Global Racist Contagion Following Donald Trump’s Election

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The election of Donald Trump was followed by a spike in the number of hate crimes and online harassment targeting minorities (Hauslohner 2016; Levin and Grisham 2017; Muller and Schwarz 2018; Potok 2017). One explanation holds that this sequence of events reflected a shift in social norms: while pro-racial equality attitudinal trends in the United States had spread optimism about the future of race relations, Donald Trump’s win signaled that social norms had shifted towards a greater acceptance of racist attitudes (Bursztyn, Egorov and Fiorin 2017; Crandall, Miller and White 2018; Rushin and Edwards 2018).

While the available evidence pertains to the United States, concerns that Donald Trump’s win legitimized racist attitudes abroad were voiced in media across the globe (see, for example, Shabi 2016). Al-Jazeera even worried that ‘Trump’s electoral victory has been a wake call for all democratic nations to consider the solidification of the global right-wing and discriminatory politics in Europe and beyond’ (Cherkaoui 2016). To explore whether those concerns were founded, we test whether the election of Donald Trump increased racial bias in policy attitudes outside the United States.

To identify the effect of Donald Trump’s election, we exploit the coincidence of the 2016 US presidential election with the fieldwork period of thirteen developed countries sampled by the European Social Survey (ESS). The ESS provides individual-level information about political attitudes, including attitudes on race-targeting policies. Most of all, the day of the interview can be considered as good as random with respect to the day of the election (Bar-Tal and Labin 2001; Legewie 2013; Perrin and Smolek 2009). Using a quasi-experimental approach, we compare self-reported racial bias among respondents interviewed after the election (the treatment group) with those interviewed before the election (the control group).

We find evidence that the probability of reporting a racial bias increased by 2.3 percentage points within an interval of ±15 days around the election of Donald Trump. The treatment effect is statistically significant and robust to several econometric specifications, including different sets of controls, time intervals, clustering and covariate-balancing strategies. As the ESS has typically run from September to January every even year since 2002, we can replicate the main analysis for previous US elections. The main result is unlikely to be spurious. We find that self-reported racial bias significantly decreased when Barack Obama, arguably the near-perfect opposite of Donald Trump on race-related issues, won his first mandate. Conversely, elections that did not change the status quo, such as when George W. Bush and Barack Obama received their second mandates in 2004 and 2012, respectively, had no significant effect on self-reported attitudes displaying racial bias.
We interpret our findings in light of the literature studying the effect of social norms on reporting sensitive attitudes (see, for example, Bursztyn, Egorov and Fiorin 2017; Holbrook and Krosnick 2009; Janus 2010; Kuklinski, Cobb and Gilens 1997a; Kuklinski et al. 1997b, Weber et al. 2014). In a world where the social norm of racial neutrality is mainstream, reporting a racial bias entails a social cost that racially biased respondents may avoid by insincerely reporting no bias. However, the election of Donald Trump, a candidate with racially biased views, signaled that the social norm of racial neutrality was less mainstream than previously assumed. Consequently, the expected social cost of expressing racist attitudes decreased, making them \textit{ceteris paribus} more likely to be reported (Bursztyn, Egorov and Fiorin 2017). By the same token, the first election of Barack Obama signaled an increase in the social desirability of racial neutrality and hence lowered the probability of reporting racially biased attitudes, whereas elections confirming the incumbent did not provide novel information about social norms and hence did not affect the report of racially biased attitudes.

The mechanism relies on the assumption that the election of Donald Trump, and those of his predecessors, did not affect race-related attitudes, but rather the likelihood of reporting them. While this assumption cannot be directly tested in our setting, further analysis corroborates its validity. We show that the treatment effect does not reflect a gradual change in race-related attitudes occurring around the election. In particular, neither pre-electoral campaign effects nor post-electoral bandwagon effects fully account for our main finding.

The nexus between Donald Trump’s win and the likelihood of reporting sensitive attitudes has found early empirical evidence in lab experiments (Bursztyn, Egorov and Fiorin 2017; Crandall, Miller and White 2018; Huang and Low 2017). We contribute to that literature in two ways. First, while Huang and Low (2017), Bursztyn, Egorov and Fiorin (2017) and Crandall, Miller and White (2018) rely on lab experiments, we mimic a natural experiment design based on representative samples from observational data. Secondly, while those works focus on the impact of Donald Trump’s election on attitudes and norms of behavior in the United States, we are the first to provide an exploratory analysis of the transnational contagion of racially biased attitudes.

**Empirical analysis**

**Sample**

We use data from Round 8 of the ESS, which includes eighteen countries. Donald Trump’s election fell inside the survey fieldwork period for thirteen of them. The fieldwork periods, detailed in the Appendix, typically lasted three to four months. The survey is constructed using highly rigorous translation protocols and conditional monetary incentives are granted to respondents upon the completion of face-to-face interviews.

**Empirical Model**

To identify racially biased attitudes, we use questions B38 and B39 of the ESS. Question B38 reads, ‘To what extent do you think the country should allow people of the same race or ethnic group as most people of the country to come and live here?’ Question B39 asks, ‘How about people of a different race or ethnic group from most people?’ Answers to both questions range from (1) ‘Allow many’ to (4) ‘Allow none’.

Because the two questions only differ in the race dimension, the differences in answers can only be driven by differences in the perception of migrants according to their race. By giving different answers to the two questions, respondents therefore knowingly reveal a racial bias. Most of all, because interviews are conducted face to face, respondents are subject to a stronger social pressure than in internet surveys, where racially biased opinions can be revealed anonymously (Seth 2013).
Denoting respondent $i$’s opposition to different- and same-race immigration by $y_{1i}$ and $y_{2i}$, respectively, the dependent variable *Self-reported Racial Bias in Policy Attitudes*, $y_i$, is defined as a dummy variable taking a value of 1 if $y_{1i} > y_{2i}$ and 0 otherwise. In the relevant sample, 32.70 per cent of individuals report stronger opposition to different-race immigration than same-race immigration, while 64.80 per cent report equal opposition to different-race immigrants. While such operationalization of the dependent variable has the advantage of simplicity, we discuss its limits and test the robustness of our results to using two alternatives in the Appendix. Reports of racist attitudes ($y_i = 1$) account for 31.49 per cent in the control group and 34.06 per cent in the treatment group.¹

Defining $Y_{i,c} = \ln([Pr(y_{i,c} = 1))/(1 − Pr(y_{i,c} = 1))]$, we use the following specification:

$$ Y_{i,c} = \alpha + \beta T_i + \gamma^I X_{i,c} + \mu_c + \epsilon_{i,c} $$

$T_i$ is the treatment variable. It takes a value of 1 if respondent $i$ was interviewed after 8 November 2016 (election day) and 0 otherwise. Even though asymmetric levels of respondents’ reachability as well as geographic imbalance may induce a non-random selection of respondents among the control and treatment groups (Legewie 2013; Munoz, Falco-Gimeno and Hernandez 2018), the timing of each interview is as good as random with respect to the timing of the US election. $T_i$ can therefore be interpreted as an exogenous signal of the decrease in the social desirability of reporting racially neutral attitudes. Accordingly, measures the effect of Donald Trump’s win on the propensity to report racist attitudes, $\alpha$, is a constant.

$X_{i,c}$ summarizes individual-level characteristics. In a first model, we only control for demographic characteristics including age, age squared, sex, household status (having at least one child living at home) and ethnic minority status (0 if majority, 1 if minority). We then add socio-economic characteristics: highest educational attainment (1–7), a dummy capturing economic insecurity (0 if the respondent experienced short-run unemployment during the previous year, and 1 otherwise) and household income (1 = living comfortably; 4 = living with strong difficulties). We subsequently add a dummy equal to 1 if the respondent voted in the latest general election to capture interest in politics. Note that we only select proper covariates – those that could not be affected by the treatment. To control for unobserved country heterogeneity, we include country fixed effects $\mu_c$. $\epsilon_{i,c}$ is an idiosyncratic error term, with:

$$ E[\epsilon|T, X, \mu] = 0. $$

Finally, we weight observations by the design weights provided by the ESS to control for the relative likelihood of each observation to be sampled.

Respondents in the treatment and control groups may differ in the distribution of key covariates. While the ESS is meant to be representative of each country’s population in the overall period, there is no guarantee that representativeness holds within particular sub-periods, for instance because of reachability issues. Following Hainmueller (2012), we therefore weight control units such that the distribution of covariates in the control group matches the moment conditions (until skewness) of the treatment group. After this pre-processing, covariate imbalance between control and treatment groups becomes negligible.

We fit the model with a binary logit estimator and report the average marginal effect. Since both the treatment and the output variables are dummies, the marginal effect is easy to interpret: it provides the difference in percentage points between the treated and control groups in the probability that a respondent will exhibit a racial bias. We base our main analysis on an interval of ±15

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¹Specifically, 2.49 per cent of respondents opposed same-race immigration more than different-race immigration, displaying ‘positive racism’. In an alternative specification, we allow the dependent variable to take the value $−1$ in the latter case. As the number of respondents reporting positive racism is extremely limited, the treatment effects are very close in the two cases.
days before and after the election. This bandwidth choice reflects a trade-off between statistical power, which is greater the larger the bandwidth, and attribution, which is more accurate the smaller the bandwidth. In the Appendix we discuss this rather arbitrary choice in greater detail and report treatment effects for alternative bandwidths.

The ideal dataset to study a ‘global contagion’ should include the race-related attitudes of each individual in each country both before and after the election. Instead, we had to run the analysis on a sample of thirteen countries. We therefore face sample uncertainty (Abadie et al. 2017). Moreover, race-related reports are observed either before or after the election. Consequently, we also face design uncertainty (Abadie et al. 2017). For these reasons, we cluster errors at the country level.

Results

Main Test

Table 1 shows that the hypothesis that Donald Trump’s win increased global self-reported racial bias in policy attitudes cannot be rejected. In Column 1, the treatment effect, computed as a simple mean difference, is equal to 3.2 percentage points and is statistically significant at p < 0.01. In Column 2, we control for country fixed effects only. Being interviewed after Donald Trump’s election increases the likelihood of reporting a racial bias by 1.7 percentage points, and the outcome is now significant at p < 0.1. Columns 3 and 4 add control variables pertaining, respectively, to demographic and socioeconomic characteristics. The treatment effect slightly exceeds 2 percentage points.

In Column 5, we include self-reported turnout in the latest national election. The treatment effect is nearly unaffected and stays significant at p < 0.01. A key comparison is the one between Columns 5 and 6. When the control units are weighted to match the covariates’ distribution of treated units, the treatment effect hardly increases. This suggests that sample imbalance is not severe. In the full specification of Column 6, which we use to address further identification issues, Donald Trump’s election increases the probability of reporting a racial bias by 2.3 percentage points, significant at p < 0.01.

The Appendix digs deeper into the temporal and spatial dimensions of the treatment effect. As the lengths of the fieldwork periods are limited and different among countries, we remain agnostic about medium-run effects. However, we show that the main result is robust to alternative bandwidths. We also compare each country’s specific treatment effect with the aggregate effect. We document that the treatment effect is significantly below average in Sweden, Finland and Estonia, suggesting the presence of a Scandinavian cluster of non-updaters. The treatment effect is significantly above average in Austria, Switzerland and the Netherlands, suggesting the presence of a continental cluster of stronger updaters, paralleled by Israel.

We also perform two robustness checks. First, we show that alternative modelling strategies as well as alternative operationalizations of the dependent variable yield qualitatively and quantitatively close treatment effects. Secondly, to deal with the sample selection issues that our pre-treatment matching strategy may not capture, we run the same analysis again after balancing for reachability and geographic imbalance. We obtain very similar treatment effects. These tests are reported in the Appendix.

Threats to Identification

Donald Trump vs. previous elections

Our strategy rests on the contention that Donald Trump’s election marked a change in the status quo toward lower social desirability of racial equality. To see what would have happened to race-related attitudes if the status quo had changed toward higher racial equality, we apply our
design to Barack Obama’s first election, which is arguably the closest to a perfect opposite of Donald Trump’s. Figure 1a shows that the effect of his first election was the opposite of Donald Trump’s: the report of racial bias in policy attitudes decreased significantly at $p < 0.05$.2

A second question regarding counterfactuals is: What would have happened if the status quo had not changed? George W. Bush’s 2004 and Barack Obama’s 2012 elections represent appealing counterfactuals, since the incumbents were confirmed, hence there was no change in the status quo. Figure 1a shows that the effect of the 2004 and 2012 elections, which granted second mandates, respectively, to George W. Bush and Barack Obama, is not statistically distinguishable from zero.

Racist vs. immigration attitudes
The two questions we combine to construct our dependent variable differ only in the racial background of immigrants. However, race-targeting policies may face opposition due to welfare concerns, rather than racism (Bobo and Kluegel 1993). Survey respondents may have simply used race as a proxy for specific labor market skills or the demand for public goods (Dustmann and Preston 2007). In that case, expressing greater opposition to different- vs. same-race immigration would be driven by welfare concerns, rather than racism. However, the results shown in Figure 1b show that the documented effect on self-reported racial bias is not driven by economic-related immigration concerns. In the Appendix, we show that policy attitudes less ostensibly related to Donald Trump’s campaign – including redistribution, environmental protection and gay rights – were not affected by the election.

Electoral vs. campaign effect
Schaffner (2017) shows that being exposed to Donald Trump’s campaign increased individuals’ willingness to express xenophobic opinions against minorities. Morrison et al. (2018) moreover show that assault frequency increased on days and in cities where candidate Donald Trump

2One may argue that the informational content of Donald Trump’s win was stronger than that of Barack Obama’s 2008 win. In the first case, the electoral outcome was unexpected given pre-election polls. In the latter case, the outcome was less unexpected: Barack Obama and John McCain had close approvals until October, but Obama gained an edge over McCain during the last month. This may make the election itself less informative. It cannot be denied, however, that the election of the first African-American president in the United States marked an important discontinuity in world politics from the perspective of a global audience.

Table 1. Effect of Donald Trump’s election on self-reported racial bias

<table>
<thead>
<tr>
<th>Racial bias (0–1)</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
</tr>
</thead>
<tbody>
<tr>
<td>Treatment (0–1)</td>
<td>0.031</td>
<td>0.022</td>
<td>0.017</td>
<td>0.020</td>
<td>0.021</td>
<td>0.023</td>
</tr>
<tr>
<td>s.e. (0.011)</td>
<td>(0.009)</td>
<td>(0.008)</td>
<td>(0.009)</td>
<td>(0.009)</td>
<td>(0.009)</td>
<td>(0.008)</td>
</tr>
<tr>
<td>Observations</td>
<td>7,904</td>
<td>7,904</td>
<td>7,855</td>
<td>7,717</td>
<td>7,717</td>
<td>7,717</td>
</tr>
<tr>
<td>Country effects</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Demographics</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Socioeconomics</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Voting</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Entropy balancing</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
</tbody>
</table>

Note: coefficients for treatment effect: average marginal effects following logit estimation. Standard errors clustered at the country level in each model. The analysis is based on 4,064 effective control and 3,653 effective treated units. Countries: Austria, Belgium, Switzerland, Germany, Estonia, Finland, UK, Israel, Norway, Sweden and Slovenia. Demographics: age (15–105), age squared, gender (0–1), household status (0–1), minority status (0–1), and domicile (1–4). Socioeconomics: education attainment (1–7), income status (1–4), and a dummy capturing whether the respondent experienced short-run unemployment during the last year (0–1). Voting takes a value of 1 if the respondent voted in the latest general election (0–1). Entropy balancing weights units so that the distribution of covariates of the control group matches the distribution of covariates of the treated group, until skewness. Design weights apply.

Source: ESS, round 8.
As the campaign was covered worldwide, his xenophobic rhetoric may have also changed individuals’ willingness to report racist attitudes abroad prior to the election. This would bias our treatment effect downward. However, Figure 1c establishes that moving the treatment one week, fifteen days, three weeks or thirty days before the actual election, keeping a symmetric interval of time around it, yields no significant treatment effect. Although Donald Trump’s rise in popularity during the campaign may have affected race-related attitudes, it does not threaten the validity of our estimates of the effect of his election per se.

**Electoral vs. bandwagon effect**

The election of Donald Trump may have affected a broad set of political attitudes due to a standard bandwagon effect, leading individuals to rally with the winning opinion (Fleitas 1971). The observed change in race-related attitudes may then simply reflect a wider alignment on the positions of Donald Trump or on the perceived new stance of the United States. Figure 1b, however, shows that some of the most archetypal political attitudes, including left–right placement and support for right-wing populist parties, remained constant, suggesting there was no generalized bandwagon effect.

**Conclusion**

Our analysis combines a methodological and substantive contribution. While the extant literature focuses on the effect of Donald Trump’s election on domestic social norms, we study its effect on social norms abroad and provide evidence consistent with a phenomenon of contagion. The study of norm diffusion in world politics is so far limited, as the field of international organization has focused on an institutional top-down channel, whereby a country’s local social norms change following institutional decisions inspired or imposed by a focal country (Acharya 2004; Klotz 1995). Our article suggests that another informational channel, based on citizens’ reactions...
to election results abroad, may also cause shifts in global norms. Which mechanisms underlie and moderate the contagion? How do the institutional and informational channels interact? These questions represent an interesting avenue for future research.

Supplementary material. Data replication sets are available in Harvard Dataverse at: https://doi.org/10.7910/DVN/YPYMH8 and online appendices at: https://doi.org/10.1017/S0007123419000449.

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