Global Racist Contagion Following Donald Trump's Election

Marco Giani   Pierre-Guillaume Méon

October 4, 2019

Supplementary Material

A Descriptive statistics

- Table 2 provides the complete descriptive statistics for Donald Trump's election. Table 3 provides descriptive statistics for previous elections.

B Theoretical background

Our theoretical framework builds in the concept of social desirability. It is well-known that surveys usually overestimate turnout in elections. Such phenomenon is often attributed to intentional misrepresentation by respondents who did not vote and would be embarrassed to admit it (Holbrook and Krosnik, 2009). Since voting is a "socially desirable" duty, some respondents who abstained prefer to lie in face to face interviews, to avoid incurring the "stigma" associated with abstention. The same happens *mutatis mutandis* for race-related attitudes. In this case, since racial-neutrality is socially desirable, racially-biased individuals may avoid to report their true opinion to an interviewer, causing the aggregate descriptive statistics to underestimate the actual level of racial bias in a society (see *e.g.* Kuklinski et al., 1997).

How can an electoral result change social norms? The theoretical mechanism that we favour builds on an adaptation of a framework of "pluralistic ignorance" (Katz and Allport, 1931) in the realm of self-reports. The theory builds on a specific micro-level mechanism whereby respondents will only state their true preference if they believe that the interviewer shares it with a large enough probability (Benabou and Tirole, 2011; Burstyn et al, 2018). More formally, on a specific issue - race-related attitudes in our case - individuals hold mutually exclusive opinions. In our case, the may have race neutral ($y_i = 0$) or racially biased ($y_i = 1$) preferences. Individuals prefer to report their true opinion than to lie. For simplicity, and defining $x_i \geq 0$ as the "intensity of preferences" they obtain a payoff of $1 \times x_i$ if they report the truth and a payoff of 0 if they lie. However, they prefer to lie if the "stigmatization cost" c associated with their racially biased opinion is "high enough". As such, at this stage, a racially biased individual would truthfully report if $c < x_i$. The more intense the preferences of the racially biased individual with respect to the stigmatization cost, the more likely she is to report.

As long as "race neutrality" is socially desirable, some actually racially-biased individuals will prefer to lie and report no racial bias even if they have one. This mechanism is known to result in an underestimation of aggregate reports of racial attitudes, as well as, for instance, an overestimation of aggregate self-reported turnout, for the same reason, *i.e.* voting is a socially desirable duty (Raghavarao et al., 1979; Sniderman, 1993).

While this cost might be negligible in our context if the survey was administrated online, it may be large enough in a face-to-face survey like the ESS where respondents face a stronger social desirability bias than in anonymous contexts (Seth, 2013). Moreover, the interviewer is unknown to the respondent. This is an important difference with respect to reports in the social media, where the social desirability bias is lower because of the ability to select peers (DiGrazia et al., 2013). Consequently, the respondent can only guess the position of the interviewer base on signals of the distribution of preferences like election results.

Now the "stigmatization cost" of reporting racial bias in policy attitudes depends on the number of individuals holding racially neutral v. biased attitudes. Suppose that 99% of the whole "reference community" was racially neutral. Then, the remaining racially biased 1% would be highly stigmatized. In other words, the stigmatization cost c is a function that increases in the share of the population ΔP holding it: $c(\Delta P)$ such that $c'(\Delta P) > 0$.

The other crucial aspect is that the share of the population holding a particular view is not perfectly assessed. Individuals do not know ΔP and form expectations $\mathrm{E}[\Delta P]$. Hence, their stigmatization cost is $c(\mathrm{E}[\Delta P])$. They may observe that their racially-biased opinion is minoritarian in the debate. However, "silent majorities" may sometimes exist. Hence, the "truthful report condition" becomes simply $c(\mathrm{E}[\Delta P]) < x_i$. As such, (i) the more intense the preferences of the racially biased individual, and (ii) the lower her expectation about the number of people holding racially-neutral attitudes, the more likely she is to report.

The win of Donald Trump revealed that the likely number of individuals holding racially-biased attitudes was actually larger than expected. By lowering the expectation on $\mathrm{E}[\Delta P]$, respondents perceive a decrease in the stigmatization cost associated with holding racially-biased attitudes $c(\mathrm{E}[\Delta P])$, making it more likely, *ceteris paribus*, that a racially biased individual report her racial-bias instead of lying.

According to our simple "truthful report condition", the win of Trump (Obama 2008) corresponds to an increase (decrease) in $\mathrm{E}[\Delta P]$ and hence, in a related manner, in $c(\mathrm{E}[\Delta P])$. Hence, $c(\mathrm{E}[\Delta P]_{\text{after}}) > c(\mathrm{E}[\Delta P]_{\text{before}})$ and, holding constant x_i, the condition $c(\mathrm{E}[\Delta P]_{\text{after}}) < x_i$ more likely to be met than the condition $c(\mathrm{E}[\Delta P]_{\text{before}}) < x_i$. Importantly, the election of Trump should make previously unwilling to report individuals willing to report. Hence, it should act on individuals with relatively low intensity of preferences x_i.

Summing up, actually racially biased individuals may become more likely to report their racial bias following Donald Trump's election, because the election of a world leader who openly held racially-biased positions revealed that a lower than previously believed fraction of the population strongly opposed racism, making the latter less stigmatized and more likely to be reported. This conceptualization offers a plausible interpretation of our outcomes. It implicitly relies on the idea that the election of Donald Trump did not make individuals "more racist" but rather "more willing to report their racism". We favor this interpretation, because we share the premise of Huang and Low (2017) and Burstin et al (2018) that the result of an election is more likely to provide information about others' preferences than to change one's preferences.

C Robustness Checks

- **Imbalance: Mean and Variance.** Table 1 provides the descriptive statistics for each covariate used in the main analysis. It provides information about the imbalance in covariates between treated and control units before and after applying entropy balancing. Imbalance is somewhat limited in the aggregate analysis, as one can already suspect by looking in Table 1 at the difference between the treatment effect with and without entropy balancing weighting. The latter became extremely small once entropy balancing was applied.

- **Imbalance: Skewness.** Table 1 allows observing means and variances, but it does not provide information about skewness, which is only relevant for continuous covariates. Figure 1 provides the kernel density for the four continuous covariates used in the analysis. Some degree of skewness persists after balancing on the "age" variable, whereas it essentially disappears for domicile, education and income. For all of these covariates, the kernel density of the treated and control groups are almost indistinguishable.

- **Alternative specifications.** Table 4 provides additional specifications. As suggested by Hainmueller and Xu (2013), entropy balancing is preceded by the extraction of outliers operationalized through pre-treatment coarsened exact matching. We first run an imbalance test on covariates. We then match control and treated units with coarsened exact matching on imbalanced covariates within each country. Units without match are treated as outliers and pruned before running the analysis. Details are provided in the table's footnote. In column (i), we provide the treatment effect after outliers' extraction and prior to entropy balancing. In column (ii), we apply entropy balancing after having extracted outliers. In both cases, the treatment effect is slightly larger than in column (vi) of Table **??**. Column (iii) displays the outcome of an alternative, more ambitious, balancing strategy, in which entropy weighting is constructed at the country level. The treatment effect gets slightly larger under this specification.

- With this by country balancing strategy, however, we are not always able to achieve balance at the third moment. Column (iv) and (v) replicate the analysis of, respectively, columns (ii) and (iii), but account for the fact that, since the number of clusters is relatively low (13 countries), standard clustering may underestimate standard errors. We re-estimate the main result using a wild cluster bootstrap. The level of significance only decreases from $p < .01$ to $p < .05$ for the country-level balancing specification.

- Finally, in column (vi), we provide the treatment effect for an augmented set of covariates. In the main text, we deliberately restrict the set of controls to proper covariates. The effect of the treatment might therefore be driven by a shift in ideology. To rule that possibility out, we control for three further attitudes: left-right placement, political interest, and satisfaction with democracy. The treatment effect after outliers' extraction, entropy balancing, and wild cluster bootstrapping is about 2 percentage points, significant at $p < .01$.

- **Alternative dependent variable.** Call y_{same} the answer to the "same race migrants" question and y_{diff} the answer to the "different race migrants" question. Recalling that higher values on the 1-4 scale mean higher opposition, we construct three different dependent variables.

  1. *Main* (in the text):
  $$y_i = \begin{cases} 1 & \text{if } y_{\text{diff}} > y_{\text{same}} \\ 0 & \text{else} \end{cases}$$
  This approach has the advantage of simplicity, because reported treatment effects can be interpreted directly. It has the disadvantage of not capturing increases in racial bias for already racially biased individuals thereby underestimating its actual increase.

2. *Alternative I*:

$$y_i = y_{\text{diff}} - y_{\text{same}}$$

This alternative has the advantage of capturing the intensity of the racial bias. However, an already racially biased individual (say $y_{\text{diff}} = 3, y_{\text{same}} = 1$) increasing his racial bias ($y_{\text{diff}} = 4$) would count the same as a racially unbiased becoming racially biased. An increase in racial bias may accordingly be driven by a polarization effect.

3. *Alternative II*:

$$y_i = \begin{cases} 1 & \text{if } y_{\text{diff}} > y_{\text{same}} \\ 0 & \text{if } y_{\text{diff}} = y_{\text{same}} \\ -1 & \text{else} \end{cases}$$

In this case, we account for the fact that some respondents may favor different race over same race immigrants. This group is extremely limited (2.78% of our sample), which is why we used a dummy rather than a categorical variable in our baseline specification.

We estimated the baseline specification with each alternative definition of the dependent variable in Table 5. The three approaches lead to very close empirical conclusions.

- **Sampling selection issues: Reachability.** Pre-treatment matching accounts for imbalance in covariates among respondents. An additional issue arises as sampled individuals may be more or less likely to complete the survey before or after the election. The election of an American president *per se* unlikely induces self-selection. However, the respondents who completed the survey at the first attempt made by the interviewer are more likely to be in the control group than those for whom several attempts had to be made prior to completing the survey (Muñoz, Falcó-Gimeno and Hernández, 2018). This is the issue of *reachability*: the more reachable a household, the more likely it is, *ceteris paribus*, to be interviewed before the election. Reachability would bias our results if it correlated with characteristics that we do not account for. The left panel of Figure 2 plots the fraction of households interviewed at the

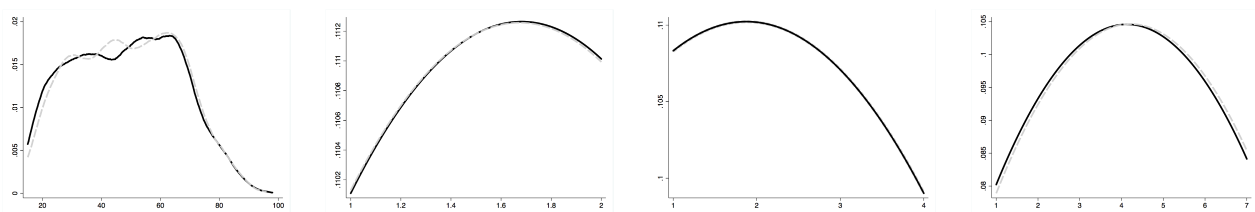
1st, 2nd, nth attempt in the relevant control and treatment groups. The right panel considers the whole sample. Figure 2 confirms that the fraction of "easily reachable" respondents is indeed slightly higher in the control group. The average number of attempts to get the survey administered is 2.586 in the control and 2.659 in the treatment group.

To check whether and to which extent reachability biases outcomes, we run again the main regression controlling for the "reachability" variable, given by the number of attempts needed for the interviewer to interview the sampled household. We do this in two ways: (i) by controlling for reachability in the main regression, and (ii) by dropping individuals with particularly low levels of reachability. Figure 2 shows that treatment effects according to (i) and (ii) yield outcomes that are extremely close to those presented in the main table.

- **Sampling selection issues: Geographic imbalance.** For most countries, ESS implements strict probability sampling. Multi-stage is only used for countries lacking reliable addresses of households (see ESS8 Sampling guidelines, page 6). As a result, there is a decent balance of before/after collection by region: for most regions there are data both before and after the election (Figure 3). As some imbalance persists, and regions may correlate with features that our matching strategy does not capture, we run two additional analyses. In the first, we add region fixed effects. In the second, we also use entropy weighting to make the control and treatment groups balanced on the fraction of surveys collected in each region. Both those treatment effects are extremely close to baseline estimates (see Figure 4).

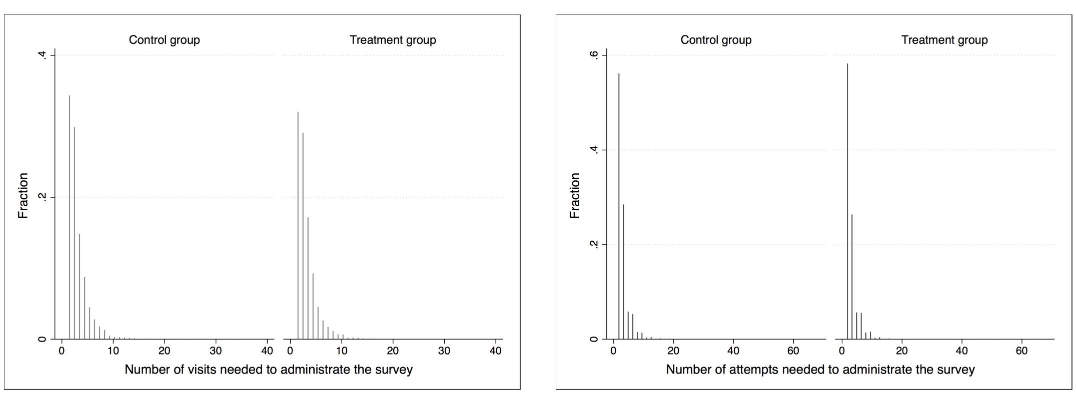
	Range	Treated		Control				Imbalance	
		(Unconditional)		(Unconditional)		(After balancing)		(Unconditional)	(After balancing)
		Mean	Variance	Mean	Variance	Mean	Variance	△ Mean	△ Mean
Age	15-120	48.91	313.9	49.48	337.8	48.91	313.9	-.57	0
Female	0-1	.52	.25	.52	.25	.52	.25	0	0
Domicile	0-3	1.72	1.62	1.86	1.59	1.72	1.62	-.14	0
Household status	0-1	.67	.22	.68	.22	.67	.22	0	0
Minority status	0-1	.06	.05	.05	.05	.06	.05	.01	0
Education attainment	1-7	4.22	2.89	4.28	2.96	4.22	2.89	-.06	0
Income	1-4	1.87	.64	1.88	.66	1.87	.64	.01	0
Unemployment	0-1	.73	.19	.73	.19	.73	.19	0	0
Voted at latest election	0-1	.72	.20	.72	.20	.72	.20	0	0

Table 1: **Imbalance: 1.** Descriptive statistics and imbalance: before and after entropy balancing.



(a) Age. (b) Domicile. (c) Income. (d) Educational attainment.

Figure 1: **Imbalance: 2.** Kernel density of continuous covariates among treatment and control groups after entropy balancing. The black regular line (grey dashed) plots treated (control) units.



(a) Relevant sample (± 15 days). (b) Entire sample.

Figure 2: Reachability in the control and treatment group.

Dependent Variables	N. obs	Mean	Std. Dev	Min	Max
Racial bias	7,904	.33	.47	0	1
Same race immigration	7,951	2.16	.87	1	4
Different race immigration	7,929	2.57	.91	1	4
Oppose refugees	7,923	3.39	1.78	1	5
Oppose poor migrants	7,906	2.62	.92	1	4
Immigration harms economy	7,848	4.94	2.45	0	10
Immigration harms culture	7,895	5.17	2.64	0	10
Left-right placement	7,415	5.21	2.18	0	10
Support Populist	3,710	.16	.37	0	1
Oppose Redistribution	7,950	2.36	1.07	1	5
Oppose Gay rights	7,861	2.78	1.35	1	5
Independent Variables	**N. obs**	**Mean**	**Std. Dev**	**Min**	**Max**
Age	8,035	48.36	18.42	15	105
Female	8,053	.52	.50	0	1
Children at home	8,053	.32	.47	0	1
Minority status	8,053	.96	.23	0	1
Domicile	8,047	2.78	1.26	1	5
Income	7,981	1.88	.81	1	4
Education	8,013	4.08	1.72	1	7
Unemployed	8,033	.26	.44	0	1
Voting	8,023	.69	.46	0	1

Table 2: Descriptive statistics, Donald Trump (2016).

Dependent Variables	N. obs	Mean	Std. Dev	Min	Max
Racial bias (Bush, 2004)	11,269	.25	.43	0	1
Racial bias (Obama, 2008)	7,402	.27	.44	0	1
Racial bias (Obama, 2012)	8,208	.31	.46	0	1
Independent Variables (Bush, 2004)	**N. obs**	**Mean**	**Std. Dev**	**Min**	**Max**
Age	11,637	45.86	18.59	15	99
Female	11,673	.52	.50	0	1
Children at home	11,672	.41	.49	0	1
Minority status	11,676	.96	.20	0	1
Domicile	11,683	3.01	1.17	1	5
Income	11,533	1.97	.85	1	4
Education	11,661	3.32	1.72	1	5
Unemployed	11,633	.25	.43	0	1
Voting	11,600	.70	.46	0	1
Independent Variables (Obama, 2008)	**N. obs**	**Mean**	**Std. Dev**	**Min**	**Max**
Age	7,617	47.35	18.57	15	123
Female	7,629	.52	.50	0	1
Children at home	7,586	.37	.48	0	1
Minority status	7,605	.95	.21	0	1
Domicile	7,615	2.90	1.23	1	5
Income	7,617	1.84	.80	1	4
Education	7,563	3.13	1.37	1	5
Unemployed	7,586	.25	.43	0	1
Voting	7,589	.74	.44	0	1
Independent Variables (Obama, 2012)	**N. obs**	**Mean**	**Std. Dev**	**Min**	**Max**
Age	8,506	48.36	18.42	15	96
Female	8,519	.53	.50	0	1
Children at home	8,516	.39	.49	0	1
Minority status	8,461	.93	.26	0	1
Domicile	8,510	2.79	1.24	1	5
Income	8,466	1.73	.87	1	4
Education	8,399	4.11	1.81	1	7
Unemployed	8,449	.26	.44	0	1
Voting	8,464	.771	.45	0	1

Table 3: Descriptive statistics, other elections.

	Racial bias (0-1)					
	(i)	(ii)	(iii)	(iv)	(iv)	(v)
Treatment (0-1)	.025***	.026***	.024**	.025***	.025**	.020***
SE	(.008)	(.008)	(.010)	*NA*	*NA*	*NA*
N.obs	7,335	7,335	7,335	7,335	7,335	6,694
Country Effects	✓	✓	✓	✓	✓	✓
Demographics	✓	✓	✓	✓	✓	✓
Socioeconomics	✓	✓	✓	✓	✓	✓
Voting	✓	✓	✓	✓	✓	✓
Outliers' extraction (CEM)	✓	✓	✓	✓	✓	✓
Entropy balancing (pooled)		✓		✓		✓
Entropy balancing (by country)			✓		✓	
Wild Cluster bootstrapping				✓	✓	✓
Further political attitudes						✓

*: significant at .1, **: significant at .05, ***: significant at .01. Coefficients: average marginal effects following a logit estimation. Errors clustered at country level. Countries: Austria, Belgium, Switzerland, Czech Republic, Germany, Estonia,Finland, UK, Israel, the Netherlands, Norway, Sweden, and Slovenia. Demographics: age, age squared, gender, household status, minority status, and domicile. Socioeconomics: education attainment, income, and recent short-run unemployment. Entropy balancing designed to satisfy moment conditions until skewness. Outliers' extraction following coarsened exact matching on imbalanced covariates (Age and domicile). Age is coarsened through intervals of 5 years while domicile is coarsened according to exact matching algorithm. Matching prunes 422 units. Wild cluster bootstrapping is run through OLS after 1000 successful resamples. Further political attitudes include: left-right placement (0-10), political interest (1-4) and satisfaction with democracy (0-10). Design weights apply. Source: ESS, round 8.

Table 4: **Alternative specifications.** Effect of Donald Trump's election on self-reported racial bias, further specifications.

	Alternative I	*Alternative II*	MAIN
	(1-7)	(-1, 0 ,1)	(0,1)
Treatment (0-1)	.123***	.124***	.119***
SE	(.043)	(.037)	(.040)
N. Obs	7,717	7,717	7,717
Country Effects	✓	✓	✓
Demographics	✓	✓	✓
Socioeconomics	✓	✓	✓
Voting	✓	✓	✓
Entropy balancing	✓	✓	✓

*: significant at .1, **: significant at .05, ***: significant at .01. Coefficients: Ordered Logit coefficient for Alternative I and Logit coefficient for Alternative II and MAIN. Errors clustered at country level. Countries: Austria, Belgium, Switzerland, Germany, Estonia, Finland, UK, Israel, Norway, Sweden, and Slovenia. Demographics: age, age squared, gender, household status, minority status, and domicile. Socioeconomics: education attainment, income , and recent short-run unemployment. Entropy balancing to satisfy moment conditions until skewness. Design weights apply. Source: ESS, round 8.

Table 5: Effect of Donald Trump's election on alternative dependent variables.

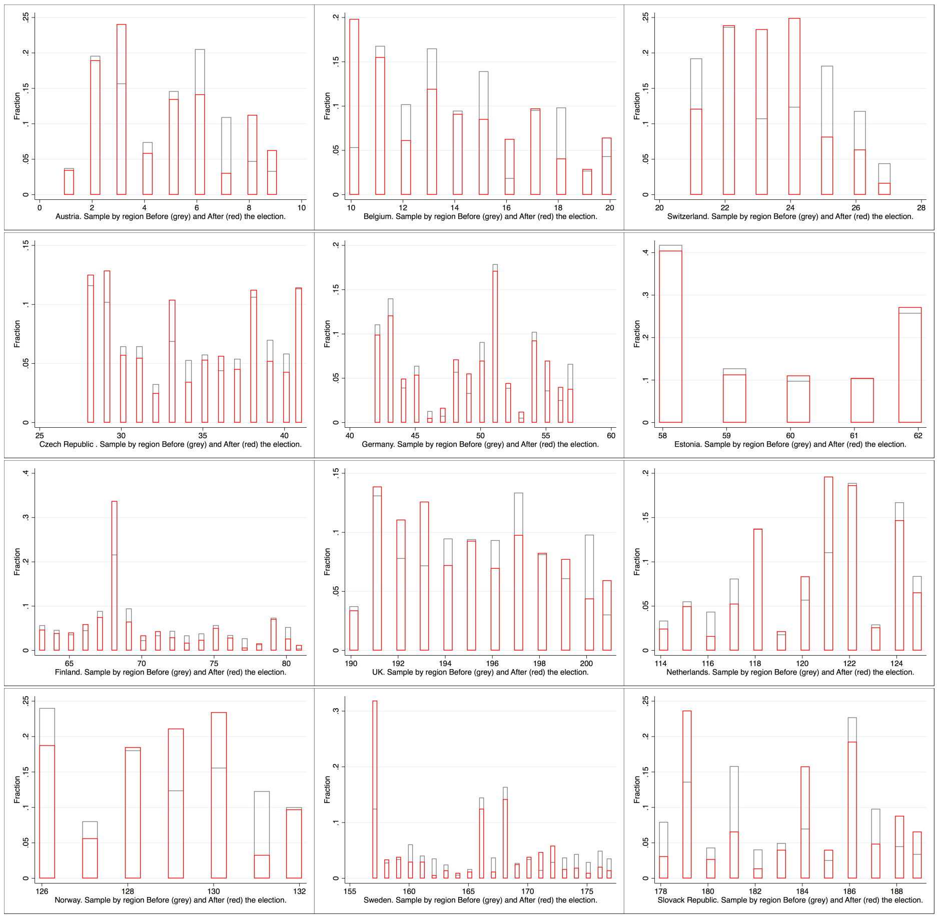
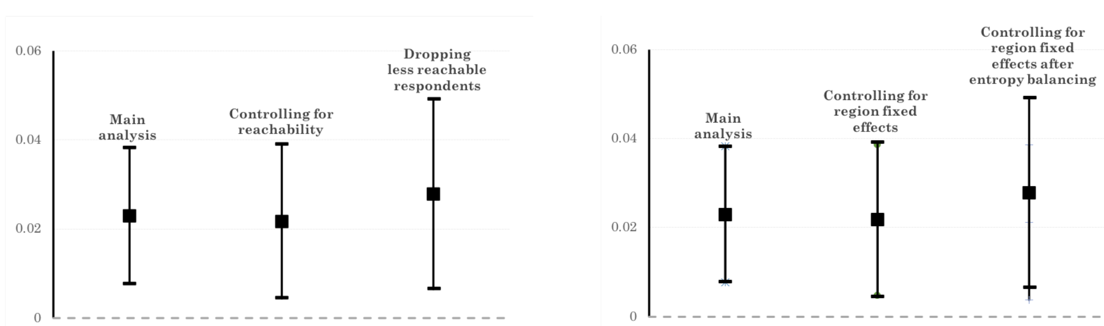


Figure 3: Survey collection by region before and after the election. No information available for Israel.



(a) Reachability.

(b) Geographic imbalance.

Figure 4: Treatment effects accounting for further sampling issues.

- **Trump v. previous elections.** Table 6 shows that, apart from Barack Obama's 2008 election, none of the previous elections, studied in exactly the same way, caused any change in self-reported racial bias. The first three columns report the coefficient plotted in Figure 1. In the last three columns, we show that the treatment effect of Donald Trump's election is not only positive and significant, but also significantly larger than the ones from previous elections. We define $D_i \in \{0,1\}$ as a dummy variable taking the value 1 if unit i was interviewed in 2016 and 0 if she was interviewed in a previous round (2004, 2008 or 2012). Following our main specification, we can write the empirical model as

$$Y_{i,c,p} = \alpha + \beta T_{i,p} + \delta_0 D_i + \delta_1 D_i \times T_{i,p} + \gamma' X_{i,c,p} + \mu_{c,p} + \epsilon_{i,c,p}.$$

where we control for country-year fixed effects, $\mu_{c,p}$, and cluster errors at the country-year level. We report the main coefficient of interest, δ_1, which is a difference in difference estimate. As an alternative, we can also test whether the treatment effect following Donald Trump's election according to the main specification is significantly larger than the one following previous elections with a $z - test$. Comparing the treatment effect following Donald Trump's election with that of George W. Bush 2004, and those of Obama 2008 and 2012, we obtain respectively $z = -3.04$, $z = -3.56$, and $z = -2.44$. Hence the treatment effect of Donald Trump's election is significantly larger than the one in previous elections in a one-sided test at $p < .01$.[1]

- **Electoral v. Campaign effect.** Table 7 reports the treatment effects that would be obtained applying the same method as in the baseline estimations around placebo election dates. We moved the treatment by intervals of 5 days until 30 days before the actual election day and kept symmetric intervals to

[1] As an additional test, we test whether this outcome holds true when restricting the sample to countries common to both elections. This exercise cuts substantially the sample, as the set of countries that happened to be fielded during U.S. elections differs from round to round. Nevertheless, the outcome remains significant. Comparing regression coefficients on common countries only yields $z = -2.11$, $z = -2.45$, and $z = -1.66$, when comparing the treatment effect following Donald Trump's election with George W. Bush 2004, and Barack Obama 2008 and 2012. Hence, the comparison is significant at $p < .05$ in the first case, $p < .01$ in the second case, and $p < .1$ in the last case.

avoid the inclusion of actually treated units. None of the placebo treatment-dates before the real election yielded any change in self-reported racial bias.

- **Racist v. Immigration attitudes.** Table 9 reports estimates of the effect of Donald Trump's election on four other survey items related to immigration. Those items read:
  - Question C: The government should be generous in judging people's applications for refugee status. (1: Agree strongly, ... , 5: Disagree strongly);
  - Question B40: Allow many/few immigrants from poorer countries outside Europe. (1: Allow many, ... , 4: Allow none);
  - Question B41: Would you say it is generally bad or good for [country]'s economy that people come to live here from other countries? (1: Bad, ... , 10: Good);
  - Question B42: Would you say that [country]'s cultural life is generally undermined or enriched by people coming to live here from other countries? (1: Undermined, ... , 10: Enriched).

  Consistent with our main result, we find no effect of Donald Trump's election on welfare-related immigration attitudes, while we find that the only significant treatment effect is on cultural concerns for immigration (at $p < .1$)

- **Electoral v. Bandwagon effect.** Table 9 provides the outcome relative to the effect of Donald Trump's election on four other survey items related to ideology. Those items read:
  - Question B26: Placement on a left-right scale. (1: Left, ... , 10: Right);
  - Question B24: Which party do you feel close to. (0: Any or none, ... , 1: a right-wing populist party);
  - Question B23: Government should reduce differences in income levels. (1: Agree strongly, ... , 5: Disagree strongly);
  - Question B36: Gay and lesbian couples should have the same rights to adopt children as straight couples. (1: Agree strongly, ... , 5: Disagree strongly).

We find that the treatment effect is null for each of these cases. We can therefore interpret our main finding as signaling that Donald Trump's election had no general effect on the opinions of respondents. It therefore specifically increased the willingness to report opinions that discriminate migrants of a different race.

- **Attrition.** The number of non-respondents on the main dependent variable is low and well balanced between the treatment and control groups. Specifically, we have only 2.04% non-responses in the control group and 2.09% in the treatment group. Consistently, creating a dummy variable equal to 1 if the response to our racial-bias proxy is missing and 0 else, and running the main regression, we observe no effect of Donald Trump's election on the likelihood of observing missing data. In addition to that, we run a regression in which the same set of covariates used in the main specification are used to predict missingness of data on the main dependent variable. We run two regression: one to predict missingness before the election of Donald Trump, and one after. As such, we can check whether each dependent variable's predictive power was altered by the election. We show in Table 8 that in no single case do the election of Trump significantly change the predictive power of covariates.

- Following Legewie (2013), we randomly re-assign the binary treatment variable within countries. Internal validity is strengthened if the number of cases in which the random treatment effect exceeds the actual one is limited. This test helps ruling out a decisive role of sampling issues in driving the documented treatment effect. Testing for "permuted treatment effects" on the main dependent variable reveals that the number of cases in which the random treatment effect exceeds the actual one is only 3.2% after $1,000$ Montecarlo simulations.

D Further Analysis

- **Alternative bandwidths.** Following Depetris-Chauvín and Durante (2017), Giani (2017) and Mikulasche, Pant and Tesfaye (2017), we base our main analysis on an interval of ± 15 days before and after the election. On the one hand, the chosen interval balances out two necessities. ESS questionnaires feature a large number of questions, with no collection during weekends. The rate of data collection per country is therefore relatively small. This requires selecting a sufficiently wide time interval. On the other hand, as race-related attitudes may vary according to several channels and further events, the observed treatment effects can be credibly attributed to the election outcome only if intervals are sufficiently close to the election day.

- To make sure that our results are not driven by the choice of the time interval, Table 10 provides the treatment effects over alternative intervals. The magnitude of the effect remains similar throughout the first month. If we consider an interval of ± 45 days, the treatment effect is still positive and significant at $p < .1$, and stays so exactly until Christmas day, while it becomes null when using larger time intervals, *e.g.* a time interval of ± 60 days.

  We deliberately remain agnostic in what regards medium-term treatment effects for three reasons. Firstly, we can only credibly attribute observed changes to transnational electoral spillovers by focusing on the very short run, especially in light of the additional analysis presented in subsection 2.3. Conversely, longer intervals of time make confounding factor possibly decisive, and hence attribution problematic. Secondly, our objective is to disentangle an "electoral effect" from other effects that may concur to the evolution of race-related attitudes, including learning. Thirdly, as Figure 6 details, if we use an interval of ± 15 days, each country's fieldwork period is active. Conversely, using an interval of e.g. ± 45 days, Austria and the Czech Republics' fieldworks are closed. Hence, the increase in sample size from the ± 30 days interval to the ± 45 days interval is driven by a set of countries different from the one considered in the main analysis, thereby introducing a confounding factor.

  For these reasons, the observed difference in the treatment effect over different study windows is consistent with different interpretations. It may be that the observed drop in the treatment effect when the bandwidth includes the

Christmas holidays is due to Christmas itself. In line with this interpretation, we observe the same phenomenon in 2004 and 2012 (there are no data for 2008). In turn, this "Christmas effect" could be due both to the different likelihood of different households to be willing to complete surveys during Christmas holidays, or to a mitigating effect of religious values on racial bias. It may also be that other events for which we are not accounting, possibly unrelated to American Politics, imposed further changes to race-related attitudes. Finally, it may also be due to the fact that contagion is a rather short-lived phenomenon, meaning that as the salience of the U.S. presidential election decreased, race-related attitudes returned to their original levels.

- **Analysis by country.** Figure 6 provides the timing of the interviews for each of the countries used in our analysis. The 2016 election of Donald Trump fell inside the survey period range of 14 of them. Iceland, however, has only 5 respondents before the election and was therefore discarded. Table 11 interacts the main dependent variable with each country dummy at a time. It shows that our main result is always significant at least at $p < .05$, and hence is not driven by outliers. Austria, Israel, the Netherlands, and Norway have a significantly higher treatment effect than the average, whereas in Estonia, Finland, Sweden, and Slovenia the outcome is significantly lower than the average. It would be interesting to run a comparative analysis, and identify the country-level variables that shape country-level treatment effects. However, the number of control and treated units per country is small when focusing on the relevant interval of time, making this analysis difficult.

- **Online search.** Figure 7 compares trends of Google searches in our sample of countries and in the U.S. for both "Trump" (Figure 7a) and "Racism" (Figure 7b). In both cases and for both geographic units, searches are the highest on November 9, the after-election day, confirming that Donald Trump's election was connected with racism. Searches of Trump are more concentrated on November 9, 2016, for our sample than for the U.S., while the opposite is true for "racism" (and its translation in each country's language).

	Racial bias (0-1)					
	Bush 2004	Obama 2008	Obama 2012	Trump-Bush	Trump-Obama	Trump-Obama
Treatment (0-1)	-.010	-.033**	-.017	.032***	.055***	.034**
SE	(.008)	(.013)	(.014)	(.011)	(.015)	(.015)
N.obs	10,773	7,146	7,894	18,108	14,908	15,661
Country Effects	✓	✓	✓	✓	✓	✓
Demographics	✓	✓	✓	✓	✓	✓
Socioeconomics	✓	✓	✓	✓	✓	✓
Voting	✓	✓	✓	✓	✓	✓
Entropy balancing	✓	✓	✓	✓	✓	✓

*: significant at .1, **: significant at .05, ***: significant at .01. Coefficients: average marginal effects following a logit estimation. Errors clustered at the country level and country-year for the difference in difference analysis. Demographics: age, age squared, gender, minority status, household status, and domicile. Socioeconomics: education attainment, income, and recent short-run unemployment. Education attainment ranges from 1 to 5 for Bush 2004 and Obama 2008. Entropy balancing is defined to satisfy moment conditions until skewness, separately for each round of the survey. Design weights apply. Bush 2004: Belgium, Switzerland, Czech Republic, Germany, Denmark, Estonia, Spain, Finland, UK, Luxembourg, the Netherlands, Norway, Poland, Portugal, Slovenia, Sweden, and Slovakia. Obama 2008: Switzerland, Cyprus, Germany, Denmark, Spain, Finland, France, UK, Israel, the Netherlands, Norway, Portugal, Slovenia, and Sweden. Obama 2012: Austria, Belgium, Switzerland, Cyprus, Germany, Estonia, Finland, UK, Ireland, Israel, Island, the Netherlands, Norway, Poland, Portugal, Russia, Sweden , Slovenia, and Slovakia. Source: ESS, rounds 2 - 4 - 6 -8.

Table 6: **Trump v. previous elections.** Effect of *past elections* on self-reported racial bias.

	Racial bias (0-1)			
Fake treatment date	November 1	October 24	October 18	October 9
Treatment (0-1)	.005	-.004	.002	-.001
SE	(.013)	(.017)	(.009)	(.008)
N. Obs	3,773	6,512	8,626	11,961
Country Effects	✓	✓	✓	✓
Demographics	✓	✓	✓	✓
Socioeconomic	✓	✓	✓	✓
Voting	✓	✓	✓	✓
Entropy balancing	✓	✓	✓	✓

*: significant at .1, **: significant at .05, ***: significant at .01. Coefficients: average marginal effects following Logit estimation. Countries: Austria, Belgium, Switzerland, Germany, Estonia, Finland, UK, Israel, Norway, Sweden, and Slovenia. Demographics: age, age squared, gender, household status, minority status, and domicile. Socioeconomics: education attainment, income, and recent short-run unemployment. Entropy balancing designed to satisfy moment conditions until skewness. Design weights apply. Source: ESS, round 8.

Table 7: **Electoral v. Campaign effect.** Effect of *fake treatments* election on self-reported racial bias.

	Missing response		Prob > chi2
	Before	*After*	
Age	.028	-.007	.39
SE	(.025)	(.026)	
Sex	-.043	.348	.13
SE	(.134)	(.162)	
Domicile	-.062	-.083	.85
SE	(.073)	(.050)	
Household status	.043	-.069	.60
SE	(.136)	(.140)	
Minority background	.324	-.224	.31
SE	(.411)	(.320)	
Education	-.008	.020	.65
SE	(.047)	(.039)	
Income	.298	.233	.67
SE	(.066)	(.116)	
Employment status	.020	-.059	.54
SE	(.097)	(.129)	
Voting	-.273	-.434	.35
SE	(.182)	(.230)	
N. Obs	13928	10385	
Country Effects	✓	✓	
Entropy balancing	✓	✓	

Table 8: Missing data analysis.

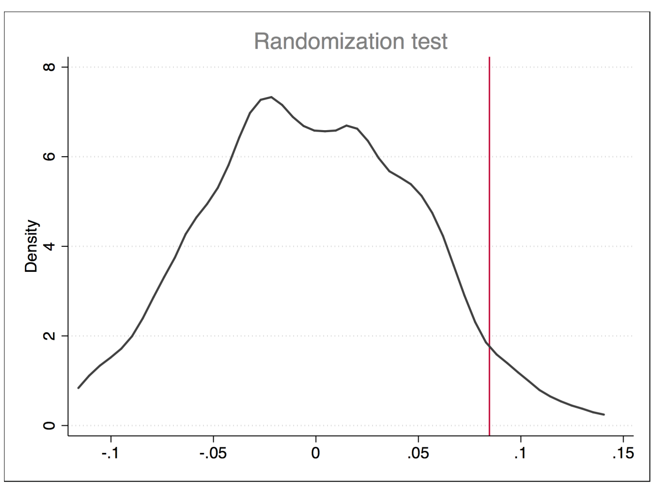


Figure 5: Kernel density of the permuted treatment effects.

	Racist v. Immigration attitudes			
	Oppose refugees (1-4)	Oppose poor migrants (1-4)	Immigration harms economy (0-10)	Immigration harms culture (0-10)
Treatment (0-1)	-.047	-.014	.042	.049
SE	(.058)	(.048)	(.042)	(.040)
N. Obs	7,733	7,717	7,661	7,712
	Electoral v. Bandwagon effect			
	Left-right placement (1-10)	Support Populist (0-1)	Oppose Redistribution (1-5)	Oppose gay rights (1-5)
Treatment (0-1)	.014	-.133	-.003	-.047
SE	(.053)	(.107)	(.043)	(.055)
N. Obs	7,258	3,632	7,753	7,675
Country Effects	✓	✓	✓	✓
Demographics	✓	✓	✓	✓
Socioeconomic	✓	✓	✓	✓
Voting	✓	✓	✓	✓

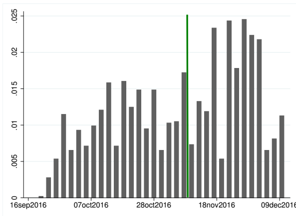
*: significant at .1, **: significant at .05, ***: significant at .01. Coefficients: outputs from ordered logit regressions. Errors clustered at the country level. Countries: Austria, Belgium, Switzerland, Germany, Estonia, Finland, UK, Israel, Norway, Sweden, and Slovenia. Demographics: age, age squared, gender, household status, minority status, and domicile. Socioeconomics: education attainment, income , and recent short-run unemployment. Entropy balancing to satisfy moment conditions until skewness. Design weights apply. Source: ESS, round 8.

Table 9: **Racist v. Immigration attitudes and Electoral v. Bandwagon effect**. Effect of Donald Trump's Election on ideology and further immigration-related attitudes.

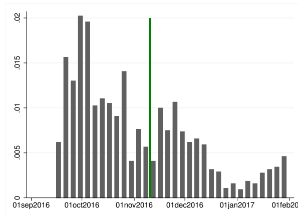
	Racial bias (0-1)						
	7 days	15 days	21 days	30 days	45 days	60 days	All
Treatment (0-1)	.019**	.023***	.014***	.013*	.010*	.001	-.011
SE	(.009)	(.008)	(.006)	(.007)	(.006)	(.006)	(.010)
N. Obs	3,879	7,717	10,166	13,917	18,278	21,161	23,757
Country Effects	✓	✓	✓	✓	✓	✓	✓
Demographics	✓	✓	✓	✓	✓	✓	✓
Socioeconomic	✓	✓	✓	✓	✓	✓	✓
Voting	✓	✓	✓	✓	✓	✓	✓
Entropy balancing	✓	✓	✓	✓	✓	✓	✓

*: significant at .1, **: significant at .05, ***: significant at .01. Coefficients: average marginal effects following a logit estimation. Errors clustered at the country level. Countries: Austria, Belgium, Switzerland, Germany, Estonia, Finland, UK, Israel, Norway, Sweden, and Slovenia. Demographics: age, age squared, gender, household status, minority status, and domicile. Socioeconomics: education attainment, income , and recent short-run unemployment. Entropy balancing satisfies moment conditions until skewness. Design weights apply. Source: ESS, round 8.

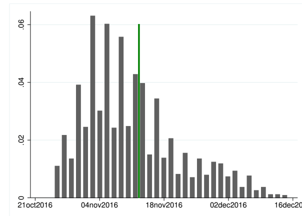
Table 10: Effect of Donald Trump's election on self-reported racial bias, by time interval.



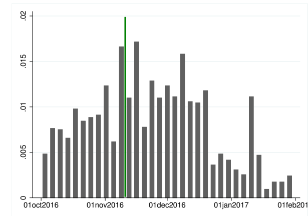
(a) Austria



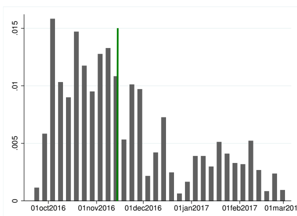
(b) Belgium



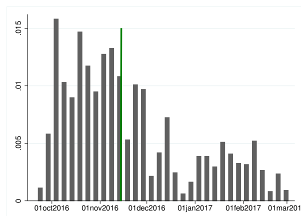
(c) Czech Republic



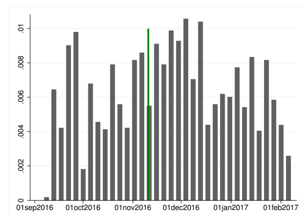
(d) Estonia



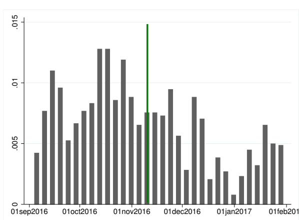
(e) Finland



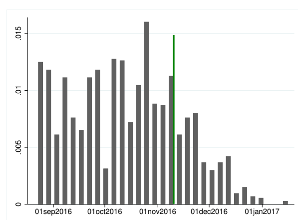
(f) Germany



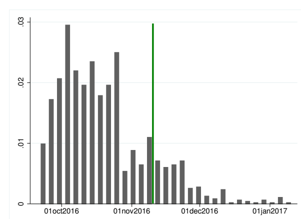
(g) Israel



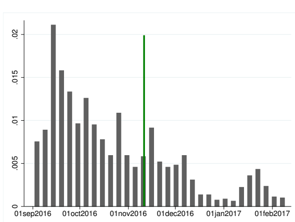
(h) Netherlands



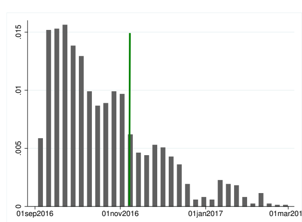
(i) Norway



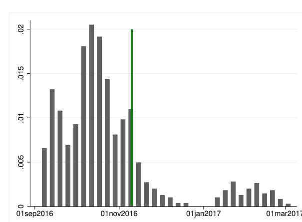
(j) Slovenia



(k) Sweden

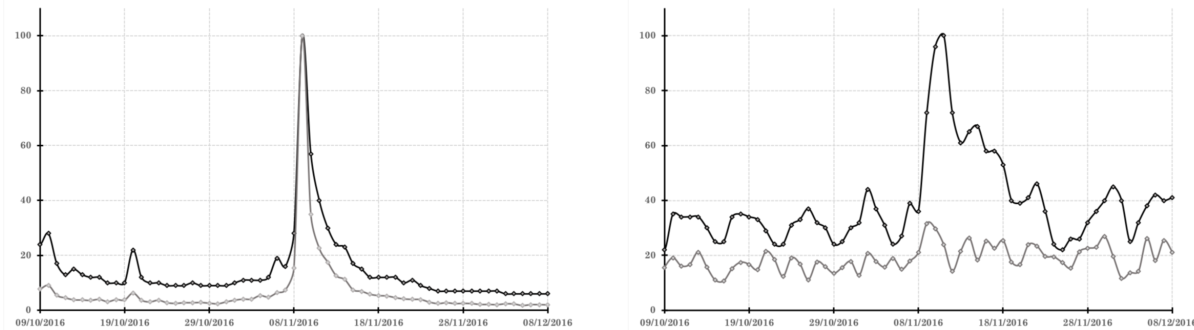


(l) Switzerland



(m) UK

Figure 6: Data distribution by country. The green line represents the date of the US Presidential election (November 8, 2016).



(a) Trump. (b) Racism.

Figure 7: Google trends on "Trump" and "racism" one months before and after the election. Black line: U.S, grey line: our sample. Units in the $y-$axis use information on search traffic on Google browser to compute means relative to an arbitrary initial value with respect to which each data point is scaled. For our sample, we first collect data for each country and then average them out. For "racism", we also collect the country translation (*e.g.* for Germany, we separately collected "racism" and "rassismus", and averaged them out).

	Racial bias (0-1)						
	AT	BE	CH	CZ	DE	EE	FI
Treatment	.020**	.023**	.023**	.020*	.023**	.026***	.028***
SE	(.008)	(.008)	(.008)	(.009)	(.008)	(.007)	(.006)
Treatment × Country	.025***	.004	-.004	.014	.008	-.034***	-.056***
SE	(.007)	(.009)	(.008)	(.009)	(.009)	(.008)	(.007)
	UK	IL	NL	NO	SE	SI	
Treatment	.024***	.021**	.023**	.021***	.025***	.024***	
SE	(.008)	(.008)	(.008)	(.008)	(.007)	(.008)	
Treatment × Country	-.001	.030***	.007	.044***	-.051***	-.028***	
SE	(.008)	(.009)	(.009)	(.008)	(.007)	(.008)	

*: significant at .1, **: significant at .05, ***: significant at .01. Coefficients: average marginal effects following a logit estimation. Errors clustered at the country level. Entropy balancing to satisfy moment conditions until skewness. Design weights apply. Source: ESS, round 8.

Table 11: Effect of Donald Trump's election on self-reported racial bias, interacting with each country.