###### Online Appendix

## 1 Descriptive statistics

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
| VARIABLES | N | mean | sd | min | max |
| Redistribution | 171,551 | 2.180 | 1.052 | 1 | 5 |
| Xenophobia | 169,165 | 5.717 | 2.515 | 1 | 10 |
| Racist prejudice | 20,384 | .409 | .492 | 0 | 1 |
| Social trust | 174,239 | 5.246 | 2.393 | 1 | 10 |
| Political trust | 170,782 | 4.688 | 2.483 | 1 | 10 |
| University | 174,233 | 0.251 | 0.434 | 0 | 1 |
| Female | 174,451 | 1.532 | 0.499 | 1 | 2 |
| Age | 173,748 | 52.33 | 16.28 | 25 | 123 |
| University: Father | 174,591 | 0.0620 | 0.241 | 0 | 1 |
| University: Mother | 174,591 | 0.105 | 0.307 | 0 | 1 |
| Immigration: Father | 173,867 | 1.120 | 0.325 | 1 | 2 |
| Immigration: Mother | 174,283 | 1.118 | 0.322 | 1 | 2 |
| Household income | 139,912 | 5.830 | 2.642 | 1 | 10 |
| Subjective income | 170,678 | 1.905 | 0.827 | 1 | 4 |
| Long-run unemployment | 174,141 | 0.134 | 0.340 | 0 | 1 |
| Labor market outsider | 174,591 | 0.360 | 0.480 | 0 | 1 |
| Relative skill specificity | 150,545 | 0.272 | 0.445 | 0 | 1 |
| Religion | 172,266 | 1.389 | 0.488 | 1 | 2 |
| Political Interest | 174,281 | 2.522 | 0.926 | 1 | 4 |
| Left right scale | 157,176 | 5.056 | 2.092 | 0 | 10 |
|  |  |  |  |  |  |

## 2 Endogeneity issues

Throughout the paper, we analyze the association between educational attainment and redistribution preferences, positing a causal relationship between the two. Well aware that the challenge of causality can not be entirely overcome, we address the standard endogeneity issues: reverse causality, omitted variable bias, and measurement errors.

### 2.1 Reverse causality

What if educational choices be affected by beliefs about the role of government? Even if educational choices are irreversible whereas attitudes are malleable, and even if educational choices are undertaken at early stages of life, attitudes towards the role of government may partly affect the decision to go to university. We may conjecture, for instance, that a teenager with strong opposition to redistribution may be more likely, *ceteris paribus*, to acquire human capital so as to reduce her future expected reliance on welfare. Similarly, we may conjecture that a teenager with strong interest towards societal issues may be more likely, *ceteris paribus*, to enroll in a bachelor degree so as to deepen her understanding of the role of government. It is not our objective to dismiss the possible relevance of this reverse causal channel. Rather, we provide additional arguments and analysis that further validate our causal channel.

Our analysis suggests that university reduces support for redistribution. If university matters *per se*, we should be able to observe a discernible effect of university on preferences at the time when the university diploma is obtained. This would allow us to rule out the possibility that education only shapes preferences gradually. We try to address this identification threat by relying on a classic regression discontinuity design approach (Thistlethwaite and Campbell, 1960).

Crucial to this design is the availability of a variable that throughout all rounds of the  tells us how many years of education respondents have completed. Denoting the latter, “forcing” variable by  where the same subscripts as usual apply, we run:



A couple of caveats are worth a mention. Firstly, while the discontinuity is set in a rather sharp way, we have no information about the exact number of years that each individual had to incur on in order to successfully complete her university diploma. We set this discontinuity at  years, as this is the norm in most countries, but we acknowledge that the choice is somewhat arbitrary. Hence, we run a sharp RDD design with rather fuzzy data. We are interested in controlling for the effects that education has though top-down or bottom-up ideational processes, rather than in demonstrating that the attainment of the university diploma makes a difference.

Results in Table 3 are key to our analysis. Column () simply reports the university-effect from the main specification (model  in Table 1). Column () and column () add, progressively, () the forcing variable and () the interaction of the forcing variable with the university dummy. Table 3 shows that the university effects decreases. It now adds a % right-wing bias in redistribution preferences. This small reduction, however, does not threaten significance. Moreover, neither the forcing variable nor its interaction with the university dummy are significant. Somewhat interestingly, the latter coefficient is negative (though non-significant). It may hence be the case that further education (*i.e*, Master degree, PhD) mitigates anti-redistributive preferences.

**Table 3.** Reverse Causality.

|  |  |  |  |
| --- | --- | --- | --- |
|  | (*i*) | (*ii*) | (*iii*) |
|  | Redistribution: | | |
|  | () Support ; – ; () Oppose | | |
| University diploma (0-1) | .130 | .111 | .113 |
| Std Error | (.026) | (.025) | (.025) |
| Years of education (0-30) |  | .004 | .006 |
| Std Error |  | (.002) | (.036) |
| University diploma  Years of education |  |  | -.005 |
| Std Error |  |  | (.005) |
| N.obs | 121,265 | 120,857 | 120,857 |
| Coefficients are obtained by  regression as specified in equation  Standard errors clustered | | | |
| at country level are reported in parentheses. For   and  the set of controls is as in model | | | |

### 2.2 Omitted variable bias

In the paper, we relied on a large set of controls to minimize omitted-variable based endogeneity. Here we provide further analysis, extending even further the set of socioeconomic controls, particularly in what concerns the confounding role of parental background and the confounding role of future expected income. We also discuss in detail the matching specification briefly discussed in the paper.

#### 2.2.1 Strengthen controls for Parental background

The first and second columns in Table 4 simply report the regression coefficients in the Main Table in the paper, to provide a benchmark. In column (), we run again the main regression after augmenting the set of controls that account for parental background by including (i) maternal and paternal socioeconomic status, as well as (ii) maternal and paternal immigration background. To create the first variable, we combine information from two separate survey items. The first item is composed of two dummy variables, taking value 1 if the mother (or father) were unemployed when the respondent was 14 years old. We consider unemployment the lowest status, with value  Then, we exploit a categorical variable that provides information about parental professional background. The survey item reads:

• Father’s (Mother’s) occupation when respondent was 14 years old:

- Routine manual and service occupations;

- Semi-routine/ manual/ service occupation;

- Technical and craft occupations;

- Traditional professional occupations;

- Modern professional occupations;

- Clerical and intermediate occupations;

- Senior manager or administrators;

- Middle or junior managers.

The first three categories are given value  and correspond to an intermediate socioeconomic status. The last  categories are coded as high socioeconomic status, with a value of . As such, parental background is a variable ranging from  to  where  is unemployment,  is blue-collar occupations, and  is white collar occupations.

Column () adds controls related to income and column () controls for economic insecurity. Finally, in column (), we run again the main regression after extending the set of controls that account for the development of attitudes that may be related to the acquisition of education and may in turn affect preferences for redistribution.

Table 4 reports the coefficients of our augmented regression. Comparing the education effect on redistribution preferences in Table 4 with those found in Table 2 reveals that our analysis is extremely robust to the introduction of both further socioeconomic variables and further attitudes. It is of interest to remark that parental socioeconomic status presents the same gender bias observed in the paper, that is, father’s status matters whereas mother’s status does not. Immigration background, instead, does not affect preferences for redistribution. Finally introducing satisfaction for democracy entails minimal effects on the overall education effect. For each specification, the negative effect of education on redistribution preferences is significant at 

#### 2.2.2 Controlling for future expected income

Higher education may affect expectations about future income. In turn, expecting a higher future income may reduce current support for redistribution. As such, educated survey respondents may *ceteris paribus* support a lower level of redistribution because they expect their income to rise. This represents an additional potential omitted variable bias.

Following Rueda et al (2014), we reconstruct income expectations based on the labor economics literature on life-cycle profiles. They can be estimated in a relatively simple way, following three steps. Firstly, we assume that income at any time is a function of skill development (proxied by years of education) as well as professional experience (proxied by the number of years of professional experience labeles PE). Secondly, controlling for country and time effects, we run



and store the estimated values of . Finally, we calculate expected future earnings up to the retirement age of 65 abstracting from future income discounting. We then add  as a control variable in the main specification. Table 5 shows that higher income expectations decrease support for redistribution for each specification. However, the overall effect of education on redistribution preferences stays similar.

#### 2.2.3 Matching to increase sample overlapping

Ideally, testing the causal effect of education requires that higher education be randomly assigned to control and treatment groups that are otherwise identical. In the absence of a full-scale randomized experiment, matching techniques can be used within an observational study to mimic the experimental method. Matching techniques date back to the attempts of Rubin (1973) and Hollande (1986) to address the issue of causality in observational studies. The basic idea is simple and powerful: to match untreated (low-educated) observations that are equal on relevant covariates, with treated (high-educated) observations. The underlying logic is that comparing individuals who are equal across all relevant covariates and only differ on the treatment variable is logically equivalent to comparing individuals randomly assigned to different treatments in an experiment (Dehejia and Wahba, 2002).

When treatment and control groups are unbalanced a simple regression model produces non-valid estimates of the average causal treatment effect. When there is some overlap between the control and treated group, the estimates of OLS or Logit models will not capture the effect of the treatment in non-overlapping segments of the data (Gelman and Hill, 2007). In the case of education, this problem can be severe (see Kam and Palmer (2011) and Persson (2016) who implement similar strategies to analyze the effect of education on political participation). As observed by Persson (2014), if the dataset lacks individuals with a low SES family background who gain higher education and individuals from high SES family backgrounds without higher education, the dataset lacks overlap. Our final model therefore uses a matching procedure to test , trading off representativeness for internal validity.

Matching estimators are based on the potential-outcome model, in which each individual has a well-defined outcome for each treatment level. In the binary-treatment potential-outcome model,  is the potential outcome obtained by an individual if given treatment level 1 and  is the potential outcome obtained by each individual  if given treatment-level 0.

The problem posed by the potential-outcome model is that only  or  is observed, never both.  and  are realizations of the random variables  and  with  subscripts denoting realizations of the corresponding, unsubscripted random variables. ATE is then computed as



More formally, consider the vector of covariates  for observation  (abusing notation, we abstract from clustering and time dimensions). The distance between  and  is parameterized by the vector norm



where  is a given symmetric, positive-definite matrix. We find that the set of nearest-neighbor indices for observation  restricted to one in our case unless ties apply, is



where hence  and where the metric chosen for the scaling matrix  is the Malhanobis distance.

**Table 4.** Omitted variable bias: Increasing controls.

|  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- |
|  | (*i*) | (*ii*) | (*iii*) | (*iv*) | (*v*) | (*vi*) |
|  | Redistribution: | | | | | |
|  | () Support ; – ; () Oppose | | | | | |
| University (0-1) | .273 | .258 | .118 | .120 | .120 | .135 |
| SE | (.028) | (.026) | (.024) | (.026) | (.028) | (.021) |
| Female (0-1) |  | -.135 | -.137 | -.127 | -.121 | -.114 |
| SE |  | (.020) | (.020) | (.019) | (.020) | (.020) |
| Age (25-99) |  | -.009 | -.008 | -.016 | -.014 | -.010 |
| SE |  | (.003) | (.002) | (.003) | (.003) | (.003) |
| Age squared |  | .000 | .000 | .000 | .000 | .001 |
| SE |  | (.000) | (.000) | (.000) | (.000) | (.000) |
| University: Father (0-1) |  |  | .137 | .109 | .110 | .098 |
| SE |  |  | (.014) | (.016) | (.016) | (.013) |
| University: Mother (0-1) |  |  | .034 | .034 | .031 | .036 |
| SE |  |  | (.017) | (.017) | (.018) | (.015) |
| Socioeconomic status: Father (0-1) |  |  | .046 | .045 | .040 | .021 |
| SE |  |  | (.016) | (.007) | (.007) | (.005) |
| Socioeconomic status: Mother (0-1) |  |  | .031 | .045 | .030 | .000 |
| SE |  |  | (.019) | (.028) | (.016) | (.017) |
| Immigration: Father (0-1) |  |  | -.013 | .016 | .013 | .019 |
| SE |  |  | (.017) | (.019) | (.022) | (.022) |
| Immigration: Mother (0-1) |  |  | -.024 | .005 | .018 | .005 |
| SE |  |  | (.016) | (.014) | (.015) | (.014) |
| Household income (1-10) |  |  |  | .043 | .040 | .035 |
| SE |  |  |  | (.005) | (.005) | (.004) |
| Subjective income (1-4) |  |  |  | .120 | .115 | .095 |
| SE |  |  |  | (.013) | (.012) | (.010) |
| Relative skill specificity (0-1) |  |  |  |  | -.044 | -.038 |
| SE |  |  |  |  | (.010) | (.009) |
| Labor market outsider (0-1) |  |  |  |  | -.045 | -.033 |
| SE |  |  |  |  | (.012) | (.010) |
| Long-run unemployment (0-1) |  |  |  |  | -.100 | -.070 |
| SE |  |  |  |  | (.023) | (.020) |
| Left-right scale (1-10) |  |  |  |  |  | .103 |
| SE |  |  |  |  |  | (.012) |
| Political interest (1-4) |  |  |  |  |  | .007 |
| SE |  |  |  |  |  | (.005) |
| Religion (0-1) |  |  |  |  |  | -.020 |
| SE |  |  |  |  |  | (.025) |
| Satisfaction for Democracy (1-10) |  |  |  |  |  | .027 |
| SE |  |  |  |  |  | (.005) |
| N.Obs | 170,651 | 169,945 | 130,952 | 130,952 | 109,214 | 99,181 |
| R-squared | .10 | .10 | .13 | .13 | .14 | .18 |
| Country effects | yes | yes | yes | yes | yes | yes |
| Time effects | yes | yes | yes | yes | yes | yes |
| Standard errors clustered at country level are reported in parentheses after  estimation. Source | | | | | | |

**Table 5.** *Omitted variable bias*: Considering future expected income*.*

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
|  | (*i*) | (*ii*) | (*iii*) | (*iv*) |
|  | Redistribution: | | | |
|  | () Support ; – ; () Oppose | | | |
| University (0-1) | .273 | .108 | .110 | .129 |
| SE | (.028) | (.025) | (.027) | (.021) |
| Female (0-1) |  | -.151 | -.149 | -.114 |
| SE |  | (.022) | (.024) | (.020) |
| Age (25-99) |  | .004 | .004 | .006 |
| SE |  | (.003) | (.003) | (.003) |
| Age squared |  | .000 | .000 | -.000 |
| SE |  | (.000) | (.000) | (.000) |
| University: Father (0-1) |  | .094 | .096 | .090 |
| SE |  | (.017) | (.016) | (.013) |
| University: Mother (0-1) |  | .019 | .024 | .033 |
| SE |  | (.015) | (.015) | (.014) |
| **Future income** |  | .107 | .097 | .110 |
| SE |  | (.020) | (.023) | (.025) |
| Household income (1-10) |  | .110 | .104 | .088 |
| SE |  | (.015) | (.015) | (.013) |
| Subjective income (1-4) |  | .106 | .100 | .093 |
| SE |  | (.020) | (.009) | (.009) |
| Relative skill specificity (0-1) |  |  | -.051 | -.046 |
| SE |  |  | (.011) | (.010) |
| Labor market outsider (0-1) |  |  | -.052 | -.036 |
| SE |  |  | (.014) | (.012) |
| Long-run unemployment (0-1) |  |  | -.104 | -.078 |
| SE |  |  | (.024) | (.024) |
| Left-right scale (1-10) |  |  |  | .115 |
| SE |  |  |  | (.012) |
| Political interest (1-4) |  |  |  | .000 |
| SE |  |  |  | (.006) |
| Religion (0-1) |  |  |  | -.031 |
| SE |  |  |  | (.025) |
| N.Obs | 170,651 | 101,822 | 86,599 | 79,546 |
| R-squared | .10 | .13 | .14 | .18 |
| Country effects | yes | yes | yes | yes |
| Time effects | yes | yes | yes | yes |
| Standard errors clustered at country level reported in parentheses after  estimation. Source | | | | |

### 2.3 Measurement errors

We identify two potential measurement errors that may jeopardize the validity of our estimates. Firstly, while we treat our dependent variable as a continuous one, it is actually an ordered one. A non-linear model may therefore provide a better fit to the true underlying data-generating process. In this regards, we fit model  in Table 2 through an Ordered logit. Table 6 shows that the coefficient of main interest is positive and significant at  ruling out the possibility that the main result is upward biased by OLS.

Secondly, we rely on a specific measure of redistribution preferences that respondents may understand differently depending on their level of education. For robustness, we select another proxy relating to redistribution. It reads:

• [*Fairness*] For fair society, differences in standard of living should be small . (1): Agree, ... , (5): Disagree.

The effect of education on this alternative dependent variable coefficient is reported in column (). The effect of education using this alternative dependent variable is close as the one reported in the main text.

**Table 6.** Measurement issues.

|  |  |  |
| --- | --- | --- |
|  | (*i*) | (*ii*) |
|  | Redistribution | Fairness |
|  | Ordered Logit | OLS |
|  | () Support ; – ; () Oppose | () Support ; – ; () Oppose |
| University (0-1) | .266 | .039 |
| Std Error | (.015) | (.014) |
| N.obs | 103,826 | 25,755 |
| Standard errors clustered at country level are reported in parentheses. Source: | | |

## 3 Conditional effects

### 3.1 Cross-country comparison

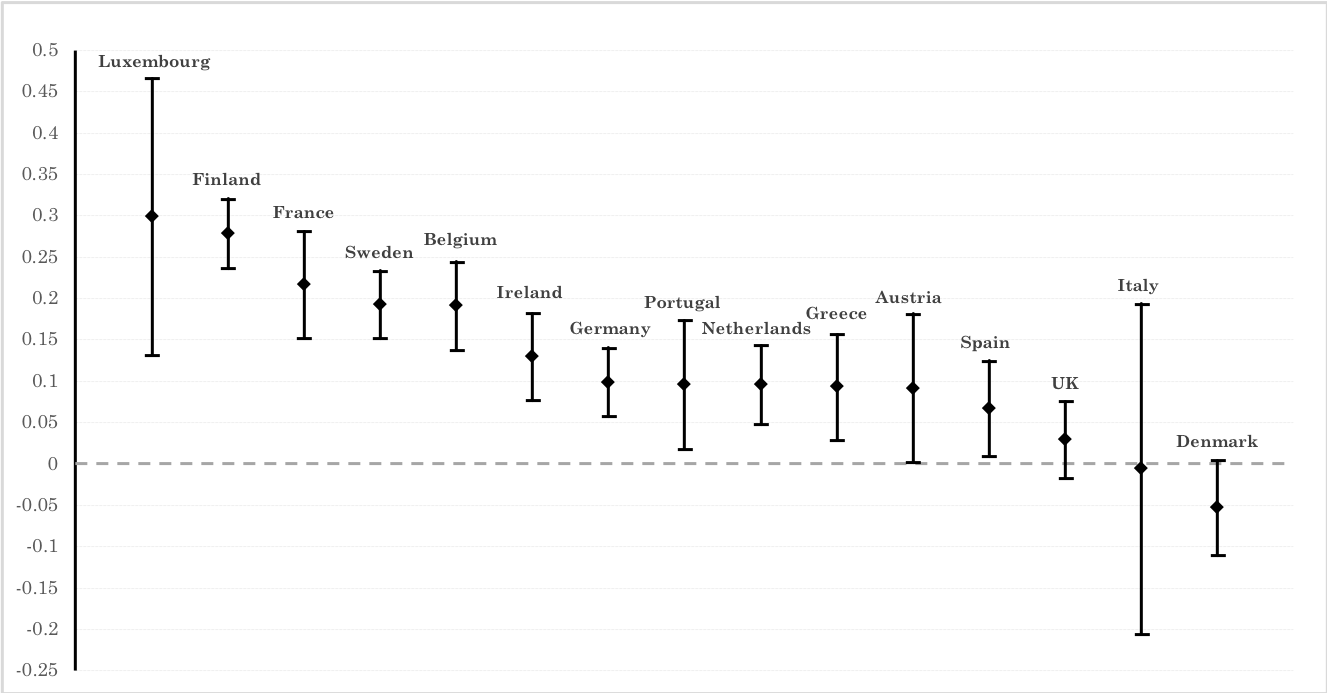
The relationship between education and support for redistribution does not hide large heterogeneity at the country level. Figure 2 ranks the EU-15 according to the strength of the education effect on support for redistribution. The figure plots coefficients and % confidence intervals. It shows that the education effect is positive and significant at  in  countries, amounting 80% of the sample. It is positive and significant at  in the UK, while it is non significant in Italy and Denmark.

We can see that the extent to which education - net of economic security - reduces support for redistribution does not correspond to established institutional typologies such as the ‘three worlds’ of welfare capitalism (Esping-Andersen, 1990) or the ‘four worlds’ of education finance (Garritzmann, 2016). While it is beyond the scope of this paper to explore how institutional context affects the relationship between education and support for redistribution, disaggregating our analysis shows that our main result is surprisingly regular across countries, and not driven by outliers.

### 3.2 Fields of studies

The ESS does not provide information about subjects studied, and even at the country-level the necessary information is extremely scant. We download data from Eurostat on enrollment by field of study, and create attitudinal scores on the dependent variable ‘redistribution’ by collapsing ESS data at the country-year. Our dependent variable is thus ‘redistribution’ at the country-year level. Our main independent variable is the share of the population enrolled in any of the following fields: Economics, Finance, banking and insurance, Management and administration, and Marketing and advertising. We control for the overall rate of enrollment in tertiary education, GDP, GDP growth, Population,  spending and Gini index. The analysis is necessarily limited to = 34. Table 7 shows that a higher rate of student enrollment in any of the aforementioned fields decreases support for redistribution. The result is significant for most specifications. However, as the erratic evolution of regression coefficients printed in Table 7 signals, the statistical power is rather limited, and results should be taken with caution.

**Figure 2.** A comparative look: Education and redistribution, by country.



**Table 7.** Effect of business and economic studies on redistribution*.*

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
|  | (*i*) | (*ii*) | (*iii*) | (*iv*) |
|  | Redistribution: | | | |
|  | () Support ; – ; () Oppose | | | |
| Share of business graduates | .005 | .111 | .211 | 2.184 |
| SE | (.102) | (.054) | (.115) | (.289) |
| GDP |  | 1.054 | 2.828 | -14.395 |
| SE |  | (.032) | (.928) | (2.684) |
| GDP growth |  | .024 | .071 | .848 |
| SE |  | (.036) | (.056) | (.104) |
| Population |  | 3.604 | -17.281 | -236 |
| SE |  | (2.752) | (7.561) | (29.838) |
| Gini index |  |  | .036 | -.291 |
| SE |  |  | (.124) | (.093) |
| R&D spending |  |  | .339 | -1.682 |
| SE |  |  | (.189) | (.330) |
| *ESS* Share of graduates |  |  |  | 3.585 |
| SE |  |  |  | (.288) |
| Gender gap in employment |  |  |  | 6.389 |
| SE |  |  |  | (.458) |
| N.Obs | 37 | 34 | 29 | 29 |
| Country effects | yes | yes | yes | yes |
| Time trend | yes | yes | yes | yes |
| Standard errors reported in parentheses after  estimation. Source  and | | | | |