Whistle the Racist Dogs: Political Campaigns and Police Stops

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Abstract

Do right-wing populists radicalize xenophobes? Using data from nearly 12 million traffic stops, we show that the probability that a police officer stops a Black driver increases by 4.2% after a rally led by Donald Trump during his 2015-2016 campaign. The effect is immediate, lasts for up to 50 days after the rally, and is due only to discretionary stops. Moreover, we observe significantly larger effects in areas with more racist attitudes today, or those that historically relied more heavily on slavery. By contrast, we neither observe an increase in the rates of stops of White or other racial minority drivers, nor any effect of the campaign events of the other 2016 candidates to the Republican nomination or to the presidency. Results from a 2016 online experiment confirm 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 the expression of the latter in a critical and potentially 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, *The Atlantic*, 3 June 2020.

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

1 Introduction

Identity politics has played an increasing role in most advanced democracies in recent years (Gennaioli and Tabellini, 2019), becoming 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 remains an open question.

In this paper, we explore how Trump’s 2015-2016 political campaign affected the expression of racial prejudice and discrimination against Blacks in one of its most fundamental and potentially violent dimensions: police behavior. Indeed, police behavior and alleged racially-motivated brutality have come to symbolize racial bias and discrimination against African-Americans, especially since the BlackLivesMatter movement began in 2013.[1] The focus here is on the most frequent type of police-citizen interaction: traffic stops.[2] We use data on nearly 12 million traffic stops carried out by the police in the 142 counties shown in Figure 1 where Trump held a campaign rally, either as a candidate for the Republican nomination or for the presidency. To measure racially-directed police behavior, we rely on the racial classification of the motorist stopped (following

[1]Racial bias and discrimination in police behavior have been studied in several papers, including but not limited to Antonovics and Knight (2009); Anbarci and Lee (2014); Anwar and Fang (2006); Coviello and Persico (2015); Feigenberg and Miller (2020); Fryer (2019); Goncalves and Mello (2017); Grogger and Ridgeway (2006); Horrace and Rohlin (2016); Knowles, Persico and Todd (2001). Prejudice against African-Americans is certainly not limited to police behavior, but pervades the entire justice system, as well as 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 the Bureau of Justice Statistics, 8.6% of US residents aged 16 and over (more than 20 million people) were pulled over by the police for a traffic stop in 2015 (Davis, Whyde and Langton, 2018). See https://www.bjs.gov/content/pub/pdf/cpp15.pdf, accessed July 7, 2020.

e.g. Knowles, Persico and Todd (2001); Anwar and Fang (2006); Antonovics and Knight (2009); Anbarci and Lee (2014); Goncalves and Mello (2017)). We address the potential endogenous selection of counties with a campaign event by restricting our sample to the counties with at least one Trump campaign rally. A difference-in-differences methodology (DiD) allows to identify the effect of campaign stops by 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 did influence police behavior towards Blacks. Our baseline estimate suggests that the probability of a Black driver being stopped by the police increases by 0.78 percentage point on average in the month following a rally, a 4.2% increase. The effect is immediate and materializes the day after a Trump rally. While the effect slowly fades, it lasts for at least 50 days. This outcome 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, implying that the effect reflects a change in the behavior of the police officers rather than any modification of the composition of the police force on duty. We also conduct placebo specifications, observing no effect on police behavior of campaign events carried out by either the Democratic contender to the presidency, Hilary Clinton, or by the other main Republican opponent, Ted Cruz.[4]

To guide our understanding of the mechanism, we rely on a 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

[3]Recent econometric literature on staggered difference-in-differences shows that two-way fixed effects estimate a weighted average of each treatment effect where the weights may be negative. We first follow the recommended diagnostics by de Chaisemartin and d'Haultfoeuille (2020) and show that none of the weights are negative for our specification. We then follow the estimation procedure proposed by Abraham and Sun (2018) and find similar results to our baseline DiD.

[4]The ability of police data to determine racial discrimination has been discussed by various scholars. In particular, Knox, Lowe and Mummolo (2019) argue that estimates of racial discrimination based on samples of people stopped\detained by the police may be biased as racial discrimination could have already selected these samples. Our analysis abstracts from this issue for several reasons. Given that we are using a DiD methodology, we rely only on changes in the probability of a Black driver being stopped, and as such do not attempt to infer racial discrimination from the average probability of a police stopping a Black driver. Moreover, in our baseline estimates, we do not study the behavior of the police after the traffic stop.

et al., 2020). This body of work focuses on attitudes towards the groups directly targeted by Trump's xenophobic speeches – Latinos and Muslim migrants. While Trump's campaign did not explicitly disparage African Americans[5], numerous political science and law studies show how certain speech has a hidden message only understood by a targeted subgroup– a phenomenon known as the "dog-whistle effect" (Lohrey, 2006; Goodin, 2008; Fear, 2007; Haney-Lopez, 2014).[6] Dog-whistling has been variously defined. Here, we are particularly interested in a specific dimension of the latter: when coded language triggers strongly rooted stereotypes about groups perceived as threatening (Haney-Lopez, 2014). Specifically, we explore whether Trump's campaign activated prejudice against Blacks, namely a view associating Blacks with violence and crime, and particularly amongst individuals with deep-seated bias against Blacks. To answer this question, we proceed in three steps.[7]

First, we leverage the high granularity of our traffic stop data to establish the racially-charged and specifically anti-African American nature of our results. We show that the change in police behavior is, in fact, specific to Black drivers and does not reflect an increase in criminal behavior or a change in driving patterns, either overall or specifically by Black drivers. Meanwhile, we observe no increase in either the total number of traffic stops or those of Whites or Asian/Pacific Islander drivers. The probability of stopping a Hispanic driver actually decreases, potentially suggesting a substitution effect, although the effect is not as robust as the effect found for Black drivers. Moreover, we observe an increase only in the discretionary stops of Blacks on the part of the police (e.g., for

[5]As excerpts from Trump's rally speeches, which we included in Appendix 3, show, when Trump talked about African Americans in his 2015-2016 rallies, he generally describes them as victims of poverty and crime.

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

[7]Recently, scholars have argued that Trump has moved beyond dog-whistling by breaking racial discourse norms and making explicit and direct racial appeals (see, e.g., Smith (2020)). While this paper does not debate the extent to which the current President's discourse on race is implicit or explicit, we do observe that the latter has primarily targeted migrants, against whom it is necessary to "build a wall" rather than Blacks. The excerpts from Trump's rally speeches, which we included in Appendix 3, show how Trump systematically associates foreigners with crime (including "brutal drug cartels"). From that association, flows the necessity of building a "wall" (with Mexico).

"careless driving"), as opposed 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. Local anti-African American views are proxied 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 proxies of racial animus inherited from the pre-Civil War era. We follow Acharya, Blackwell and Sen (2016), who show how the prevalence of slavery shaped racial prejudice against Blacks in the US, and continues to do so to this day. Specifically, we use the number of slaves in the year 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 today have deeper racial resentment, as well as in those that had more slaves in 1860 and whose agricultural endowments were more suitable to slavery. In contrast to 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 either across counties more or less affected by import competition with China (Autor, Dorn and Hanson, 2013), or between majority Democrat or Republican counties.

Third, to provide direct evidence of a dog-whistle effect, which could operate beyond police behavior and concern 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 his campaign speeches. It is thus well suited to providing the population counterpart of our study of police behavior. While their paper focuses solely on the acceptance of discrimination against Latinos, the authors also collected data on prejudice against Blacks. We use the latter, to the best of our knowledge for the first time, in this paper. Employing their sample and data, we show that respondents with above-median (or above mean) pre-existing prejudice against 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 of being rapists.[8] 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 bias against other minority groups, including Hispanics, even for respondents who were initially prejudiced against these populations. Consistent with the theoretical conceptualization of dog-whistling appealing particularly to the stereotype of a threatening and dangerous group, the effect here is also specific to a distinct dimension of prejudice: the belief that Blacks are *violent*, as opposed to being, for example, lazy, or lacking intelligence, which is also measured in the experiment. We take these findings as evidence that Trump's rhetoric resonates especially with individuals already prone to thinking that Blacks are violent, and radicalizes these views even further.

Overall, our results show how Trump's speech has fueled racial resentment against African Americans. Moreover, the experimental findings demonstrate that it is a *particular* dimension of prejudice that is affected – the bias that Blacks are violent – thus suggesting more than a simple activation of indiscriminate racial bigotry. Our analysis cannot, however, fully untangle whether Trump's rhetoric aggravated prejudice or simply activated or normalized preexisting prejudice. Regardless, we are able to highlight the direct and real consequences not only relative to the views expressed by the population but also in terms of racially-directed behavior by the police. Moreover, since the effect is stronger, or found only, in more racist areas, or amongst most bigoted individuals, our results indicate a radicalization of prejudice against Blacks. 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.[9] Arguably, coded language is potentially even more damaging

[8]During his presidential announcement speech on June 16, 2015, Trump remarked: "When Mexico sends its people, they're not sending their best [...] They're sending people that have lots of problems, and they're bringing those problems with them. They're bringing drugs. They're bringing crime. They're rapists."

[9]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.

to democratic politics, as politicians can win both the vote of the groups they appeal to as well as those of an inattentive majority – or even of the group unknowingly targeted by the dog-whistle.

Our findings contribute to an 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. A novel and important difference of our paper is that we do not examine attitudes towards the group directly targeted by Trump's xenophobic rhetoric. Rather, we show how Trump's statement, though seemingly not targeted against African Americans, has in fact stimulated prejudice against this group, especially among those with the most deeply ingrained preexisting prejudice.[10] As such, our results speak to two related strands of literature on hidden values, or "crypto-morality," and on the dog-whistle effect. In fact, while a plethora of recent studies have documented the persistence of values and norms,[11] some values may remain hidden, a phenomenon described as "crypto-morality" by Greif and Tadelis (2010).[12] In the context of the US, racial inequality was the dominant accepted social norm[13] into the early twentieth century, until it was supplanted by a norm of racial equality in the post-Civil Rights era (Mendelberg, 2001; Newman et al., 2020). Yet, negative racial views did not simply vanish; they are hidden and continue to shape political preferences (Hutchings and Valentino, 2004; Mendelberg, 2008). Furthermore, experimental and survey evidence suggests that negative racial predispositions can be activated by implicit racial cues (see Mendelberg (2008) for a meta-analysis). Politicians

[10] Feinberg, Branton and Martinez-Ebers (2019) document a correlation between the counties that hosted one of Trump's presidential rallies and incidence of hate crimes. They find that this relationship remains even after controlling for various observable characteristics likely related to hate crime prevalence. Lilley and Wheaton (2019) show that this correlation does not hold after controlling for population size.

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

[12] 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, moderated by the social acceptability of expressing these views. Studies on voting behavior are similarly constrained by the supply of political parties. For example, Cantoni, Hagemeister and Westcott (2019) argue that the lack of supply of party platforms restricted the expression of populist right-wing views in Germany.

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

can thus appeal to racial bias and activate racial resentment using coded languages and symbols, and in doing so gain an electoral advantage – the dog-whistle effect (Valentino, Hutchings and White, 2002; Haney-Lopez, 2014; Valentino, Neuner and Vandenbroek, 2018).[14] Our work therefore builds on a recent literature exploring the ways leaders legitimize political preferences and mobilize their followers (Dippel and Heblich, 2018; Cagé et al., 2020), even to the point of prompting them to perpetrate acts that would otherwise be morally repugnant (Cagé et al., 2020).[15] By showing how dog-whistle politics radicalize already-prejudiced individuals, this paper also complements studies on political radicalization and polarization.[16]

A notable contribution of our work is that we examine the effect of Trump's political campaign on police behavior.

The rest of the paper is organized as follows. The next section describes the data used in the analysis of the effect of Trump's campaign rallies on police traffic stops.The analysis and the results are described in section 3, and the underlying mechanism in section 4. We provide experimental evidence on the effect of exposure to Trump's racially inflammatory speech on racial prejudice in the population in section 5. We then show how such racially inflammatory speech also amplifies the effect of Trump rallies on traffic stops of Black drivers in section 6. Last, we discuss broader implications.

2 Data

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

Police Stops: Our data on police traffic stops comes from Pierson et al. (2020), who have made the information publicly available on the Stanford Open Policing Project website. To construct a national database of traffic stops, these authors filed public records

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

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

[16]See, e.g., Abramowitz and Saunders (2008); Gentzkow (2016); Abramowitz (2018); Gennaioli and Tabellini (2019); Bordalo and Yang (2020) for recent contributions documenting polarization in the US. While many, in particular Abramowitz (2018), argue that Trump's rise to power was the consequence of polarization, we focus instead on how his campaign further deepened divisions.

requests with all 50 state patrol agencies and over 100 municipal police departments. More details on the data collection can be found in Pierson et al. (2020). Altogether, the data comprises approximately 95 million stops from 21 state patrol agencies and 35 municipal police departments from 2011-2018. While for all the traffic stops we know the date it took place, some also include information on the driver's race, age, and sex. For a limited set of stops, information is available on the reason the driver was stopped; what happened during the stop (i.e., whether or not 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: Data on the rallies held by the 2016 presidential candidates comes from the Democracy in Action website (Appleman, 2019), which documents presidential candidates' schedules, from pre-campaign to presidential inauguration. We geocode Donald Trump's presidential rallies starting June 17, 2015, a day after he formally announced his campaign to November 7, 2016, a day before the 2016 general elections. We also geo-code information on the 2016 Democratic presidential candidate, Hillary Clinton, as well as the other main Republican contender, Ted Cruz.

We then match traffic stop data with the campaign rally information. For each stop, we match the date of the closest rally. Altogether, 190 Trump campaign rallies (out of 324) in 142 counties overlap with data on 19,186,644 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, which show that although Blacks represent only 11.22% of our sample population, as much as 20.39% of drivers stopped are Black, while 51.50% are White, and 24.10% Hispanic. We have information on whether a search was conducted for 7,521,505 stops, and on the outcome of the stop and search for 7,071,236 stops. 82.03% of stops end in a simple citation or a warning, 13.27% in a summons, and 4.70% in an arrest; while 6% of stops result in a vehicle search. 29% of the vehicle searches (1.73/5.99) are conducted on Black drivers. 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 (less severe) citations or warnings.

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

County Characteristics: Data on the number of Blacks, the number of Hispanics, ethnic fractionalization, average income, and average college completion comes 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 over-represented 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), while the information on 2012 county-level vote shares for Obama comes from Leip (2016).

Racial Resentment: We derive our measure of racial resentment from the 2012 and 2014 Cooperative Congressional Election Surveys (Schaffner and Ansolabehere, 2015) (hereafter, CCES). We chose the 2012 and 2014 waves to obtain a measure of pre-existing racial resentment, before the launch of the Trump campaign. Specifically, we take advantage of questions CC442a and CC422b, or "Racial Resentment A" and "Racial Resentment B," which 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 those who somewhat or strongly disagree with the second statement. Higher values therefore indicate greater resentment. For ease of interpretation, we normalize all variables to have a mean of 0 and a standard deviation of 1. We also use a proxy of deep-seated racial animus based on slavery during the pre-Civil War era: the number of slaves per capita in 1860.[17] To deal with the potential endogeneity of slavery to local cultural and political factors, we use cotton suitability as an exoge-

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

nous predictor of slavery, following Acharya, Blackwell and Sen (2016) and Masera and Rosenberg (2020).

The experiment and data are described in Section 5.

3 Empirical Strategy and Results

3.1 Difference-in-Differences Analysis

3.1.1 Empirical Specification We conduct our analysis at the stop level, estimating whether a Trump campaign rally e leads to an increase in the probability that the driver stopped by the police in stop i in county c on date t is Black. We first define $D_{c,t}^{(a,b)}$ as a dummy variable equal to 1 if day t is within a and b days from any event in county c. Formally, $D_{c,t}^{(a,b)} = Max(\mathbb{1}(a \leq d_{c,t,e} \leq b)_{e=1,\ldots,N_c})$, where $d_{c,t,e}$ is the distance (in days) of day t from Trump rally e in county c. $d_{c,t,e}$ is positive if day t is after the event and negative if day t is before the event. A given county can have more than one event, and up to N_c events. Out of the 142 counties in our sample, 99 have exactly one Trump rally, while 23 have 3 or more rallies. With a slight abuse of notation $D_{c,t}^{(-\infty,a)}$ is 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 traffic 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 pulled over by the police in stop i in county c on date t is Black.

One concern may be that counties in which Trump held a rally differ systematically from other counties. For example, Trump or his campaign team might target counties as a function of their underlying racism or police behavior. We account for time-invariant county institutional or cultural characteristics, including racism, permanent police capacity, legislative differences, or geographic differences, by county fixed effects α_c. Moreover, to further address potential selection issues, in the estimation of Equation 1 we rely only on the set of counties in which Trump has ever held a rally. 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 traffic stops; for instance, across different days of the week, holidays, or end of the month effects.

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

Our main parameter of interest is β. The variable that captures the treatment is a dummy variable that takes the value of one for k days following any Trump rally in that county and zero otherwise. To address potential concerns about the selection of the treatment window, we adopt a flexible approach. Specifically, we estimate Equation 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 $D_{c,t}^{(-\infty,-k-1)}$, which is equal to one for the days prior to the comparison window k, and $D_{c,t}^{(k+1,\infty)}$, which is equal to one for the days following our treatment period. For counties with multiple events, we allocate each stop i in county c to each possible event e in the county and define the windows around each event.

In order to define the dummy variables that capture the different windows, it is thus necessary to take the maximum of the indicator variables that capture the possible windows for each possible event. For example, if we consider a treatment window k of 30 days after a rally, the omitted comparison window is 30 days before the rally (for all possible rallies in the county), $D_{c,t}^{(-\infty,-31)}$ indicates more than 30 days before a rally, and $D_{c,t}^{(31,\infty)}$ more than 30 days after the rally. β therefore captures the change within a county in the probability that a traffic 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 officer stopping a Black driver 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 the 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 consequently adjust standard errors for two-way clustering at the county and day level. Furthermore, a possible concern for 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 findings of an event-study analysis that shows that there are no pre-trends in the rate of Black stops across counties. The event studies results also show that the only period in which we pick up a treatment effect is precisely that immediately following a Trump rally, lasting up to 50 days.

3.1.2 Results Table 2 shows the estimates of Equation 1 for increasing windows k around a Trump event. We observe that the probability that the driver pulled over by the police in a traffic 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 slowly fades away. The effect remains statistically significant for up to 100 days after a rally, although at the 100-day window it is only half in magnitude compared to the largest effects immediately after the rally, suggesting that the effect returns 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 and is no longer significant after 50 days. In what follows, we retain a 30-day window after a rally as the main focus of our analysis. In Table A1, we verify that our results are robust to including county-specific flexible time trends, either linear or quadratic. If anything, the inclusion of linear or quadratic time trends improves the statistical precision of our results. In Section 2.2 of the Appendix and particularly 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 and up to 30 days following a Trump campaign rally suggests a 0.78 percentage point increase in the probability of a Black driver being pulled over. Given that in the 30 days before a Trump rally, the probability that the driver being 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-day window prior to any rally is 575,042. Thus, our analysis reveals that Trump's rallies led to 4,485 additional stops of Black people by the police in the month following the event. Note that this number is an underestimate, since we only have information on 190 out of the 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 traffic 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 thus suggest that we may underestimate the total effect by a factor of 6.65,[18] implying that Trump's rallies led to nearly 30,000 additional stops of Black people by the police in the months following the various rallies.

Recent econometric literature on staggered difference-in-differences highlights potential issues with the two-way fixed effect estimator employed here. One of the main insights of this work is that the estimated parameter is a weighted average of each treatment (in our context, each rally) where the weights may be negative. We consequently follow the recommended diagnostics by de Chaisemartin and d'Haultfoeuille (2020) and show that for our specification none of the weights are negative. However, it is worth noting

[18] 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).

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 more similar to our own, Abraham and Sun (2018) propose estimating the treatment effect for each event and then averaging it out. To this regard, Figure A7 displays the distribution of the estimated difference-in-differences parameters for each county. Following their technique, we combine these estimates and find that the probability of a Black driver being stopped increases from 0.78 in our baseline to 0.87 percentage points, suggesting that our baseline estimate is, if anything, underestimated.

Our analysis thus far considers all the rallies held during the campaign, both those for the nomination and those for the presidency. Yet, the rallies held for the nomination took place when Trump was still a marginal player. We thus expect that the rallies held during his presidential campaign had bigger effects. Indeed, the increase in Trump's visibility and popularity, from when he became the Republican nominee and throughout his presidential campaign, may have had an emboldening effect. For example, the experiment by Bursztyn, Egorov and Fiorin (2019) reveals that signals of Trump's popularity make xenophobic respondents more likely to express their views. Moreover, the presidential rallies involved bigger crowds and thus likely had a greater impact than the smaller nomination rallies. That said, Enke (forth.) shows that Trump's campaign became more moderate after he secured the Republican nomination, which would instead imply that the presidential rallies had a smaller effect on Black stops.[19] 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 the rallies. The probability that a Black driver is stopped by the police increases after a Trump presidential rally. At 30 days, this probability increases by 1 percentage point,

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

or a 5.8% increase with respect to 30 days prior to a rally.[20] In other words, the effect of Trump presidential campaign rallies is about 38% larger in magnitude than all his 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 allows 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, we can study how the effect changes, time period by time period, rather than averaging over the whole window k as we did before. The event-study specification is as follows:

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

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

Figure 2 shows the estimates of β_τ in Equation 2. We see that Trump rallies result in a substantial and immediate spike in Black traffic stops following a rally. The probability that the individual stopped by the police is Black increases by 0.8 p.p. in the first ten days following a rally by Trump. It remains stable 30 days after a rally and reaches its highest magnitude (nearly 1 p.p.) at that point. The effect is nevertheless transitory

[20] The baseline probability of a Black traffic stop 30 days before a presidential campaign is 0.175.

and has faded away by 50 days following a rally. The figure also shows the trend in the share of Black stops prior to a rally by Trump, which is very stable and does not change significantly in either direction up to 105 days leading up to a Trump rally. This result suggests that Trump did not specifically time his rallies in certain counties as a function of local police behavior.

4 Mechanisms

We adopt a series of strategies to unpack several potential mechanisms behind the effect of Trump rallies on police traffic stops of Black drivers. First, the effect could simply be due to the occurrence of a political rally, particularly by a Republican candidate given this party's reputation for being tough on crime. Second, the result might derive from compositional changes to the police force after a rally, following, for example, local political decisions, as opposed to the behavior of individual police officers. Yet another explanation for our results could lie in a supply-side effect, or 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 consequently examine the impact on police behavior of rallies held 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.

This may, however, not be the right comparison. The change in police behavior towards Black drivers after a Trump rally could be due to the fact that he was running for the Republican party, known for its conservative and tough-on-crime policies. If this were the case, we should observe similar changes in police behavior after rallies held by Ted Cruz, the other main contender for the Republican nomination in 2016. Cruz was closely trailing Trump during the Republican primaries, and ran on a socially conservative, pro-gun, anti-crime, and pro-police platform.[21] We thus again estimate Equation 1 using the same windows as in Table 2 after Cruz 2016 rallies. Table 4 (Panel B) shows that his 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 could occur after a Trump rally due to 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. In what follows, we provide evidence against each of these possible mechanisms.

4.2.1 State Troopers and Within-Officer Comparisons To rule out changes in traffic 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. In column 3 of Table 3 we provide a more direct test, which rules out the possibility that the Trump

[21] 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.

effect is due only to a change in the deployment of different units or individuals after a rally, or any other compositional change of the police force. Specifically, we add officer-level fixed effects to Equation 1. 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 citizen 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, in columns 4 to 9 of Table 3 we present the results of additional specifications in which we use as alternative dependent variables the total number of stops, the probability of a vehicle search, or (in increasing order of severity) warnings, citations, summons, or arrests after a Trump rally. In these specifications we do not just focus on black drivers but consider all the police stops. 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 a vehicle search, warnings, citations, summons, or arrests, our specification is identical to Equation 1, but we use these probabilities as the dependent variables rather than the probability that the driver pulled over is Black. We observe no change in any of these outcomes, suggesting that shifts in driving patterns or criminal behavior cannot explain our results. These findings also rule out the explanation that the change in police behavior stems from a general behavioral change by the force, 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. For example, Trump rallies might 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 being stopped for the other reported races or ethnicities in the data: Whites, Hispanics, Asians/Pacific Islanders. We do not observe change in the stop probabilities for either Whites or Asians/Pacific Islanders. Meanwhile, we find a *de*crease in the probability of being stopped for Hispanics, 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, columns 7 and 8 of Table A3 show 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 the police. This suggests that the effect is driven more by the behavior of Hispanic drivers than by the police, although we observe no significant change in the outcome of the stop in columns 3 to 6 of Table A3.[22] Rather, this result may reflect a substitution behavior by the police, whereby the police may choose to patrol more Black neighborhoods rather than Hispanic neighborhoods. In any case, the negative - or at least nil- effect on stops of Hispanics is particularly remarkable, given that Trump explicitly targeted Hispanics, rather than Blacks, in his campaign speeches. We delve deeper into this issue 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 of Blacks being violent, but does not affect those concerning Hispanics.

Even though we do not observe an overall change in either the number of stops, the rates of citations, searches, and arrests, or 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

[22]The results for Hispanics should also be taken with some caution, given evidence of misreports of Hispanics as Whites documented in the literature (Luh, 2019). It is thus 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 in stops of Hispanic drivers is potentially compatible with a model in which the police substitute Black arrests for Hispanic arrests. This might occur to remain within quota limits for minority stops or because of redeployment to different - more Black - areas to fulfill an objective of stopping more Black drivers. Hence fewer Hispanics might be stopped due simply to a reduced police presence in Hispanic rather than Black areas. We plan to tackle this question in a future version of the paper, using more disaggregated data on where the stops occur. We also aim to use speech data from different rallies to investigate which words might aggravate or attenuate the effect on Black stops.

of Black driver traffic stops stems from police – rather than Black driver - behavior.

4.3 Establishing Racially-Directed Police Behavior: Additional Information on the Causes and Outcomes of Black Stops

If the increase in stops of Black drivers was justified by Black driver 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. Yet, Columns 1 to 5 of Table 5 show that this is not the case. We do not observe any 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 arrests, Col 5) and never statistically significant.[23] These results show that the increase in stops is not due to a serious - or, in fact, any type of - offense, but instead likely reflects discretionary and unjustified stops by the police. The next set of results confirm that this is the case.

We exploit information on the justifications for a stop, differentiating between discretionary stops and events that trigger an automatic stop or police intervention. In our data, the latter include: 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 sorts of offenses that we categorize as discretionary 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 Columns 6 and 7 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 actually negative (-0.185) although not statistically signifi-

[23] Vehicle search is a separate category from the outcome of the stop (citation, warning, summons, arrest).

cant. Meanwhile, the point estimate of β for discretionary stops is 0.889. In other words, the increase in traffic 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 only observe an increase in traffic stops of Black drivers. A potential explanation offered by the literature on dog-whistling is that Trump's rallies and speeches activated negative stereotypes of the police towards Black people. Recent studies have, in fact, established that (especially White) police officers have a preference for discrimination against Blacks (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 precisely those who 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 this group. 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 bias 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. The interaction term therefore captures differential changes from the underlining trend of the probability that the police stop 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 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 as proxies of deep-seated racial animus the number of slaves per capita in 1860, as well as cotton suitability as an exogenous predictor of slavery, as described in Section 2.

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 a Black driver being stopped by the police is between 51% (Column 2) and 64% (Column 1) larger in counties that are one standard deviation above mean racial resentment with respect to counties with mean racial resentment, as measured in the CCES survey, depending on which measure we use (B or A). It is 36% higher in counties with one standard deviation above the mean share of slaves per capita in 1860 (Column 3) 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, 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, such as that stemming 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 influence the impact of Trump rallies on the expression of racial prejudice on the part of the police, regardless of whether we use the actual or instrumented measure (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, these authors focus 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" (footnote 6) become more prejudiced – not against the group singled out in the speech, Hispanics, but rather against *African Americans*. Specifically, respondents who are initially already highly prejudiced become even further radicalized in terms of their view of African Americans as *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: speech supposedly targeting foreign nationals, but radicalizing the most bigoted respondents in their opinion that African Americans are violent.

In what follows, 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 was provided by Collingwood (2020).[24] The authors conducted an online two-wave panel survey experiment using Amazon's Mechanical Turk platform. The experiment took place during the 2016 Trump campaign. Their investigation is thus perfectly well suited to validate, in a randomly selected sample of respondents, the results of our preceding analysis of police behavior during Trump's 2016 campaign. The study includes 1,287 adults in the first wave (conducted between March 19 and April 23, 2016) and 997 in the follow-up, which took place 3 days afterward. As is standard for M-Turk samples, the group is more left-leaning and more highly educated compared to a nationally representative sample, suggesting that the obtained estimates related to prejudice may provide a lower bound of the population

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

treatment effect.

In addition to socio-demographic and political orientation information, the authors collected measures of racial prejudice against several groups (Blacks, Asians, Hispanics, and Whites) using a negative stereotype index from the 2008 American National Elections Studies (ANES). Prejudice was measured at baseline, in Wave 1, thereby providing a measure of initial prejudice prior to the treatment. The authors then administered the treatment in Wave 2 and measured prejudice again. The published paper by Newman et al. (2020) focuses on how Trump's anti-Hispanic speech affects prejudice and discriminatory behavior against Hispanics. However, the fact that they also measure prejudice against other groups and along several dimensions provides a perfect opportunity to study the extent to which Trump's seemingly anti-Mexican rhetoric activated prejudice against Blacks.

Their experimental treatment consisted of exposing respondents to political speeches that vary in their content (racially inflammatory or not) and protagonist (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. The primary treatment condition – "Trump Prejudice" – instead presented the respondents with the racially inflammatory remarks made by Trump against Mexican immigrants (the quote from Trump's announcement speech about Mexicans being criminals, drug dealers, and rapists already quoted in footnote 6). This statement was – in all appearances– targeted against Mexican immigrants and also contained a strong reference to crime. Crucially, the study also differentiates prejudice along several dimensions: laziness, lack of intelligence, tendency for violence, or in the US illegally. For each dimension, respondents are asked to rate on a scale from 1 to 5 whether an adjective - either "lazy," "intelligent," "violent," or " here illegally" - describes the racial group. All the answers are rescaled so that 1 captures the lowest level of prejudice and 5 the highest. Given the connection of the statement to crime and to an outgroup that might activate fear, we specifically expect that exposure to the treatment activates the bias that African Americans are *violent*. This dimension of prejudice is also arguably that which most influences how the police perceive African Americans (LeCount, 2017). At baseline, the average opinion that African Americans are violent is 2.40 (s.d. 1.18), as opposed to 2.16 (s.d. 1.01) for Whites or 1.38 for Asians (s.d. 1.18).

To isolate the effect of racially inflammatory speech from the effect of simply mentioning immigration or the effect of Trump alone, the study includes two additional treatments. First, an "Immigration Prime" treatment featured a discussion of immigration, but by Jeb Bush as opposed to Donald Trump as the Republican candidate. Second, the "Trump Prime" treatment showed Trump discussing campaign finance reform rather than immigration. The researchers also added two treatments measuring the extent to which other elite condoned or condemned Trump's racially inflammatory speech. While we have no prediction concerning how endorsement or condemnation by other political elite might impact the dog-whistle effect, we retain these last two treatment conditions in the analysis to preserve power (the results are robust to excluding these treatments from the estimation sample, see Figures A3 to A6 in the Appendix).

5.2 Empirical Specification and Results

The study by Newman et al. (2020) focuses on attitudes towards Hispanics and uses as the dependent variable the acceptability of discriminatory behavior against this group, measured in a specific vignette experiment. Here, we use a different dependent variable, not employed by the authors in their study. As we are interested in the expression of prejudice against African Americans, especially the bias that they are violent, we use as the dependent variable the endline measure of such prejudice, after the treatment.[25] We also control for initial prejudice, measured before the treatment, in all specifications.[26]

Dog-whistling resonates only - or more strongly - among the subgroups who hold the views that are being harnessed. We therefore hypothesize that the Trump Prejudice

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

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

condition will only prompt bias among those who are already in the higher end of the distribution of prejudice in the population. To test this, we include an interaction between one's position in the prejudice distribution (e.g., above the median)[27] and the treatment condition. We also present the results of placebo specifications in which we use the same dimension of prejudice but against other racial groups, 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 instead use OLS estimation.

Our estimation equation is as follows:

$$PrejViol_{i2} = \alpha + \beta\, Treat_i {*} PrejViol_{i1} + \gamma PrejViol_{i1} + \theta\, Treat_i + \eta X_i + u_i, \tag{3}$$

$PrejViol_{it}$, 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. More precisely, $PrejViol_{i1}$ is a dummy variable that indicates whether the respondent's initial level of prejudice is above sample median. $Treat_i$ denotes the treatment condition for i. X_i is a vector of individual-level controls, all measured at baseline. We use the same set of individual controls as in Newman et al. (2020): age, education, gender, race (Black, Hispanic, Asian, or White), employment status, party identification, and politicization (i.e., whether the respondent pays attention to elections).

In Figure 3, we present the coefficients associated with each treatment condition, controlling for baseline prejudice and the full set of controls, but without including the interaction between treatment and baseline prejudice described in Equation 3. In other words, this represents the average effect of each treatment across all respondents in the sample. Regression results is presented in Appendix Table A5. None of the treatment conditions significantly affects the average prejudice against any racial group. As in

[27] 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.

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[28] for us to detect an average effect of inflammatory speech.

Yet, among the most prejudiced individuals, the picture is strikingly different. In Figure 4 (full results in the 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 bias. 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 respondents' prejudice against Whites, Asians, or Hispanics; even among respondents who are particularly prejudiced against each of these groups. This finding is especially remarkable as Trump's statement is not - at least at a superficial level- directed against African Americans. The fact that we observe an activation of prejudice against African Americans but not against Hispanics provides a direct validation of the dog-whistle effect. Since we only observe a significant increase in prejudice among the individuals who are already most prejudiced, our results show a radicalization of prejudice.

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

We provide further evidence that the effect is tied specifically to the activation of the prejudice that *Blacks* are *violent*. To this end, Figure 5 presents the results of a similar analysis for the other dimensions of prejudice against Blacks measured in the survey: laziness, lack of intelligence, illegal presence in the US. The coefficients associated with the treatments are positive for those respondents who were already more prejudiced than the median along these dimensions, but not statistically significant. The effect of exposure to Trump's racially inflammatory speech is largest in aggravating the prejudice that Blacks

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

lack intelligence, as opposed to the prejudice that they are lazy or in the US illegally. The effect is statistically significant at the 10 percent level, but not robust to the inclusion of individual controls (see regression results in Appendix Table 5). Furthermore, in Figure A2 in Appendix we verify that the treatment does not make individuals with a high preexisting prejudice that African Americans are *violent* more likely to express prejudice in these *other* dimensions. To conclude, we observe that exposure to Trump's racially inflammatory speech radicalizes prejudice against African Americans, but is robust only for the prejudice that African Americans are violent.

Additional results displayed in Table 7 indicate that the effect is only significant, and larger in magnitude, for White respondents. For those in this group above the (sample) median prejudice, exposure to Trump's inflammatory statement increases declared prejudice by 0.55 points, a 24% increase with respect to the sample mean. By contrast, we observe no difference across education levels. The point estimate of the coefficient associated with the interaction of baseline prejudice and exposure to Trump's inflammatory statement is identical for college graduates (0.38) and those who did not complete a university education (0.37).[29]

Columns 7 to 9 of Table 7 show the effect of Trump's inflammatory speech along political lines. Note that although Republicans generally display higher prejudice associating Blacks with violence, far from all prejudiced individuals are Republican. Of the 20.90% of respondents in the sample who self-identify as Republican, 59.81% are above the sample median prejudice level, compared to 40.46% among the self-identified Democrats or Independents. Although this difference is statistically significant, its magnitude is far from a perfect sample split, where all Republicans would stand above-median prejudice and Democrats below. Moreover, Column 7 shows that Republicans are not more likely than Democrats or Independents to respond to the treatment. However, Columns 8 and 9 show that already-prejudiced Republicans increase their level of prejudice when exposed to Trump's racially inflammatory statements, when other Republican elites condone such

[29]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).

speech ("Trump Condone" treatment). This suggests that the attitude of the Republican leadership toward Trump may play an important role in moderating the effect of Trump's divisive speech on racial attitudes of self-identified Republican voters. The magnitude of the effect of exposure to Trump's racially inflammatory speech among other political affiliations is less than half in magnitude but still statistically significant. In other words, all prejudiced individuals, regardless of their political affiliation, become radicalized in their prejudice that blacks are violent when exposed to Trump's inflammatory statements. For self-identified Republicans, the effect is larger in magnitude and moderated by the broader position of the Republican leadership.

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

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

One explanation for our results is that talk of crime, rape, and drugs, or the evocation of a potentially threatening outgroup activates among the subgroup of most prejudiced Americans their long-standing and ingrained threatening stereotype: that African Americans as responsible for violence and crime. This is all the more surprising that the outgroup evoked in the speech does not consist of African Americans, but rather of foreign nationals (Mexicans). Simultaneously, this enables Trump maintains deniability that his campaign is in any way anti-African American while also avoiding alienating those strongly attached to norms of equality and tolerance. Meanwhile, the most prejudiced voters understand these words differently – in a way that radicalizes their prejudice even further. Altogether, this evidence is consistent with theoretical conceptualization of the dog-whistle effect. We now go back to Trump's campaign speeches and our police data to verify whether police behavior is similarly triggered by those words when spoken at Trump's rallies, particularly in more racist areas.

6 Trump's Rally Speech and Police Stops

In this section, we go back to our police data and examine how the words included in the experimental statement moderate the effect of a rally on police behavior when spoken at Trump's rallies. First, we establish that the same inflammatory words found to radicalize prejudice among the online random sample in the previous section also channel the effect of a Trump rally on police stops of Black drivers. We then show that this effect is only found in counties that historically relied more heavily on slavery.

To perform our analysis, we obtain the content of speeches at each campaign rally from The American Presidency Project (Peters and Woolley, 2020).[30] We are able to obtain speech data for 75 rallies in 58 counties, but only 39 counties for which we have police data. Our final sample consists of 48 rallies in 39 counties.

We focus on the (stem) words from Trump's statement used in the experiment described in the previous section: crim, drug, rap, and Mexic. Drug and rap are actually quite rare in the speeches, so we only retain crim and Mexic for our analysis. Descriptive statistics suggest that Trump says crim at least once in 79% of the speeches for which we have data, Mexic in 56% of them, and crime and Mexic together in 48% of them. In only 6 speeches, does he mention Mexic without mentioning crim. To simplify our empirical analysis, we only focus on a 30-day period on either side of a rally (i.e. 60 days in total). We estimate Equation 1 with $k = \mathit{30}$, adding an interaction term between $D_{c,t}^{(1,30)}$ and a dummy variable that takes value one if the rally speech includes Mexic or crim. Finally, because we hypothesize that our mechanism of dog-whistling operates more

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

strongly among prejudiced subjects, we include a triple interaction between an indicator of whether the county in which the rally takes place is above the median county in the sample in terms of historical slave numbers (or cotton suitability for robustness), whether the speech mentions either Mexic or crim, and the post-rally dummy. We therefore estimate:

$$\begin{aligned} Black_{i,c,t} = \alpha_c + \theta_t + \eta D_{c,t}^{(0,0)} + \beta D_{c,t}^{(1,30)} + \gamma D_{c,t}^{(1,30)} {*} \mathbb{1}Word_{c,t} \\ + \kappa \mathbb{1}Word_{c,t} {*} \mathbb{1}Slavery_c + \delta D_{c,t}^{(1,30)} {*} \mathbb{1}Word_{c,t} {*} \mathbb{1}Slavery_c + u_{i,c,t} \end{aligned} \tag{4}$$

where $\mathbb{1}Word_{c,t}$ takes value one in county c in the days around a rally in which Trump mentions a specific word (Mexic or crim) and $\mathbb{1}Slavery_c$ indicates whether more slaves were historically held in county c compared to the median county in our sample.

An important caveat of this approach is that the choice of words in Trump's rallies may be endogenous to underlying county characteristics. Accordingly, in Appendix Table A8, we show how the probability that a rally speech in a given county includes Mexic or crim correlates with several characteristics of the county where the rally takes place. Trump is more likely to talk about Mexic or crim in more populous and richest counties, as well as in counties where the share of Blacks is higher, but less likely to do so in counties that are more ethnically fragmented. The results discussed in this section should therefore only be interpreted as suggestive of a correlation between speech content, historically persistent racial attitudes and police behavior.

Nevertheless, results in Table 8 show that rallies in which Trump mentions Mexic or crim are associated with more traffic stops of Black drivers by the police, but only in counties that relied more heavily on slavery historically – our proxy for long standing racial prejudice against Blacks. In Columns 1 and 2, we show the estimation results when we only include the interaction between our post-rally dummy and whether the rally mentioned Mexic or crim. The interaction is positive for Mexic, suggesting an additional effect of rallies on Black stops when Trump says Mexic, but the effect is not statistically significant. The interaction is negative for crim, but not significant. However, the picture is very different when we consider the estimation results of Equation 4 in Columns 3 and 4. Consistent with our hypothesis, the only significant coefficient is δ: the coefficient associated with the triple interaction between our post-rally dummy, whether the speech delivered at the rally contains words referring to either Mexico (Column 3) or crime (Column 4) and whether the county in which the rally takes place held above median number of slaves historically. The triple interaction is positive and significant both for Mexic and for crim. These results show that the effect of Trump rallies on traffic stops of Black drivers is only statistically significant in more racist areas, and when speeches refer to either crime or a potentially threatening outgroup. The coefficient associated with the main effect of rallies, i.e. the effect of rallies in which the words of interest are not spoken, remains positive and large but fall short of statistical significance in this reduced sample.[31] The results are similar when we use another proxy for historical slavery, cotton suitability (see Table A9 in the Appendix).

Notwithstanding the limitations due to the potential endogeneity of word choice, these results add two important pieces of evidence that concord with our experimental results and that validate our hypothesized mechanism of the dog-whistle effect. First, they confirm that the effect of Trump speeches on prejudice against Blacks is triggered by mentions of specific words that do not explicitly target Blacks but stroke fear, either by mentioning crime or by invoking a potentially threatening outgroup. Second, the effect hinges on pre-existing attitudes. At the individual level, only already prejudiced individuals react to those words and became even more prejudiced. At the police stop level, rallies mentioning those words increase the probability of Black traffic stops, but only in areas that have a long-standing history of racial animosity.[32]

[31] The coefficient associated with the interaction between the post-rally dummy and the word dummy is positive for Mexic, but negative for crim, suggesting that in areas that did not rely heavily on slavery, police still react to Trump's references to Mexico by arresting more Blacks, but react in the opposite way when hearing him talk about crime. However, none of these coefficients is statistically significant.

[32] A question left open is whether the effect is triggered more by the mention of crim, rather than Mexic. However, given that Trump mentions Mexico without also talking about crime in the same speech in only 6 speeches, we lack the power to investigate that question. A more complete analysis of Trump's speeches is beyond the scope of this paper.

7 Conclusion

In this paper, we show how Trump's political campaign radicalized racial prejudice against African Americans as well as its expression in a critical and potentially violent dimension: police behavior. Our estimates suggest that Trump's campaign events led to 4,480 additional traffic 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 historically more prevalent. Combining our data with an online survey experiment conducted during Trump's 2016 campaign, we confirm that the effect is specific to prejudice against Blacks, particularly the bias that they are violent. Moreover, the effect is stronger, or only present, in areas or among individuals that were initially more racist. Overall, our results show how politicians can radicalize stereotypes of a given group through language that, at least superficially, appears either innocuous or unrelated.

Traffic stops represent, by far, the majority of police-citizen interactions in the US. More than 20 million people are pulled over by the police every year in the US (Davis, Whyde and Langton, 2018), with Black drivers over-represented by a factor of two compared to their proportion in the population (our estimate in Section 2). Although fatal encounters between police officers and Black civilians capture greater media attention, the over-enforcement of minor infractions and the kind of unjustified stops by the police that we document in this paper highlight a daily and generalized expression of discrimination against minorities, with potentially dire consequences.[33] Ensuing feelings of discrimination and under-representation undermine minorities' faith in the police and public institutions at large. This produces a vicious circle, whereby daily and unjustified harassment reduces trust in the police and leads citizens to shy away from relying on law

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

enforcement and instead to seek other, informal, and possibly violent means of protection and retributive justice (Giffords Law Center, 2020). Moreover, unjustified police repression can translate into voter suppression, when disenchanted citizens extend their lack of trust in the police to all public institutions. Recent research, in fact, shows how historical discrimination and violence against Blacks is associated with lower voter registration by Black voters (Williams, 2020). Estimating the impact of police discriminatory behavior on voting behavior is thus a crucial direction for future research.

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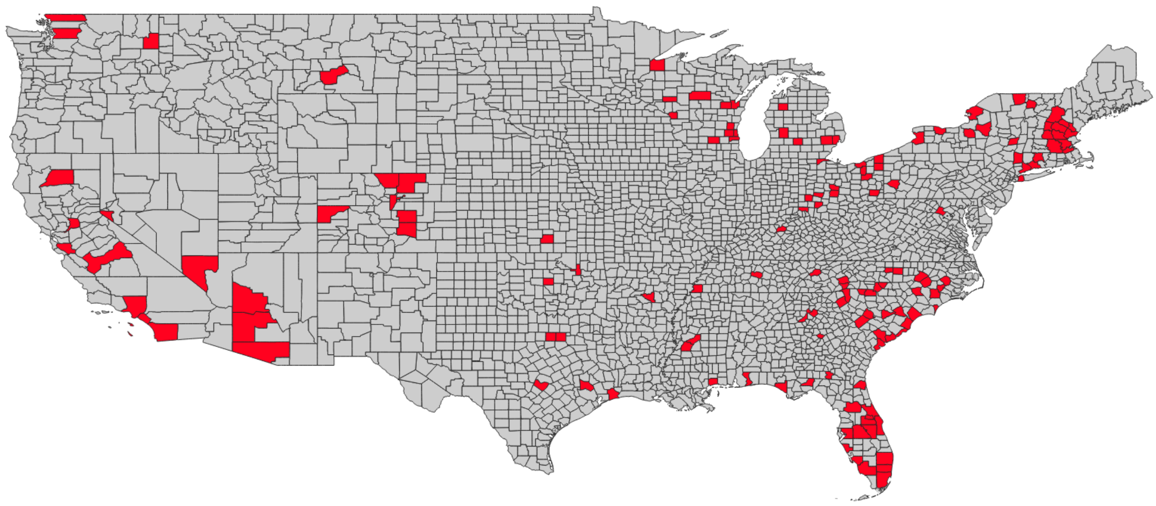
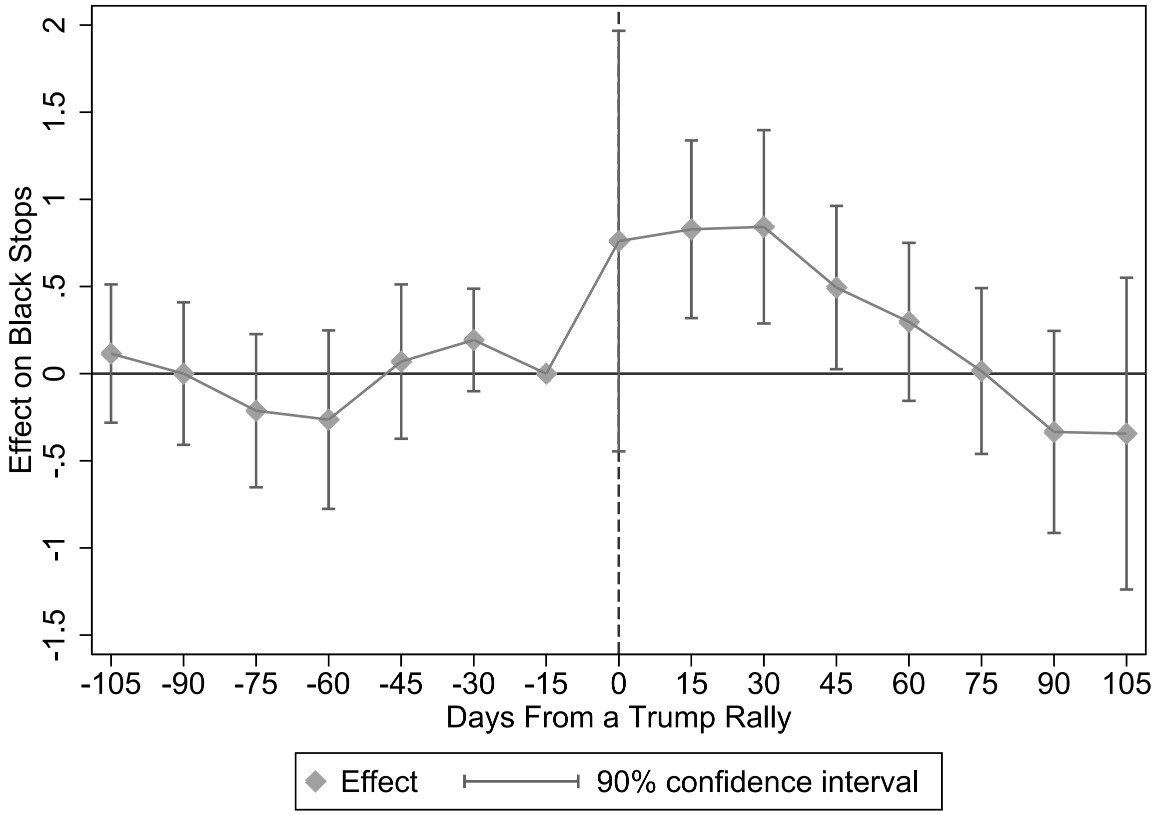


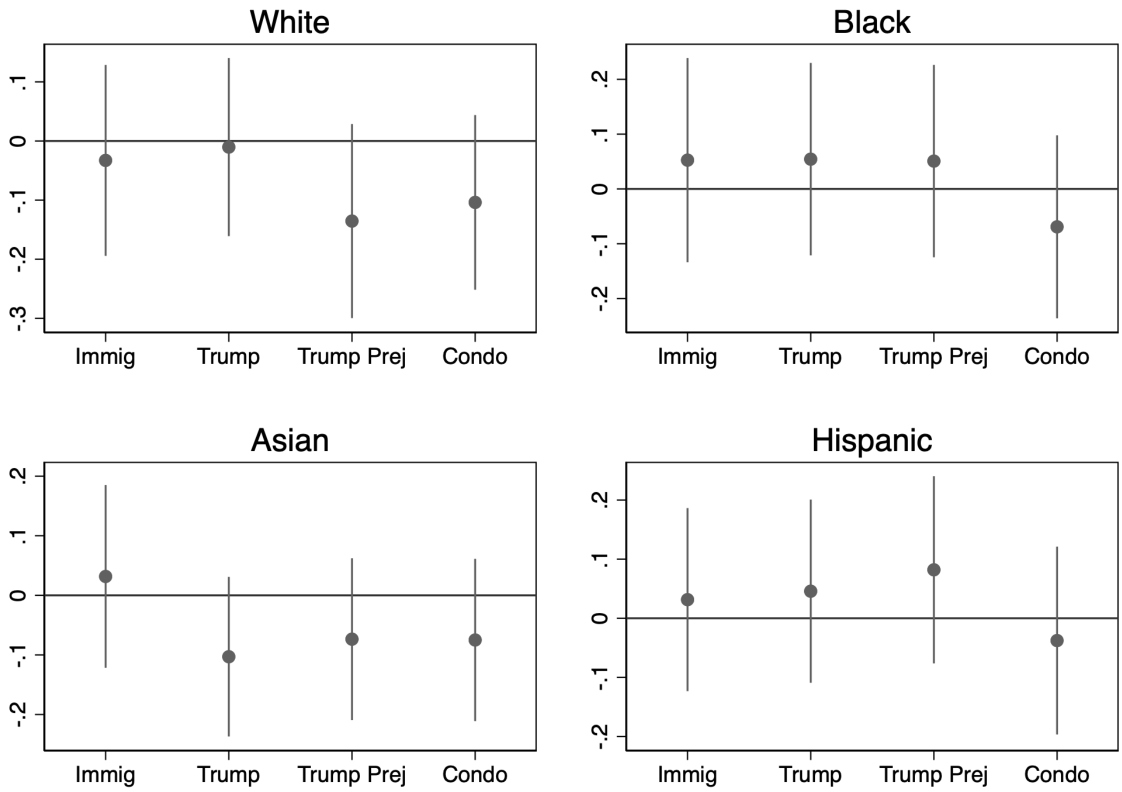
Notes: This map shows the 142 counties for which we have data on police stops and 2015-2016 Trump's campaign events.

Figure 1: Counties with campaign events and police stops



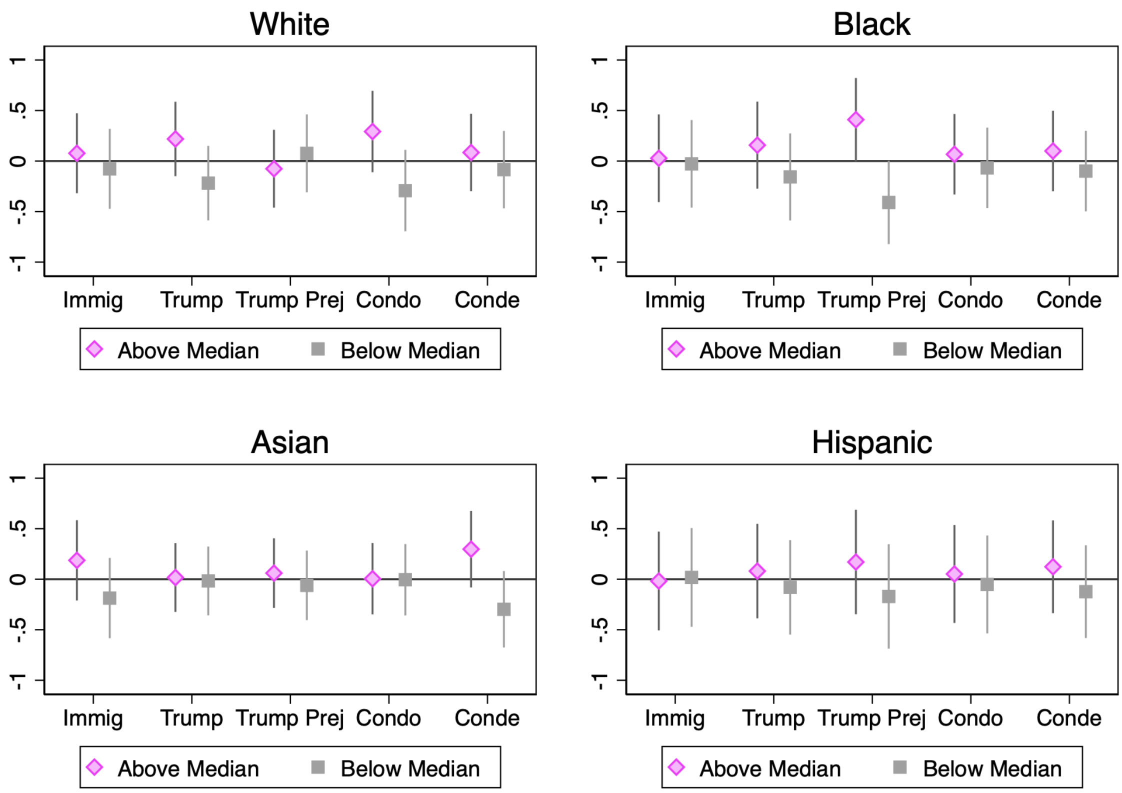
Notes: The figure plots OLS coefficients with 90% confidence intervals (vertical lines). The plotted coefficients are the β_τ coefficients associated with each month (as indicated divided by 30), described in Equation 2. Standard errors are corrected for two-way clustering at the county and date level.

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



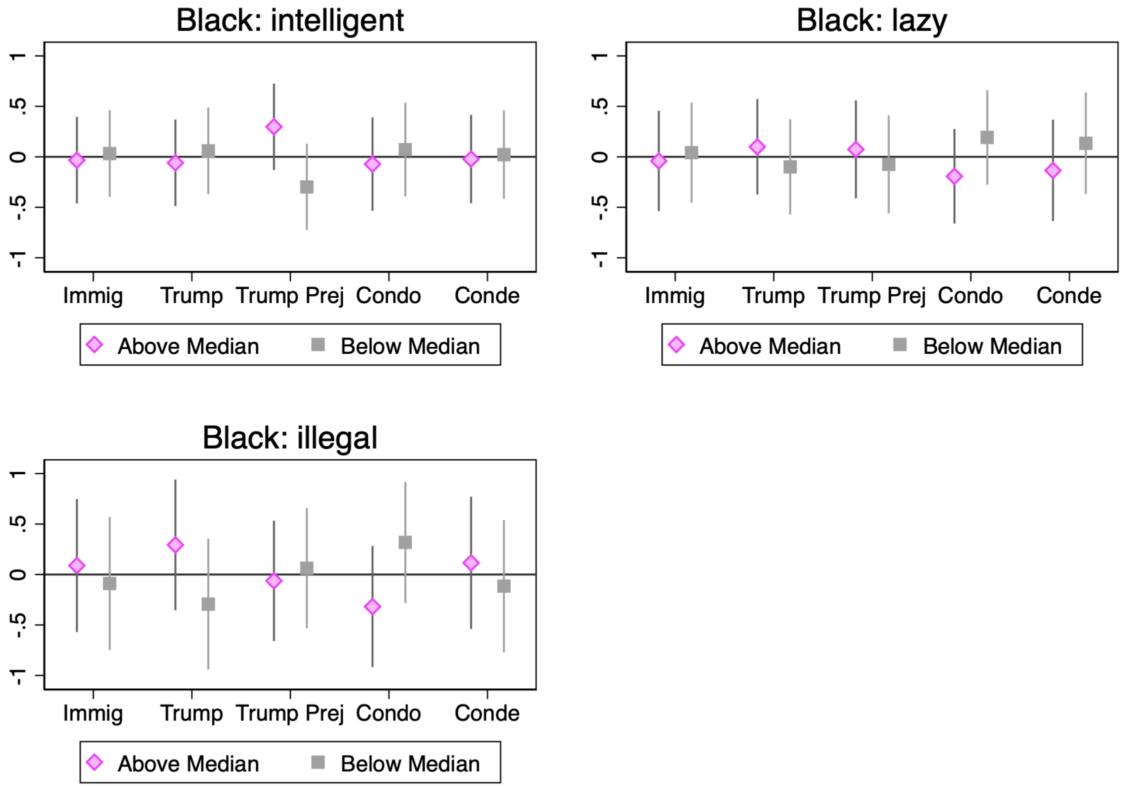
Notes: The figure plots OLS coefficients with 90% confidence intervals (vertical lines). The plotted coefficients are the coefficients associated with each treatment condition ("Immig": Immigration prime; "Trump": Trump prime; "Trump Prej": Trump Prejudice; "Condo": Trump Condone; "Conde": Trump Condemn, see Table A1 for more detail), 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 against any racial group for the average respondent.

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



Notes: The figure plots OLS coefficients with 90% confidence intervals (vertical lines). The dependent variable is the endline prejudice that each racial group (as indicated in the header of each panel) is violent. The plotted coefficients are the coefficients associated with the interaction between each treatment condition (as indicated) and an indicator variable for being above (triangle) or below (square) the median (in separate regressions). All regressions control for the indicator variable separately, for the main effect of the treatment condition and for the full set of controls described in Equation 3. Standard errors are corrected for heteroskedasticity. See Table A6 for regression results with the interaction with the indicator for above sample median prejudice. We display results for the last two treatments ("Condo" and "Conde") for completeness, although we do not have predictions for either of these treatments. See Figure A4 when these two treatments are excluded from the estimation sample.
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 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

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. 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.

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

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	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 Blacks, 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 become even more prejudiced when exposed to the treatment (Col. 2). Prejudiced Republicans, as well as - to a lesser extent - prejudiced Independents or Democrats, also become even more prejudiced when exposed to the treatment, although the effects are not statistically significant at conventional levels (Col. 8 and 9). 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. $^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8: Effect of Trump's rally speech on police stops of Black commuters

	(1)	(2)	(3)	(4)
VARIABLES		100 x $\mathbb{P}$(Black Stop= 1)		
POST-Trump	1.540	2.749	1.590	2.765
	(1.511)	(2.302)	(1.511)	(2.298)
POST-Trump * $\mathbb{1}$(Word)	0.439	-1.899	0.104	-2.239
	(0.773)	(2.167)	(0.810)	(2.261)
POST-Trump * $\mathbb{1}$(Word) * $\mathbb{1}$(Slavery)			2.755**	3.117*
			(1.308)	(1.692)
Observations	323,064	323,064	323,064	323,064
R-squared	0.131	0.131	0.131	0.131
Interaction	Mexic	crim	Mexic	crim
County FE	YES	YES	YES	YES
Daily FE	YES	YES	YES	YES

Notes: This table shows that exposure to content of Trump's inflammatory speech affects differently racially prejudiced and other areas. The table reports OLS regressions with a constant of Equation 4. $\mathbb{1}Word$ takes value one in county c in the days around a rally in which Trump mentions a specific word (Mexic or crim) and $\mathbb{1}Slavery$ indicates whether more slaves were historically held in county c compared to the median county in our sample. All regressions control for the county and day fixed effects. Standard errors are two-way clustered at the county and at the day level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Online Appendix
(NOT FOR PUBLICATION)

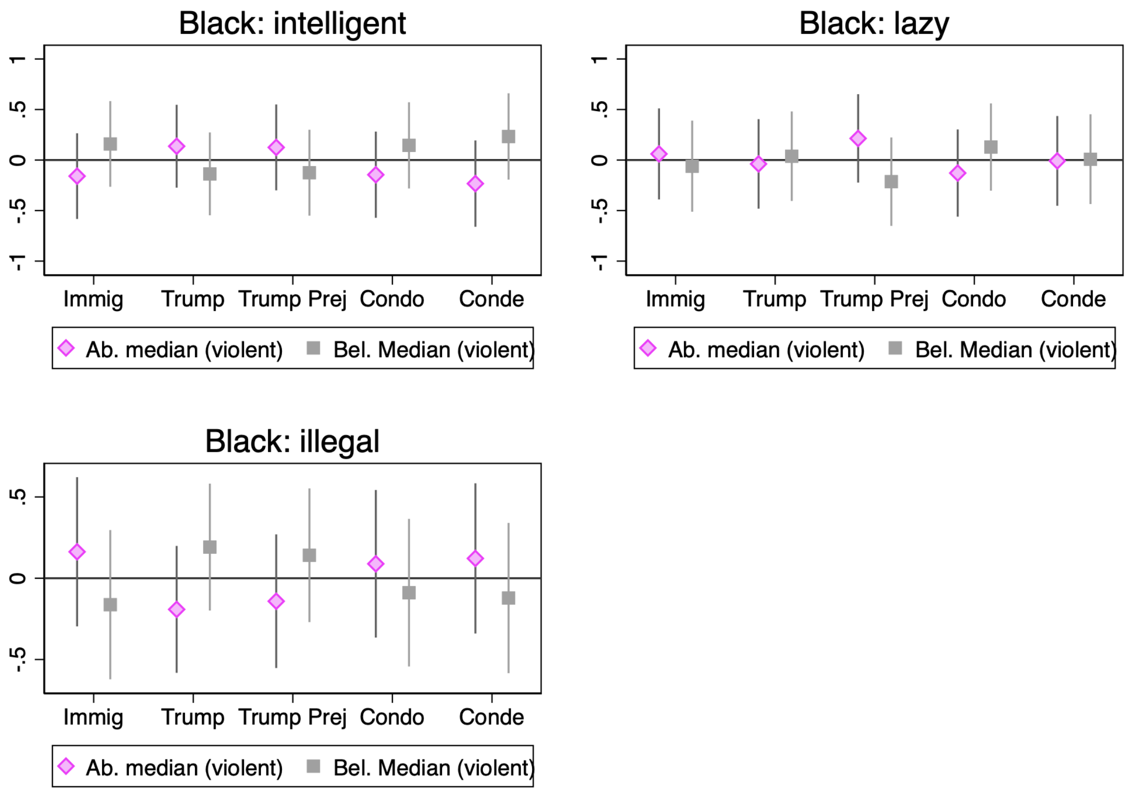
	Name	Candidates	Political issue	Prejudiced speech	Other elite signal
1.	Control	Clinton/Bush	Campaign finance reform	None	None
2.	Immigration Prime	Clinton/Bush	Immigration reform	None	None
3.	Trump Prime	Clinton/Trump	Campaign finance reform	None	None
4.	Trump Prejudice	Clinton/Trump	Immigration reform	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 of randomly assigning a respondent to read one article about the 2016 presidential election. The articles were created by the authors, drawing on real election content. The table describes the content of the six different versions of the article respondents were asked to read in Wave 2 of the authors' online panel survey experiment.

Figure A1: Experimental Treatments

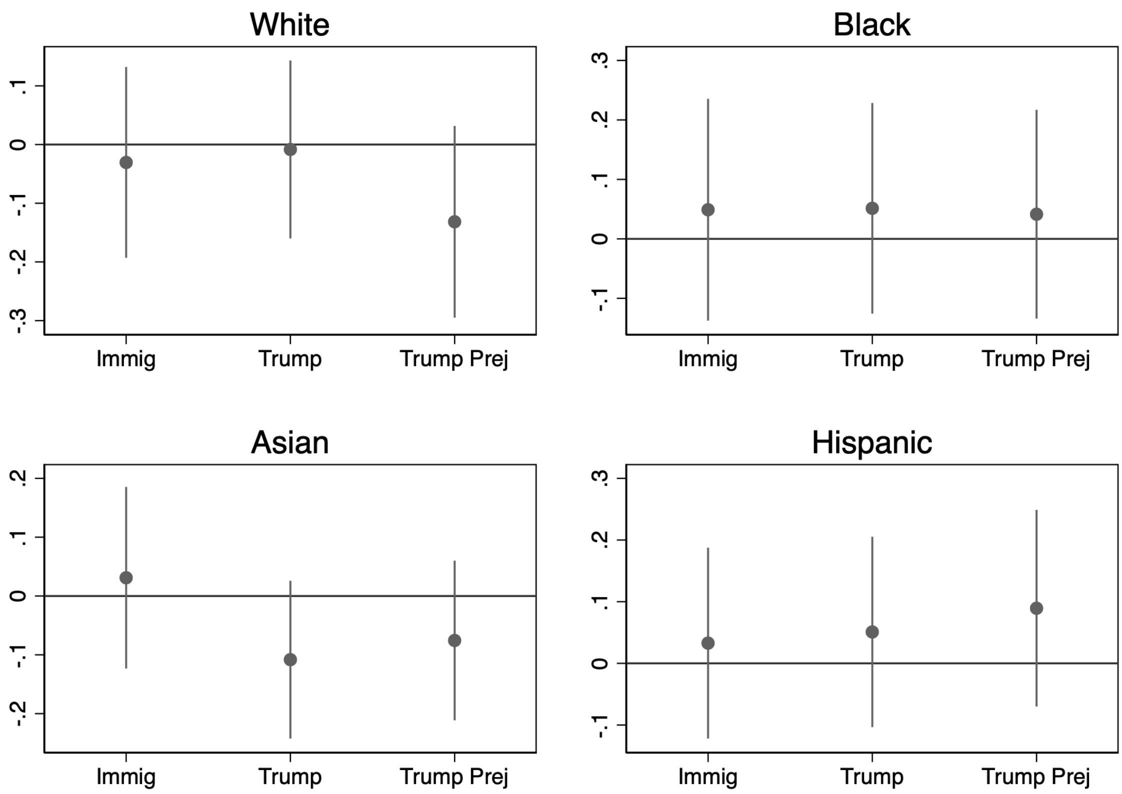
1 Additional Figures

The following figures replicate Figures 3 to A2 in the paper but exclude respondents in the "Trump Condone" and the "Trump Condemn" conditions. The sample size is reduced to 656 respondents. All the results in the paper carry through in this subsample.



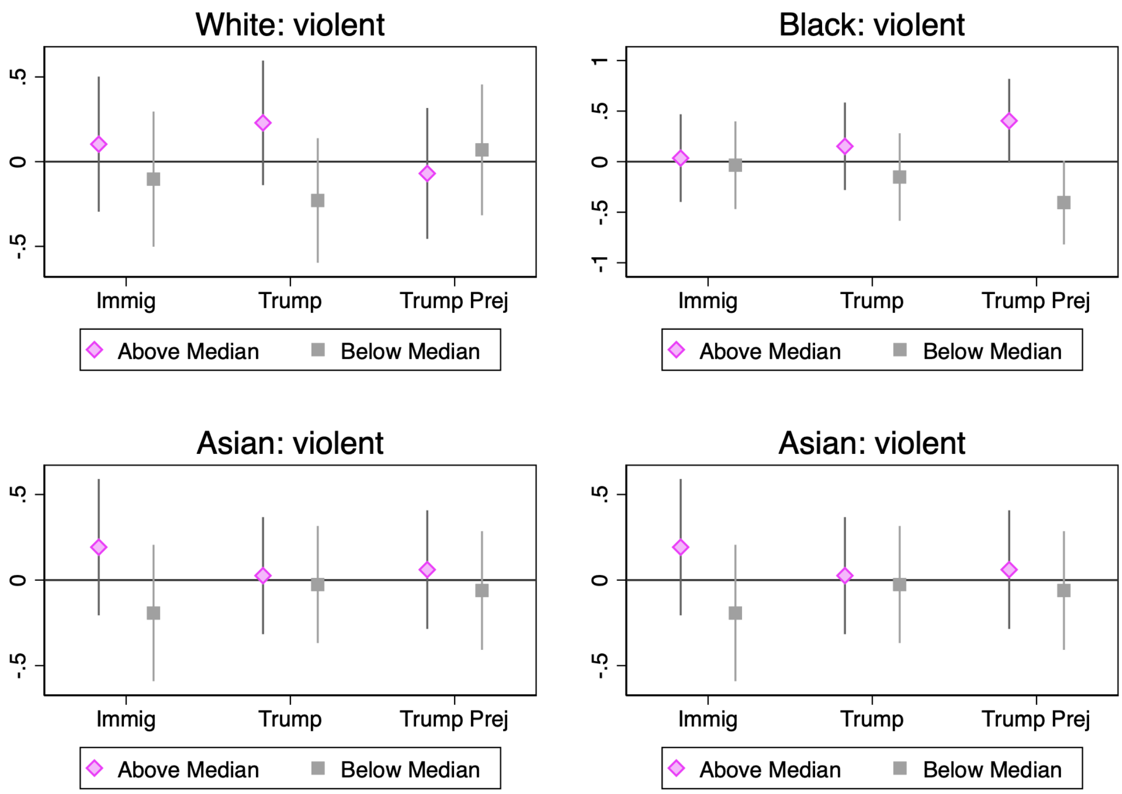
Notes: See notes to Figures 4 and 5. The only difference here is that we interact the treatment with pre-existing prejudice that Blacks are violent (as opposed to the same dimension of prejudice as the dependent variable). The Figure shows that none of the treatment conditions activates prejudice against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

Figure A2: 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.



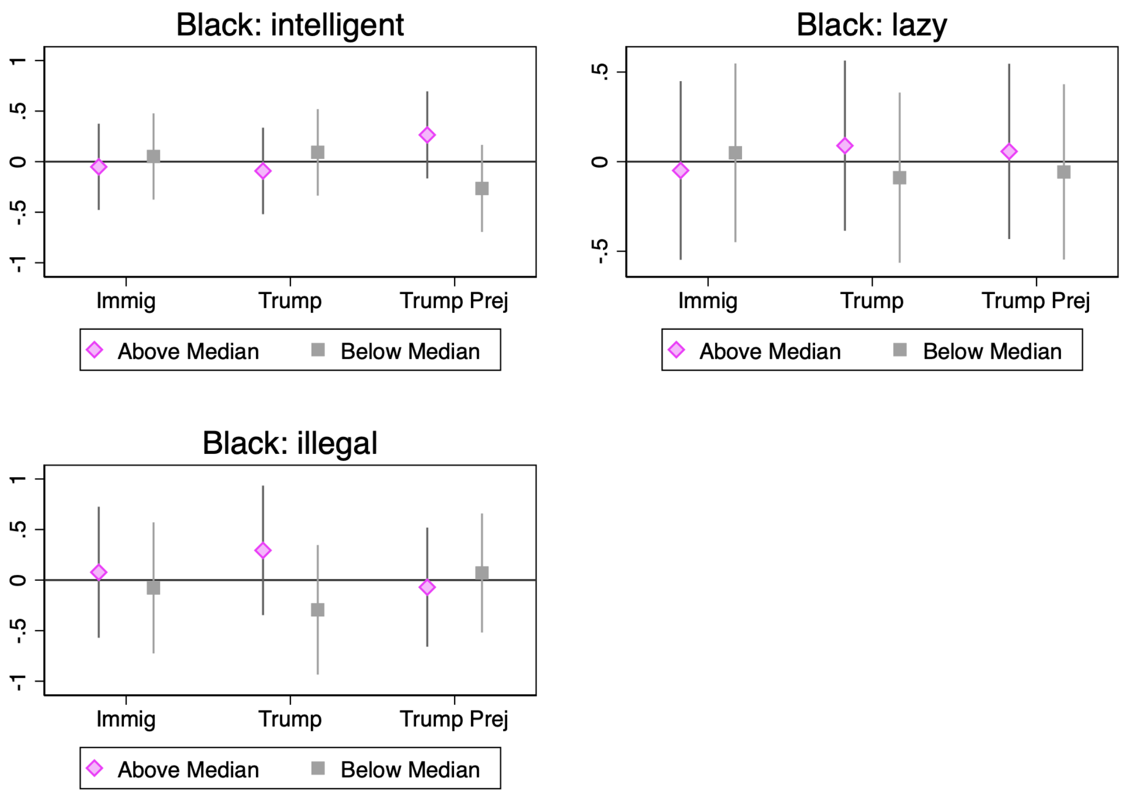
Notes: See notes to Figure 3. The only difference is that we exclude here the 341 respondents in the "Trump Condone" and in the "Trump Condemn" conditions. The results commented in the notes to Figure 3 and in the paper carry through.

Figure A3: Violent Prejudice: Figure 3 without the last 2 treatment conditions.



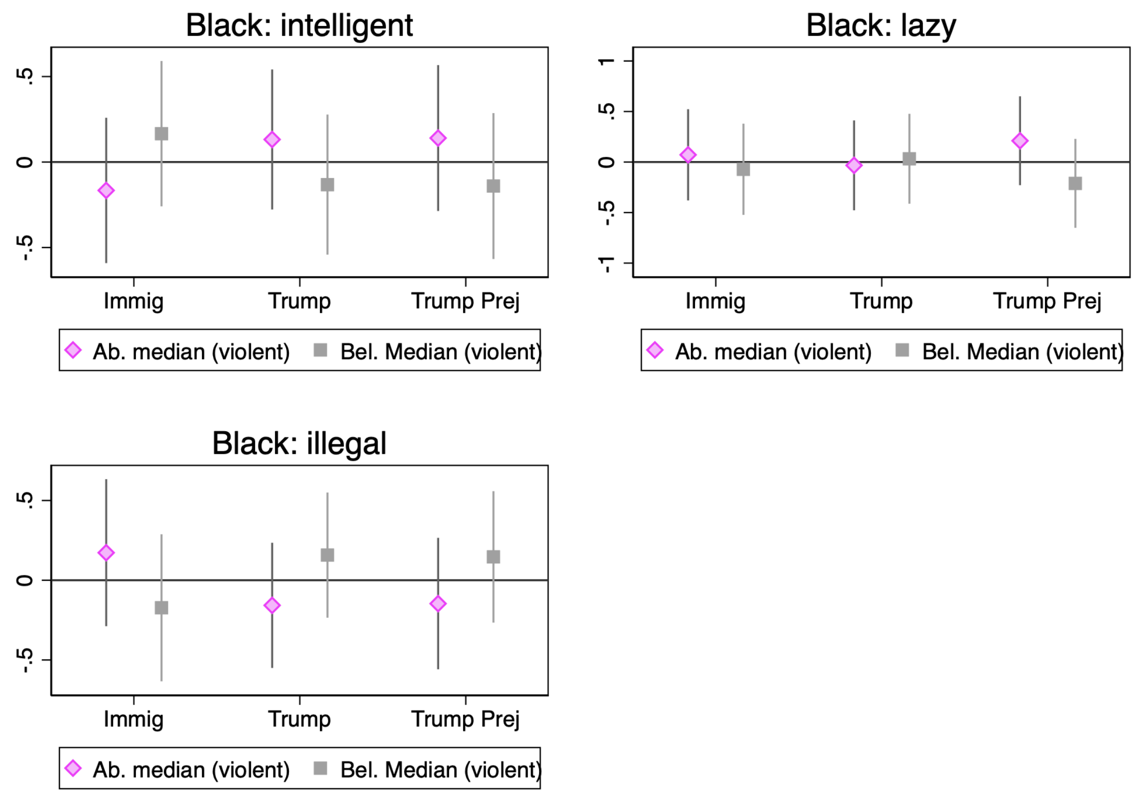
Notes: See notes to Figure 4. The only difference is that we exclude here the 341 respondents in the "Trump Condone" and in the "Trump Condemn" conditions. The results commented in the notes to Figure 4 and in the paper carry through.

Figure A4: Trump's inflammatory speech activates the prejudice that Blacks are violent among biased respondents: Figure 4 without the last 2 treatment conditions.



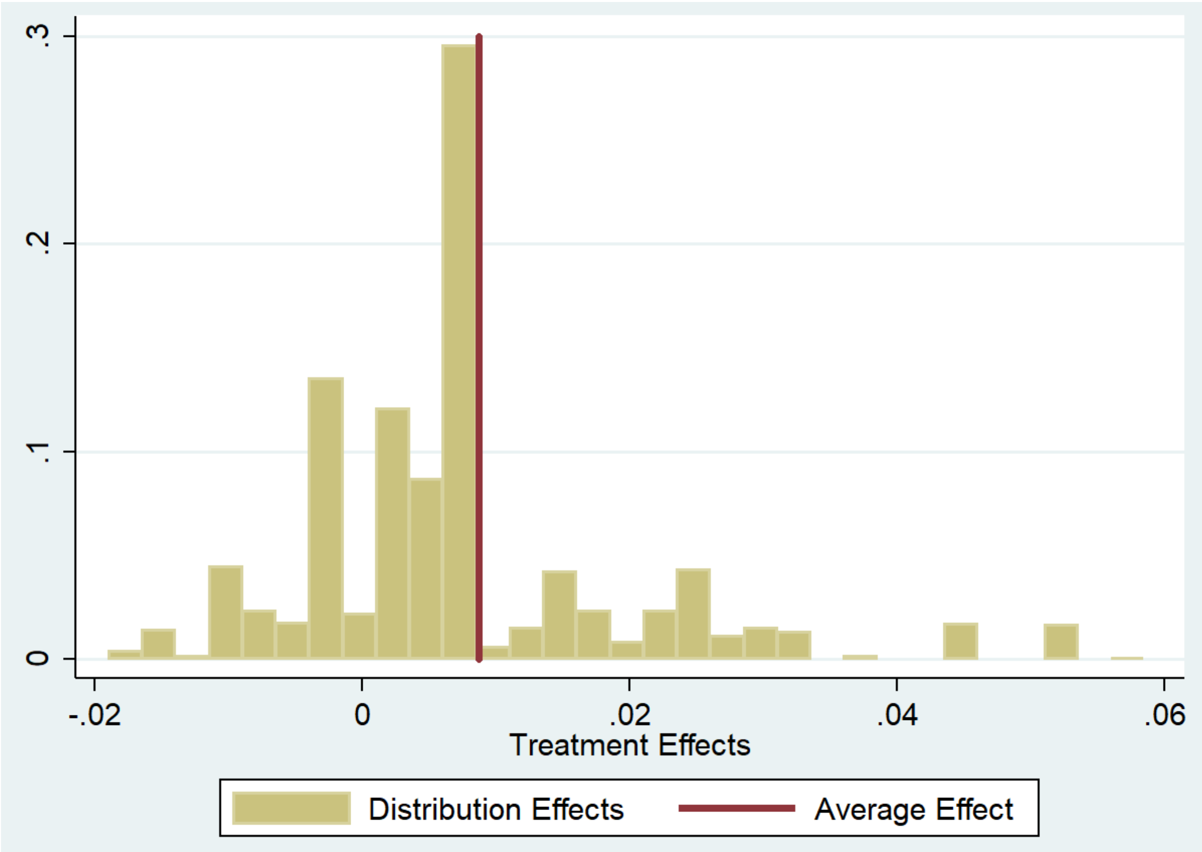
Notes: See notes to Figure 5. The only difference is that we exclude here the 341 respondents in the "Trump Condone" and in the "Trump Condemn" conditions. The results commented in the notes to Figure 5 and in the paper carry through.

Figure A5: 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 A2. The only difference is that we exclude here the 341 respondents in the "Trump Condone" and in the "Trump Condemn" conditions. The results commented in the notes to Figure 4 and in the paper carry through.

Figure A6: Trump's inflammatory speech does not activate other dimensions of anti-Black bias, even among those who have a strong prejudice that Blacks are violent: Figure A2 without the last 2 treatment conditions.



Notes: This figure shows the distribution of the estimated difference-in-differences estimator for each county. The vertical bar is the average of these estimated coefficients

Figure A7: Distribution Difference-in-Differences Estimator

2 Additional Tables

2.1 Robustness of Police Results

In this section, we present the robustness analysis for the results on traffic 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. Column 3 shows the probabilities of a traffic stop leading to a vehicle search for Hispanic drivers. Columns 4, 5 and 6 show the outcomes of a stop: the probabilities of a traffic stop leading to (in order of increasing severity): a warning, citation, or arrest for Hispanic drivers. Column 7 shows the probability of a Hispanic driver being stopped due to a visible offense that would also automatically trigger a stop for a Black driver, while column 8 shows the probability of a discretionary stop of a Hispanic driver. 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 a Black driver being stopped in the following specification:

$$Black_{ict} = \alpha_c + \theta_t + \eta \mathbb{1}(D_{cte} = 0)_{e=1,\dots,N_c} + \beta Max(\mathbb{1}(0 < D_{cte} \leq k)_{e=1,\dots,N_c}) + u_{ict}, \quad (5)$$

where $Black_{ict}$ is a dummy equal to one if the driver stopped was Black in county c on date t. $\mathbb{1}(0 < D_{cte} \leq k)_{e=1,\dots,N_c}$ 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, here we estimate Equation 5 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 drivers stopped as a fraction of total stops in the 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. In Columns 1 to 6, we present the 10 to 100 days (with increments of 10 days) of post-rally observations in our sample.

Table A4 shows the estimates of Equation 5. We fix the pre-rally part of the sample as hundred days before a rally, and consider different post-rally parts of the sample around a Trump rally. From Columns 1 to 10, we present the 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 point (p.p.) increase in the probability of Black driver being stopped in the first ten days after the rally. The mean value (standard deviation) of the probability of a traffic 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 a Trump rally. The effect peaks at 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 were very different from the those visited for the presidential campaign. In what follows, we show that this is not the case along a wide range of dimensions, including pre-trends in police behavior, racial composition, education, recent economic shock, or underlying racial resentment.

In Table **??**, 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 2 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 A2.

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 respective 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). Results in the odd columns are plotted in Figure 3. Standard errors are corrected for heteroskedasticity. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 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 respective 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 2, the included variable is an indicator variable for being above the median sample prejudice that Whites are violent; in columns 3 and 4, the included variable is an indicator variable for being above the median sample prejudice that Blacks are violent, etc.), as well as the interaction between this indicator variable and the treatment condition, as indicated. Individual controls are included in the odd columns. As in all specifications, the individual controls consist of: age, education, gender, race (Black, Hispanic, Asian, or White), employment status, party identification, and politicization. The coefficients associated with the main effect of each treatment and its interaction with above median prejudice in the odd columns are displayed in Figure 4. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. This table shows that exposure to Trump's inflammatory speech increases the prejudice that Blacks are violent among respondents who are initially prejudiced (i.e., above the 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 the odd columns are displayed in Figure 5. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. This table shows that exposure to Trump's inflammatory speech does not increase other dimensions of prejudice against African Americans, namely the prejudice that Blacks are lazy, or lack intelligence, even for respondents who to begin with are highly prejudiced.

2.5 Trump's Rally Speech and Police Stops: Additional results

In this section, we provide additional analyses underlying Section 6.

Table A8: Pair-wise correlations: Trumps' rally speech and county covariates

	Mexic	Crim	Slavery	Cotton	Pop	White	Black	Hispanic	Rac-HHI	Age	Income	DEM	HS	Coll	China
Mexic	1														
Crim	0.74*	1													
Slavery	-0.01	-0.01	1												
Cotton	0.07	-0.06	0.51*	1											
Population	0.22*	0.27*	-0.05*	-0.07	1										
White	-0.03	-0.04*	-0.31*	-0.41*	-0.19*	1									
Black	0.04*	0.04*	0.80*	0.54*	0.08*	-0.47*	1								
Hispanic	0.01	0.01	-0.17*	0.05	0.11*	-0.73*	-0.14*	1							
Racial HHI	-0.07*	-0.09*	-0.46*	-0.43*	-0.28*	0.85*	-0.58*	-0.44*	1						
Median Age	-0.03	-0.04*	-0.13*	-0.12	-0.18*	0.43*	-0.19*	-0.26*	0.44*	1					
Income	0.09*	0.12*	-0.16*	-0.15	0.24*	0.06*	-0.18*	0.02	-0.07*	-0.05*	1				
DEM share	0.10*	0.11*	0.16*	-0.26*	0.27*	-0.45*	0.41*	0.09*	-0.29*	-0.17*	0.18*	1			
High School	-0.11*	-0.14*	0.19*	0.05	-0.22*	-0.19*	0.18*	0.17*	0.05*	0.09*	-0.63*	-0.16*	1		
College	0.14*	0.18*	-0.13*	-0.05	0.32*	0.01	-0.10*	0.00	-0.12*	-0.17*	0.68*	0.30*	-0.84*	1	
China Shock	-0.02	-0.02	0.11*	-0.18*	-0.02	0.10*	0.10*	-0.19*	0.09*	-0.00	-0.19*	0.04*	0.18*	-0.13*	1

Notes: This table shows that pair-wise correlation between a specific word (Mexic or crim) mentioned in a rally and the following characteristics of the county where the rally takes place: slaves per capita in 1860, soil suitability for growing cotton, share of Whites, share of Blacks, share of Hispanics, Racial HHI, median age, median household income, vote share of Obama in 2012 presidential election, share of high school graduates, share of college graduates, and the China import competition shock from Autor, Dorn and Hanson (2013). * $p < 0.05$

Table A9: Robustness: Effect of Trump's rally speech on police stops of Black commuters

VARIABLES	(1)	(2)	(3)	(4)
	100 x $\mathbb{P}$(Black Stop= 1)			
POST-Trump	1.925	2.644	2.037	2.595
	(1.733)	(2.011)	(1.747)	(2.054)
POST-Trump * Word	-0.066	-0.109	-0.161	-0.108
	(0.180)	(0.100)	(0.194)	(0.101)
POST-Trump * Word * $\mathbb{1}$(Slavery)			0.486**	0.109
			(0.182)	(0.227)
Observations	323,064	323,064	323,064	323,064
R-squared	0.131	0.131	0.131	0.131
Interaction	Mexic	crim	Mexic	crim
County FE	YES	YES	YES	YES
Daily FE	YES	YES	YES	YES

Notes: This table shows that exposure to Trump's inflammatory speech results in significantly more traffic stops of Black drivers in areas that historically held more slaves. The table reports the results of OLS estimation of Equation 4 (see more detail on estimation in the main text). "Word" counts the number of times a rally in county c mentions a specific word (Mexic or crim). $\mathbb{1}Slavery$ indicates that more slaves were historically held in county c compared to the median county in our sample. All regressions control for county and day fixed effects. Standard errors are two-way clustered at the county and at the day level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3 Trump's Rally Speeches: Examples

This section provides excerpts of Trump's rally speeches referring to African-Americans, crime, foreigners, or Mexico. The speeches usually associate crime and foreigners, as can be seen in the excerpts. From there, speeches often get to the subjects of either building a wall with Mexico, or "brutal drug cartels". We therefore group crime and foreigners together in one section.

3.1 On African-Americans

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

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

Table A10: Robustness: Effect of Trump's rally speech on police stops of Black commuters

	(1)	(2)	(3)	(4)
VARIABLES		100 x $\mathbb{P}$(Black Stop= 1)		
POST-Trump	1.540	2.749	1.389	2.765
	(1.511)	(2.302)	(1.368)	(2.298)
POST-Trump * $\mathbb{1}$(Word)	0.439	-1.899	-0.575	-2.239
	(0.773)	(2.167)	(1.031)	(2.261)
POST-Trump * $\mathbb{1}$(Word) * $\mathbb{1}$(Cotton)			3.116***	3.117*
			(0.922)	(1.692)
Observations	323,064	323,064	323,064	323,064
R-squared	0.131	0.131	0.132	0.131
Interaction	Mexic	crim	Mexic	crim
County FE	YES	YES	YES	YES
Daily FE	YES	YES	YES	YES

Notes: This table shows that exposure to Trump's inflammatory speech results in significantly more traffic stops of Black drivers in areas that historically were more suitable for cotton growing. The table reports the results of OLS estimation of Equation 4 (see more detail on estimation in the main text). "Word" counts the number of times a rally in county c mentions a specific word (Mexic or crim). $\mathbb{1}Cotton$ indicates that the county c is more suitable for growing cotton compared to the median county in our sample. All regressions control for county and day fixed effects. Standard errors are two-way clustered at the county and at the day level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3.2 On crime, foreigners, and Mexico

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

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

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

We will 100 percent build the wall, 100 percent.

Are you ready? Are you ready?

And who is going to pay for the wall?

[Audience]: ”Mexico”

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

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

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

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

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

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

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

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

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

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

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

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