354)	(0.350)	(0.322)	(0.336)	(0.348)	(0.414)	(0.361)	(0.376)
Observations	4,713,907	4,713,907	4,713,907	4,713,907	4,713,907	4,713,907	4,713,907	4,713,907	4,713,907	4,713,907
R-squared	0.086	0.086	0.086	0.086	0.086	0.086	0.086	0.086	0.086	0.086
County FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Day FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
County Trend	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Window	10	20	30	40	50	60	70	80	90	100

Notes: See notes to Table 5. We augment the specification with variables accounting for Trump rallies. All estimations include county and day fixed effects, and county linear time trends. Standard errors are two-way clustered at the county and day level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: 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

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	100 x $\mathbb{P}$(Black Stop= 1)					
	Panel A: Variables related to race					
POST-Trump	0.808***	0.767**	0.806***	0.788***	0.792***	0.824***
	(0.295)	(0.308)	(0.263)	(0.300)	(0.273)	(0.261)
POST-Trump * X	0.518***	0.383**	0.295*	0.579*	0.296*	0.387*
	(0.152)	(0.173)	(0.164)	(0.324)	(0.173)	(0.199)
Observations	11,931,161	11,931,161	11,931,161	11,931,161	11,931,161	11,931,161
R-squared	0.090	0.090	0.090	0.090	0.090	0.090
X=	Racial Resentment-A	Racial Resentment-B	Slaves p.c. 1860	Cotton Suitability	Executions	Lynchings
County FE	YES	YES	YES	YES	YES	YES
Daily FE	YES	YES	YES	YES	YES	YES
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel B: Other variables					
POST-Trump	0.904***	0.817***	0.834***	0.785***	0.761***	0.794***
	(0.310)	(0.299)	(0.296)	(0.257)	(0.256)	(0.258)
POST-Trump * X	0.243	0.150	0.114	0.093	-0.006	-0.080
	(0.174)	(0.196)	(0.179)	(0.192)	(0.182)	(0.162)
Observations	11,931,161	11,931,161	11,451,866	11,931,161	11,805,287	11,805,287
R-squared	0.090	0.090	0.086	0.090	0.090	0.090
X=	County Racial-HHI	County DEM share'12	County HH income	County College	China Shock	China Shock-IV
County FE	YES	YES	YES	YES	YES	YES
Daily FE	YES	YES	YES	YES	YES	YES

Notes: The Table shows OLS estimation of Equation 4 with the addition of an interaction term between POST-Trump, which is equal to $D^{(1,30)}_{c,t}$, and the following predetermined county characteristics: two measures of racial resentment from the 2012 and 2014 CCES (Schaffner and Ansolabehere, 2015), slaves per capita in 1860, soil suitability for growing cotton, executions of Black people, lynchings of Black people (Panel A); racial HHI diversity index, vote share of Obama in 2012 presidential election, median household income, share of college graduates, the China import competition shock from Autor, Dorn and Hanson (2013), and the instrumental variable for the China import competition shock from Autor, Dorn and Hanson (2013). All 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

2.6 Experiment: Regression results

In this section, we present all estimation results underlying Figures 3 to 5.

Table A9: Regression results for Figure 3

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable	Endline prejudice race is violent							
	White		Black		Asian		Hispanic	
Immigration Prime	−0.050	−0.033	0.056	0.053	0.020	0.032	0.019	0.032
	(0.083)	(0.082)	(0.097)	(0.095)	(0.079)	(0.078)	(0.080)	(0.079)
Trump Prime	−0.030	−0.010	0.050	0.054	−0.102	−0.103	0.043	0.046
	(0.076)	(0.077)	(0.090)	(0.089)	(0.068)	(0.068)	(0.080)	(0.079)
Trump Prejudice	−0.135	−0.135	0.060	0.051	−0.078	−0.074	0.087	0.082
	(0.084)	(0.084)	(0.092)	(0.089)	(0.069)	(0.069)	(0.081)	(0.081)
Trump Condone	−0.117	−0.104	−0.063	−0.069	−0.085	−0.075	−0.041	−0.038
	(0.075)	(0.075)	(0.085)	(0.085)	(0.070)	(0.069)	(0.081)	(0.081)
Trump Condemn	−0.122	−0.111	0.008	0.003	−0.011	−0.013	−0.001	0.006
	(0.080)	(0.079)	(0.093)	(0.091)	(0.074)	(0.073)	(0.078)	(0.077)
Baseline prejudice race violent	0.596***	0.575***	0.665***	0.634***	0.455***	0.442***	0.591***	0.565***
	(0.029)	(0.029)	(0.025)	(0.026)	(0.047)	(0.046)	(0.029)	(0.032)
Main effect: treatment conditions	YES	YES	YES	YES	YES	YES	YES	YES
Individual controls	NO	YES	NO	YES	NO	YES	NO	YES
Observations	997	996	997	996	997	996	997	996
R^2	0.394	0.408	0.472	0.490	0.209	0.223	0.360	0.374

Notes: The Table shows OLS estimation of Equation 6 without the inclusion of the interaction effect. The unit of observation is an individual. The dependent variable is the endline prejudice that each respective ethnic group (as indicated in the Column headers) is violent. The main independent variables are the different treatment conditions (see Figure A5 and the text for a description of each treatment). For the dependent variable for each race, we present the results of an uncontrolled specification (even columns) and a fully controlled specification (odd columns). Results in the odd columns are plotted in Figure 3. Standard errors are corrected for heteroskedasticity. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A10: Regression results for Figure 4

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable	Endline prejudice race is violent							
	White		Black		Asian		Hispanic	
Immigration Prime	−0.103	−0.103	0.075	0.085	−0.043	−0.024	0.024	0.055
	(0.098)	(0.096)	(0.129)	(0.125)	(0.082)	(0.082)	(0.096)	(0.093)
Trump Prime	−0.096	−0.086	0.005	0.053	−0.135*	−0.136*	0.010	0.015
	(0.097)	(0.099)	(0.114)	(0.115)	(0.075)	(0.076)	(0.095)	(0.094)
Trump Prejudice	−0.082	−0.083	−0.064	−0.056	−0.105	−0.100	0.063	0.068
	(0.099)	(0.098)	(0.117)	(0.112)	(0.074)	(0.074)	(0.091)	(0.088)
Trump Condone	−0.188**	−0.173*	−0.091	−0.093	−0.102	−0.087	−0.067	−0.058
	(0.088)	(0.090)	(0.112)	(0.115)	(0.076)	(0.076)	(0.093)	(0.092)
Trump Condemn	−0.150	−0.148	−0.061	−0.041	−0.092	−0.087	−0.050	−0.033
	(0.100)	(0.100)	(0.127)	(0.125)	(0.077)	(0.078)	(0.092)	(0.089)
Interaction: > median baseline prejudice ***race*** *is violent and:*								
Immigration Prime	0.004	0.077	0.065	0.028	0.210	0.187	0.038	−0.018
	(0.204)	(0.202)	(0.227)	(0.221)	(0.206)	(0.202)	(0.248)	(0.249)
Trump Prime	0.182	0.219	0.267	0.157	0.010	0.017	0.094	0.081
	(0.191)	(0.188)	(0.221)	(0.220)	(0.172)	(0.174)	(0.238)	(0.238)
Trump Predjudice	−0.086	−0.076	0.471**	0.410*	0.060	0.061	0.243	0.170
	(0.200)	(0.196)	(0.212)	(0.210)	(0.173)	(0.175)	(0.256)	(0.263)
Trump Condone	0.292	0.292	0.090	0.067	0.024	0.005	0.076	0.052
	(0.207)	(0.205)	(0.205)	(0.203)	(0.182)	(0.180)	(0.243)	(0.247)
Trump Condemn	0.050	0.085	0.177	0.099	0.316	0.297	0.191	0.123
	(0.198)	(0.195)	(0.208)	(0.203)	(0.193)	(0.193)	(0.229)	(0.234)
> median baseline prejudice race is violent	1.011***	0.936***	1.119***	1.080***	0.532***	0.520***	0.949***	0.912***
	(0.135)	(0.133)	(0.152)	(0.150)	(0.135)	(0.135)	(0.177)	(0.183)
Main effect: treatment conditions	YES	YES	YES	YES	YES	YES	YES	YES
Individual controls	NO	YES	NO	YES	NO	YES	NO	YES
Observations	997	996	997	996	997	996	997	996
R^2	0.292	0.317	0.329	0.358	0.190	0.203	0.245	0.271

Notes: The Table shows OLS estimation of Equation 6. The dependent variable is the endline prejudice that each respective ethnic group (indicated in the column headers) is violent. The table displays the coefficients associated with: the treatment condition (see Figure A5 and the text for a description of each treatment), the baseline prejudice that each race is violent (i.e., in columns 1 and 2, the included variable is an indicator variable for being above the median sample prejudice that Whites are violent; in columns 3 and 4, the included variable is an indicator variable for being above the median sample prejudice that Black people are violent, etc.), as well as the interaction between this indicator variable and the treatment condition (as indicated on the vertical axes). Individual controls are included in the odd columns. As in all specifications, the individual controls consist of: age, education, gender, race (Black, Hispanic, Asian, or White), employment status, party identification, and politicization. The coefficients associated with the main effect of each treatment and its interaction with above median prejudice in the odd columns are displayed in Figure 4. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. This table shows that exposure to Trump's inflammatory speech increases the prejudice that Black people are violent among respondents who are initially prejudiced (i.e., above the median baseline prejudice that Black people are violent); while it does not affect the prejudice that any other race is violent, even among respondents who are highly prejudiced against those other races.

Table A11: Regression results for Figure 5

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Endline prejudice African Americans are:					
	Lazy		Lack intelligence		In US illegally	
Immigration Prime	0.058	0.099	0.197*	0.191	0.118	0.122
	(0.111)	(0.109)	(0.118)	(0.116)	(0.115)	(0.115)
Trump Prime	−0.008	0.015	0.088	0.108	−0.151*	−0.131
	(0.105)	(0.103)	(0.118)	(0.116)	(0.090)	(0.091)
Trump Prejudice	−0.019	0.011	0.005	0.017	0.015	0.019
	(0.108)	(0.103)	(0.115)	(0.112)	(0.105)	(0.105)
Trump Condone	0.043	0.052	0.065	0.100	0.215*	0.220*
	(0.107)	(0.110)	(0.115)	(0.113)	(0.119)	(0.119)
Trump Condemn	−0.085	−0.052	−0.058	−0.090	0.173	0.170
	(0.105)	(0.101)	(0.115)	(0.117)	(0.118)	(0.117)
Interaction: > median baseline prejudice Blacks: are lazy (Col.1, 2); lack intelligence (Col. 3, 4); in the US illegally (Col. 5, 6) and:						
Immigration Prime	0.083	−0.041	−0.026	−0.033	0.066	0.089
	(0.257)	(0.253)	(0.232)	(0.218)	(0.345)	(0.336)
Trump Prime	0.182	0.099	0.073	−0.059	0.261	0.293
	(0.247)	(0.241)	(0.232)	(0.218)	(0.329)	(0.330)
Trump Prejudice	0.201	0.074	0.382*	0.298	−0.051	−0.063
	(0.248)	(0.247)	(0.229)	(0.218)	(0.300)	(0.304)
Trump Condone	−0.161	−0.192	0.059	−0.072	−0.272	−0.318
	(0.251)	(0.239)	(0.248)	(0.236)	(0.307)	(0.306)
Trump Condemn	0.002	−0.135	0.059	−0.022	0.102	0.116
	(0.260)	(0.256)	(0.232)	(0.223)	(0.330)	(0.334)
> median baseline prejudice Blacks are lazy	1.241***	1.234***				
	(0.191)	(0.190)				
> median baseline prejudice Blacks lack intelligence			1.074***	1.055***		
			(0.177)	(0.163)		
> median baseline prejudice Blacks are in the US illegally					0.458*	0.456*
					(0.240)	(0.236)
Individual controls	NO	YES	NO	YES	NO	YES
Observations	997	996	997	996	997	996
R^2	0.309	0.352	0.249	0.295	0.044	0.063

Notes: The Table shows OLS estimation of Equation 6 for different types of prejudice. The dependent variables are the endline prejudices that African Americans are: lazy (Columns 1 and 2) or lack intelligence (Columns 3 and 4). The Table displays the coefficients associated with: the treatment condition (see Figure A5 and the text for a description of each treatment), an indicator variable for being above the median sample baseline prejudice that Black people are lazy (Columns 1 and 2) or lack intelligence (Columns 3 and 4) as well as the interaction between this indicator variable and the treatment condition (as indicated on the vertical axes). Individual controls are included in odd columns. As in all specifications, the individual controls consist of: age, education, gender, race (Black, Hispanic, Asian, or White), employment status, party identification, and politicization. Standard errors corrected for heteroskedasticity are reported in parentheses. The coefficients associated with the interaction terms in the odd columns are displayed in Figure 5. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. This table shows that exposure to Trump's inflammatory speech does not increase other dimensions of prejudice against African Americans, namely the prejudice that Black people are lazy, or lack intelligence, even for respondents who are highly prejudiced to begin with.

3. Trump's Rally Speeches: Examples and Rally County Correlates

This section provides excerpts of Trump's rally speeches referring to African Americans, crime, foreigners, or Mexico. The speeches usually associate crime and foreigners, as can be seen in the excerpts. From there, speeches often become the subject of either building a wall with Mexico, or "brutal drug cartels". We therefore group crime and foreigners together in one section. Last, we show how the probability that specific (stem) words figure in a Trump rally speech correlates with the characteristics of the county where the rally takes place.

3.1 On African Americans

"The people who will suffer the most as a result of these riots, are law-abiding African American residents who live in these communities. Nationwide, approximately 60% of murder victims under the age of 22 are African American. This is a national crisis, and it is the job of the next President to work with our governors and mayors to address this crisis and save African American lives." (Remarks at the Shale Insight TM Conference at the David L. Lawrence Convention Center in Pittsburgh, Pennsylvania, September 22, 2016.)

"The Democratic Party has run the inner cities for fifty, sixty, seventy years and more. 4 in 10 African American children live in poverty, including 45% of those under the age of six. 2,900 people have been shot in Chicago since the beginning of the year." (Remarks at the US Cellular Center in Asheville, North Carolina, September 12, 2016).

3.2 On Crime, Foreigners, and Mexico

"She [Hillary Clinton] refused when she was secretary of state to make foreign countries take back their criminal aliens. In other words, we have somebody in from a foreign country illegally. The person kills somebody, hurts somebody, robs a store, robs a bank, does all sorts – sells drugs all over the place, poisons our youth. And we want to send them back to the country from which they came and they bring them back and the country says, we're not taking them. Because they're smart. They don't want them. So they bring them back into the United States and put him into our society. I guarantee you, and you can mark my words, and there's a lot of tape running, that won't happen once when Donald Trump is president; not once, guarantee you that.

One convicted criminal alien, she allowed to go free – totally free; killed a young girl named Casey Chadwick; beautiful young girl. The corrupt establishment in Washington wants to surrender America's borders, even as they send our troops overseas to protect the borders of other countries. She'll protect those borders, but not our borders.

Casey Chadwick was a great example, and he was let free, and she should be alive today. Jamiel Shaw has a son – had a son, a great young man, killed violently. And I don't know if you know, but killed violently by an illegal immigrant, shot three times in the face for no reason; somebody that wasn't supposed to be here. A Trump administration will secure, control, defend and protect the borders of the United States, as sure as you are standing here today.

We will 100 percent build the wall, 100 percent.

Are you ready? Are you ready?

And who is going to pay for the wall?

[Audience]: "Mexico"

100 percent. They just don't know it yet. They don't know it yet, but 100 percent. They'll pay for the wall.

Hey, look, we're going to have a great relationship with Mexico and China and all of these countries that are ripping us off and now have no respect. They're going to make less and they're going to like us more. Funny how that works, but it's true. Mexico is making a fortune with trade. They're taking our jobs. So many things. They are going to respect us finally. We will stop the drugs from pouring into our country. Florida has seen a 470 percent increase in heroin deaths. Think of that think if you have a child 470 percent increase in heroin deaths since 2007. Think of that. Think of your kids and your grandkids.

We're going to stop it. And most of it's coming across the southern border. And let me tell you, when we allow ICE and when we allow the Border Patrol agents and when we give them that big beautiful wall with a door in it so people can come in, but they have to come in legally; we want them to come in, but they have to come in legally.

We will stop the drugs from poisoning our youth and others.

But I have a message for the drug dealers, for the gang members and the criminal cartels: Your days are numbered, believe me; your days are numbered. [applause]

Hillary also wants a 550 percent increase in Syrian refugees. We don't know anything about them. [...]

And here I am saying, we want strong borders. We don't want drugs coming in. We don't want crime coming in." (Remarks at the Southeastern Livestock Pavilion in Ocala, Florida, October 12, 2016.)

"She [Hillary Clinton] will – she will allow people into our country that will do damage, they will do damage, folks. You look at what's going on in the world – look at France; take a good look at France. I have friends that go to France. They used to love France.

They say, no thank you. They don't expect to be going back anytime for a long time.

I have one friend, I said, how was your trip this year to France? He said, France isn't France anymore. We're not going. And so many people are saying that.

Look at what's going on with Germany. Look at what's happening with Germany. Look at the crime; look at the problems. [...] So, terrible. Hillary and President Obama refused to use the term radical Islamic terrorism. Big problem. Big problem. Hillary wants to release violent criminals and criminal offenders from prison, that's wonderful, enjoy yourselves. I want to work with our police. Our police are so incredible, they're not getting the respect they deserve." (Remarks at a Rally at the University of North Carolina in Wilmington, August 09, 2016.)

"We must discuss, as well, the ongoing catastrophe of crime in our inner cities. According to the Chicago Tribune, there has already been more than 2,000 shooting victims in Chicago this year alone. This epidemic of violence destroys lives, destroys communities, and destroys opportunity for young Americans. Violent crime has increased in cities across America. The New York Times described "a startling rise in murders," in our major cities. Brutal drug cartels are spreading their reach into Virginia and Maryland." (Remarks in Virginia Beach, Virginia, July 11, 2016.)

4. Proofs

In this section, we provide the proofs for proposition 1. First, we show that q_j increases in racial bias λ_j and in the probability that the driver is Black p_j. Then, we show that if $g_{1,j}$ first order dominates $g_{1,j'}$ then $q_j > q_{j'}$. If $g_{0,j'}$ first order dominates $g_{0,j}$ then $q_j > q_{j'}$.

$$\frac{\partial q_j}{\partial \lambda_j} = \frac{p_j g_{1,j}(-\lambda_j)[(1-p_j)(1-G_{0,j}(0))]}{[p_j(1-G_{1,j}(-\lambda_j)) + (1-p_j)(1-G_{0,j}(0))]^2} > 0$$

$$\frac{\partial q_j}{\partial p_j} = \frac{(1-G_{1,j}(-\lambda_j))(1-p_j)(1-G_{0,j}(0)) + p_j(1-G_{1,j}(-\lambda_j))(1-G_{0,j}(0))}{[p_j(1-G_{1,j}(-\lambda_j)) + (1-p_j)(1-G_{0,j}(0))]^2} > 0$$

Let $W_j = G_{1,j}(-\lambda)$

$$q_j = \frac{p_j(1-W_j)}{p_j(1-W_j) - (1-p_j)(1-G_{0,j}(0))}$$

$$\frac{\partial q_A}{\partial W_j} = \frac{p_j[(1-p_j)(1-G_{0,j}(0))]}{[p_j(1-W_j) - (1-p_j)(1-G_{0,j}(0)]^2} > 0$$

Therefore let officer j and j' have:

- $p_j = p_{j'}$
- $G_{0,j}(x) = G_{0,j'}(x)\ \forall x$
- $\lambda_j = \lambda_{j'}$
- $G_{1,j}(-\lambda) < G_{1,j'}(-\lambda)$

$q_j > q_{j'}$

Given that the only way $G_{1,j}$ affects q_j is through its value at $-\lambda_j$ then a fortiori $q_j > q_{j'}$ also if $g_{1,j}$ first order dominates $g_{1,j'}$.

Let $W_j = G_{0,j}(0)$

$$q_j = \frac{p_j(1 - G_{1,j}(\lambda_j))}{p_j(1 - G_{1,j}(\lambda_j)) - (1 - p_j)(1 - W_j)}$$

$$\frac{\partial q_j}{\partial W_j} = \frac{[-p_j(1 - G_{1,j}(\lambda))](1 - p_j)}{[p_j(1 - G_{1,j}(\lambda)) - (1 - p_j)(1 - W_j)]^2} < 0$$

Therefore let officer j and j' have:

- $p_j = p_{j'}$
- $G_{1,j}(x) = G_{1,j'}(x)$
- $\lambda_j = \lambda_{j'}$
- $G_{0,j}(0) > G_{0,j'}(0)$

$q_j > q_{j'}$

Given that the only way $G_{0,j}$ affects q_j is through its value at 0 then a fortiori $q_j > q_{j'}$ if $g_{1,j'}$ first order dominates $g_{1,j}$.