Whistle the Racist Dogs: Political Campaigns and Police Stops

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Abstract

Do right-wing populists merely reflect, or also aggravate xenophobia? Using data from nearly 12 million police stops, we show that the probability that a police officer stops a black driver increases by 4.2% after a rally by Donald Trump during his 2015-2016 campaign. The effect is immediate, lasts for up to 50 days after a rally, and is only due to discretionary stops. We observe significantly larger effects in areas with more racist attitudes today or that relied more on slavery historically. By contrast, we observe no increase in the rates of stops of white or other racial minority drivers; nor any effect of campaign events of other 2016 candidates to the Republican nomination or to the presidency. Evidence from a 2016 online experiment confirms that Trump's inflammatory racial speech specifically aggravated respondents' prejudice that *blacks* are *violent*. We take this as evidence that Trump's campaign radicalized racial prejudice against blacks as well as its expression in one of its most fundamental and violent dimension: police behavior.

Keywords: Police stops, political campaign, racial prejudice.
JEL Codes: D73, K42.

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"Donald Trump is the first president in my lifetime who does not try to unite the American people – does not even pretend to try. Instead, he tries to divide us." Former Secretary of Defense General James Matthis, 3 June 2020.

1 Introduction

While identity politics has played an increasing role in most advanced democracies in recent years (Gennaioli and Tabellini, 2019), it has become particularly divisive in the US. In 2016, the election of Donald Trump, a candidate with openly xenophobic and sexist views, signified to many the victory of "white supremacy" (Coates, 2017). However, whether Trump's rise to prominence was merely the symptom of racial resentment or a cause of its radicalization is still an open question.

In this paper, we explore how Trump's political campaign has affected the expression of racial prejudice and discrimination against blacks in one of its most fundamental and violent dimensions: police behavior. Police behavior and alleged racially-motivated brutality have come to symbolize racial bias and discrimination against African-Americans, especially since the BlackLivesMatter movement in 2013.[1] We focus on the most frequent type of police-citizen interaction: traffic stops.[2] We use data on nearly 12 million vehicle stops by the police in 143 counties shown in Figure 1 where Trump held a campaign rally, either as a candidate for the Republican nomination or as a candidate for the Presidency. To measure racially-directed police behavior, we rely on the racial classification of the motorist stopped (following Knowles, Persico and Todd (2001); Anwar and Fang (2006); Antonovics and Knight (2009); Anbarci and Lee (2014); Goncalves and Mello (2017)). To deal with the potential endogenous selection of counties with a campaign event, we restrict our sample to the counties with at least one Trump campaign rally. To identify

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

[2] According to Bureau of Justice Statistics, 8.6% of US residents aged 16 and over, more than 20 million people, were pulled over by the police during a traffic stop in 2015 (Davis, Whyde and Langton, 2018). See https://www.bjs.gov/content/pub/pdf/cpp15.pdf, accessed July 7, 2020.

the effect of campaign stops, we use a Difference-in-Differences methodology, comparing the probability that the driver of the vehicle stopped by the police is black, just before and immediately after the campaign event, controlling for county and day fixed effects.[3]

We find evidence that the Trump campaign influenced police behavior towards blacks. Our baseline estimate suggests that the probability of a stop of a black driver by the police increases by 0.78 percentage point on average in the month following the rally, a 4.2% increase. The effect is immediate and materializes from the day after a Trump rally. The effect slowly fades away and lasts for at least 50 days. The effect is robust to varying the observation window to 10 to 100 days around the event, as well as to including flexible county-specific time trends. It is also robust to controlling for officer-level fixed effects, showing that the effect reflects a change in the behavior of police officers rather than a change in the composition of the police force on duty. We also conduct placebo specifications, in which we see no effect on police behavior of campaign events by the Democratic contender to the Presidency Hilary Clinton in 2016, nor by the other main contender to the Republican investiture in 2016, Ted Cruz.[4]

To guide our understanding of the mechanism, we refer to recent literature that shows how Trump's campaign and election changed the expression, and acceptability, of xenophobic views and discrimination (Bursztyn, Egorov and Fiorin, 2019; Newman et al., 2020). This literature focuses on attitudes against the groups directly targeted by Trump's xenophobic speeches – Latinos and Muslim migrants. By contrast, Trump's campaign did not directly and explicitly targeted African Americans. Nevertheless, a vast literature in political science and law shows how certain speech can resonate in a particular way

[3]Recent econometric literature on staggered Differences-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 Abraham and Sun (2018) and find similar results to our baseline DiD.

[4]There has been an important discussion related to the ability of using police data to determine racial discrimination. In particular, Knox, Lowe and Mummolo (2019) highlight that estimates of racial discrimination based on samples of people stopped\detained by the police may be biased, because racial discrimination may have already selected those samples. We abstract from this issue because of several reasons. Given that we are using a DiD methodology, we are relying only on changes in the probability of the stop of a black driver, rather than trying to infer racial discrimination from the average probability of a police stop of a black driver. Moreover, in our baseline estimates we are not studying the behavior of the police after the police stop.

to a targeted subgroup– a phenomenon labeled the Dog-whistle Effect (Lohrey, 2006; Goodin, 2008; Fear, 2007; Haney-Lopez, 2014).[5] Dog-whistling has various definitions, but we are most particularly interested here in one specific dimension: when the coded language of Dog-whistling appeals to deep-seated stereotypes of groups that are perceived as threatening (Haney-Lopez, 2014). Hence we ask whether Trump's campaign activated stereotypes against blacks, namely those associating blacks to violence and crime, and particularly amongst individuals with deep-seated prejudice against blacks. To answer, we proceed in three steps.

First, we leverage the high granularity of our police stops data to establish the racially-charged and specifically anti-African American nature of our results. We show that the change in police behavior is specific to black drivers and does not reflect an increase in criminal behavior or a change in driving patterns, overall or by black drivers. We observe no increase in the total number of stops or stops of whites or Asians and Pacific Islander drivers, while the probability of stopping Hispanic drivers decreases, suggesting a substitution effect. Moreover, we only observe an increase in discretionary stops of blacks by the police (e.g., for "careless driving") rather than stops due to offenses that would automatically trigger a stop (e.g., car accident, fleeing, speeding, or using a mobile phone).

Second, we show that the effect of Trump campaigns on police behavior is larger in magnitude in areas with stronger, and deeper-seated anti-African American sentiment. We proxy for local anti-African American sentiment by county-average responses to two "racial resentment" questions included in the 2012 and 2014 Cooperative Congressional Election Surveys (Schaffner and Ansolabehere, 2015). We then use measures of deep-seated racial animus inherited from the pre Civil War era. We follow Acharya, Blackwell

[5] 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 racists views with coded language that enabled him to maintain plausible deniability by avoiding overtly racist language (Lohrey, 2006; Goodin, 2008; Fear, 2007). Haney-Lopez (2014) describes Dog-Whistle techniques in American politics in detail. Recently, scholars have argued that Trump has moved beyond Dog-Whistling by breaking norms of racial discourse and making explicit and direct racial appeals (see, e.g., Smith (2020)). We do not engage here in any debate about the implicit or explicit nature of Trump's discourse on race. We nevertheless observe that it targeted primarily Hispanics and migrants, particularly Muslim migrants, rather than blacks.

and Sen (2016), who show how the prevalence of slavery shaped racial prejudice against blacks in the US, to this day. We use the number of slaves in 1860 and, in order 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. We find that Trump's campaign rallies have a significantly larger effect on the probability of black drivers being stopped by police in counties that have deeper racial resentment today, as well as those that had more slaves in 1860, and those whose agricultural endowments were more suitable to slavery. By contrast with racial attitudes, other potential sources of heterogeneity, such as average income, college education, or racial fragmentation, play no role in moderating or aggravating the effect of a Trump rally on police behavior. We similarly observe no differential effect across counties more or less affected by import competition with China (Autor, Dorn and Hanson, 2013); nor between majority Democrat or Republican counties.

Third, to provide direct evidence of the Dog-whistle effect that may operate beyond the police, in the population as a whole, we revisit the experiment conducted by Newman et al. (2020). This experiment took place during Trump's 2016 campaign and presented respondents with Trump's campaign speeches. It is thus perfectly suited to provide the population counterpart of our study of police behavior. The paper by Newman et al. (2020) solely focuses on the acceptance of discrimination against Latinos. Yet, the authors also collected data on prejudice against blacks, which we use, to the best of our knowledge, for the first time in this paper. Using their sample and data, we show that respondents with above-median (or above mean) pre-existing prejudice against blacks become even more prejudiced when exposed to Trump's anti-immigration rhetoric, specifically when he accused Mexican migrants of bringing drugs and crime and being rapists.[6] No effect is observed for respondents who were not initially prejudiced before reading Trump's statement. Moreover, the effect is specific to prejudice against *blacks*: no effect is observed for prejudice towards other minority groups, including Hispanics, even

[6]During his presidential announcement speech on June 16, 2015, Trump remarked about Mexican immigrants: "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."

for respondents who were initially prejudiced against those groups. Consistent with the theoretical conceptualization of Dog-whistling appealing to stereotypes of a threatening and dangerous group, the effect is also specific to one particular dimension of prejudice: the belief that blacks are *violent*, as opposed to other dimensions of prejudice, for example, that they might be lazy, or lack intelligence. We take these findings as evidence that Trump's rhetoric whistles to the ears of people prone to think that blacks are violent, and radicalizes these views further.

Overall, our results show how Trump's rhetoric has acted upon racial resentment against African Americans. Our experimental results show that only a specific dimension of prejudice is affected – the opinion that blacks are violent, suggesting more than a simple activation of indiscriminate racist prejudice. Yet, our results cannot fully speak to whether Trump's rhetoric really aggravated prejudice or only activated or normalized preexisting prejudice. Regardless, we highlight the direct and real consequences not only on views expressed by the population but also on racially-directed behavior by the police. 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.[7] In a way, coded language can be more damaging to democratic politics, as politicians can win both the vote of the groups they appeal to as well as the views of the inattentive majority – or even of the group unknowingly targeted by the dog-whistle.

Our findings contribute to emerging literature, namely by Bursztyn, Egorov and Fiorin (2019), 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. An important difference with this work is that we do not examine attitudes towards the group directly targeted by Trump's xenophobic rhetoric. Instead, we show how Trump's rhetoric, while at least seemingly not targeted against African Americans, has activated prejudice against that group, especially among those with the most deeply ingrained

[7] 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.

preexisting prejudice.[8] As such, our findings speak to two related literatures on hidden values, or "crypto-morality", and on the Dog-whistle effect. Recent literature has documented the persistence of values and norms.[9] However, while persistent, some values may remain hidden, a phenomenon labeled "crypto-morality" by Greif and Tadelis (2010).[10] In the context of race in the US, while racial inequality was the dominant accepted social norm[11] into the early twentieth century, 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 vanish; they were hidden but kept shaping political preferences (Hutchings and Valentino, 2004; Mendelberg, 2008). In this context, experimental and survey evidence suggests that negative racial predispositions can be activated by implicit racial cues (see Mendelberg (2008) for a meta-analysis). Politicians can appeal to racial bias and activate racial resentment, using coded languages and symbols, and gain an electoral advantage – the Dog-whistle effect (Valentino, Hutchings and White, 2002; Haney-Lopez, 2014; Valentino, Neuner and Vandenbroek, 2018).[12] Our work is thus related to the recent literature on how leaders can legitimize political preferences and mobilize their followers (Dippel and Heblich, 2018; Cagé et al., 2020), to the point of conducting them to perpetrate acts that would otherwise be morally repugnant (Cagé et al., 2020).[13] By showing how Dog-whistle politics radicalizes already-prejudiced individuals, we also contribute to the literature on political radicalization and polarization.[14]

[8] Closest to our focus on a group implicitly targeted by Trump's coded rhetoric, recent work by Feinberg and Martinez-Ebers (2019) documents a correlation between the counties that hosted one Trumps presidential rallies and incidents of hate crimes. This relationship is still present even after controlling for various observable characteristics likely related to hate crime prevalence.

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

[10] This highlights a limitation of studies that measure norms in surveys. A potential confound is that opinions revealed in surveys may reflect deeply held values and moderated by the social acceptability of these views. Studies on voting behavior are similarly constrained by the supply of political parties. For example, Cantoni, Hagemeister and Westcott (n.d.) argues that the lack of supply of party platforms constrained the expression of populist Right-wing views in Germany.

[11] A dominant social norm can be defined as an "informal standard of social behavior accepted by most members of the culture and that guides and constrains behavior" (Mendelberg, 2001)

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

[13] A related literature shows how social media rather than leaders can facilitate the coordination of xenophobic attacks (Bursztyn et al., 2019).

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

A notable contribution of our work is that we examine the effect of Trump's political campaign on behavior by the police. [15] Recent work highlights the pro-social inclinations of police officers (Friebel, Kosfeld and Thielmann, 2019), suggesting that self-selection of cooperative agents in the bureaucracy, and the police in particular, could act as a counter-power against the unraveling of norms stemming from the behavior of political leaders. While a large body of work has already established racially-biased behavior of the police, and the entire justice system more broadly (see footnote 1), our work shows how political events can exacerbate this behavior, limiting the hopes of an independent bureaucracy providing a buffer against racial politics.

2 Data

In the following section, we describe the data sources used in the paper.

Police Stops: Our data on stops by the police comes from Pierson et al. (2020), who have publicly made available the data on the Stanford Open Policing Project website. To construct a national database of traffic stops, Pierson et al. (2020) 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 contains approximately 95 million stops from 21 state patrol agencies and 35 municipal police departments from 2011-2018. The data contains information on the date of the stop for all stops. For some stops, the data contains information on the driver's race, age, and sex. For a limited set of stops, we also have information on the reason the commuter was stopped; what happened during the stop (i.e., whether a search was performed); and the outcome of the stop (i.e., whether the driver was issued a citation, a warning, or arrested).

Campaign Rallies: We collect data on the rallies by 2016 Presidential candidates

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

[15] We thereby contribute to the literature that studies the behavior of the bureaucracy. While some work focuses on the power of financial incentives (Ager and Voth, 2020; Bertrand et al., 2019), our work is more connected to the literature on social incentives and organizations (see Ashraf and Bandiera (2018) for a review)

from the Democracy in Action website (Appleman, 2019). The website documents the schedule of presidential candidates ranging from the pre-campaign to the presidential inauguration. We geo-code the presidential rallies by Donald Trump starting from June 17th, 2015, a day after he announced his Presidency to November 7th, 2016, a day before the 2016 general elections. We also geo-code information on the Democratic presidential candidates in 2016, Hillary Clinton, and the other main contender to the Republican investiture in 2016, Ted Cruz.

We then match police stops data with the campaign rallies data. For each police stop, we match the date of the closest rally. Altogether, we have 190 Trump campaign rallies (out of 324) in 142 counties that overlap with data on 19,186,644 police stops for which we have information on the driver's race. These counties are plotted in Figure 1. Race is recorded as "asian/pacific islander", "black", "Hispanic", or "white". We restrict our dataset to 2015, 2016, and 2017, and are left with 11,931,161 police stops.

Summary Statistics: Summary statistics are provided in Table 1. 20.39% of drivers stopped are black, 51.50% white, and 24.10% Hispanic. We have information on whether a search was conducted for 7,521,505 stops, and information on the outcome of the stop and search for 7,071,236 stops. 6% of stops result in a vehicle search. 82.03% of stops ensue in a citation or a warning, 13.27% in a summons, and 4.70% in an arrest. 1.73% of the stops represent black drivers whose vehicles were searched. Roughly 29% of the vehicle searches (1.73%/5.99%) are conducted on black drivers. Yet, black drivers represent "only" 10.69% of the summons, and 17.18% of the arrests – much lower proportions than the proportion of black drivers in stops or vehicle searches – while they represent 22.13% of the citations or warnings.

In addition, we exploit the following county-level data to explore heterogeneous effects:

County Characteristics: Data on the number of blacks, the number of Hispanics, ethnic fractionalization, average income, and average college completion is from the 2015 American Community Survey. For an average stop, 11.22% of the county population is black, and 24.73% Hispanic. Black drivers are thus overrepresented among stops by a factor of two, while Hispanics are not. Data on county-level import competition shock is from Autor, Dorn and Hanson (2013), and data on 2012 county-level vote shares for Obama are from Leip (2016).

Racial resentment: Our measure of racial resentment comes 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. We use questions CC442a and CC422b, "Racial Resentment A" and "Racial Resentment B". The questions ask respondents how much they agree, on a scale of 1 to 5, to the following statements: "Racial Resentment A": "*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 B": "*Generations of slavery and discrimination have created conditions that make it difficult for Blacks to work their way out of the lower class*". We calculate the share of people who somewhat or strongly agree with the first statement and the share of people who somewhat or strongly disagree with the second statement. Therefore, higher values indicate higher resentment. For ease of interpretation, we normalize all variables to have a mean 0 and a standard deviation of 1.

3 Empirical Strategy and Results

3.1 Difference-in-Differences Analysis

3.1.1 Empirical Specification Our analysis is conducted at the stop level. We estimate 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 at 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, $D_{c,t}^{(a,b)} = Max(\mathbb{1}(a \leq d_{c,t,e} \leq b)_{e=1,...,N_c})$. 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. 99 out of 142 counties in our sample have exactly one Trump rally, while 23 have 3 or more rallies. With a slight abuse of notation $D_{c,t}^{(-\infty,a)}$ is 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 as 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 $D_{c,t}^{(a,\infty)} = Max(\mathbb{1}(d_{c,t,e} \geq a)_{e=1,\ldots,N_c})$.

To estimate the effect of Trump rallies on police stops we then estimate the following regression:

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

where $Black_{ict}$ is a dummy that takes value one if the driver stopped by the police in stop i county c on date t is black.

One may be concerned that counties in which Trump held a rally may differ systematically from other counties. For example, Trump or his campaign team may 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 accounted for by county fixed effects α_c. Moreover, to abstract from any further consideration related to the selection of counties in which Trump held a campaign rally, we only rely on the set of counties in which Trump has ever held a rally to estimate Equation 1. We focus our analysis on police stops in the years 2015-2017. Day fixed effects θ_t account for daily fluctuations in the nature of the police stops. These fixed effects, for instance, account for differences in police stops across different days of the week, holidays, or end of the month effects.

The rally may disrupt the daily routine of police departments in several ways. Due to the organization of a large-scale public event, it could be 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, authorities may want to enhance security in their local area and increase the patrolling of roads by deploying officers who do not normally patrol roads. We control for such disruptions using an indicator that takes value one for county c at 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 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, and we estimate Equation 1 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 $D_{c,t}^{(-\infty,-k-1)}$ that is equal to one for the days prior to the comparison window k, and $D_{c,t}^{(k+1,\infty)}$ that 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 e in the county and define the windows around each event.

For this reason, to define the dummy variables that capture the different windows, we have 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 $D_{c,t}^{(31,\infty)}$ indicates more than 30 days after the rally. β therefore captures the change within a county in the probability that a police stop involves a black driver 30 days after any Trump rally when compared to the 30 days prior. In the potential outcome framework, our identification assumption thus only requires that after controlling for day fixed effects, the probability of a police stop of a black commuter would not change in the k days after a Trump rally compared to k days before, in the absence of the rally. We check that our results are not subject to potential issues with two-way fixed effects estimators highlighted by Abraham and Sun (2018) and de Chaisemartin and d'Haultfoeuille (2020).

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, therefore, adjust standard errors for two-way clustering at the county and day level. A potential threat to the identification assumption is that the precise timing of Trump rallies across counties may be correlated with county-specific trends in the daily stops of black drivers. We address this in two ways. First, we include linear or quadratic county-specific time trends. Second, we present the results of an event-study analysis that shows that there are no pre-trends in the rate of black stops across counties. The event studies result also shows that the only period at which we pick up a treatment effect is exactly immediately after a Trump rally and up to 50 days later.

3.1.2 Results Table 2 shows the estimates of Equation 1 for increasing windows k around a Trump event. The Table shows that the probability that the driver stopped by the police in a stop is black increases after a Trump rally. The effect is immediate, constant in magnitude for the first 30 days after a rally, and then fades away slowly. The effect remains statistically significant for up to 100 days after a rally, although it is only half in magnitude at a 100 days window compared to the largest effects immediately after the rally, suggesting that the effect goes back to zero after 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 in magnitude and is no longer significant after 50 days. In what follows, we retain a 30-days window after a rally as the main focus of our analysis. We check in Table A1 that our results are robust to including county-specific flexible time trends, including linear or quadratic trends. If anything, the inclusion of linear or quadratic time trends improves the statistical precision of our results. In Appendix Table A4, we also show that our results are robust in a different Difference-in-Differences specification in which we only include observations that fall within the k days of a rally, for $k = 10, ..., 100$.

The magnitude of the effect immediately after a Trump's campaign rally and up to 30 days after it suggests a 0.78 percentage point increase in the probability of a black stop. Given that in the 30 days before a Trump rally, the probability that a driver that is stopped is black is 18.65% in our estimation sample, this amounts to a 4.2% increase. The total number of stops in our sample in a 30 days window prior to any rally is 575,042. Thus, our analysis reveals that Trump's rallies led to 4,480 additional stops of black people by the police in the month following the events. This number is an underestimate since we only have information on 190 out of roughly 320 campaign rallies and on a subset of law enforcement agencies. A recent report by the Bureau of Justice Statistics estimates that there are a total of 20 million stops a year, or about 55,000 a day (Davis, Whyde and Langton, 2018). Our sample contains information on 10,886 stops a day, on average. These figures suggest that we may underestimate the total effect by a factor of 6.65,[16] suggesting that Trump's rallies led to nearly 30,000 additional stops of black people by the police in the month following the events.

Recent econometric literature on staggered Differences-in-Differences highlights potential issues with the two-way fixed effect estimator we are using here. One of the main insights of this literature is that the estimated parameter is a weighted average of each treatment (in our context, each rally) where the weights may be negative. We first follow the recommended diagnostics by de Chaisemartin and d'Haultfoeuille (2020) and show that for our specification none of the weights are negative. However, it is worth noting that our setting does not perfectly match the situations studied by de Chaisemartin and d'Haultfoeuille (2020). First, the treatment only lasts for 30 days (in our baseline). 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 30 days. In a context closer to our situation Abraham and Sun (2018) proposes to estimate the treatment effect for each event and then average it out. In figure A6, we display the distribution of the estimated Differences-in-Differences parameters for each county. Following the technique suggested by Abraham and Sun (2018), we combine these estimates and find that the probability of stop of a black driver increases by 0.87 percentage points.

Our analysis so far deals with all the rallies held during the campaign, either for the nomination or for the Presidency. Many rallies held for the nomination took place when Trump was still a marginal player. We expect rallies held during the presidential campaign to have bigger effects for several reasons. 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 (2019) reveals that signals of Trump's popularity makes

[16] 6.65 is the sum of 5.05, from the fact that we have information on 10,886 out of 55,000 stops, and 1.6, from the fact that we have information on 200 out of 320 rallies.

xenophobic respondents more likely to express their views. Moreover, presidential rallies involved bigger crowds and likely had a larger effect than marginal nomination rallies. On the other hand, Enke (forth.) shows that Trump's campaign became more moderate after he secured the Republican nomination, which would imply that the presidential rallies would have a smaller effect on black stops.[17] In Column 1 of Table 3, we restrict the estimation sample to rallies held during the presidential campaign only. The results are robust and larger in magnitude compared to the analysis that includes all rallies. The probability that black drivers are stopped by the police increases after a Trump presidential rally. At 30 days, the probability that the driver stopped by police is black increases by 1 percentage point, or a 5.8% increase with respect to 30 days prior to a rally.[18] In other words, the effect of Presidential campaign rallies is about 38% larger in magnitude than all rallies combined.

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

3.2 Event-Study Analysis

In this section, we perform an event-study analysis, which offers several advantages. First, it enables us to check for the existence of trends in the probability of black stops 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, it enables us to study how the effect changes, time period by time period, instead of averaging over the whole window k as we did before. The event-study specification is as follows:

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

[17] Last, it could be the case that places visited during the nomination campaign were very different from places visited during the presidential campaign, because the marginal county or state is very different for the nomination compared with the presidential election. Additional analysis in Appendix Section 2.3 suggests that the counties in which Trump held campaign rallies for the Republican nomination or the Presidency did not differ in a statistical way along with a wide range of dimensions, including pre-trends in the number of police stops or the share of black stops.

[18] The baseline probability of a black stop 30 days before a presidential campaign is 0.175.

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

Figure 2 shows the estimates of β_τ in Equation 2. We see that Trump rallies result in a substantial and immediate spike in black stops following a rally. The probability that a police stop is of a black driver increases by 0.8 p.p. in the first ten days following a rally by Trump. It stays stable 30 days after a rally and reaches its highest magnitude (nearly 1 p.p.) 30 days after a rally. The effect is nevertheless transitory and fades away completely 50 days following a rally. In the figure, we also see the trend in the share of black stops prior to a rally by Trump. The share of black stops was very stable and did not change significantly in either direction up to 105 days leading to a Trump rally. This result argues against the possibility that Trump may have specifically timed his rallies in certain counties as a function of local police behavior.

4 Mechanisms

We adopt a series of strategies to unpack the mechanism behind the effect of Trump rallies on police stops of black drivers. Several potential mechanisms could explain the effect. First, it could simply be due to the occurrence of a political rally, particularly by a Republican candidate, given the Republican party's reputation for being tough on crime. Second, the result could derive from compositional changes to the police force after a rally, which would, for example, follow from local political decisions, rather than the behavior of individual police officers. Another explanation for our results could lie in a supply-side effect and a change in criminal and driving behavior after a rally. We present a series of placebo and robustness results that deal with each of these possible explanations and establish the specific link between exposure to a Trump rally and racially-directed police behavior towards black drivers. Finally, we show that the effect is larger in areas with stronger and deeper-seated racial animus against African Americans.

4.1 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 study the impact on police behavior of rallies by the other presidential candidate Hillary Clinton during her 2016 campaign. We estimate Equation 1 using the same windows after each rally as in Table 2. Table 4 (Panel A)shows that Clinton rallies did not result in any change in police behavior towards black drivers. The estimated coefficients are sometimes positive, sometimes negative, and never statistically significant.

However, Clinton may not be the right comparison. The change in police behavior towards black drivers after a Trump rally may be due to the fact that Trump was running for the Republican party, a party reputed 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 the other main contender for the Republican nomination in 2016, Ted Cruz. Ted Cruz was closely trailing Donald Trump during Republican primaries in 2016. He ran on a socially conservative, pro-gun, anti-crime and pro-police platform.[19] We again estimate Equation 1 using the same windows as in Table 2 after Cruz 2016 rallies. Table 4 (Panel B) shows that Cruz's rallies did not result in any change in racially-directed police behavior. The estimates range from -0.28 to 0.32 p.p. and are never statistically significant.

4.2 Within-officer comparisons and supply-side channel

We consider two additional alternative explanations for our results. The first consists of a compositional change in the police force, which may occur after a Trump rally due to

[19]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.

local (e.g., mayor or county sheriff) political decisions about which units or officers to deploy. The second stems from a possible supply-side change in overall crime and driving patterns after a rally. We provide here evidence that argues against each of these possible explanations.

4.2.1 State troopers and within-officer comparisons To rule out changes in stop patterns that may be due to local (e.g., mayor or county sheriff) politics, 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 2 of Table 3 show that the effect of Trump rallies on black stops remains statistically significant at the 1% level and is larger in magnitude. We provide in Column 3 of Table 3 a more direct test that rules out the possibility that the Trump effect could be due only to a change in the deployment of different units or individuals after a rally, or any other compositional change in the police force. We add to Equation 1 officer-level fixed effects. This ensures that any effect of a Trump rally on the probability of black stops is due to a change in the behavior of police officers themselves. Since information on individual officer identifiers is only available for a subset of stops, the estimation sample drops by 30.7%. Nevertheless, our results remain robust.

4.2.2 Supply side channel Another potential explanation for our results lies in the supply side, i.e., changes in citizens' driving or criminal behavior after a Trump rally. If this were the case, we would expect a change in the overall number of stops and the rate of vehicle searches or arrests. To examine this possibility, we present in Columns 4 to 9 of Table 3 the results of additional specifications in which we use as alternative dependent variables the total number of stops, the probability of vehicle search, or (in increasing order of severity) warnings, citations, summons, or arrests after a Trump rally. To examine the effect of a Trump rally on the total number of stops, we aggregate our dataset at the county-day level. For the probability of vehicle search, warnings, citations, summons, or arrests, our specification is identical to Equation 1, but we use these probabilities rather than the probability that the driver pulled over is black as the dependent variables. We observe no change in any of these outcomes, suggesting that changes to driving patterns or criminal behavior cannot explain our results. These results also rule out that the change in police behavior stems from a general behavioral change by the police, characterized, for example, by increased zealousness, rather than behavior specifically targeted against black drivers.

It could still be the case, however, that drivers change their behavior along racial lines. It may be the case, for example, that Trump rallies appease white drivers, but antagonize racial minority drivers, thereby leaving the overall number of stops or the overall probabilities of searches, citations, warnings, summons or arrests, unchanged, but justifying the increase in stops of minority drivers. To test for this hypothesis, we examine the stops of whites and other racial minorities in Columns 10 to 12 of Table 3. We estimate Equation 1 for the probability of stops for the other reported races or ethnicities in the data: whites, Hispanics, Asians, or Pacific Islanders. We do not observe any change in the probabilities of a stop of whites and Asians or Pacific Islanders. For Hispanics, we observe a decrease in the probability of stop, although this result is not robust to including linear trends, or to restricting our sample to state troopers (see Table A2 and Table A3 in the Appendix). Moreover, we observe in Columns 7 and 8 of Table A3 that contrary to the results for black drivers, the change in stops of Hispanic drivers is only due to automatically triggered stops, with no significant change in the probability of a discretionary stop by police. This suggests that the effect is driven more by the behavior of Hispanic drivers rather than police, although we observe no significant change in the outcome of the stop in Columns 3 to 6 of Table A3.[20] The negative - or at least nil- effect on stops of Hispanics is particularly remarkable, given that Trump explicitly targeted

[20]The results for Hispanics may also be taken with some caution, given evidence of misreports of Hispanics as whites documented in the literature(Luh, 2019). Given the potential for misreporting Hispanics as whites, it is possible that the estimated coefficient is downward biased, explaining the significant and negative coefficient associated with the effect of a Trump rally. The increase in stops of black drivers combined with a decrease of stops of Hispanics is compatible with a model in which police substitutes black arrests for Hispanic arrests either to remain within the limits of quotas of minority stops or because of redeployment to different - more black- areas to fulfill their objective of stopping more black drivers - thereby arresting fewer Hispanics not because of an active decision but due to police's reduced presence in Hispanic rather than black areas. We aim to use more disaggregated data on where the stops occur to tackle this question in a future version of the paper. We also aim to use speech data at different rallies to investigate which words aggravate or attenuate the effect on black stops.

Hispanics, rather than blacks, in his campaign speeches. We come back to this issue in more detail in Section 5, where we show in an experiment with a representative online panel of the population that Trump's campaign speech reinforces negative stereotypes against blacks being violent, but does not affect stereotypes against Hispanics.

Even though we do not observe an overall change in the number of stops, the rates of citations, searches, and arrests, nor an increase in the probability of stops of other racial minorities, it could still be the case that *black* drivers commit more driving offenses after a Trump rally, justifying their stopping by the police. We leverage additional information on stops in the next subsection to show that the increase in the probability of stops of black drivers stems from police – rather than black drivers' - behavior.

4.3 Establishing racially-directed police behavior: additional information on causes and outcomes of black stops

If the increase in stops of black drivers was justified by black drivers' criminal or offensive behavior, this should be reflected in the severity of the stop outcome. For example, we should observe an increase in the probability of a stop leading to a vehicle search or an arrest. Columns 1 to 5 of Table 5 show that this is not the case. We also do not observe an increase in the probability of a vehicle search (Col. 1), which requires that the officer has probable cause to believe that evidence or contraband is present in the vehicle (Hendrie, 2005). The probabilities of a simple citation or warning (Col. 2 and 3) go up, but the effect is not statistically significant at conventional levels. As for the probabilities of summons or arrests, the estimated coefficient is close to zero (0.040 for summons, Col. 4) or even negative (-0.111 for arrest, Col 5) and never statistically significant. These results show that the increase in stops is not due to serious - or, in fact, any type of offense but instead likely reflect discretionary and unjustified stops by police. Columns 6 and 7 of Table 5 confirm that this is the case.

We exploit information on the justifications for a stop, and we differentiate between discretionary stops and stops that would be automatically triggered. The kind of event or offense that triggers an automatic stop or police intervention in our data consists of:

accident or collision, criminal offenses including assault, fleeing the scene of a crime, fleeing the police, speeding, driving an unregistered car or a car with a defective license plate, or brake violations. Other offenses that we call discretionary stops are either only revealed after a stop (e.g., driving without a license) or subject to value judgment and discretion by the officer (e.g., following another vehicle too closely, driving in the wrong lane, or generally "careless" driving). Results in the last two columns of Table 5 show that automatically triggered stops do not explain the increase in stops of black drivers. The point estimate of β for non-discretionary stops of black drivers after a Trump rally is negative (-0.185) although not statistically significant. The point estimate of β for discretionary stops is 0.889. In other words, the increase in police stops of black drivers stems exclusively from discretionary stops.

Taken together, our results suggest that the change in police behavior after a Trump rally is unjustified and racially-motivated, specifically against blacks. In the next subsection, we show that this effect is larger in magnitude in areas with stronger, and deeper-seated, prejudice.

4.4 Heterogeneity across counties

Even though Trump's xenophobic rhetoric explicitly targeted Hispanics rather than blacks, we observe an increase in police stops of black drivers. A potential explanation offered by literature on Dog-whistling is that Trump's rallies and speeches activated negative stereotypes of the police towards black people. Recent literature has established that police officers (especially white) have a preference for discrimination against blacks (Roland G. Fryer, 2019; Goncalves and Mello, 2017), are more racially resentful and are more likely to see blacks as violent compared to non-police (LeCount, 2017; Ba and Rivera, 2020). The dog-whistle theory would predict that the most prejudiced police officers are the ones that should respond to the Dog-whistle. Unfortunately, we do not have enough information on individual police officers, including their race, to test for heterogeneity across police officers. However, we can exploit cross-county variation and study whether Trump rallies have a larger effect on police behavior in more racially resentful areas. We provide a more direct test of the activation of the prejudice that blacks are violent at the individual level in Section 5.

To understand which underlying characteristics amplify or alleviate the effect of Trump rallies on police behavior, we analyze the heterogeneous impact of Trump rallies by including an interaction term between $D_{c,t}^{(1,30)}$ and pre-determined county characteristics in the estimation of 1. We also control for linear trends based on these pre-determined county characteristics. Therefore the interaction term captures differential changes from the underlining trend of the probability that the police stops a black driver as a function of pre-determined county characteristics.

Our primary focus is on underlying racial resentment at the county level. We proxy racial resentment by the county-average responses to the two racial resentment questions included in the 2012 and 2014 Cooperative Congressional Election Surveys (Schaffner and Ansolabehere, 2015) described in Section 2. We also use a measure of deep-seated racial animus inherited from the pre-Civil War era: the number of slaves per capita in 1860.[21]. 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).

Results are presented in Columns 1 to 4 of Table 6. Consistently, 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 stop of black drivers by the police is between 51% (Column 2) and 64% (Column 1) bigger in counties that are one standard deviation above 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) and almost double in counties whose soil conditions are one standard deviation above mean cotton suitability (Column 4).

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,

[21]We use the Census of 1860 as the last official recorded the number of slaves prior to the abolition of slavery

such as ethnic fragmentalization. While the interaction with the share of black people in the county is positive and significant (Column 5), the interaction with ethnic fragmentalization is not (Column 6). Moreover, we show that the effect of racial prejudice and the long-lasting effect of slavery is more important than other potential sources of heterogeneity, stemming for example from political partisanship, as measured by the vote share for Obama in the 2012 Presidential elections (Column 7), differences in income (Column 8), or education (Column 9). Finally, we show that the trade shock with China, which has been shown to influence voting (Autor et al., 2017, n.d.) does not play any role in moderating the impact of Trump rallies on the expression of racial prejudice by the police, whether we use the actual or instrumented measure (Columns 10 and 11) from Autor, Dorn and Hanson (2013).

5 Experimental Study

In this section, we first describe the experimental study implemented by Newman et al. (2020) during the 2016 Trump campaign. As explained in the Introduction, the paper by Newman et al. (2020) focuses on whether Trump's campaign made people more accepting of discriminatory behavior against Hispanics. We instead show how respondents exposed to Trump's (in)famous racially inflammatory speech about "Mexico [...] sending people [...] bringing their problems"," bringing drugs", "bringing crime", and being "rapists" become more prejudiced, not against the group singled out in this speech, Hispanics, but instead against *African Americans*. Specifically, respondents who are initially highly prejudiced become radicalized in their prejudice that African Americans are *violent*. No effect is observed against other racial minorities nor for other dimensions of prejudice against African Americans. We take this as evidence of the Dog-whistle effect: the sheer evocation of crime, drugs, and rape, even when supposedly associated with a group of foreign nationals, radicalizes the most bigoted respondents in their long-standing prejudice that African Americans are violent.

We use the same sample and data as Newman et al. (2020) but implement an entirely novel analysis.

5.1 Data

The data comes from the study by Newman et al. (2020) and is provided by Collingwood (2020).[22] For this study, the authors conducted a two-wave panel online survey experiment using Amazon's Mechanical Turk platform. The experiment took place during the 2016 Trump campaign. This study 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 experimental 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 standard for M-Turk samples, the sample is more left-leaning and more highly educated compared to a nationally representative sample, suggesting that obtained estimates related to prejudice may provide a lower bound of the population treatment effect.

In addition to socio-demographics and information on political orientation, the authors collected measures of racial prejudice towards 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-Hispanics speech affects prejudice and discriminatory behavior against Hispanics. However, the fact that they measure prejudice against other groups, and along several dimensions of prejudice provides a perfect opportunity to study the extent to which Trump's seemingly anti-Mexican rhetoric activated prejudice against blacks.

The experimental treatment consisted of exposing respondents to political speeches that vary in their content (racially inflammatory or not) and in their author (Jeb Bush, Hilary Clinton, or Donald Trump). Respondents were randomly assigned to one of six experimental conditions, which are reproduced in Figure A1 in the Appendix. In the control condition, respondents were neither exposed to Trump nor racially inflammatory speech.

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

The primary treatment condition – "Trump Prejudice" – presented the respondents with racially inflammatory statements made by Trump against Mexican immigrants (the quote from Trump's announcement speech about Mexicans being criminals, drug dealers, and rapists already quoted in footnote 4). This quote was – in all appearances– targeted against Mexican immigrants but also contained a strong reference to crime, and may resonate in the ears of prejudiced respondents who associate crime with African Americans. We, therefore, test whether prejudice against African Americans increases among respondents exposed to this condition. The study differentiates prejudice along several dimensions: laziness, lack of intelligence, tendency for violence, or in the US illegally. For each dimension, respondents are asked to rate on a scale from 1 to 5 whether an adjective - either "lazy", "intelligent", "violent", or " here illegally" describes the race. 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, we expect specifically that exposure to the treatment activates the prejudice that African Americans are *violent*. This dimension of prejudice is also arguably the one that most influences how the police apprehend 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 just mentioning immigration or the effect of Trump alone, the study includes two additional treatments. Firstly, the "Immigration Prime" treatment featured a discussion of immigration but involving Jeb Bush as opposed to Donald Trump as the Republican candidate. Secondly, the "Trump Prime" treatment featured 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. We have no prediction concerning how endorsement or condemnation by other political elite would affect the Dog-whistle effect. However, we retain these last two treatment conditions in the analysis to preserve power (the results are robust to excluding these treatments from the estimation sample, see Figures A2 to A5 in Appendix).

5.2 Empirical specification and Results

The study by Newman et al. (2020) is focused on attitudes towards Hispanics and uses as the dependent variable the acceptability of discriminatory behavior against Hispanics, measured in a specific vignette experiment. We use a different dependent variable, which the authors do not use in their study. Our focus is on the expression of prejudice towards African Americans, and especially the prejudice that African Americans are violent. Therefore, we use as the dependent variable the endline measure of such prejudice, after the treatment.[23] We also control for initial prejudice, measured before the treatment, in all specifications.[24]

By nature, Dog-whistling resonates only - or more strongly- among the subgroups who hold the views that are being harnessed. We hypothesize that the Trump Prejudice condition will only activate prejudice 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 median)[25] and the treatment condition. We also present the results of placebo specifications in which we use the same dimension of prejudice against other racial groups, as well as other dimensions of prejudice against Blacks.

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

Our estimation equation is as follows:

[23] The vignette implemented by Newman et al. (2020) to measure the acceptability of discriminatory behavior was only implemented for a Hispanic target of discrimination, not black. By contrast, the endline prejudice was measured for all racial groups. It is unclear to us why the authors did not also use the endline measure of prejudice as their dependent variable, as we do in this paper.

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

[25] 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.

$$PrejViol_{i2} = \alpha + \beta Treat_i X PrejViol_{i1} + \gamma PrejViol_{i1} + \theta Treat_i + \eta X_i + u_i, \qquad (3)$$

$PrejViol_{it}$, for $t = 1, 2$, is the measure of prejudice that a racial group is violent declared by individual i at time t, where $t = 1$ is the pre-treatment period and $t = 2$ the post-treatment period. $Treat_i$ 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 3. In other words, this represents the average effect of each treatment across all respondents in the sample. The full set of results is in Appendix Table A5. 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 and tolerance norms are too widely shared in the population[26] for us to detect an average effect of inflammatory speech.

However, among the most prejudiced individuals, the picture is strikingly different. In Figure 4 (full results in Appendix Table A6), when we differentiate between people above or below the median baseline prejudice that Blacks are violent, we see that those exposed to Trump's racially inflammatory statement become significantly further radicalized in their prejudice. The result is only statistically significant for prejudice against Blacks: none of the interaction coefficients are significant for any of the other racial groups. In other words, Trump's racially inflammatory statements do not affect the prejudice of respondents towards Whites, Asians, or Hispanics; even among respondents who are

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

particularly prejudiced against each of these groups. This is particularly striking as the statement is not - at least at a superficial level- directed against African Americans but Mexicans. The fact that we observe an activation of the prejudice against African Americans but not against Hispanics provides a direct validation of the Dog-whistle effect. When Trump says "crime", even when supposedly talking about another racial minority, prejudiced Americans hear "African American".

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 associating *blacks* to *violence*. In Figure 5, we provide 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. The coefficients associated with the treatments are never statistically significant, even among the respondents who are most prejudiced along these dimensions to start with. We also check in Figure 6 that the treatment does not make those with a high preexisting prejudice that African Americans are violent more likely to express prejudice in these other dimensions.

Additional results displayed in Table 7 indicate that the effect is only significant, and larger in magnitude, for white respondents. For white respondents 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 college education (0.37).[27]

Columns 7 to 9 of Table 7 show how the effect of Trump's inflammatory speech is mod-

[27] 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).

erated along political lines. It is important to stress that although Republicans display higher prejudice associating blacks with violence, far from all prejudiced individuals are Republican. 20.90% of respondents in the sample self-identify as Republicans. 59.81% of them are above the sample median prejudice level, compared to 40.46% of 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, exposure to Trump's racially inflammatory statement is not moderated along party lines: 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 speech, especially when other Republican elites condone such speech ("Trump Condone" treatment). 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, and self-identified Republicans even more so.

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.

One question is whether the effect we observe is due to a simple increase in the willingness to express prejudice or a real increase in prejudice. To an extent, exploiting other dimensions of anti-African American prejudice enables us to address this question. If the treatment simply liberated the expression of prejudice, respondents would be more likely to express any form of prejudice against African Americans. This is not the case. Trump's speech did not activate *any* kind of prejudice against blacks. For example, it does not affect the prejudice that African Americans are lazy or lack intelligence. Just the prejudice that blacks are violent. This suggests that the effect goes beyond the liberation of racially prejudiced speech.

An explanation for our results is that talks of crime, rape, and drugs, even seemingly associated with another group, activate, among the subgroup of most prejudiced Americans their long-standing and ingrained prejudice that African Americans are responsible for violence and crime. By associating these words to foreign nationals (Mexicans), Trump maintains deniability that his campaign was not anti-African American and avoids alienating those strongly attached to norms of equality and tolerance. But the most prejudiced voters understand these words differently –and for them these words radicalizes their prejudice even further.

6 Conclusion

In this paper, we show how Trump's political campaign radicalized racial prejudice against African Americans as well as its expression through one of its most fundamental and violent dimension: police behavior. Our estimates suggest that Trump's campaign events led to 4,480 additional stops of black drivers in our sample, and up to 30,000 overall. These stops would not have occurred otherwise and were not justified by either an automatic trigger or a serious offense. Consistent with our interpretation that Trump's campaign activated racial stereotypes, we find that the effect is more pronounced in areas that are more racist today, or where slavery was more prevalent historically. Combining our data to an online survey experiment that was collected during Trump's 2016 campaign, we confirm that the effect is specific to prejudice against blacks and specific to threatening stereotypes associating blacks with violence. Overall, our results show how politicians can radicalize threatening stereotypes against specific groups, by resorting to language that, at least at a superficial level, 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 overrepresented by a factor of two compared to their proportion in the population (our estimate in Section 2). Although fatal encounters between police officers and black civilians tend to capture more of the media's attention, over-enforcement of minor infractions and the kind of unjustified stops by the police that we document in this paper provide a daily and generalized expression of discrimination against minorities. The consequences are dire.[28] Ensuing feelings of discrimination and underrepresentation undermine minorities' trust in the police and public institutions at large. This opens a vicious circle, whereby daily and unjustified harassment reduces trust in the police and leads citizens to turn away from relying on the police and to seek other, informal, and ultimately violent ways of protection and retributive justice (Center, 2020). Moreover, unjustified police repression can act as a way of voter suppression, when disabused citizens extend their lack of trust in the police to all public institutions. Recent research shows how historical discrimination and violence against blacks is associated with lower voter registration by black voters (Williams, 2020). Estimating the impact of discriminatory behavior by the police today on voting behavior is left for future research.

[28] Manski and Nagin (2017) offers a recent discussion of the negative consequences of confrontational policing, including traffic stops. Durlauf (2005) discusses equity considerations in the context of racial profiling and Durlauf (2006) concludes that the benefits of profiling are not established while the harm to those who are innocent and stopped is high.

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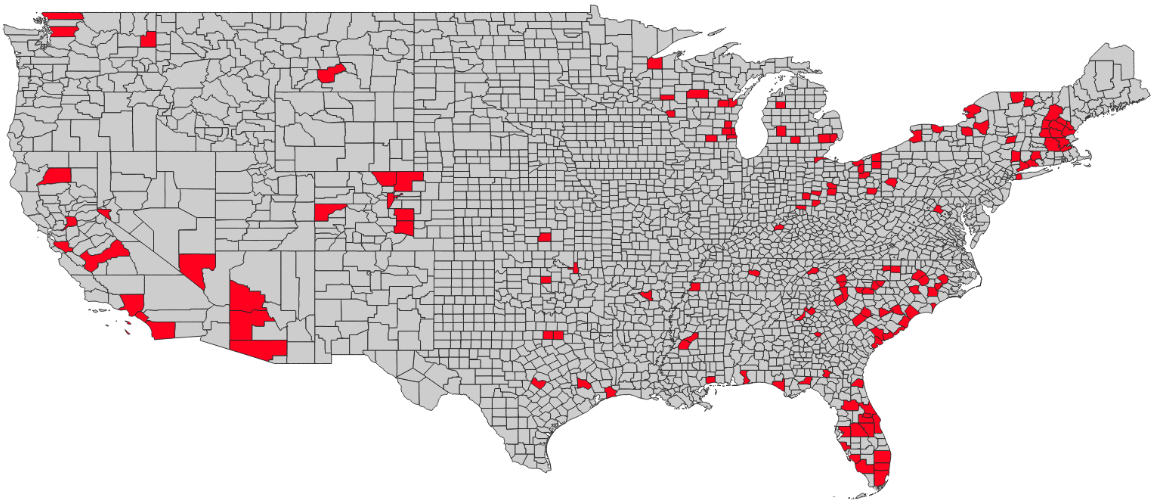
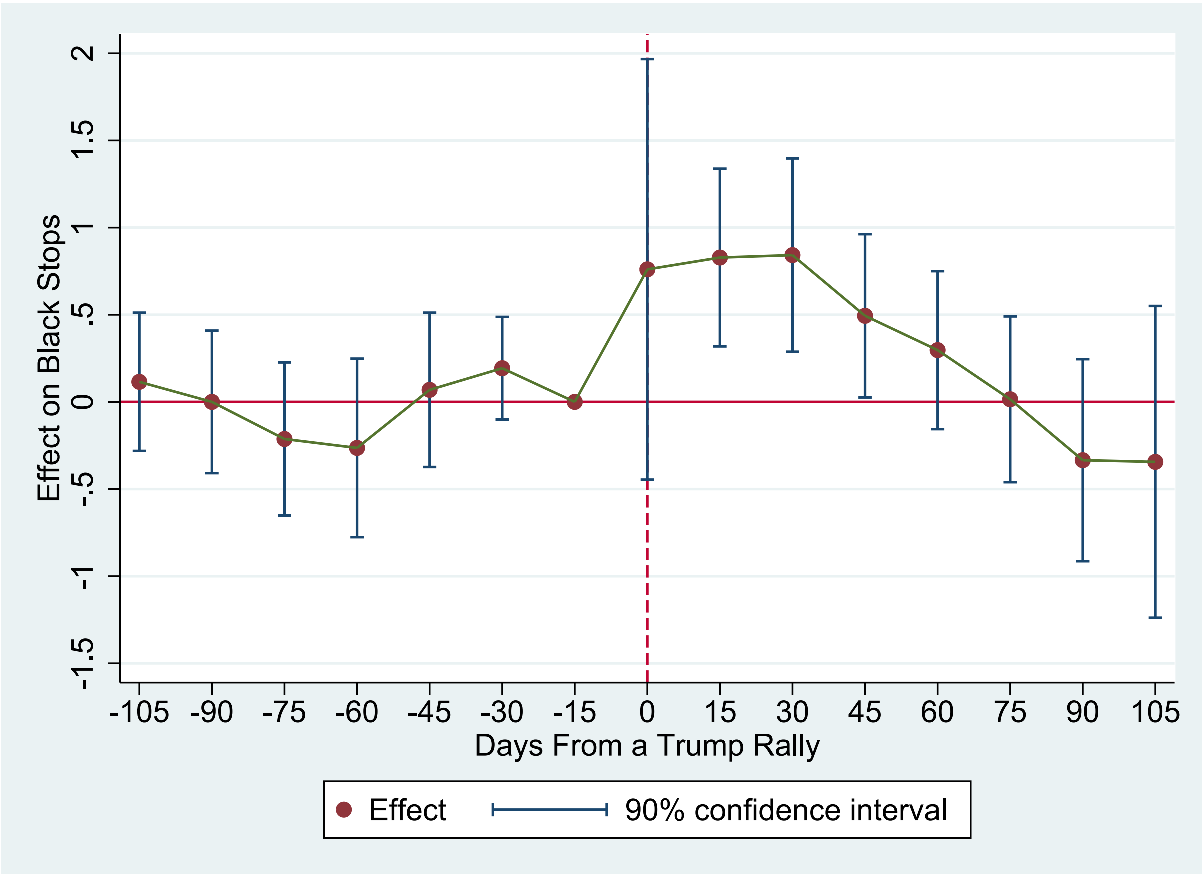


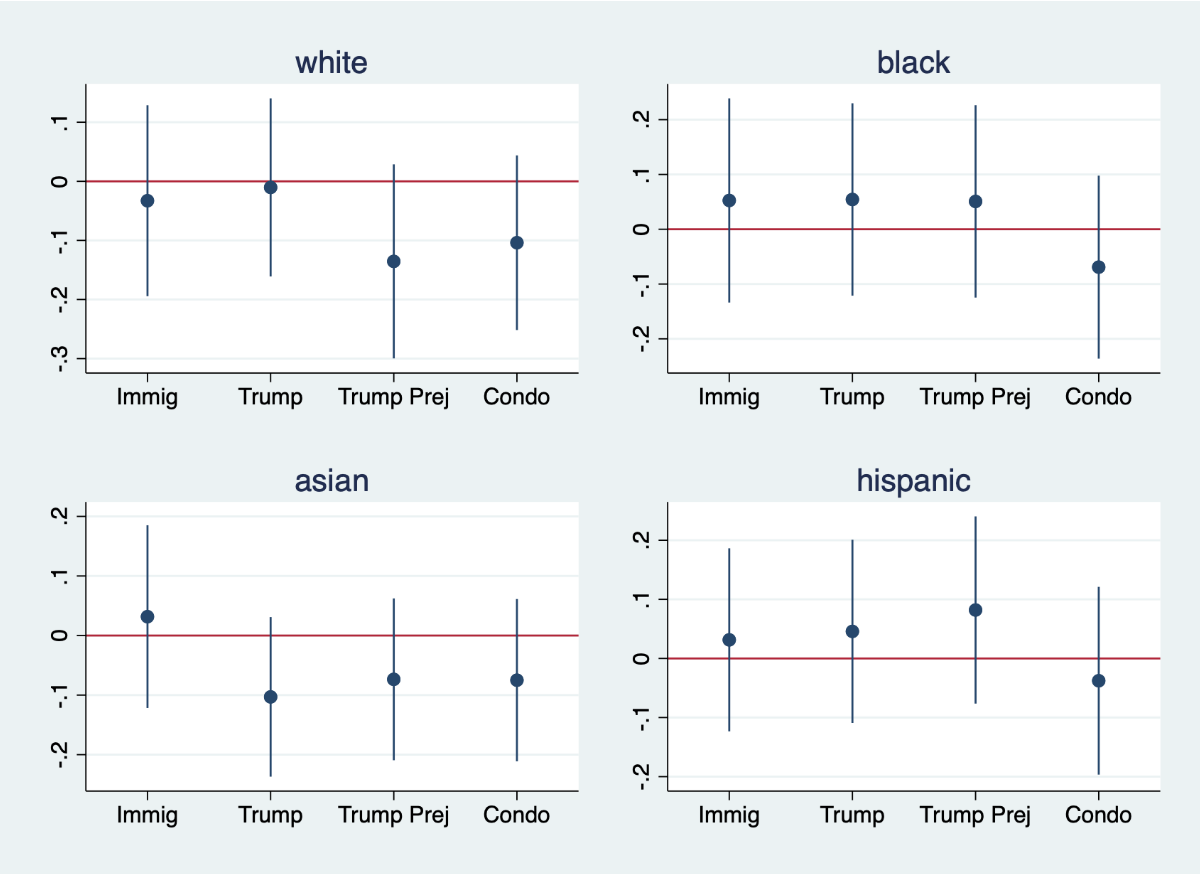
Notes: This map shows the 142 counties for which we have data on police stops and 2015-2016 Trump's campaign events.

Figure 1: Counties with campaign events and police stops



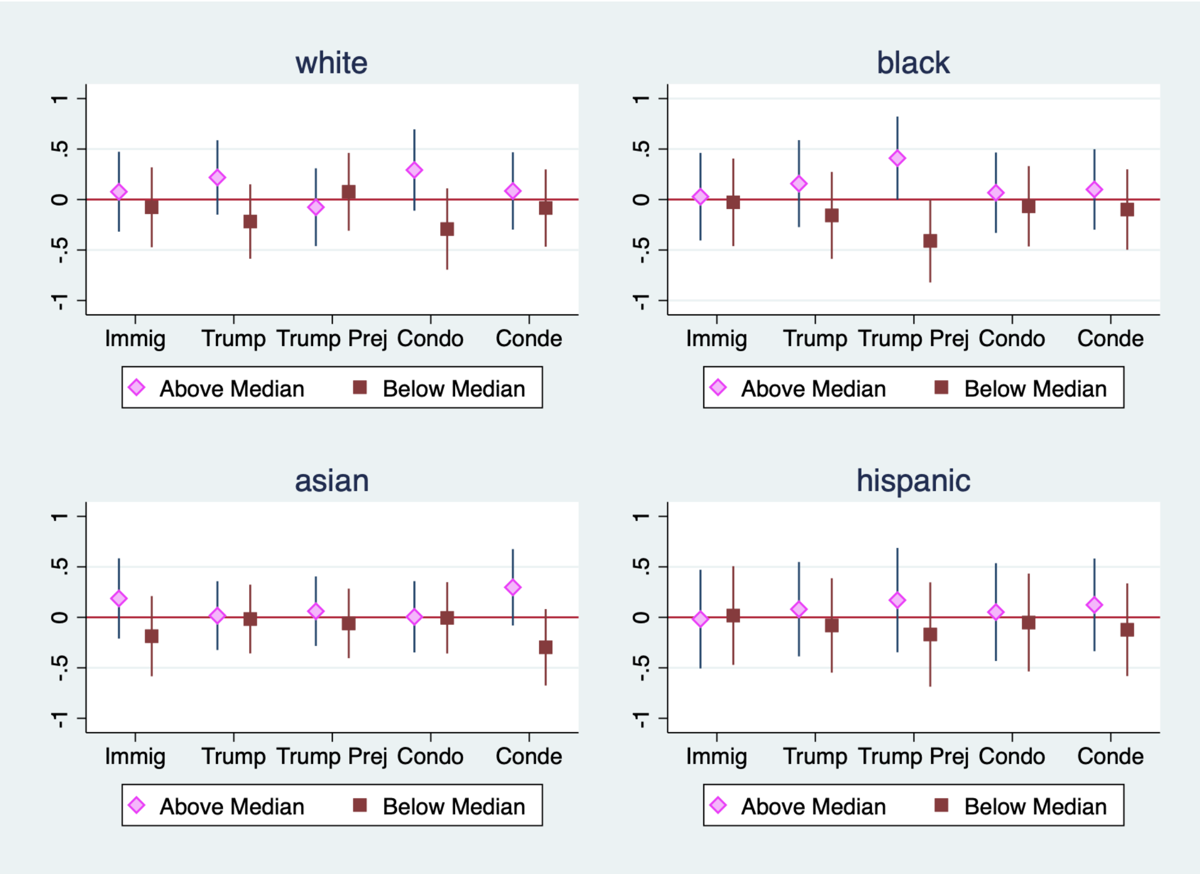
Notes: The figure plots OLS coefficients with 90% confidence intervals (vertical lines). The plotted coefficients are the β_τ coefficients associated with each month (as indicated divided by 30), described in Equation 2. Standard errors are corrected for two-way clustering at the county and date level.

Figure 2: Impact of Trump rallies on police stops of Black commuters



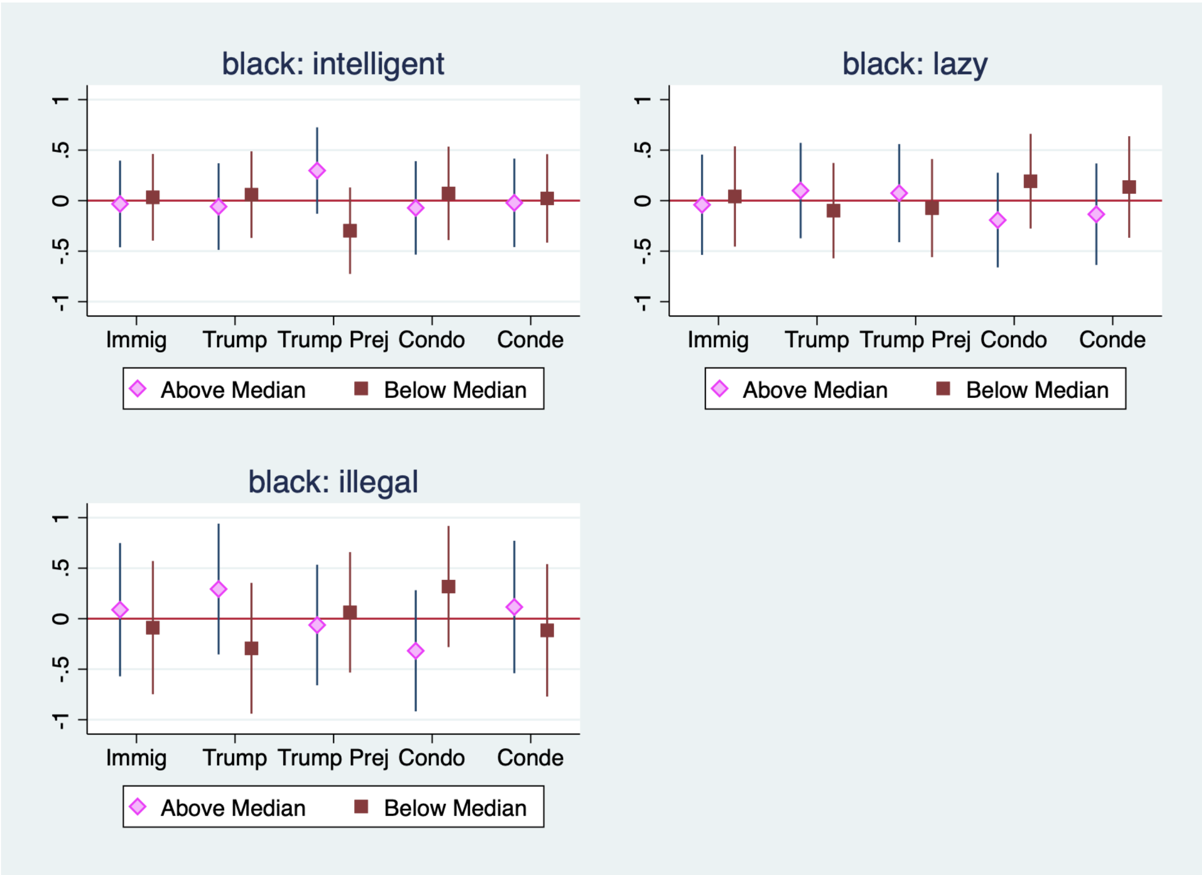
Notes: The figure plots OLS coefficients with 90% confidence intervals (vertical lines). The plotted coefficients are the coefficients associated with each treatment condition (as indicated), controlling for baseline prejudice and the full set of controls, but without including the interaction effect described in Equation 3. 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 A5 for the full regression results. The Figure shows that the treatments do not affect prejudice for the average respondent.

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



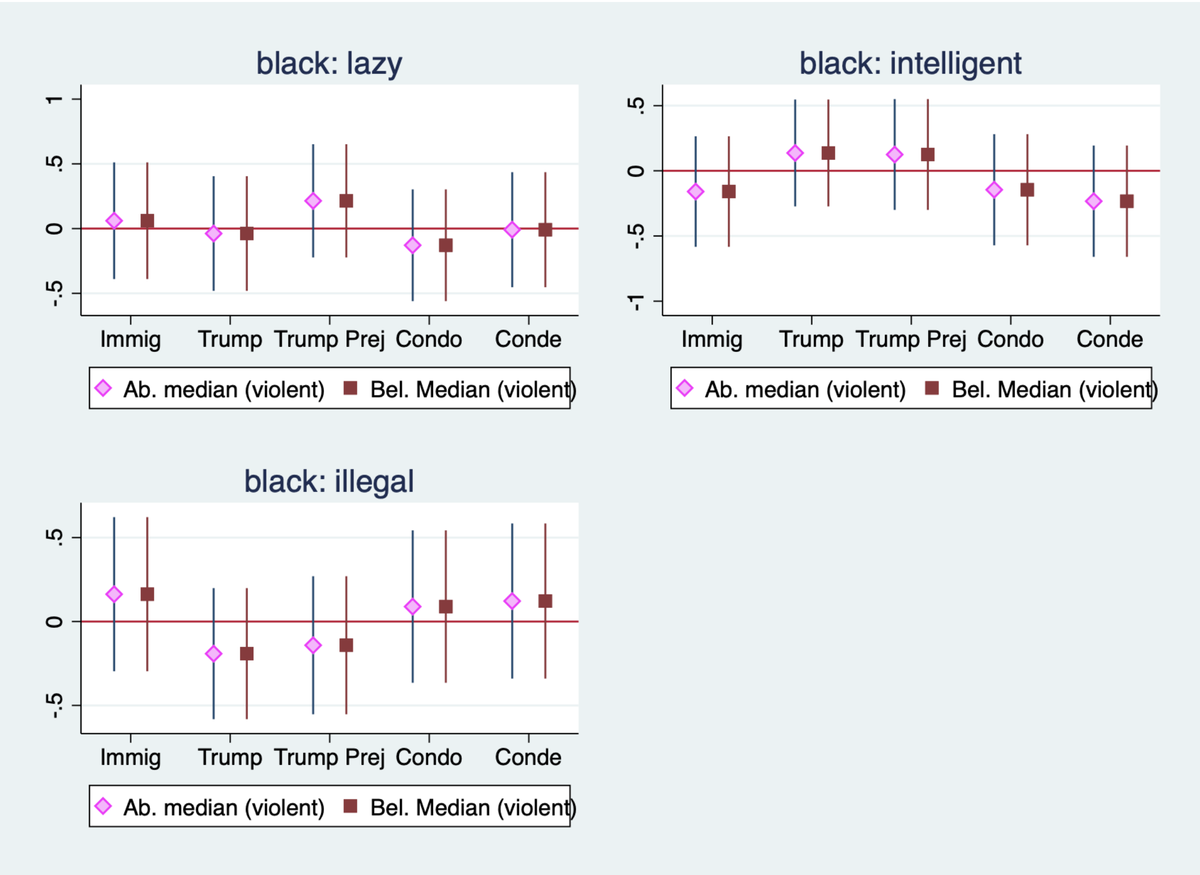
Notes: The figure plots OLS coefficients with 90% confidence intervals (vertical lines). The plotted coefficients are the coefficients associated with the interaction between each treatment condition (as indicated) and an indicator variable for being above (triangle) or below (square) the median. All regressions control for the indicator variable separately and for the full set of controls described in Equation 3. 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 A6 for the full regression results. The figure shows that Trump's inflammatory speech activates the prejudice that blacks 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 blacks are violent among prejudiced respondents



Notes: See notes to Figures 4 as well as Table A7 for full regression results. We focus here only on prejudice against blacks in dimensions other than violence, which are measured in the survey. We examine the prejudice that blacks lack intelligence, are lazy, or are in the US illegally. The Figure shows that none of the treatment conditions activates prejudice against blacks 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



Notes: See notes to Figures 5. The only difference here is that we interact the treatment with pre-existing prejudice that blacks 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 blacks along these dimensions, even for respondents who are initially highly prejudiced that blacks are violent.

Figure 6: Trump's inflammatory speech does not activate other dimensions of anti-black prejudice even among respondents who are initially highly prejudiced that blacks are violent.

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
Search	7,521,505	5.99	23.72
Warning	7,071,236	30.18	45.90
Citation	7,071,236	51.85	49.97
Summons	7,071,236	13.27	33.93
Arrest	7,071,236	4.70	21.16
Black-Search	7,521,505	1.73	13.04
Black-Warning	7,071,236	6.87	25.29
Black-Citation	7,071,236	11.28	31.64
Black-Summons	7,071,236	1.42	11.83
Black-Arrest	7,071,236	0.81	8.95
Black-Non-Discretionary	5,387,948	5.18	22.16
Black-Discretionary	5,387,948	15.51	36.20
POST-Trump	11,931,161	0.05	0.21

Notes: The Table shows summary statistics for the main analysis. The data for variables from Black to Black-Discretionary is constructed from Stanford Open Policing Project and POST-Trump is constructed from Democracy in Action website. The unit of observation is a police stop. Black is equal to one if the stopped commuter is black and zero otherwise. White is equal to one if the stopped commuter is white and zero otherwise. Hispanic is equal to one if the stopped commuter is Hispanic and zero otherwise. Search, Warning, Citation, Summons, and Arrest are equal to one if the stop resulted in a vehicle search, warning, citation, summons, and arrests, respectively, and zero otherwise. Black-Search, Black-Warning, Black-Citation, Black-Summons, and Black-Arrest are equal to one if the stopped commuter is black and the stop resulted in a vehicle search, warning, citation, summons, and arrests, respectively, and zero otherwise. 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 police stops of black commuters

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
VARIABLES	100 x $\mathbb{P}$(Black Stop= 1)									
POST-Trump	0.753***	0.817***	0.779***	0.703***	0.670***	0.594***	0.541**	0.490**	0.430**	0.388*
	(0.268)	(0.236)	(0.253)	(0.229)	(0.225)	(0.215)	(0.219)	(0.217)	(0.213)	(0.228)
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	0.090	0.090	0.090	0.090	0.090	0.090	0.090	0.090	0.090	0.090
County FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Day FE	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 OLS estimation of Equation 1. The unit of observation is a police stop. The dependent variable is a dummy equal to one if the stopped commuter is black and zero otherwise. The main independent variable is POST-Trump: our treatment window, which is equal to one for days after Trump rally, and is zero otherwise. We vary the size of this treatment window in increment 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, respectively. Other independent variables are: (i) Day-of-Rally an indicator that takes on a value equal to one on the day of the rally for county where the rally takes place, and zero otherwise; (ii) Pre-Rally: an indicator that takes on a value equal to equal to one for the days prior to a Trump rally, and zero otherwise; (iii) Post-Post-Rally: an indicator that takes on a value equal to equal to one for the days after the treatment window, and zero otherwise. For consistency, the windows for Pre-Rally and Post-Post-Rally are defined as symmetric to the treatment window. In other words, if the treatment window is 10 (resp. 100) days, Pre-Rally takes values 1 for the 10 (resp. 100) days before the rally; and Post-Post-Rally takes values 1 for the 10 (resp. 100) days after the treatment window, i.e. 20 (resp. 200) days after the rally. All estimations include county and day fixed effects. Standard errors are adjusted for two-way clustering at county and day level. The table shows that the probability that a commuter arrested in a police interaction is black increases in the days after a Trump rally. The effect is immediate and lasts for about 80 days. For a 10 days window, the effect is not significant, as the treatment is longer lived, so that including days after the 10 first days in the control groups biases the coefficient towards zero. The treatment effect is statistically significant for treatment windows of 20 to 80 days, and then fades away.

Table 3: Presidential elections, within-officer comparisons and supply side channel

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	100 x $\mathbb{P}$(Black Stop= 1)			Stops	Search	Warning	Citation	Summons	Arrest	White	Hispanic	Asian
POST-Trump	0.993***	0.509***	0.320**	13.164	-0.018	0.535	-0.002	0.534	-1.068	-0.273	-0.496***	-0.016
	(0.369)	(0.143)	(0.131)	(28.397)	(0.285)	(1.287)	(0.642)	(0.640)	(0.821)	(0.251)	(0.137)	(0.101)
Observations	4,584,686	8,272,885	6,635,064	95,074	7,521,505	7,071,236	7,071,236	7,071,236	7,071,236	11,931,161	11,931,161	11,931,161
R-squared	0.131	0.106	0.154	0.954	0.310	0.248	0.372	0.489	0.151	0.165	0.186	0.027
Sample/Specification	GE-Rally	State-PD Stops	Officer-FE	County-Day	Stops	Stops	Stops	Stops	Stops	Stops	Stops	Stops
County FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Day FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Notes: See notes to Table 2. POST-Trump is equal to $D_{c,t}^{(1,30)}$. Column 1 restricts the estimation sample to presidential election rallies by Trump. Column 2 restricts the estimation sample to state troopers. Column 3 instead adds officer-level fixed effects. Column 4 shows results of a specification at the county-day level using the total number of stops as the dependent variable. Column 5 shows the probability of a vehicle search. Columns 6 to 9 show the overall probabilities (in increasing order of severity) of warnings, citations, summons, and arrests. Columns 10 to 12 show the estimation results of Equation 1 using as the dependent variable the probability of stop of: whites, Hispanics, Asians/Pacific-Islanders. Standard errors are two-way clustered at the county and at the day level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. This table shows that the increase in black stops after a Trump rally is not due to compositional changes in the police or to supply side change in overall crime and driving behavior.

Table 4: Impact of Clinton and Cruz rallies on police stops of Black drivers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A	100 x $\mathbb{P}$(Black Stop= 1)									
POST-Clinton	-0.773	-0.386	-0.145	0.082	0.147	0.252	0.393	0.436	0.505	0.597
	(0.515)	(0.536)	(0.499)	(0.472)	(0.448)	(0.415)	(0.406)	(0.408)	(0.431)	(0.425)
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	0.175	0.175	0.175	0.175	0.175	0.175	0.175	0.175	0.175	0.175
	(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
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 for the rallies held by Hillary Clinton during 2016 Presidential campaign, and Ted Cruz in 2015 and 2016 for the Republican nomination. We use a thirty day event window after a candidate's rally to define POST-Clinton and POST-Cruz. Standard errors are two-way clustered at the county and at the day level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The Table shows that there is no positive effect of Clinton's or Cruz's rallies on stops of black drivers.

Table 5: Black stop details

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES		Stop of a Black commuter lead to:				Stop of a Black commuter was:	
	Search	Warning	Citation	Summons	Arrest	Non-Discretionary	Discretionary
POST-Trump	0.005	0.251	0.339	0.040	-0.111	-0.185	0.889**
	(0.107)	(0.270)	(0.212)	(0.067)	(0.106)	(0.163)	(0.353)
Observations	7,521,505	7,071,236	7,071,236	7,071,236	7,071,236	5,387,948	5,387,948
R-squared	0.140	0.125	0.102	0.055	0.025	0.089	0.098

Notes: See notes to Table 2. POST-Trump is equal to $D_{c,t}^{(1,30)}$. Columns 1 to 6 show the overall probabilities of stop leading to arrests for black drivers, citations for black drivers, warnings for black drivers, vehicle search for black drivers, stop performed due to a visible offense that would automatically trigger a stop for black drivers, and discretionary stops for black drivers. Standard errors are two-way clustered at the county and at the day level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Heterogeneous effect of Trump rallies on police stops of Black commuters

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
VARIABLES	100 x $\mathbb{P}$(Black Stop= 1)										
POST-Trump	0.808***	0.766**	0.806***	0.787**	0.855***	0.896***	0.813***	0.821***	0.784***	0.760***	0.794***
	(0.295)	(0.309)	(0.263)	(0.303)	(0.280)	(0.307)	(0.298)	(0.290)	(0.255)	(0.255)	(0.259)
POST-Trump * X	0.517***	0.389**	0.293*	0.602*	0.914***	0.227	0.140	0.118	0.094	-0.010	-0.081
	(0.154)	(0.179)	(0.160)	(0.330)	(0.244)	(0.172)	(0.193)	(0.172)	(0.188)	(0.182)	(0.160)
Observations	11,931,161	11,931,161	11,931,161	11,931,161	11,931,161	11,931,161	11,931,161	11,451,866	11,931,161	11,805,287	11,805,287
R-squared	0.090	0.090	0.090	0.090	0.090	0.090	0.090	0.086	0.090	0.090	0.090
X=	Racial Resentment-A	Racial Resentment-B	Slaves p.c. 1860	Cotton Suitability	County Blacks	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	YES	YES	YES	YES	YES
Daily FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Notes: The Table shows OLS estimation of Equation 1 with the addition of an interaction term between POST-Trump, which is equal to $D_{c,t}^{(1,30)}$. and the following predetermined county characteristics: the 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, share of black, 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 value of each county from the mean value and dividing the result by 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 7: Experimental Results: Marginal effects of treatment conditions on endline prejudice that Blacks 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 affect differently Whites and Non-Whites (Col. 1), college educated or not (Col. 4), or Republican or not (Col. 7). However, *prejudiced* Whites and *prejudiced* Republicans become even more prejudiced when exposed to the treatment (Col. 2a nd 8). Prejudiced Independents or Democrats also become more prejudiced when when exposed to the treatment (Col. 9), but the effect is smaller in magnitude for them. The effect of exposure to the treatment is not at all moderated by college education, even among prejudiced individuals (Col. 5 and 6). The table reports OLS regressions 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. $^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Online Appendix
(NOT FOR PUBLICATION)

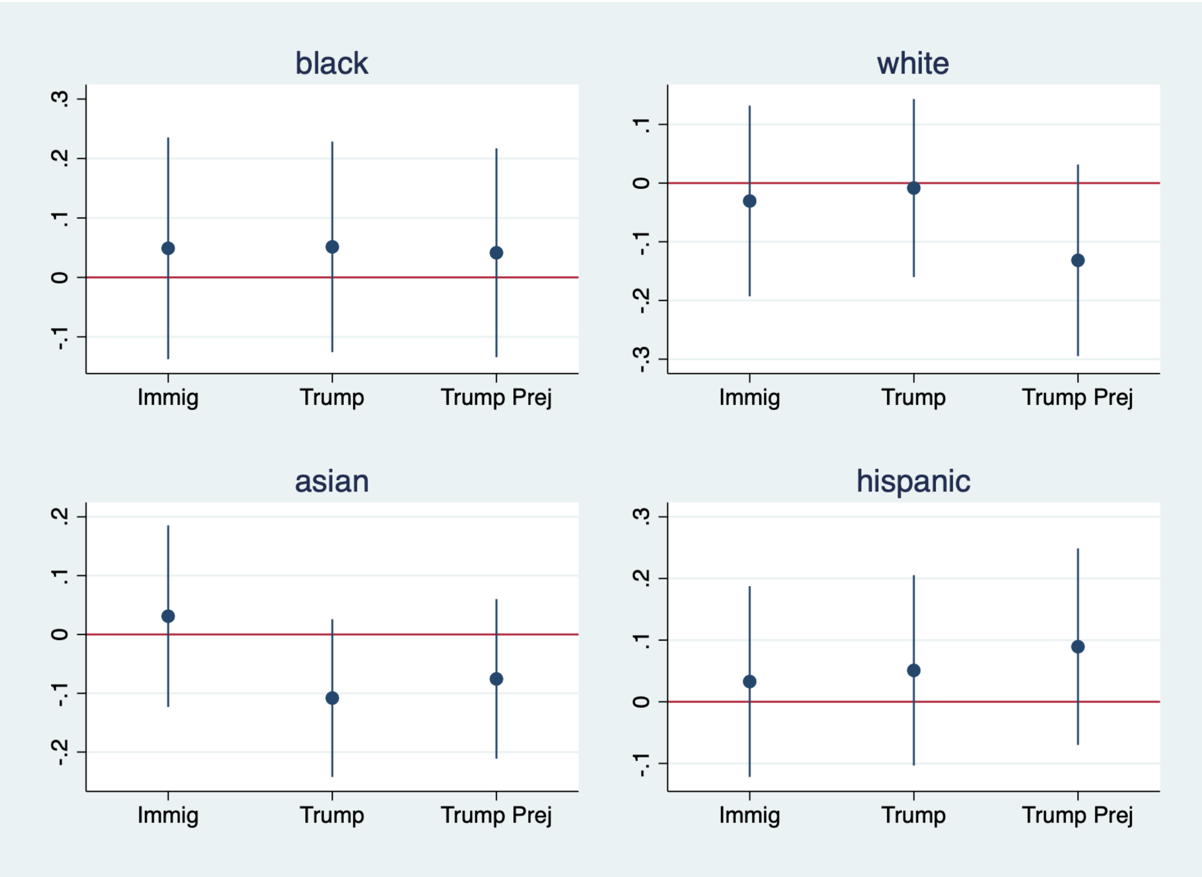
	Name	Candidates	Political issue	Prejudiced speech	Other elite signal
1.	Control	Clinton/Bush	Campaign finance reform	None	None
2.	Immigration Prime	Clinton/Bush	Immigration reform	None	None
3.	Trump Prime	Clinton/Trump	Campaign finance reform	None	None
4.	Trump Prejudice	Clinton/Trump	Immigration reform	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 in 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 online panel survey experiment by the authors.

Figure A1: Experimental Treatments

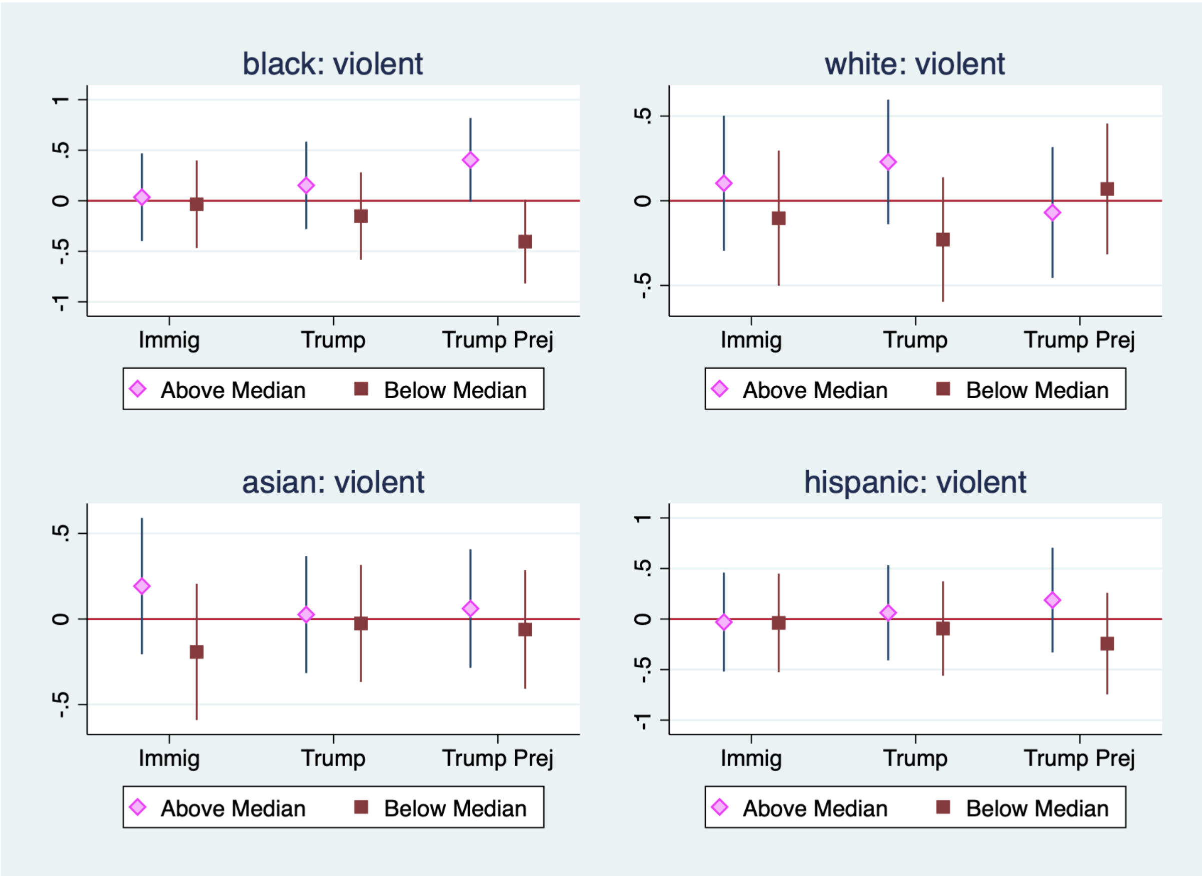
1 Additional Figures

The following figures replicate Figures 3 to 6 in the paper but excluding respondents in the "Trump Condone" and in the "Trump Condemn" conditions. The sample size is reduced to 656 respondents. All the results in the paper carry through in this subsample.



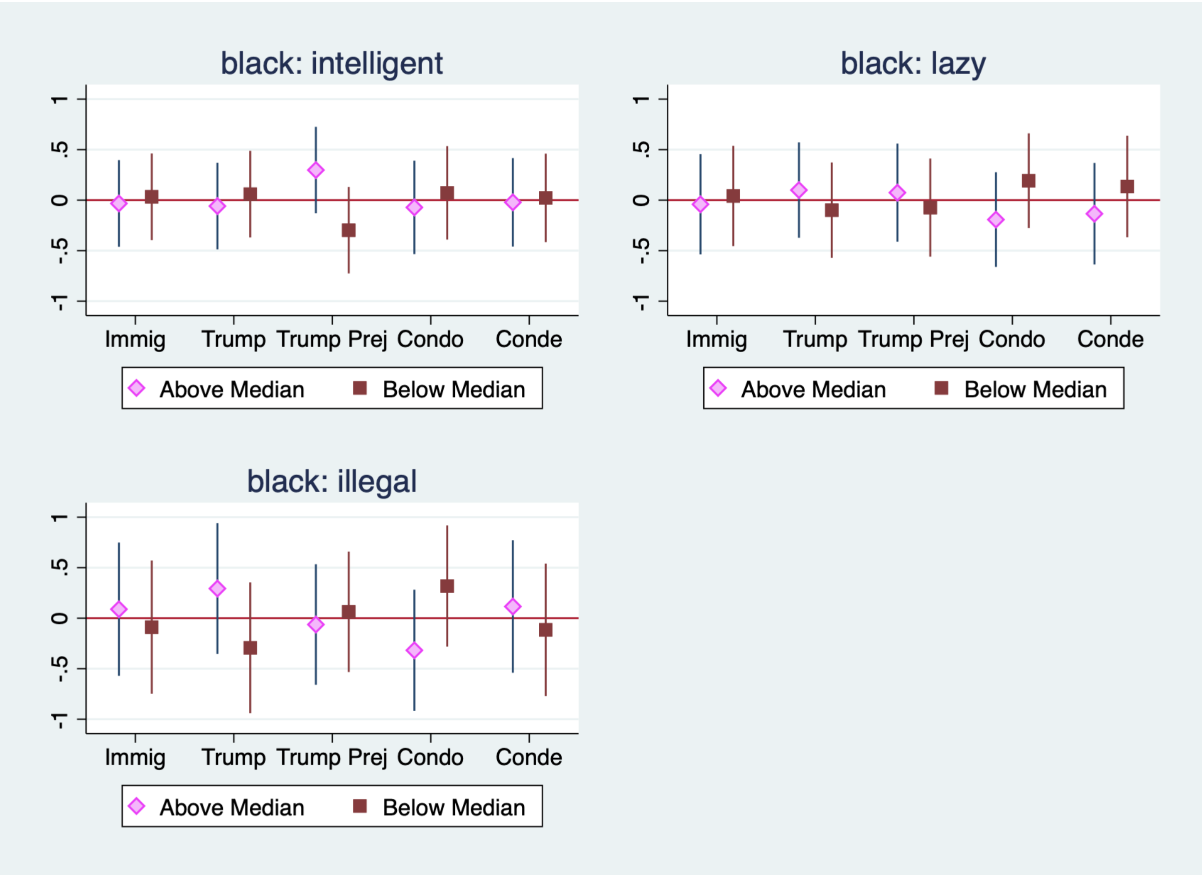
Notes: See notes to Figure 3. The only difference is that we include here the 341 respondents in the "Trump Condone" and in the "Trump Condemn" conditions. The results commented in the notes to Figure 3 and in the paper carry through.

Figure A2: Violent Prejudice: Figure 3 without the last 2 treatment conditions.



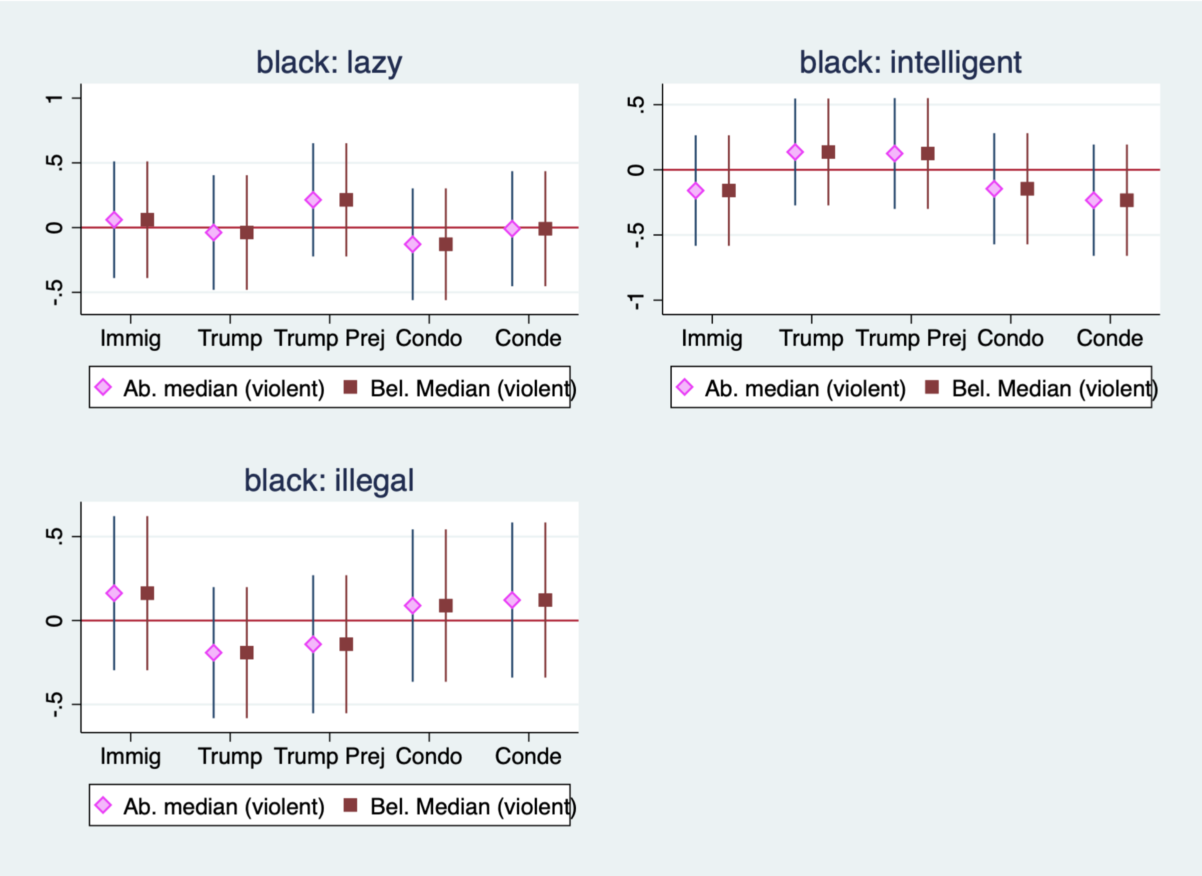
Notes: See notes to Figure 4. The only difference is that we include here the 341 respondents in the "Trump Condone" and in the "Trump Condemn" conditions. The results commented in the notes to Figure 4 and in the paper carry through.

Figure A3: Trump's inflammatory speech activates the prejudice that blacks are violent among prejudiced respondents: Figure 4 without the last 2 treatment conditions.



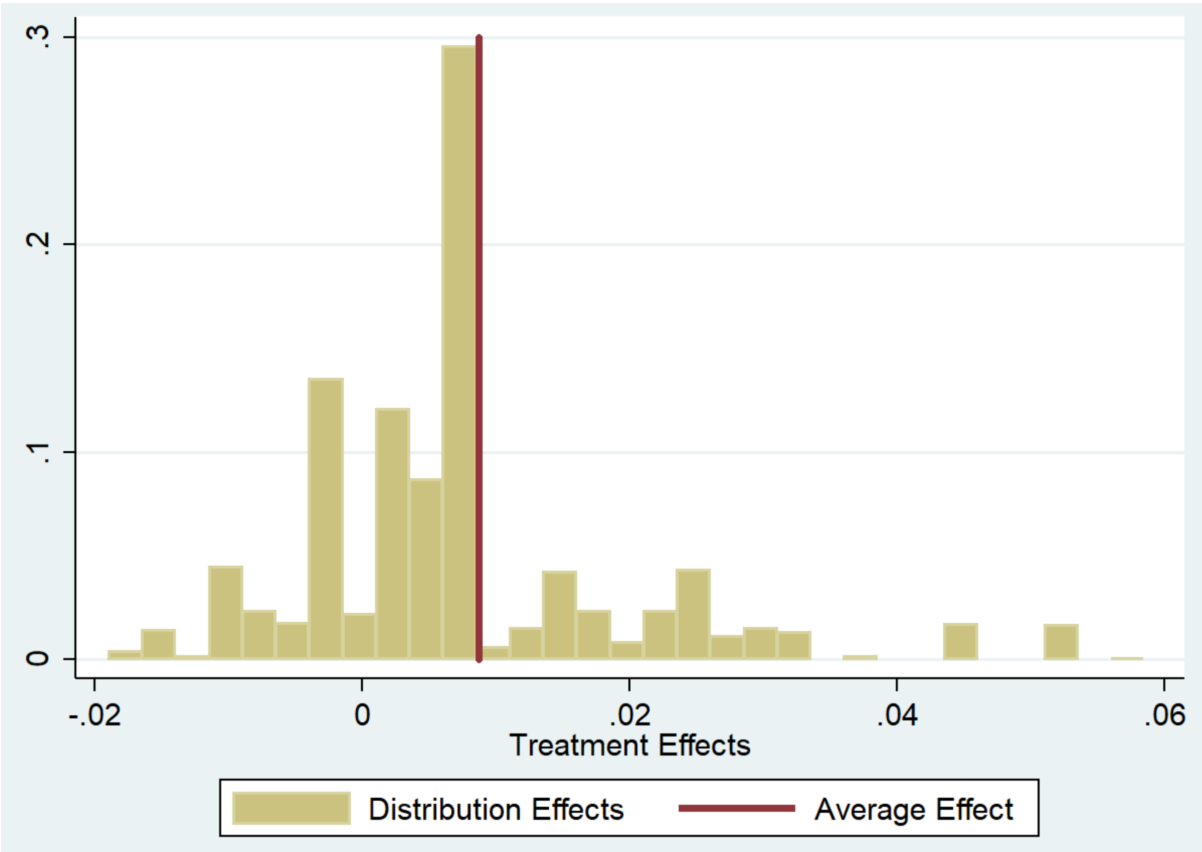
Notes: See notes to Figure 5. The only difference is that we include here the 341 respondents in the "Trump Condone" and in the "Trump Condemn" conditions. The results commented in the notes to Figure 5 and in the paper carry through.

Figure A4: Trump's inflammatory speech does not activate other dimensions of anti-black prejudice: Figure 5 without the last 2 treatment conditions.



Notes: See notes to Figure 6. The only difference is that we include 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 A5: Trump's inflammatory speech does not activate other dimensions of anti-black prejudice even among those who are highly prejudiced that blacks are violent: Figure 6 without the last 2 treatment conditions.



Notes: This figure shows the distribution of the estimated Differences-in-Differences estimator for each county. The red bar is the average of this estimated coefficients

Figure A6: Distribution Differences-in-Differences Estimator

2 Additional Tables

2.1 Robustness of Police results

In this section, we present robustness analysis for the results on police stops. First, we check that our results are robust to the inclusion of flexible time trends. We include county-specific linear and quadratic time trends, which account for potential county-varying trends in police stops of black drivers. Our results on black stops are robust and increase in magnitude when a linear or quadratic time trend is added.

Table A1: Impact of Trump rallies on police stops of black commuters: Different FE

	(1)	(2)	(3)	(4)
VARIABLES		100 x $\mathbb{P}$(Black Stop= 1)		
POST-Trump	0.941***	0.794***	0.474***	0.679***
	(0.268)	(0.239)	(0.176)	(0.231)
Observations	11,931,161	11,931,161	11,931,161	11,931,061
R-squared	0.091	0.091	0.099	0.094
Robustness	Linear Trend	Quadratic Trend	LEA FE	State-Day FE

Notes: See notes to Table 2 The fist Column reports the result from Table 2 with POST-Trump, which is equal to $D_{c,t}^{(1,30)}$.

Table A2: Impact of Trump rallies on police stops of Hispanic commuters: Different FE

	(1)	(2)	(3)	(4)
VARIABLES		100 x $\mathbb{P}$(Hispanic Stop= 1)		
POST-Trump	-54.318	-66.863**	-25.015*	-58.695**
	(34.312)	(28.043)	(13.234)	(27.186)
Observations	11,931,161	11,931,161	11,931,161	11,931,061
R-squared	0.188	0.188	0.191	0.188
Robustness	Linear Trend	Quadratic Trend	LEA FE	State-Day FE

Notes: See notes to Table A1.

Table A3: Impact of Trump rallies on police stops of Hispanic commuters

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Hispanic	Hispanic	Stop of a Hispanic commuter lead to:				Stop of a Hispanic commuter was:	
	Stop	Stop	Search	Warning	Citation	Arrest	Non-Discretionary	Discretionary
POST-Trump	-24.841	-33.379**	-0.288	-0.333	0.086	0.075	-0.217**	0.573
	(15.847)	(15.453)	(0.210)	(0.279)	(0.277)	(0.068)	(0.088)	(0.375)
Specification	State-PD	Officer FE						
Observations	8,272,885	6,635,064	7,071,236	7,071,236	7,071,236	7,521,505	5,387,948	5,387,948
R-squared	0.183	0.242	0.046	0.086	0.042	0.049	0.059	0.152

Notes: See notes to Table 2. In Column 1, we restrict the sample to state troopers. Column 2 instead adds officer fixed effects. Columns 3 to 6 show the probabilities of stop leading to arrest, citation, warning, vehicle search for Hispanic drivers. Columns 7 shows the probability of a stop of a Hispanic driver due to a visible offense that would automatically trigger a stop for black drivers, while Column 8 shows the probability of a discretionary stop of a Hispanic drivers. Standard errors are two-way clustered at the county and at the day level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

2.2 Police results: Simple Difference-in-Differences Analysis

In this section, we present an analysis that complements the results discussed in Section 3 of the paper. We adopt a slightly different approach, with a more classic Difference-in-Differences specification, in which we capture whether Trump rallies lead to a change in the probability of stop of a black driver in the following specification:

$$Black_{ict} = \alpha_c + \theta_t + \eta \mathbb{1}(D_{cte} = 0)_{e=1,\ldots,N_c} + \beta Max(\mathbb{1}(0 < D_{cte} \leq k)_{e=1,\ldots,N_c}) + u_{ict}, \quad (4)$$

where $Black_{ict}$ is dummy equal to one if the driver stopped was 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 in which we estimate 1 in the same sample but varying the treatment (and control) windows, we estimate Equation 4 for different samples around a Trump rally. We use 100 days before a Trump rally, and up to 100 days after the rally. The treatment window is defined as the post rally period, and varies accordingly from 10 to 100 days.

Table A4: DID Estimates of Impact of Trump rallies on police stops of Black commuters

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
VARIABLES					100 x $\mathbb{P}$(Black Stop= 1)					
POST-Trump	0.817***	0.843***	0.940***	0.949***	1.001***	0.977***	0.939***	0.912***	0.860***	0.811***
	(0.299)	(0.281)	(0.298)	(0.292)	(0.294)	(0.276)	(0.261)	(0.250)	(0.236)	(0.227)
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	0.114	0.114	0.113	0.112	0.112	0.111	0.110	0.110	0.109	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 1. The unit of observation is a police stop. The dependent variable is the proportion of black commuters stopped as a fraction of total stops in county. The main independent variables are: (i) Post-Rally, which is equal to one for days after Trump rally, and is zero otherwise. (ii) Day-of-Rally is an indicator that takes on a value equal to one on the day of the rally for county where the rally takes place, and is zero otherwise. All estimations include county and day fixed effects. Our sample consists of hundred days before each Trump rally and up to hundred days after each Trump rally. From Columns 1 to 6, we have 10 to 100 days (with increments of 10 days) of post-rally observations in our sample.

Table A4 shows the estimates of Equation 4 using rallies by Trump. We fix our pre-rally part of the sample as hundred days before a rally, and consider different post-rally part of the sample around Trump rally. From Columns 1 to 10, we have 10 to 100 days (with increments of 10 days) of post-rally observations in our sample. Column 1 shows that rallies by Trump lead to a 0.8 percentage points (p.p.) increase in the probability of stop of a black driver in the first ten days. The mean value (standard deviation) of the probability of stop of a black driver 100 days around a Trump rally in our sample is 0.20 (0.40). This means that the coefficient implies an increase equivalent to 4% of the mean value. In other words, there is an increase equivalent to 0.02 standard deviations in the share of Black stops.

In Column 2, we see that the coefficient increases slightly from 0.8 p.p. to 1.0 p.p. over the first 50 days, suggesting that the effect increases in force between ten to fifty days after rally by Trump. The effect peaks to 1.0 p.p. in the 50 days following a Trump rally. The estimates gradually decline from 1.0 p.p. to 0.8 p.p. in 50 to 100 days after a Trump rally, suggesting that the effect begins to fade away fifty days after the Trump rally (Columns 5 to 10).

2.3 Comparing Nomination vs Presidential rallies

The magnitude of the effect of a Trump rally on the probability of a black driver being stopped by the police is higher when we restrict our attention to Presidential rallies. We interpret this result in the paper as suggestive that the increasing visibility and popularity of Trump, who started out as a marginal candidate, emboldened the police to act on potential prejudice. However, an alternative explanation for the difference in the magnitude of the results could be that the type of counties visited for the nomination campaign could be very different from the type of counties visited for the Presidency campaign. We show in this section that along a wide range of dimensions, including pre-trends in police behavior, racial composition, education, recent economic shock, or underlying racial resentment, this is not the case.

Variable	Presidential Rally	
Total Stops	0.023	(0.025)
Black Stops	-242.69	(200.379)
County Population	-13992.296	(173773.52)
County White	-2.407	(2.961)
County Blacks	0.182	(2.203)
County Hispanics	3.615*	(1.949)
County Racial HHI	-0.024	(0.029)
County Median Age	-0.025	(0.038)
County HH income	0.019	(0.048)
County DEM share'12	0.182	(2.203)
County REP share'12	-0.419	(1.813)
County High School	503.126	(934.814)
County College	0.005	(0.016)
China Shock	-0.017	(0.02)
Racial Resentment-A	-0.007	(0.023)
Racial Resentment-B	-0.068	(0.353)

Notes: The Table shows OLS regressions of various county characteristics on a dummy variable that takes value one if a Trump's rally for the Presidency was held, as opposed to a Trump's rally for the Republican nomination (the 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 rally in the campaign for the Presidency do not differ from counties in which Trump held a rally for the Republican nomination in pre-trends in police behavior, defined both in terms of total stops (Col. 1) and share of black stops (Col. 2). The two types of counties are also statistically similar along a wide range of socio-economic characteristics, including county total population (Col. 3), racial composition (measured by percentage of population that is white (Col. 4), black (Col. 5) or Hispanic (Col. 6)) and racial fragmentalization (Col. 7), median age (Col. 8), 2012 election vote shares for the Democrat (Col. 10) or Republican (Col. 11) candidate, high school (Col. 12) and college completion (Col. 13), import competition shock with China from Autor, Dorn and Hanson (2013) (Col. 14), or the two measures of underlying racial resentment in the 2012 and 2014 Cooperative Congressional Election Survey (Schaffner and Ansolabehere, 2015) (Col. 15 and 16). The only difference, statistically significant at the 10% level, is in median income (Col. 9).

In this section, 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 of the paper for a description of the data sources).

2.4 Experiment: Regression results

In this section, we present all estimation results underlying Figures 3 to 6.

Table A5: 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 3 without the inclusion of the interaction effect. The unit of observation is an individual. The dependent variables are the endline prejudice that each different ethnic group (indicated in the Column headers) is violent. The main independent variables are the different treatment conditions (see Table A1 and the text for a description of each treatment). For each dependent variable, we present the results of an uncontrolled specification (even columns) and a fully controlled specification (odd columns). The results in 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 A6: 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 3. The dependent variables are the endline prejudice that each different ethnic group (indicated in the Column headers) is violent. The Table displays the coefficients associated with: the treatment condition (see Table A1 and the text for a description of each treatment), the baseline prejudice that each race is violent (i.e. in Columns 1 and 3, 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 blacks are violent, etc), as well as the interaction between this indicator variable and the treatment condition, as indicated. 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. The coefficients associated with the interaction terms in 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 blacks are violent among respondents who are initially prejudiced (i.e. above median baseline prejudice that blacks 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 A7: Regression results for Figure 5

	(1)	(2)	(3)	(4)
Dependent variable:	Endline prejudice African Americans are:			
	Lazy		Lack intelligence	
Immigration Prime	0.058	0.099	0.197*	0.191
	(0.111)	(0.109)	(0.118)	(0.116)
Trump Prime	−0.008	0.015	0.088	0.108
	(0.105)	(0.103)	(0.118)	(0.116)
Trump Prejudice	−0.019	0.011	0.005	0.017
	(0.108)	(0.103)	(0.115)	(0.112)
Trump Condone	0.043	0.052	0.065	0.100
	(0.107)	(0.110)	(0.115)	(0.113)
Trump Condemn	−0.085	−0.052	−0.058	−0.090
	(0.105)	(0.101)	(0.115)	(0.117)
Interaction: > median baseline prejudice Blacks: are lazy (Col.1, 2); lack intelligence (Col. 3, 4) and:				
Immigration Prime	0.083	−0.041	−0.026	−0.033
	(0.257)	(0.253)	(0.232)	(0.218)
Trump Prime	0.182	0.099	0.073	−0.059
	(0.247)	(0.241)	(0.232)	(0.218)
Trump Prejudice	0.201	0.074	0.382*	0.298
	(0.248)	(0.247)	(0.229)	(0.218)
Trump Condone	−0.161	−0.192	0.059	−0.072
	(0.251)	(0.239)	(0.248)	(0.236)
Trump Condemn	0.002	−0.135	0.059	−0.022
	(0.260)	(0.256)	(0.232)	(0.223)
> 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)
Individual controls	NO	YES	NO	YES
Observations	997	996	997	996
R^2	0.309	0.352	0.249	0.295

Notes: The Table shows OLS estimation of Equation 3 for different types of prejudice. The dependent variables are the endline prejudice 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 Table A1 and the text for a description of each treatment), an indicator variable for being above the median sample baseline prejudice that blacks 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. 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 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 blacks are lazy, or lack intelligence, even for respondents who are highly prejudiced to start with.