CEB Working Paper

Centre Emile Bernheim
Research Institute in Management Sciences

Solvay Brussels School
Economics & Management

# Global Racist Contagion Following Donald Trump's Election

**Marco Giani and Pierre-Guillaume Méon**

Exploiting the coincidence of the U.S. Presidential Elections with the fieldwork period of the European Social Survey, we show that Donald Trump's win significantly increased self-reported racial bias in policy attitudes outside the U.S. We show that the opposite occurred following Barack Obama's first election in 2008, while no significant effect occurred when he and George W. Bush were reelected in 2012 and 2004, respectively. We show that the increase in self-reported racial bias is not driven by welfare-related immigration concerns, campaign effects, or bandwagon effects, suggesting a decrease in the social desirability of racial equality.

Keywords: Cross-Sectional Models, Spatial Models, Treatment Effect Models, Quantile Regressions, Political Processes: Rent-Seeking, Lobbying, Elections, Legislatures, and Voting Behavior, Political Economy, Cultural Economics, Economic Sociology, Economic Anthropology

JEL Classifications: C21, D72, P16, Z1

CEB Working Paper N° 17/034
May 2018

*Previous version available on: http://www.solvay.edu/latest-updates-ceb*

*Université Libre de Bruxelles - Solvay Brussels School of Economics and Management*
*Centre Emile Bernheim*
*ULB CP114/03 50, avenue F.D. Roosevelt 1050 Brussels BELGIUM*
*e-mail: ceb@admin.ulb.ac.be Tel.: +32 (0)2/650.48.64 Fax: +32 (0)2/650.41.88*

ULB

# Global Racist Contagion Following Donald Trump's Election

Marco Giani* Pierre-Guillaume Méon†

May 11, 2018

**Abstract**

Exploiting the coincidence of the U.S. Presidential Elections with the fieldwork period of the European Social Survey, we show that Donald Trump's win significantly increased self-reported racial bias in policy attitudes outside the U.S. We show that the opposite occurred following Barack Obama's first election in 2008, while no significant effect occurred when he and George W. Bush were re-elected in 2012 and 2004, respectively. We show that the increase in self-reported racial bias is not driven by welfare-related immigration concerns, campaign effects, or bandwagon effects, suggesting a decrease in the social desirability of racial equality.

*London School of Economics and Political Science, Department of Government. Email: m.giani@lse.ac.uk

†Université libre de Bruxelles (ULB). Centre Emile Bernheim, Solvay Brussels School of Economics and Management. Email: pgmeon@ulb.ac.be

# 1 Introduction

U.S. Presidential Elections are global events that result in global consequences. The election of a candidate who proposed the construction of a wall separating the U.S. and Mexico and an immigration ban on Muslims is no exception. Concerns that Donald Trump's win might have shifted norms of behavior and legitimized racist attitudes abroad were voiced in media across the globe (see *e.g.* 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). Were those concerns founded? To answer that question, we test whether the election of Donald Trump increased racial bias in policy attitudes outside the U.S.

Identifying such an effect is challenging. Collecting detailed surveys about race-related attitudes now that the election has occurred would leave us without a counterfactual. Conversely, relying on aggregate polls would leave us without individual-level information. Finally, comparing two rounds of the same survey would not allow us to single out short-run changes. To overcome those challenges, we exploit the coincidence of the 2016 U.S. Presidential election with the fieldwork period of 13 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 is random with respect to the day of the election. Using a quasi-experimental approach, we test the effect of Donald Trump's election on self-reported racial bias in policy attitudes by comparing respondents interviewed after the election (the treatment group) and those interviewed before the election (the control group).

We find evidence in favor of the contagion hypothesis: Self-reported racial bias increased significantly by $2.3\%$ within an interval of $\pm 15$ days around the election of Donald Trump. The treatment effect is robust

to several econometric specifications, including different sets of controls, time intervals, clustering, and covariate-balancing strategies. As the $ESS$ has typically been run from September to January every even year since 2002, we can replicate the main analysis for previous U.S. 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 a first mandate. Conversely, elections that did not change the status quo, as when George W. Bush and Barack Obama received a second mandate in 2004 and 2012, had no significant effect on self-reported attitudes displaying racial bias.

We interpret our findings as changes in the propensity to truthfully report racially biased attitudes, rather than changes in preferences, and provide further analysis consistent with that interpretation. Firstly, we show that race-related attitudes were not significantly increasing prior to the election, suggesting that pre-electoral campaign effects cannot fully account for the documented result. Secondly, we show that further policy attitudes that characterized the platform of Donald Trump but do not relate to racial equality did not significantly change either, suggesting that post-electoral bandwagon effects cannot fully account for the documented result. Finally, we document that the effect of Donald Trump's election on racial bias in policy attitudes is not driven by welfare-related immigration concerns, but by race-related ones.

An emerging literature studies the effect of Donald Trump's win on changes in social norms and, relatedly, on the propensity to report particular attitudes. In a recent experiment run by Bursztyn, Egorov, and Fiorin (2017), participants were offered a bonus reward if they authorized researchers to donate to an anti-immigration organization. During the two weeks before the election, participants who expected their decision to be observed by a surveyor donated less than those whose decision was private, suggesting a social stigma associated with such donations. Interestingly, this difference vanished in the week following the election

of Donald Trump. In another experiment, Crandall, Miller, and White (2018) find that the acceptance of prejudice towards groups targeted by Donald Trump, such as Muslims, were larger after the election than before, while they had remained unchanged for stigmatized groups he had not targeted, such as the alcoholic. Huang and Low (2017) study the effect of Donald Trump's election on sexism. They ran a series of lab experiments where participants were asked to play a "battle of the sexes" game and found that male subjects became more aggressive toward female subjects after the election. Huang and Low (2017) interpret their finding as suggesting that the election of Donald Trump may have affected norms of civility and chivalry.

We contribute to this literature in two distinct ways. Firstly, while Crandall, Miller, and White (2018), Bursztyn, Egorov, and Fiorin (2017), and Huang and Low (2017) rely on experiments to analyze the effect of Donald Trump's win on the willingness to express xenophobic and sexist attitudes, we mimic a natural experiment design based on observational data. Secondly, while those works focus on the impact of Donald Trump's election on attitudes and norms of behavior in the U.S., we are the first to provide an exploratory analysis of the transnational contagion of social norms.

# 2 Empirical analysis

## 2.1 Sample

The data we use come from Round $8$ of the European Social Survey, which includes $18$ countries. The election of Donald Trump fell inside the survey fieldwork period of $13$ of these countries. Each of these countries belongs to the 2016 United Nations list of $35$ "developed countries". The survey is constructed using highly rigorous translation protocols and conditional monetary incentives are granted to units upon the completion of interviews.

## 2.2 Empirical model

We test whether *Donald Trump's win increased global self-reported racial bias in policy attitudes*. Our approach to identify the dependent variable belongs to an established tradition. As biological racism declined after World War II, race-related attitudes have often been measured by relying on race-targeting policy attitudes (Bobo et al., 2012). Researchers have accordingly tried to retrieve attitudes from salient policy issues, such as the opposition to policies that tackle school segregation or allow interracial marriage (Bobo and Kluegel, 1993; Card, Mas, and Rothstein, 2008). Given the salience of the immigration issue in contemporary political debates in developed countries, immigration attitudes provide an appealing way to identify a racial bias in policy attitudes.

To identify racially biased attitudes, we use questions $B38$ and $B39$ of the $ESS$, which pertain to the willingness of respondents to let immigrants enter their country. The two questions only differ on the race dimension. Specifically, 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 $B39$ directly follows question $B38$ and reads, "*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". The succession of the two questions explicitly primes the role of race. 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, interviews are conducted face-to-face. Respondents are therefore subject to a stronger social pressure than in internet surveys, where racially biased opinions are revealed anonymously (DellaVigna, List, and Malmendier, 2012; 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 the value $1$ if $y_{1i} > y_{2i}$ and $0$ otherwise. In the relevant sample, $32.70\%$ of individuals report stronger opposition to different race immigration than same-race immigration, while $64.80\%$ report equal opposition to different-race immigrants. Reports of racist attitudes ($y_i = 1$) account for $31.49\%$ in the control group and $34.06\%$ in the treatment group.[1] Defining $Y_{i,c} = \ln\left[\frac{\Pr(y_{i,c}=1)}{1-\Pr(y_{i,c}=1)}\right]$, we use the following specification:

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

$T_i \in \{0, 1\}$ is the treatment variable. It takes the value $1$ if respondent $i$ was interviewed after November, 8, 2016 and $0$ otherwise. As the timing of each interview is random with respect to the release of the information about the outcome of the U.S. election, $T_i$ can be interpreted as an exogenous decrease to the social desirability of race-equality. $\beta$ is therefore the treatment effect, while $\alpha$ 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). In an augmented model, we include *socioeconomic* characteristics: highest education 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's income (1 meaning living comfortably through, 4 meaning living with strong difficulties). We add a dummy variable taking the value $1$ if the respondent voted in the latest general election, to capture interest in politics. Notice that we only select proper covariates. To control for unobserved country heterogeneity, we

[1] 2.49% of respondents oppose 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 units reporting positive racism is extremely limited, treatment effects are very close in the two cases.

include country fixed effects $\mu_c$. $\epsilon_{i,c}$ is an idiosyncratic error term, with $\mathbf{E}[\epsilon|T, X, \mu] = 0$. 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 group may, however, 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. Our outcomes may therefore be biased if, for instance, respondents who were interviewed following the election were significantly less educated than respondents in the control group, making respondents in the treatment group more likely to report racially biased attitudes. Descriptive statistics in the Appendix show that this issue is not particularly severe in our application. However, following Hainmueller (2012), we weight control units such that the distribution of covariates among the control groups matches the moment conditions (until skewness) of the treatment group. After this pre-processing, covariate imbalance between control and treatment groups becomes negligible.[2]

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 percent change in the probability of a respondent in the treatment group exhibiting a racial bias. Our approach is the same as the one used by Depetris-Chauvín and Durante (2017), Giani (2017), and Mikulasche, Pant, and Tesfaye (2016) in recent working papers that exploit a similar design to study different issues. Like those authors, we base our main analysis on an interval of $\pm 15$ days before and after the election. We also report treatment effects for shorter and longer intervals in the Appendix.

[2]In the Appendix, following a suggestion of Hainmueller and Xu (2013), we first use pre-treatment matching (Coarsened exact matching, see Iacus, King and Porro, 2012) to prune outliers and then apply alternative entropy balancing strategies, which achieve balance either at the pooled level or at the levels of single countries.

The ideal dataset to study a "global contagion" should include 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 13 developed 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.[3]

# 3 Results

## 3.1 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 (i), the treatment effect, significant at $p < .01$, is computed as a simple mean-difference. In column (ii), we control for country fixed effects only. Being interviewed after Donald Trump's election increases the likelihood of reporting a racial bias by $1.8\%$, and the outcome is now significant at $p < .05$. Columns (iii) and (iv) add control variables pertaining, respectively, to demographic and socioeconomic characteristics. Once this set of covariates is controlled for, the treatment effect slightly exceeds $2\%$.

In column (v), we include self-reported turnout in the latest national election as a proxy for political interest. Doing so does not substantially alter the treatment effect, which stays significant at $p < .01$. A key comparison is the one between column (v) and (vi). Once the control units are weighted to match the covariates' distribution of treated units, the treatment effect increases very little. This suggests that sample imbal-

[3]Standard clustering may yield false positives as the number of clusters is small and odd. We therefore provide treatment effects after bootstrapping errors through Wild cluster bootstrap, following Cameron, Gelbach and Miller (2008) and Mackinnon and Webb (2017). These issues are discussed in the Appendix, where we also report treatment effects for alternative specifications.

| | Racial bias (0-1) | | | | | |
|---|---|---|---|---|---|---|
| | (i) | (ii) | (iii) | (iv) | (v) | (vi) |
| Treatment (0-1) | .031*** | .017** | .020*** | .021*** | .022*** | .023*** |
| SE | (.011) | (.009) | (.008) | (.009) | (.009) | (.008) |
| N.obs | 7,904 | 7,904 | 7,855 | 7,720 | 7,685 | 7,685 |
| Country Effects | | ✓ | ✓ | ✓ | ✓ | ✓ |
| Demographics | | | ✓ | ✓ | ✓ | ✓ |
| Socioeconomics | | | | ✓ | ✓ | ✓ |
| Voting | | | | | ✓ | ✓ |
| Entropy balancing | | | | | | ✓ |

Coefficients for treatment effect: average marginal effects following Logit estimation. significant at .1, **: significant at .05, ***: significant at .01. Standard errors clustered at 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 value one if the respondent voted at latest general election (0-1) Entropy balancing weights units in order for the distribution of covariates of the control group to match the distribution of covariates of the treated group, until skewness. Design weights apply. Source: ESS, round 8.

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

ance is not severe in our application. In the full specification of column (vi), on which we are going to rely to address further identification issues, Donald Trump's election increases the probability of reporting a racial bias by $2.3\%$, significant at $p < .01$.

In line with Bursztyn, Egorov, and Fiorin (2017) and Huang and Low (2017), we interpret changes in self-reported racial bias as changes in the propensity to report racially biased attitudes, rather than changes in preferences.[4] The next subsection provides further evidence consistent with that interpretation. The probability of reporting a racial bias in policy attitudes before the election predicted by our model is $32.1\%$. To

[4]Bursztyn, Egorov, and Fiorin (2017) show that the increase in xenophobic opinions following the election of Donald Trump was driven by an increase in the propensity to report them, whereas the actual number of xenophobic opinions did not change. This is an expected result, as the electoral outcome itself, while informative about the tolerance towards race-related norms, is unlikely to change preferences.

get a sense of the quantitative meaning of the treatment effect reported in column (vi), we consider two simulated scenarios. In the first, let us suppose that $50\%$ of the sample was actually racially biased. In that case, $17.9\%$ ($50\% - 32.1\%$) were racially biased but not willing to report it prior to Donald Trump's election. Our result would then imply that, among actually racially biased individuals, $12.8\%$ ($2.3\%/17.9\%$) switched from not-reporting to reporting due to Donald Trump's election. Let us now suppose instead that the actual share of racially biased individuals was $40\%$, then $29.1\%$ ($2.3\%/7.9\%$) of them switched from not-reporting to reporting due to Donald Trump's election.

## 3.2 Threats to identification

In this subsection, we deal with the main threats to identification. Their analysis is briefly summarized in Figure 1 and detailed in the Appendix.

**Trump v. previous elections.** Our strategy rests on the contention that Donald Trump's election marked a change in the *status quo* toward lower racial equality. What would have happened to race-related attitudes if the *status quo* had changed toward higher racial equality? An advantage of our design is that it can be applied to the last three previous U.S. presidential elections. Barack Obama is arguably the closest to a perfect opposite of Donald Trump on many issues, including issues of racial equality. 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 $p < .05$.[5] A second question regarding

[5]One may argue that the informational content of Donald Trump's win was stronger than the one of Barack Obama's 2008 win. In the first case, the electoral outcome was unexpected given pre-electoral polls. In the latter case, the outcome was less unexpected: Barack Obama and John McCain had close approvals until October, but Barack Obama gained an edge over John 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 U.S. marked an important discontinuity in world politics from the perspective of a global audience.

counterfactuals is the following: 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, and, hence, there was no change in the *status quo*. Figure 1a shows that the elections of 2004 and 2012, which granted second mandates respectively to George W. Bush in 2004 and Barack Obama in 2012, respectively, had no significant effect on self-reported racial bias in policy attitudes.

**Racist v. immigration attitudes.** The two questions we intertwine to construct the dependent variable are identical, only differing in the racial background of immigrants. Race-targeting policies, however, may face opposition due to welfare concerns, rather than racism (Bobo and Kluegel, 1993). Contextualized in our application, one may contend respondents use race simply 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 v. same-race immigration would be driven by welfare concerns, rather than racism. However, the results shown in Figure 1b eliminate this possibility. Figure 1b shows that the documented effect on self-reported racial bias is not driven by economic-related immigration concerns.

**Electoral v. campaign effect.** Schaffner (2017) shows that being exposed to Donald Trump's campaign increased individuals' willingness to express xenophobic opinions against minorities. In another recent paper, Morrison et al. (2018) show that assault frequency increased on days and in cities where candidate Donald Trump campaigned. As the campaign was covered worldwide, his xenophobic rhetoric may also have changed the 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, 15 days, three weeks, or 30 days before the actual election and keeping a symmetric

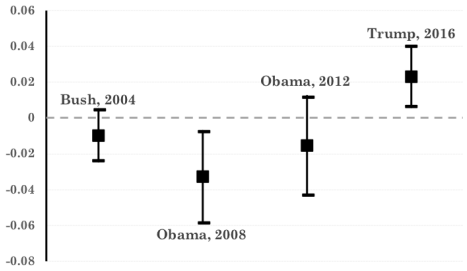
interval of time around it yields no significant treatment effect. Donald Trump's rise in popularity during the campaign may have affected race-related attitudes but does not threaten the validity of our estimates of an "electoral effect" taking place on November 8, 2016.

**Electoral v. 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 what residents of other countries perceive as being the new stance of the US. Figure 1b however shows that some of the most archetypical political attitudes, including left-right placement, the support for right-wing populist parties, and preferences towards redistribution and gay rights remained constant, suggesting no generalized bandwagon effect.

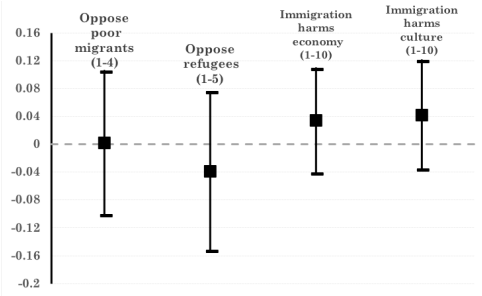
# 4 Conclusion

The configuration of racial attitudes provides vital information about the micro-level principles guiding race relations and, relatedly, about the macro-level status of racial equality. Indeed, the trend of aggregate racial attitudes largely fits the trend of actual racial divisions (Bobo et al., 2012). While pro-race equality attitudinal trends had spread optimism about the future of race relations in the U.S., Donald Trump's win signaled a potential shift in norms toward a greater acceptance of racially biased attitudes (Bursztyn, Egorov, and Fiorin, 2017; Crandall, Miller, and White, 2018). Our analysis suggests that this also holds true outside the U.S.

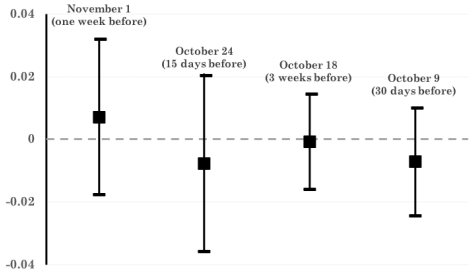
Our analysis has some limitations. For instance, it is constrained both by the data and by the nature of the design when assessing the medium- to long-run effects of elections. Firstly, the lengths of the fieldwork periods, detailed in the Appendix, are limited. But most of all,



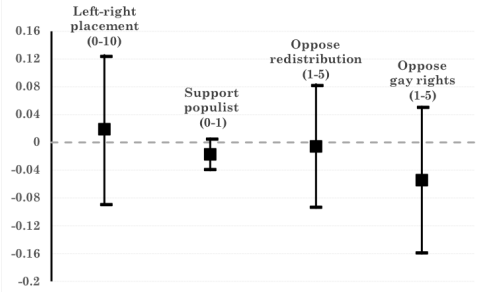

(a) Trump v. previous elections.




(b) Racist v. immigration attitudes.




(c) Electoral v. campaign effect.




(d) Electoral v. bandwagon effect.

Figure 1: Threats to identifications: treatment effects with 95% confidence interval. Coefficients are computed according to model (vi) in Table 1. For discrete dependent variables, we report ordered Logit coefficients.

identifying a long-run effect with our design would be contentious even if the available data spanned a greater length of time. Unobserved events, as well as learning, may in fact affect both the long-run trend of actual racism and the willingness to report it. It would therefore be problematic to attribute medium to long-run changes in self-reported racial bias to the election of Donald Trump.

These issues notwithstanding, we hope to offer an interesting contribution to the current debate. Our analysis combines a methodological and a substantive contribution. Our contribution is methodological on the "theory-testing" side. While the extant literature has relied on experimental approaches, we rely instead on a quasi-natural experiment based on observational data and confirm that electoral outcomes may affect the evolution of social norms. Further research should assess the external validity of our finding and theorize the mechanism through which contagion takes place. Is contagion due to the weight of the U.S. in the world, or could it be observed in other contexts? This question can be addressed by collecting other key political events related to xenophobic norms, such as *Brexit*, and studying their effect on race-related attitudes, using different surveys.

On the "theory-building" side, our contribution is substantive: 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 local social norms in a country change following institutional decisions inspired or imposed by a focal country (Acharya, 2004). For instance, U.S. sanctions against South Africa have been argued to have played a key role in fostering the global norm of racial equality and fighting against Apartheid (Klotz, 1995). Our paper suggests that another informational channel, based on the reaction of citizens to election results in a foreign country, 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 another interesting avenue for theoretical research.

# References


[1] Abadie, Alberto, Athey, Susan, Imbens, Guido W. and Jeffrey Wooldridge. 2017. "Sampling-based vs. Design-based Uncertainty in Regression Analysis," (https://economics.mit.edu/files/13161).

[2] Acharya, Amitav. 2004. "How Ideas Spread: Whose Norms Matter? Norm Localization and Institutional Change in Asian Regionalism," *International Organization* 58(2): 239-75.

[3] Bobo, Lawrence and James R. Kluegel. 1993. "Opposition to Race-Targeting: Self-Interest, Stratification Ideology, or Racial Attitudes?" *American Sociological Review* 58(4): 443-64.

[4] Bobo, Lawrence, Charles, Camille Z., Krysan, Maria and Alicia D. Simmons. 2012. *Social Trends in American Life*. Princeton University Press.

[5] Bursztyn, Leonardo, Georgy Egorov, and Stefano Fiorin, 2017. "From Extreme to Mainstream: How Social Norms Unravel," NBER working paper No. w23415.

[6] Cameron Colin A., Gelbach, Jonah B. and Douglas L. Miller. 2008. "Bootstrap-based Improvements for Inference with Clustered Errors." *Review of Economics and Statistics* 90(3): 414-27.

[7] Card, David , Mas, Alexandre and Jesse Rothstein. 2008. "Tipping and the Dynamics of Segregation," *Quarterly Journal of Economics* 123(1): 177-218.

[8] Crandall, Christian S, Jason M Miller, and Mark H. White. 2018. "Changing Norms Following the 2016 US Presidential Election: The Trump Effect on Prejudice." *Social Psychological and Personality Science* 9(2): 186-92.

[9] DellaVigna, Stefano, List, John A, and Ulrike Malmendier. 2012. "Testing for Altruism and Social Pressure in Charitable Giving," *Quarterly Journal of Economics* 127(1): 1-56.

[10] Depetris-Chauvín, Emilio and Ruben Durante, 2017. "One Team, One Nation: Football, Ethnic Identity, and Conflict in Africa," Discussion paper (https://www.rubendurante.net/research).

[11] Dustmann, Christian and Ian P. Preston, 2007. "Racial and Economic Factors in Attitudes to Immigration," *B.E. Journal of Economic Analysis & Policy* 7(1).

[12] Fleitas, Daniel W. 1971. "Bandwagon and Underdog Effects in Minimal Information Elections," *American Political Science Review*. 65(2): 434-38.

[13] Giani, Marco. 2017. "Causal Impact of Terrorism on Ethnic-Based Discrimination," (http://marcogiani.altervista.org/wp-content/uploads/2018/04/revised.pdf).

[14] Hainmueller, Jens, 2012. "Entropy Balancing for Causal Effects: A Multivariate Reweighting Method to Produce Balanced Samples in Observational Studies," *Political Analysis* (2012) 20: 25-46.

[15] Hainmueller, Jens and Yiqing Xu, 2013. "ebalance: A Stata Package for Entropy Balancing," *Journal of Statistical Software* 54(7): 1-19.

[16] Huang, Jennie and Corinne Low, 2017. "Trumping Norms: Lab Evidence on Aggressive Communication Before and After the 2016 US Presidential Election," *American Economic Review* 107(5): 120-24.

[17] Iacus, Stefano, Gary King and Giuseppe Porro. 2012. "Causal Inference Without Balance Checking: Coarsened Exact Matching," *Political Analysis* 20: 1-24.

[18] Klotz, Audie. 1995. "Norms Reconstituting Interests: Global Racial Equality and the U.S. Sanctions Against South Africa." *International Organization* 49 (3): 451-78.

[19] Mackinnon, James G. and Matthew D. Webb. 2017. "Wild Bootstrap Inference for Wildly Different Cluster Sizes," *Journal of Applied Econometrics* 32(2): 233-54.

[20] Mikulashek, Christoph, Pant, Saurabh and Tesfaye, 2016. "Winning Hearts and Minds in Civil Wars: Governance, Leadership Change, and Support for Violence in Iraq," (http://christophmikulaschek.com/wp-content/uploads/2016/12/Mikulaschek-Pant-Tesfaye_2016.pdf).

[21] Morrison, Christopher, Ukert, Benjamin, Palumbo, Aimee, Dong, Beidi, Jacoby, Sara, Wiebe, Douglas, 2018. "Assaults on Days of Campaign Rallies During the 2016 US Presidential Election," *Epidemiology*, forthcoming.

[22] Shabi, Rachel, 2016. "How Europe's far-right feasts on Trump's victory," *Al Jazeera*, 17 November 2016 (http://www.aljazeera.com/indepth/opinion/2016/11/europe-feasts-trump-victory- 161116122158503.html).

[23] Schaffner, Brian F., 2017. "Follow the Racist? The Consequences of Expressions of Elite Prejudice for Mass Rhetoric," mimeo (https://umass.app.box.com/s/x5zz210nor2z0v93m8frdlzxyobggrlm).

[24] Seth, Stephens-Davidowitz. 2013. "The Cost of Racial Animus on a Black Candidate: Evidence using Google Search Data," *Journal of Public Economics* 118(3): 26-40.

[25] Vlandas, Tim, and "Does Unemployment Matter? Economic Insecurity, Labour Market Policies and the Far-right Vote in Europe," forthcoming in *European Union Politics*.

# A Appendix

## A.1 Descriptive statistics

- Tables 4 and 4 provide the complete descriptive statistics respectively for Donald Trump's election and previous elections.

## A.2 Robustness Checks

- **Imbalance: 1.** Table 2 provides the descriptive statistics for each of the 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 null for dummy variables and extremely small for continuous covariates once entropy balancing was applied.

- **Imbalance: 2.** Table 2 allows observing means and variances, but it does not provide information about skewness, which is only relevant for continuous covariates. Figure 2 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 treated and control groups are almost indistinguishable.

- **Alternative specifications.** Table 3 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 the one in column (vi) of Table **??**. Column (iii) in Table 3 displays the outcome for an alternative, more ambitious, balancing strategy, in which entropy weighting is constructed at the country level. The treatment effect gets slightly greater 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 paper, we deliberately restrict the set of controls to proper covariates. It can be of interest, however, to rule out the possibility that the treatment effect be driven by the fact that the ideology of treated units differs substantially from the one of control units. In this last specification, we control for three further attitudes: left-right placement, political interest and satisfaction for democracy. The treatment effect after outliers' extraction, entropy balancing and wild cluster bootstrapping is about $2\%$, significant at $p < .01$.

## A.3 Threats to identification

- **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 1a. In the last three columns, we show that the treatment effect of Donald Trump's election is 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, $\delta_1$, 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 George W. Bush 2004, and 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 $p < .01$.[6]

[6] 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

- **Racist v. immigration attitudes.** Table 8 reports estimates of the effect of Donald Trump's election on four other survey items related to immigration. 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 $p < .1$) Those items read:
  - (*Oppose refugees*): The government should be generous in judging people's applications for refugee status. (1: Agree strongly, ... , 5: Disagree strongly);
  - (*Oppose poor migrants*) : Allow many/few immigrants from poorer countries outside Europe. (1: Allow many, ... , 4: Allow none);
  - (*Immigration harms economy*): 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);
  - (*Immigration harms culture*): 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).
- **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 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.
- **Electoral v. bandwagon effect.** Table 8 provides the outcome relative to the effect of Donald Trump's election on four other sur-

last case.

vey items related to ideology. We find that the treatment effect is null for each of these cases. Those items read:

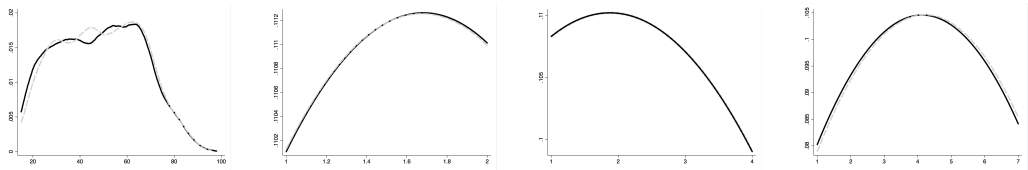
- (*Left-right placement*): Placement on left-right scale . (1: Left, ... , 10: Right);
- (*Support populist*): Which party do you feel close to. (0: Any or none, ... , 1: a right-wing populist party);[7]
- (*Oppose redistribution*): Government should reduce differences in income levels. (1: Agree strongly, ... , 5: Disagree strongly);
- (*Oppose gay rights*): Gay and lesbian couples should have the same rights to adopt children as straight couples (1: Agree strongly, ... , 5: Disagree strongly).

We can therefore interpret our main finding as signaling Donald Trump's election had no general effect on the opinions of respondents. It therefore specifically resulted in an increase in the willingness to report opinions that discriminate migrants of a different race.

[7]The list of populist parties is based on Vlandas and Halikiopoulou (2018).

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

Table 2: **Imbalance: 1.** Descriptive statistics and imbalance: before and after entropy balancing.



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

Figure 2: **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.

| | Racial bias (0-1) | | | | | |
|---|---|---|---|---|---|---|
| | (i) | (ii) | (iii) | (iv) | (iv) | (v) |
| Treatment (0-1) | .024*** | .025*** | .025*** | .025*** | .025** | .020*** |
| SE | (.008) | (.008) | (.011) | $NA$ | $NA$ | $NA$ |
| N.obs | 7,301 | 7,301 | 7,301 | 7,301 | 7,301 | 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 education). Age is coarsened through intervals of 5 years while domicile is coarsened according to Scott-break algorithm. Matching prunes 457 units (270 controls). 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 3: **Alternative specifications.** Effect of Donald Trump's election on self-reported racial bias, further specifications.

| 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 4: 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 5: Descriptive statistics, other elections.

| | Racial bias (0-1) | | | | | |
|---|---|---|---|---|---|---|
| | Bush 2004 | Obama 2008 | Obama 2012 | Trump-Bush | Trump-Obama | Trump-Obama |
| Treatment (0-1) | -.010 | -.032** | -.017 | .032*** | .055*** | .034** |
| SE | (.007) | (.013) | (.014) | (.011) | (.015) | (.015) |
| N.obs | 10,817 | 7,191 | 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) | | | |
|---|---|---|---|---|
| | - 7 days | -15 days | - 21 days | - 30 days |
| | November 1 | October 24 | October 18 | October 9 |
| Treatment (0-1) | .008 | -.008 | -.000 | -.005 |
| SE | (.013) | (.015) | (.008) | (.009) |
| N. Obs | 3,773 | 6,512 | 9,697 | 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, by intervals of time.

| | 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 | .002 | .042 | .049 |
| SE | (.058) | (.053) | (.042) | (.040) |
| N. Obs | 7,733 | 6,648 | 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 | -.009 | -.003 | -.047 |
| SE | (.053) | (.016) | (.043) | (.055) |
| N. Obs | 7,258 | 3,239 | 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 8: **Racist v. immigration attitudes and Electoral v. bandwagon effect**. Effect of Donald Trump's Election on ideology and further immigration-related attitudes.

# B For Online Publication

- Following Depetris-Chauvín and Durante (2017), Giani (2017) and Mikulasche, Pant and Tesfaye (2017), we base our main analysis on an interval of $\pm 15$ days before and after the election. The chosen interval balances out two necessities. $ESS$ questionnaires release large information about respondents and, the rate of data collection per country is therefore relatively small, with no collection during weekends. This requires selecting a sufficiently wide time interval. On the other hand, as race-related attitudes may vary according to several channels, the observed treatment effects can be credibly attributed to the election outcome only if intervals are sufficiently close to the election day. Table 9 provides the treatment effects relative to other time intervals. The latter has a similar magnitude throughout the first month.

- Table 10 provides alternative specification for our main dependent variable. In the first column, the dependent variable is the difference between the score on the opposition to different v. same race immigration. In the second column, we define the dependent variable in the following way: $-1$ ($+1$) if opposition to different race immigration is lower (larger) than same race immigration, and $0$ else. The outcomes are consistent with the one presented in the text, and significant at $p < 01$. Columns 3 and 4 show that the win of Donald Trump resulted in a wider gap between the opposition to different versus same race immigration. The latter takes place through a significant decrease in the opposition to same race immigration, rather than through an increase in the opposition to different race immigration. This result is important as it reinforces the idea that reports of racial bias were not driven by immigration concerns.

- Figure 3 provides the timing of the interviews for each of the coun-

tries 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.

- Figure 4 compares trends of Google searches in our sample of countries and in the U.S. for both "Trump" (Figure 4a) and "Racism" (Figure 4b). 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 in the November 9, 2016, for our sample as compared to the U.S., while the opposite happens for "racism" (and its translation in each country language).

- 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 at this stage. Finally, as interactions in non-linear models may be problematic, we run the same analysis with a linear probability model. Outcomes are extremely close.

| | Racial bias (0-1) | | | |
|---|---|---|---|---|
| | 7 days | 15 days | 21 days | 30 days |
| Trump 2016 (0-1) | .019** | .023*** | .014*** | .013* |
| SE | (.009) | (.008) | (.006) | (.007) |
| N. Obs | 3,879 | 7,717 | 10,166 | 13,917 |
| Country Effects | ✓ | ✓ | ✓ | ✓ |
| Demographics | ✓ | ✓ | ✓ | ✓ |
| Socioeconomic | ✓ | ✓ | ✓ | ✓ |
| Voting | ✓ | ✓ | ✓ | ✓ |
| Entropy balancing | ✓ | ✓ | ✓ | ✓ |

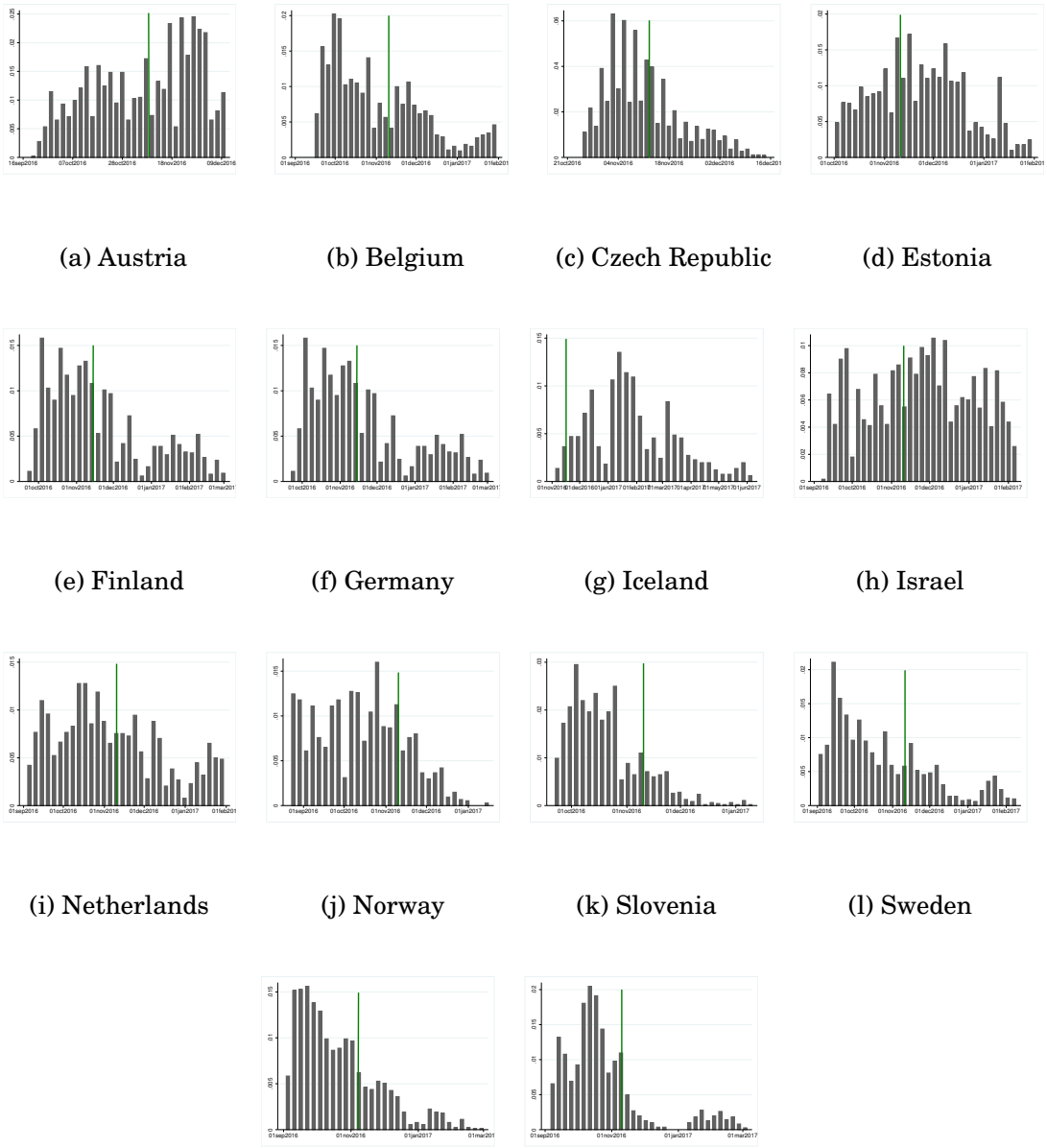
*: significant at .1, **: significant at .05, ***: significant at .01. Coefficients: average marginal effects following a logit estimation. Errors clustered at the country level. Countries: Austria, Belgium, Switzerland, Germany, Estonia, Finland, UK, Israel, Norway, Sweden, and Slovenia. Demographics: 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 9: Effect of Donald Trump's election on self-reported racial bias, by time interval.

| | $\Delta$ *Different - Same race* (1-7) | *Different* $\gtreqless$ *Same race* (-1, 0 ,1) | Opposition to *Different race* (1-4) | Opposition to *Same race* (1-4) |
|---|---|---|---|---|
| Treatment (0-1) | .123*** | .119*** | -.015 | -.111** |
| SE | (.043) | (.040) | (.058) | (.056) |
| N. Obs | 7,717 | 7,717 | 7,739 | 7,757 |
| Country Effects | ✓ | ✓ | ✓ | ✓ |
| Demographics | ✓ | ✓ | ✓ | ✓ |
| Socioeconomics | ✓ | ✓ | ✓ | ✓ |
| Voting | ✓ | ✓ | ✓ | ✓ |
| Entropy balancing | ✓ | ✓ | ✓ | ✓ |

*: significant at .1, **: significant at .05, ***: significant at .01. Coefficients: average marginal effects following Logit estimation. 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 10: Effect of Donald Trump's election on alternative dependent variables.



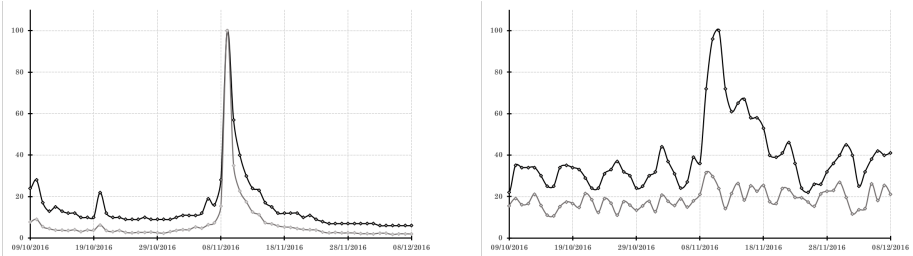
(a) Austria (b) Belgium (c) Czech Republic (d) Estonia

(e) Finland (f) Germany (g) Iceland (h) Israel

(i) Netherlands (j) Norway (k) Slovenia (l) Sweden

(m) Switzerland (n) UK

Figure 3: Data distribution by country. The green line represents the date of the US Presidential election (November 8, 2016).




(a) Trump. (b) Racism.

Figure 4: 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 average them out).

| | Racial bias (0-1) | | | | | | |
|---|---|---|---|---|---|---|---|
| | AT | BE | CH | CZ | DE | EE | FI |
| Treatment | .020** | .023** | .023*** | .021** | .023*** | .027*** | .028*** |
| SE | (.008) | (.008) | (.008) | (.010) | (.009) | (.009) | (.007) |
| Treatment × Country | .021*** | .005 | -.004 | .008 | .002 | -.033*** | -.054*** |
| SE | (.008) | (.009) | (.008) | (.011) | (.009) | (.008) | (.007) |
| | UK | IL | NL | NO | SE | SI | |
| Treatment | .023*** | .020*** | .023*** | .020*** | .024*** | .024*** | |
| SE | (.008) | (.008) | (.009) | (.008) | (.008) | (.008) | |
| Treatment × Country | -.002 | .036*** | .027*** | .094*** | -.046*** | -.006*** | |
| 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.