**Supplementary material**

Once They Are Seated: The Impact of Radical Right Parties’ Political Representation on Trust and Solidarity

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# **Readme**

This online supplement to "*Once They Are Seated: The impact of Radical Right Parties’ Political Representation on Trust and Solidarity*" contains results of the main analysis and of additional analyses. Data used in this paper comes from different sources that I have listed below. The data can be downloaded for free by following the provided links.

* European Social Survey rounds 1-9 cumulative data file (<https://www.europeansocialsurvey.org/downloadwizard/>)
* Parliaments and Governments database (<http://www.parlgov.org/>)
* Standardized world income inequality database (<https://fsolt.org/swiid/>)
* OECD (<https://data.oecd.org/migration/foreign-born-population.htm>)
* World bank ([https://data.worldbank.org/)](https://data.worldbank.org/%29)
* International Social Survey Programme (<https://www.gesis.org/en/issp/home>)
* Integrated Values Study (https://www.worldvaluessurvey.org/WVSEVStrend.jsp)

If you are interested in skimming the R code that generates these results or if you wish to replicate the results themselves and maybe alter the R code, you will need to work with the *RRP\_attitude.Rmd* file. To use the Rmd replication file, you need a working version of R, RStudio, Rmarkdown, and Latex. All mentioned software packages are open source, free to use and available for Windows, Mac, and Unix operating systems. Once you have installed these, please open RStudio and establish an RStudio project in the unzipped file directory that contains the *RRP\_attitude.Rmd* file. For further information on Rstudio projects and how to set them up, I refer to chapter 8 in R for Data science (Wickham & Grolemund, 2016a). After having set up the RStudio project, you will find all necessary files under the "Files" tab in Rstudio. Open the file by clicking on it under the "Files" tab. The file should open in the top left panel. Rstudio will automatically notify you about all user-written packages that the analysis relies on and that you have not yet downloaded and installed. Please click on "install". To replicate the whole analysis, simply click on "Knit" just below the file tab of the top left panel showing the Rmd file. If you want to alter the R code, you can work on the single R-snippets. For more information on how to edit Rmd files and R snippets see chapter 27 in R for Data science (Wickham & Grolemund, 2016b)

# **Appendices**

## **Appendix A. Identification strategies**

### ***Two-way fixed effects approach***

For this identification strategy, I restrict the sample to the 17 European countries (118 country-years) that have a pre-entrance period of a radical right party in the data. I do so, because my research interest lies in estimating within-country changes in the indicators of trust and solidarity after a radical right party (RRP) has entered parliament in country j. I do not include *always-treated* countries in the sample to avoid introducing bias because of their treatment status not changing throughout the study period. Including *always-treated* countries would introduce both negative weighting bias and misrepresentation of the control group trajectory (Ludwig & Brüderl, 2021, p. 475). After listwise deletion on all variables of interest, I end up with a sample size of 181,071 survey responses distributed across 17 countries (118 country-years).

Table A1 summarizes central information on the countries I have included in the sample. In the second column, I have listed the name of the RRP that entered parliament. If this field is empty it means that the country did not experience a RRP enter parliament during the studied period. Iceland, Ireland, Lithuania, and Luxembourg did not elect a RRP to parliament and thus constitute a comparison group. I also list the first year the RRP entered parliament, whether the country uses a proportional representation system, and whether the voting system requires that political parties exceed a legal electoral threshold to gain parliamentary representation in the national parliament. Czech Republic, France, Hungary, Slovakia, and Ukraine had a RRP enter the national parliament before the start of the data period. However, I choose to still include these countries, since their respective RRPs’ lost their seats in parliament and had not regained them by the first ESS round. When selecting radical right parties that are both characterized as ‘populist’ and ‘nativist, I rely on both Cas Mudde’s (2007, p. 307) classification and the PopuList database (Rooduijn et al., 2019). Using both sources enables me to include RRPs founded after the publication of Cas Mudde’s book (e.g., Vox in Spain).

### **Table A1: List of countries and Radical Right Parties**

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
| **Country** | **Party name** | **Entrance year** | **Proportional system** | **Threshold** |
| Cyprus | National Popular Front | 2016 | Yes | Yes |
| Czechia | Dawn | 2013 | Yes | Yes |
| Estonia | Conservative People's Party of Estonia | 2016 | Yes | Yes |
| France | National Front | 2012 | No | No |
| Germany | Alternative for Germany | 2017 | Yes | Yes |
| Greece | Popular Orthodox Rally | 2007 | Yes | Yes |
| Hungary | Jobbik | 2010 | No | Yes |
| Iceland |  |  | Yes | Yes |
| Ireland |  |  | No | No |
| Lithuania |  |  | Yes | Yes |
| Luxembourg |  |  | Yes | No |
| Portugal | Chega | 2019 | Yes | No |
| Slovakia | Slovak National Party | 2006 | Yes | Yes |
| Spain | Vox | 2019 | Yes | No |
| Sweden | Sweden Democrats | 2010 | Yes | Yes |
| UK | United Kingdom independence party | 2015 | No | No |
| Ukraine | All-Ukrainian Union “Freedom” | 2012 | Yes | Yes |

*Note: United Kingdom’s independence party (UKIP) and Slovak National Party (SNP) lose their representation in parliament during the study period. SNP reenters at the 2016 Slovak parliamentary election while UKIP loses its seats at the 2017 UK general election after having entered the House of Commons for the first time in 2015.*

### ***Regression discontinuity design approach***

In this supplementary identification strategy, I exploit the electoral rule that a RRP’s national representation in country *j* at time *t* is determined by whether the party has won enough votes to pass a nationwide electoral threshold that is either defined legally or mathematically. This procedure also means that I can include always-treated countries alongside *treated* and *never-treated* countries in the sample if their electoral systems contain an electoral threshold.

Table A2lists all the countries in the sample with an electoral threshold. The third column signals whether the threshold is defined by electoral law or calculated as an effective threshold approximating the vote share required to win one seat in parliament using the formula $T\_{effective}=\frac{75\%}{(\left(\frac{S}{E}+1\right)\*\sqrt{E}}$, where S is the total number of seats in parliament and E is the number of electoral districts (Taagepera, 1998, 2002). Since the electoral system does not completely determine the effective threshold, it is important to note that the results are similar if I only include countries in the analysis with a legally defined electoral threshold (see table H2). 28 countries (174 country-years) in the sample have electoral systems that require a political party exceeds a nationwide electoral threshold. The institutional design of the political system is exogenous to party behavior before the election, thus, eliminating this potential confounder (Abou-Chadi & Krause, 2020).

### **Table A2: List of countries and electoral thresholds**

|  |  |  |
| --- | --- | --- |
| **Country** | **Threshold** | **Type** |
| Austria | 4 | Legal |
| Belgium | 1.545 | Effective |
| Bulgaria | 4 | Legal |
| Cyprus | 3.6 | Effective |
| Czech Republic | 5 | Legal |
| Denmark | 2 | Legal |
| Estonia | 5 | Legal |
| Finland | 1.2696 | Effective |
| Germany | 5 | Legal |
| Greece | 3 | Legal |
| Hungary | 5 | Legal |
| Iceland | 5 | Legal |
| Ireland | 2.35 | Effective |
| Italy | 3 | Legal |
| Latvia | 5 | Legal |
| Lithuania | 5 | Legal |
| Luxembourg | 2.95 | Effective |
| Netherlands | 0.67 | Effective |
| Norway | 4 | Legal |
| Poland | 5 | Legal |
| Portugal | 1.36 | Effective |
| Romania | 5 | Legal |
| Slovakia | 5 | Legal |
| Slovenia | 4 | Legal |
| Spain | 1.3454 | Effective |
| Sweden | 4 | Legal |
| Switzerland | 1.69 | Effective |
| Ukraine | 5 | Legal |

In the RDD, treatment status D is determined by the RRPs vote share being below or above a defined cutoff c. A RRP is represented in the national parliament $(D=1)$ if its vote share is the same or exceeds this electoral threshold $(x\geq c)$ and not represented $(D=0)$ if the vote share does not exceed this threshold of representation $(x < c)$. According to table A2, this cutoff varies from 0.79 % of the total vote share in the Netherlands to 5 %, which is also the most common threshold. Equation (2) describes the RDD:

$$y\_{ijt}=τD\_{jt}+β\_{1}\left(x\_{jt}-c\_{j}\right)+β\_{1}\left(x\_{jt}-c\_{j}\right)\*D\_{jt}+ x\_{ijt}β\_{x}+δ\_{jt}β\_{δ} +C\_{j}+T\_{t}+ε\_{ijt} \left(2\right),$$

where $τ$ is the local average treatment effect LATE and $x\_{jt}-c\_{j}$ is the RRP vote share distance relative to the country-specific electoral threshold, which constitutes the running variable. Like in the two-way fixed effects models, I control for individual- and country-year specific characteristics that may be associated with the outcome and country- and year fixed effects. In line with standard practice, I rely on both parametric estimations using second-and third-order polynomials and a non-parametric local linear regression using different bandwidths (h) to test the proposed hypotheses (see tables G1-2 & figures G1-2). In each model, I use ESS post-stratification weights adjusted by a weighting factor that divides the total weights for the whole sample by the weights for each country-year giving each country-year equal weights (see Abou-Chadi & Finnigan, 2019 for a similar weighting procedure). To allow for between-individual correlation, I cluster standard errors at the level of country-years. Since the predictor is measured at the country-year level, the estimated regression models have 121 model degrees of freedom. For a two-tailed t-test with a significance level at $α= 0.05$ the critical value is $t=1.998$ (Elff et al., 2021).

The validity of a regression discontinuity design necessitates that (1) the continuity assumption holds and (2) that no one (e.g., RRPs themselves, their voters, or their competitors) can manipulate RRPs treatment status on either side of the cutoff. The continuity assumption entails that country-years where a RRP barely managed to enter parliament are similar to country-years where the radical right barely lost representation, so, the only change that occurs at the point of discontinuity is the shift in treatment status (de la Cuesta & Imai, 2016, p. 377). Figure H1 shows that treated and untreated units are balanced on all observed covariates except for population density and foreign-born population, which therefore constitute important controls in the estimated models. While I am still unable to eliminate the possibility of unobserved heterogeneity, it strengthens the causal interpretation of the results. Moreover, the manipulation assumption, I argue, is realistic, since manipulation is only possible through electoral fraud or by changing electoral laws (Abou-Chadi & Krause, 2020). However, in a few cases the threshold does not completely determine treatment assignment. As a robustness check, I therefore conduct a fuzzy regression discontinuity design that probabilistically defines treatment status in a two-stage regression framework, where the running variable (i.e., vote share relative to the threshold) is used as an instrument for treatment status. The results of the fuzzy RDD do not deviate from the results of the sharp RDD (see table H2). Additionally, I do not find any evidence of within-group jumps supporting the identification assumption that any effect only happens at the point of discontinuity (see table H1). Yet, it is important to stress that the results should be considered exploratory because of issues of low statistical power.

## **Appendix B. Survey items**

(1) I use principal component analysis (PCA) to construct a variable measuring *anti-immigrant sentiment*. The PCA is estimated using three items that tap into attitudes towards the cultural, societal, and economic consequences of immigration. The survey questions are worded in the following way:

* *“Would you say that it is generally bad or good for [country]’s economy that people come to live here from other countries?”*
* *“Would you say that [country]’s cultural life is generally undermined or enriched by people coming to live here from other countries?*
* *“Is [country] made a worse or a better place to live by people coming to live here from other countries?*

The items are measured on an eleven-point scale ranging from 0 (“Bad for the economy”/”Cultural life undermined”/”Worse place to live”) to 10 (“Good for the economy”/“Cultural life enriched”/“Better place to live”). I change the direction of the items so high values indicate negative and low values indicate positive attitudes towards immigrants. The variables load primarily on one principal component that explains about 79 % of the total variance, which I keep and use as a variable capturing xenophobic sentiments.

(2) To measure public *polarization of anti-immigrant sentiment*, I rely on two different measures. The first is the squared deviation of an individual’s anti-immigrant sentiment from the country-year mean of anti-immigrant sentiment: $x\_{i}^{2}-\overbar{x\_{jt}}$. The second measure I use is a rescaled version of Van der Eijk’s (2001) agreement measure that goes from 0 which represents perfect unimodality and 1 represents perfect polarization. While there is no agreed upon way to measure the polarization of attitudes, both measures has previously been used in peer-reviewed studies (Bischof & Wagner, 2019; Bohman & Hjerm, 2016; Castanho Silva, 2017).

(3) I capture *support for redistribution* with a single item running on a five-point scale asking respondents about their level of (dis)agreement with the statement that:*“The government should take measures to reduce differences in income levels”.* Answer options range from 1 completely disagree to 5 completely agree. This survey question has been used frequently to capture support for redistribution and the welfare state (e.g., Jæger, 2006).

(4) To operationalize *Welfare chauvinism*, I use the following survey item, which has been frequently used in the literature as an indicator of welfare chauvinism (Mewes & Mau, 2013; Reeskens & van Oorschot, 2012; Van Der Waal et al., 2013).

* *“Thinking of people coming to live in [country] from other countries, when do you think they should obtain the same rights to social benefits and services as a citizen already living here?”*

The answer scale is ordinal and includes five categories: (1) Immediately on arrival, (2) After living in [country] for a year, (3) only after they have worked and paid taxes for a year, (4) once they have become a [country] citizen, (5) they should never get the same rights. Even though it has been debated whether the item captures a continuous measure of more and less welfare chauvinism (Mewes & Mau, 2013, pp. 130–131; Van Der Waal et al., 2013, pp. 171–172). This discussion notwithstanding, and for reasons of simplicity, I treat the question as a (quasi-)continuous measure of welfare chauvinism. Unfortunately, the question is only part of a special module on welfare attitudes during ESS rounds 4 and 8, which therefore substantially reduces statistical power.

(5) To capture *institutional trust*, I estimate a PCA composed of three survey items that tap into trust towards the parliament, politicians, and the legal system. This institutional trust scale is based on the following three questions: *“How much do you personally trust [“the parliament”/ "politicians"/”the legal system”]?”.* The items are measured on an eleven-point scale ranging from 0 (No trust) to 10 (Complete trust). In the PCA, the items load primarily on one principal component that explains about 76 % of the total variance, which I extract and use as a variable of institutional trust.

(6) *Social trust* is measured using the validated three-item scale (Dinesen & Sønderskov, 2015; Reeskens & Hooghe, 2008; Zmerli & Newton, 2008). I capture this scale using PCA. The three questions are asked in the following way:

* *“Generally speaking, would you say that most people can be trusted, or that you can’t be too careful in dealing with people”*
* *“Do you think that most people would try to take advantage of you if they got the chance, or would they try to be fair?”*
* *“Would you say that most of the time people try to be helpful or that they are mostly looking out for themselves?”*

The items are measured on an eleven-point scale ranging from 0 (“You can’t be too careful”/“Most people would try to take advantage of me”/“People mostly look out for themselves”) to 10 (“Most people can be trusted”/“Most people would try to be fair ”/ “People mostly try to be helpful”). In the PCA, the items load on one principal component that explains about 69 % of the total variance, which I use as a variable measuring social trust.

## **Appendix C. Two-way fixed effects models**

To analyze the data, I run weighted six two-way fixed effects linear regressions with country-clustered robust standard errors; one for each outcome variable. Table C1 and figure C1 display the results. Figure C1 is identical to figure 1 in the main article.

### ***Table C1: Regression table of main findings from two-way FE approach***



*Note: Results are based on weighted linear two-way fixed effects models with cluster-robust standard errors. 95% confidence intervals are in brackets. Two-tailed p-values are indicated by +p<0.1, \* p<0.05, \*\*p<0.01, \*\*\* p<0.001*

### **Figure C1: Coefficient plot of main findings from two-way FE approach**

*Note: Point estimates and associated 90 and 95% (two-tailed) confidence intervals (thin grey and thick black lines) are based on weighted linear two-way fixed effects models with cluster-robust standard errors.*

## **Appendix D. Two-way fixed effects: Characteristics of the electoral systems**

One might be concerned that the main results are contingent on the characteristics of the electoral systems. To alleviate this concern, I re-estimate the two-way FE models separately for countries that use a proportional voting system and for countries with an electoral system that has a legally defined voting threshold. Figure D1 shows that the estimates, when restricting the analysis to these two sample definitions, are very similar to the main results presented in table C1 and figure C1.

### **Figure D1: Effect sizes across sample definitions**



*Note: Point estimates and associated 90 and 95% (two-tailed) confidence intervals are based on weighted linear two-way fixed effects models with cluster-robust standard errors.*

## **Appendix E. Two-way fixed effects: Influential case analysis**

In cross-national research, it is crucial to examine whether some countries or country-years are driving the negative relationship between the explanatory variable (national representation of a RRP) and the outcome variable (support for redistribution). To investigate this potential problem further, I conduct influential case analysis using DFbeta plots and leave-one-out models. The results from the main analysis are robust against dropping all influential country-years from the model (see figure E1 and table E1) as well as against dropping entire countries (see figure E2).

### **Figure E1: DFbeta plot**

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### **Table E1: Test of H2a with and without influential country-year cases**



*Note: Results are based on weighted linear two-way fixed effects models with cluster-robust standard errors. 95% confidence intervals are in brackets. Two-tailed p-values are indicated by +p<0.1, \* p<0.05, \*\*p<0.01, \*\*\* p<0.001*

### **Figure E2: Leave-one-out models**



*Note: Point estimates and associated 90 and 95% (two-tailed) confidence intervals are based on weighted linear two-way fixed effects models with cluster-robust standard errors.*

## **Appendix F. Two-way fixed effects: pre-and post-event dummies**

Readers might be concerned that radical right parties erode support for redistribution prior to their entrance into the national political arena, for instance through their representation in local parliaments or during campaigns. I address this concern by adding a pre-treatment dummy one term before the radical right enters parliament to the model. Figure F1 shows that the estimate of this leading version of the predictor variable is statistically insignificant at conventional levels, thus, lending empirical credibility to the argument that RRPs negative impact on public support for redistribution happens after the party’s election to the national parliament and not prior. Furthermore, I investigate whether the observed negative relationship between a RRPs national representation and public support for redistribution remains in the long-term or wanes within the first term of the radical right’s presence in parliament. Figure F2 suggests that the negative relationship lasts at least 3 electoral terms, which is roughly equivalent to 12 years in parliament.

On a technical note, I point out that the reference period (k) for a model using only post-event dummies includes the country-year observations of the never-treated countries and the pre-treatment periods of the countries where a RRP is eventually elected to parliament. However, for the model using both pre- and post-event dummies the reference period for the post-event dummies slightly changes to the pretreatment period $k < -1$ of the treated countries and the whole period of the never-treated countries. As a result, the post-event dummy estimates are more imprecise failing to reach statistical significance at conventional levels. Ludwig & Brüderl (2021, p. 465) advice against including pre-treatment dummies in a regression model unless it is informed by a theoretical argument, as these dummies may bias the treatment effects and capture other effects that are less interpretable. The bias of the post-event estimates introduced by the inclusion of a pre-event dummy means that I regard the post-treatment estimates shown in figure F2 as more credible when estimating the studied relationship(s).

### **Figure F1: Flexible two-way FE model with pre-and post-event dummies**



*Note: Point estimates and associated 90 and 95% (two-tailed) confidence intervals is based on a weighted linear two-way fixed effects model with cluster-robust standard errors. Post-event dummies after the third electoral term are truncated due to few observations (Ludwig & Brüderl, 2021)*

### **Figure F2: Flexible two-way FE model with post-event dummies**



*Note: Point estimates and associated 90 and 95% (two-tailed) confidence intervals are based on weighted linear two-way fixed effects models with cluster-robust standard errors. Post-event dummies beyond the third electoral term are truncated due to few observations (Ludwig & Brüderl, 2021)*

## **Appendix G. Replication code for the regression discontinuity approach**

To conduct the regression discontinuity design analysis, I use both a non-parametric and a parametric estimation procedure. In the first approach, I estimate local linear regressions for different bandwidths of the running variable, while, in the latter framework, I model the entire sample using second order and third-order polynomials in a linear regression framework. All models are estimated with individual- and country-year level controls and country-year clustered robust standard errors. Tables G1-2 and figure G1-2 display the results. Figure G1-G2 is identical to Figure 2 in the main article.

### **Table G1: Parametric approach to RDD: Second-order polynomial estimation**

### **Table G2: Parametric approach to RDD: Third-order polynomial estimation**

### **Figure G1: Parametric approach to RDD: Second- and third order polynomials**



*Note: Point estimates and associated 90 and 95% (two-tailed) confidence intervals are based on a sharp regression discontinuity design with cluster-robust standard errors.*

### **Figure G2: Non-parametric approach to RDD: Local linear regression at different bandwidths**



*Note: Point estimates and associated 90 and 95% (two-tailed) confidence intervals are based on a sharp regression discontinuity design with cluster-robust standard errors.*

**Appendix H. RDD: Assumptions & robustness checks**

In figure H1, I show that the continuity assumption holds for 8 out of 10 observable covariates. According to my sample of countries, RRPs seem to systematically get elected in countries with a higher population density and lower percentage foreign born residents. Thus, these two covariates constitute important controls in the RDD analysis. Additionally, table H1 displays no within-group jumps in the running variable (i.e., RRP vote share in % relative to the defined nationwide electoral threshold). Table H2-H3 further shows that the association remains if I remove countries without a threshold defined by electoral law and if I conduct a fuzzy regression discontinuity design, where the running variable (i.e., vote share relative to the threshold) is an instrument for the predictor variable (i.e., RRPs representation in parliament).

### **Figure H1: Visualization of test of continuity assumption**



*Note: Point estimates and associated 90 and 95% (two-tailed) confidence intervals are based on a sharp regression discontinuity design with cluster-robust standard errors. The dashed vertical line is the critical α value of 0.05.*

### ***Table H1: RDDs of within-group jumps***



### **Table H2: RDD among countries with legal electoral threshold and fuzzy RDD**



**Appendix I. Exploratory analysis using Mipex data**

I propose the conjecture that the migrant integration policy context moderates nationally elected RRPs erosion of public support for redistribution. Specifically, I consider whether nationally represented RRPs impact on preferences for redistribution is stronger in countries with weak migrant integration policies. My theoretical claim is that poorer integration conditions for immigrants provides fertile ground for radical right elites to construct immigrants as abusers that do not contribute to the welfare system. To explore this expectation further, I use data from the migrant Integration Policy Index (MIPEX) (2007-2019) that assigns a score between 0 and 100 as an indicator of the quality of migrant integration policies in country j at time t. The overall score assigned to each country-year is the mean of six sub-indices measuring the quality of labor market integration policies, family reunification policies, political participation, residence opportunities, citizenship rights, and anti-discrimination legislation. Because I cannot identify a country-year combination for cases in the ESS sample before 2007, I conduct the analysis on a reduced sample. Figure I1a, which is identical to figure 3 in the main article, supports my speculation. It shows that countries with weaker immigrant integration policies and where a RRP is represented in parliament have lower levels of public support for redistribution. The findings are substantially similar when using each of the sub-indices, except in the case of the citizenship rights sub-index. The linear prediction and associated 95% (two-tailed) confidence intervals (ribbon) are based on a weighted linear two-way fixed effects model with cluster-robust standard errors and individual- and country-year controls.

### ***Figure I1A-G: The migrant integration policy context***

A: Overall



B: Labor market



C: Family reunification



D: Political participation



E: Residence opportunities



F: Citizenship rights



G: Anti-discrimination



## **Appendix J. Replication using ISSP data: Support for Redistribution**

To strengthen the generalizability of the findings, I replicate the analysis of H2a (i.e., public support for redistribution) using survey data from the international social survey programme. I use the following question, which has similar wording as the ESS item: “*On the whole, do you think it should or should not be the government's responsibility to reduce the differences in income between the rich and the poor*”. However, the question is measured on a 4-point scale instead of the 5-point scale in the ESS data. Table J1 shows that the estimate has similar magnitude to the coefficient estimated using ESS data, yet it fails to reach statistical significance at conventional levels (-12% of a SD & 95% CI [-0.30;0.06]). I note that this replication analysis has substantially lower power due to the smaller sample size (43,254 individuals, 10 countries, and 31 country-years).

### **Table J1: International social survey programme – Support for redistribution**



## **Appendix K. Replication using Integrated values study data: Trust**

To strengthen the generalizability of the findings, I replicate the analysis of H3 (i.e., institutional trust) and H4 (social trust) using survey data from the Integrated Values Study, which is a merged trend file of the World Values Survey (Haerpfer et al., 2021) and European Values Study (EVS, 2021). I use a single-survey binary item to capture social trust *(“Generally speaking, would you say that most people can be trusted or that you can’t be too careful in dealing with people?” [ 0 = You can’t be too careful; 1= Most people can be trusted]*). To measure institutional trust, I conduct a principal component analysis based on two survey items asking about confidence in the parliament and confidence in the courts on a 4-point scale. The sample size is close to the European social survey sample (106,233, 21 countries, and 91 country-years). Table K1 echoes the null findings found in the main analysis (see table C1 and figure C1).

### **Table K1: Integrated values study - Trust**



## **Appendix L. Addressing the (null-)findings: Sensitivity analysis and Bayes factors**

To assess the minimum effect size the study was sensitive to, I conduct sensitivity analysis for both the two-way FE and RDD approach. I hold the sample size, alpha level, and power fixed and vary the effect size (Cohen’s D). L1-L2 show that both the two-way fixed effects model and regression discontinuity designs are underpowered to identify a small true effect size (Cohen’s D= 0.2) but has enough power to identify a medium-sized true effect (Cohen’s D= 0.5). I note that the statistical power is even lower when decreasing the bandwidth in the non-parametric regression discontinuity design approach and when adding polynomials in the parametric approach.

 As a supplement to the p-value, table L1 reports Bayes factors approximated using the Bayesian information criterion (BIC), which is given by the formula: $BIC = -2 \* loglikelihood + d \* log(N)$. A Bayes factor above 1 indicates that a non-association is more probable, while a Bayes factor below 1 favors the alternative hypothesis.

**Figure L1: Sensitivity analysis of the two-way fixed effects models**

**sFigure L2: Sensitivity analysis of the regression discontinuity design**

**Table L1. Bayes factors and p-values**

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
|   | H1a:Xenophobia | H1b:Pol. xenophobia | H2a:Redistribution | H3:Institutional trust | H4:Social trust |
| P-value | 0.8802 | 0.7327 | 0.0168 | 0.3872 | 0.8301 |
| Bayes factor(H0 over H1) | 373 | 207 | > 0.01 | 92 | 362 |

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