Human Capital and Climate Change

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Abstract

Addressing climate change requires individual behavior change and voter support for pro-climate policies, yet surprisingly little is known about how to achieve these outcomes. In this paper, we estimate causal effects of additional education on pro-climate outcomes using new compulsory schooling law data across 20 European countries. We analyze effects on pro-climate beliefs and behaviors, as well as novel data on policy preferences and voting for green parties. Results show that a year of education substantially increases pro-climate beliefs, behaviors, and policy preferences.

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I Introduction

The costs and consequences of climate change are enormous and multifaceted (Carleton and Hsiang, 2016; Graff Zivin and Neidell, 2013; Intergovernmental Panel on Climate Change, 2022; Isen, Rossin-Slater and Walker, 2017; Park, Behrer and Goodman, 2021), with monetized impacts estimated to be as large as 20% of annual global GDP within a generation (Nordhaus, 2007). On current trajectories, the world is on track to experience 2.7°C warming above pre-industrial levels within the next century, far above the global goal of 1.5°C (Climate Action Tracker, 2022). Individual behavior change and government policy are needed to dramatically alter the trajectory of emissions. Despite the urgency and scale of the challenge, current efforts are underwhelming, in part because sizable populations around the globe remain skeptical about climate change and policies to tackle it (Bechtel, Scheve and van Lieshout, 2020; Dechezleprêtre et al., 2022; Sunstein et al., 2017). Surprisingly little is known about how to overcome such resistance.

One promising approach is the accumulation of human capital through increased educational attainment.[1] More educated individuals may be better equipped to understand the complexities of climate science and trust science more in general, as well as have more awareness of the climate change risks. Descriptive correlations suggests this might be true: a global survey found people with more education were more likely to see climate change as a major threat (Pew Research Center, 2019). More education might also yield transferable skills across occupations, encouraging voting for policies which promote new industries, such as renewable energy subsidies. Yet determining the causal effect of human capital accumulation on pro-climate beliefs and behaviors is challenging. A host of variables confound the relationship between human capital and climate outcomes, such as family background. In addition, people who choose to pursue more education are, by revealed preference, forward looking and thus more concerned with the future consequences

[1] Human capital captures an individual's knowledge and skills (Becker, 1962) and is typically measured by education metrics including years of schooling (Barro, 2001) and learning (Angrist et al., 2021).

of climate change. It might not be education that is causing pro-climate beliefs and actions, but rather time preferences. Reverse causality is another challenge: individuals who believe in climate change might choose to pursue more education to better adapt to a changing world.

In this paper, we overcome causal inference challenges by assembling a new database on compulsory schooling laws (CSLs) to estimate the causal effect of human capital accumulation on a series of climate outcomes in Europe. The use of CSLs as a plausibly exogenous shift in educational attainment has a rich tradition in labor and health economics (Angrist and Krueger, 1991; Black, Devereux and Salvanes, 2008; Brunello, Fort and Weber, 2009; Gathmann, Jürges and Reinhold, 2015; Goldin and Katz, 1997; Lleras-Muney, 2005; Oreopoulos, 2006), but is much more limited on climate.[2] Moreover, due to data limitations, studies have been largely limited to single countries. We build on this nascent climate literature leveraging 41 CSL reforms in 20 countries, identified via a new reforms database. In addition, studies to date analyze limited outcomes. We study new climate outcomes which extend well beyond standard measures of beliefs and behaviors, also examining the highly consequential domains of policy preferences and voting.

Europe is an ideal setting for this study. Countries in Europe enacted dozens of education reforms in the twentieth century, expanding the number of years of education legally mandated through compulsory schooling laws. Europe also has large, harmonized multi-country surveys, enabling credible within- and cross- country analyses, with recent climate modules added to the European Social Survey (ESS) which we analyze in this study. Moreover, Europe has a robust green party movement with an explicit environmental agenda.[3] We codify a novel dataset of green party voting, enabling identification of pro-climate voting behavior.

Our analysis focuses on outcome indices as well as on specific indicators within each index,

[2]A small set of studies explore environmental outcomes (Meyer, 2015; Powdthavee, 2021). Baiardi and Morana (2020) and Hornsey et al. (2016) explore correlations between demographics and climate beliefs.

[3]Green parties' environmental focus includes climate change, pollution, and industrial agriculture.

including comparing correlations and causal estimates. We find significant impacts on nearly all pro-climate measures. Our headline results show that an additional year of education leads to an increase of 1.9 percentage points (PP) in pro-climate beliefs, 3.0 PP in behaviors, 0.8 PP in policy preferences, and 0.3 PP in green voting. Relative to status quo rates, these impacts are non-trivial, translating into 2.9% increase for beliefs, 4.3% for behaviors, 1.3% for policy preferences, and a 4.3% increase for green party voting. Effects on beliefs, behaviors, and policy preferences have high statistical significance, with p-values below <0.001. Average effects on voting are imprecise, although effects are large and statistically significant for specific countries.

These results are notable since education has been conspicuously absent from most major climate change discussions.[4] Our results show that human capital accumulation can play an important role in shaping beliefs about the costs and benefits of policies to reduce emissions (Dechezleprêtre et al., 2022) and extend directly to consequential outcomes such as policy preferences. This motivates renewed focus on policies expanding access to general education as part of the menu of approaches considered in tackling climate change.

The rest of the paper is organized as follows. The next section describes our data. Section III details our empirical strategy and Section IV presents our results. Section V concludes.

II Data

Data on pro-climate outcomes – including beliefs, behaviors, policy preferences, and voting outcomes – come from the European Social Survey (ESS).[5] The ESS is conducted biennially across dozens of European countries using stratified random sampling with a total sample size ranging from 20,000 to 40,000 individuals per round. The ESS is a large microdata set capturing informa-

[4] A recent analysis showed that only 24% of countries mention youth education in the context of the Paris Agreement (Kwauk, 2021) – a historic international treaty on climate change.

[5] European Social Survey European Research Infrastructure (ESS ERIC). (2020). ESS8 - integrated file, edition 2.2 [Data set]. Sikt - Norwegian Agency for Shared Services in Education and Research. https://doi.org/10.21338/ESS8E02_2

tion on a host of social issues and is harmonized over time and across countries. In 2016, the ESS introduced novel questions on climate outcomes, such as "how often do you do things to reduce energy use?" and "how likely are you to buy energy efficient appliances?" Moreover, the ESS collected data on policy preferences such as "to what extent are you in favour or against using public money to subsidise renewable energy such as wind and solar power?" Finally, we codify data on voting for green parties since 2002. Europe has a thriving green party movement in 32 countries. We codify a novel dataset of "green voting" across Europe based on party platforms. Many political parties around the world have broad mandates, and are thus too general to explore specific climate voting patterns. In contrast, green parties have an explicit environmental agenda, enabling identification of pro-climate voting.[6]

Table 1 shows the climate outcomes we consider in our analysis and Table A2 in the Online Appendix includes the parties we classify as "green" in each country. Each climate outcome is transformed into a binary 'pro-climate' indicator if the response is equal to or above the median. For example, a response is 'pro-climate' if the respondent answered "strongly in favor" or "somewhat in favor" when asked about policies to subsidize renewable energy, since the median response is "somewhat in favor". Alternatively, we also consider a continuous outcome, where 1 is the most pro-climate response and 0 is the least.

In addition to analyzing individual outcomes, we aggregate climate outcomes into three indices: beliefs, behaviors, and policy preferences. Table 1 lists each question and denotes the index to which it belongs; indices are simple within-individual averages. Our main results also include an indicator for whether respondents voted for a member of a green party in the last election for countries where such a party exists.

[Table 1 here]

We restrict our analysis to respondents at least 25 years old at the time they were surveyed to capture effects for those who have completed their schooling. In particular, we analyze outcomes

[6]Green voting outcomes are included using data from all ESS rounds, whereas all other climate outcomes are only included in 2016.

for cohorts who received schooling and were affected by education reforms in the 1960s through the 1980s and were adults being surveyed in the ESS from 2002 to 2018. In addition to climate and voting outcomes, the ESS data contains birth year and years of education for every individual, which are critical to mapping climate outcomes to cohorts of students affected by compulsory schooling laws, and who in turn experienced exogenous shocks to their educational attainment.

To examine the causal effect of education on climate outcomes, we leverage a new World Bank dataset on compulsory schooling laws (CSLs) in Europe. Europe has had dozens of education reforms throughout the twentieth century expanding the number of years of education legally mandated through compulsory schooling laws. Figure A1 in the Appendix includes a map of the number of compulsory schooling law reforms which can be mapped to the ESS data over this time period. For each CSL, we have information on the year it was passed, the year it came into effect, and the new minimum schooling requirement under the law. For most CSLs, we also have the school starting age, and assume this to be 6 years – the most common school starting age – for CSLs for which it is missing; this lets us calculate the birth year of the first affected cohort. We identify the CSL which applies to each respondent by finding the CSL that is applicable to their birth year cohort in a given country.

Together, these two unique datasets enable us to identify exogenous shocks to education which can be mapped directly onto climate outcomes including beliefs, behaviors, policy preferences, and voting.

III Empirical Strategy

III.A Compulsory Schooling Laws as an Instrument

Compulsory schooling laws are commonly used in the economics literature as an instrument for educational attainment. We briefly review the necessary conditions for their use in our context. First, compulsory schooling must affect educational attainment. While this may seem obvious, we show in Section III.B that this relationship holds for many reforms, but does not necessarily

hold for all. Thus, as an additional specification, we follow Oreopoulos (2006), to carefully identify reforms which bind – that is, reforms which affect a large enough share of students to have a detectable increase in educational attainment. Our primary specification includes all reforms to alleviate concerns about restricting the analysis to a selected sample. Second, compulsory schooling must affect climate outcomes through the educational attainment channel, and not be confounded by other factors. Given that the passing of compulsory schooling laws is a national, exogenous shock, resulting gains in education are largely orthogonal to other factors that would otherwise make the individual schooling decision endogenous. For example, a potential confounding variable in the education-climate relationship is individuals' valuation of the future (e.g. their discount rates or degree of present bias), which can simultaneously motivate them to pursue education as an investment in their future, as well as be concerned about the future costs of climate change. Compulsory schooling laws overcome this confounder by mandating individuals to obtain greater educational attainment, regardless of these factors.

The plausibility of the assumption that CSLs affect climate outcomes only through the education channel is further bolstered by the fact that most of the possible effects of CSLs on other mediating factors, such as income, likely increase as a direct result of the education channel. This means our estimate is the bundled effect of education, including changes in income and other mediators, that come with an exogenous increase in schooling. In line with both of these points, Table A5 in the Online Appendix shows a strong first stage on education across most countries, while Table A6 shows no statistically significant effect on other variables which should not be affected by CSL changes and would not operate through the education channel, such as gender or country of birth.

Our estimation strategy instruments for years of education using a series of indicators for whether each compulsory schooling law is designed to take effect for a given cohort of individuals. We construct these indicators cumulatively, that is, the estimated effect of the current law is the marginal effect of the law relative to the prior law. We run a two-stage least squares regression where the second stage regresses our climate outcomes on predicted education based on the ap-

plicable compulsory schooling laws, controlling for time trends and country fixed effects.[7] For a given individual i we estimate:

$$E_{icy} = \alpha_c + \beta_r \mathbf{CSL}_{icyr} + \delta T_y + \varepsilon_{icy} \tag{1}$$

$$Y_{icy} = \alpha_c + \beta_r \widehat{\mathbf{E_{icyr}}} + \delta T_y + \varepsilon_{icy} \tag{2}$$

where $\mathbf{CSL_{icyr}}$ is a binary indicator of whether an individual i in country c is a member of a cohort y affected by the reform r, and is therefore in the treatment group.[8] We estimate effects across multiple countries and reforms, with $\mathbf{CSL_{icyr}}$ representing a vector of binary indicators across all included reforms r.[9] In Equation (1) we estimate the first stage of the effect of CSLs on educational attainment E_{icy}. Since educational attainment has trended upward over time, we also condition on a time trend T_y.[10] We include country fixed effects δ_c given that we analyze results in a unified cross-country framework. Standard errors are clustered at the country-law (e.g., the CSL) level, which

[7] We further Winsorize educational attainment at the 1 percent level, to minimize outlier bias and address spurious coding in the ESS data of extreme values. With Winsorization, we have a minimum of two years of schooling and a maximum of 22 years. Without Winsorization, 414 respondents or 0.11% of our sample report at least 30 years of education, which clearly does not map to our standard notion of years of full-time education, even for individuals with a PhD, motivating Winsorization. Nevertheless, our results also hold when using raw years of education or topcoding at 20 years of education instead.

[8] This is defined based on each respondents' birth year and starting school age to derive when the reform would first take effect for a given individual.

[9] Omitted indicators are the earliest laws in each country, such that the earliest laws take the value of the country fixed effect, and each subsequent law has a positive β estimate as long as the reform i increased education relative to the country's time trend.

[10] Our estimates are robust to using linear, quadratic, and cubic time trends as well as completely flexible birth year fixed effects, implying that the functional form of the trend does not drive results.

is the level of treatment assignment. We estimate Equation (2), the causal effect of additional education on a given climate outcome Y_{icy}, with two-stage least squares, where the first stage is estimated from Equation (1) with educational attainment instrumented by CSL reforms.

This specification mirrors those most common in the economics literature (Acemoglu and Angrist, 2000; Lleras-Muney, 2005; Oreopoulos, 2006). It is important to note that these strategies all identify local treatment effects of education that are applicable to individuals on the margin of dropping out in the absence of the CSL. This is the policy-relevant estimate if the policy in question is to increase minimum schooling requirements.

In a corollary estimation in Table A3, we also estimate effects per country on green party voting using an interaction term between country and instrumented educational attainment. Country specific effects are most relevant for green party voting outcomes given the emergence and growth of green parties varied across countries and over time.

In the spirit of Callaway and Sant'Anna (2021), we can also estimate Equations (1) and (2) separately for each country and average estimates across countries into a single point estimate.[11] This procedure addresses potential concerns around negative weighting if treatment timing differs across countries when including all countries in a single regression (Callaway and Sant'Anna, 2021; de Chaisemartin and D'Haultfoeuille, 2022; Goodman-Bacon, 2021; Sun and Abraham, 2021). In the aggregation step, we weight each country proportionally to the inverse of the variance of its estimate, meaning that more precisely estimated treatment effects receive more weight. These estimates are reported as "country-averaged treatment effects" or "country-avg TE" and are very similar to standard IV estimates using the pooled regression (correlation 0.87), revealing robustness of the main estimation to cross-country treatment timing (see Online Appendix Figure A4).

[11] We thank Pedro Sant'Anna for his useful guidance on robust approaches to aggregating estimates across countries in a manner which addresses negative weighting issues highlighted in the difference-in-differences literature.

III.B First Stages: the Effect of CSLs on Education

Compulsory Schooling Laws (CSLs) legally mandate an increase in educational attainment, often by raising the minimum school leaving age. For example, in 1963, Italy increased minimum schooling from 5 years of education to 8 (equivalent to increasing the minimum school leaving age from 11 to 14 years old). As an additional robustness check, we carefully identify reforms for which there is a binding first stage – that is, where an increase in required years of schooling by CSLs increases average educational attainment, net of the time trend. While legally enforceable, changes to CSLs will only have a strong first stage if they are enforced, rolled out rapidly, and bind for those who would otherwise not proceed to attain more schooling without the law (e.g., some individuals may attain 8 years of education in Italy even before it was legally required).

Figure 1 maps the 20 countries with CSL reforms that apply to our survey sample. There are 41 reforms total, with some countries having several reforms. Online Appendix Figure A2 shows results are robust to restricting to reforms with positive and statistically significant first stages. Table A4 shows the minimum schooling attained in each country and Table A5 in the Online Appendix shows all first stages estimates with positive effects, including those that are not statistically significant. Countries in the main analysis include Austria, Belgium, Switzerland, Czechia, Germany, Estonia, Spain, Finland, France, Hungary, Iceland, Ireland, Italy, Lithuania, Netherlands, Norway, Poland, Portugal, Russia, and Sweden. We exclude countries, such as the United Kingdom, where reforms occurred at sub-national levels and do not map cleanly to the ESS data.

[Figure 1 here]

IV Results

Results on our three main pro-climate indices - beliefs, behaviors, and policy preferences - as well as green voting are shown in Table 2. An additional year of education leads to highly statistically significant increases of 1.9 percentage points (PP) in pro-climate beliefs, 3.0 PP in behaviors, 0.8 PP in policy preferences, and a nonsignificant 0.3 PP increase in green voting. These impacts translate into a 2.9% increase for beliefs, 4.3% for behaviors, 1.3% for policy preferences, and 4.3% for green party voting. Panel B of Table 2 shows the results with continuous outcomes to ensure results are not driven by binary threshold values defined as being "pro-climate"; results remain consistent. Point estimates are positive and p-values also follow a similar pattern. For example, an additional year of education has large and statistically significant effects on pro-climate beliefs and behaviors, with p-values below < 0.001 in both panels. Of note, while effect directions and statistical significance can be compared, the magnitudes in Panels A and B are not directly comparable.[12] In Online Appendix Figure A2 we include a series of robustness tests, such as various time trends and restrictions to positive and significant first stages. Results show consistently large and positive effects of education on pro-climate beliefs, behaviors, and policy preferences. Appendix Figure A3 additionally shows the robustness of these estimates to the inclusion of particular countries or reforms by plotting the distribution of leave-one-out β estimates.[13]

We further examine green party voting effects by country in Table A3. Country specific effects on voting are of particular interest given the growth of green parties varied substantially across countries and over time. While average effects on voting are imprecisely estimated, this is due to important underlying heterogeneity. Country-specific results show small, imprecise effects in half

[12] In Panel A, a one unit change in the outcome is the difference between being below and above median, whereas in Panel B, a move from 0 to 1 means changing from the most anti-climate response to the most pro-climate.

[13] We can also benchmark these results to the effect of education on income, and find a similar sized effect, 1.8%, on respondent income being above median.

of countries, with the other half showing positive, statistically significant effects of education on green party voting, including in Austria, Belgium, Czechia, Germany, Denmark, Finland, France, Hungary, and Sweden, with effect sizes ranging from 2.0 PP points to 4.9 PP.

In Figure 2, we compare the causal effects derived from IV estimates on the three pro-climate indices and green voting to their corresponding OLS correlation estimates, expressed in terms of standard deviations for comparability between outcomes.[14] In Figure 2 and Table 3 we analyze outcomes using binary indicators for ease of interpretation. Results are similarly robust whether using binary or continuous outcomes. The gains shown in Table 2 translate to 0.072 standard deviation increase for pro-climate beliefs, a 0.096 increase for behaviors, a 0.026 increase for policies, and a 0.013 increase for green party voting. Moreover, IV causal estimates are substantially larger than OLS estimates for beliefs and behaviors. One important potential explanation for these larger causal estimates is downward bias in the OLS estimates due to income effects. More educated individuals are often richer, and richer individuals are often more conservative – a standard assumption in political economy models (Meltzer and Richard, 1981) – and thus might be less pro-climate. Indeed in Table A1 in the Online Appendix we see correlations along these lines. The substantial increase in causal IV estimates relative to OLS estimates for these outcomes highlights the importance of credible causal identification of the effects of education on pro-climate outcomes.

[Table 2 here]

[Figure 2 here]

[14] To ensure comparability between IV and OLS estimates, both OLS estimates and IV estimates include the same fixed effects and time trend specifications.

While Table 2 shows our primary results, the panels of Table 3 break down each of the indices into their components, showing positive and significant estimates on most sub-outcomes. In terms of specific indicators, on beliefs, we find one year of education causes a 2.8 percentage point increase in thinking the world's climate is changing, with somewhat smaller effects on thinking that climate change has a bad impact, worrying about climate change and worrying about dependency on fossil fuels. In terms of behaviors, we find 2.7 and 2.9 percentage point increases in reducing energy use and buying energy efficient appliances, respectively, with a 3.3 PP increase in having thought about climate change before today. For policy preferences, we find a 1.1 PP increase in favoring bans on the sale of inefficient appliances and a 2.1 PP increase on favoring subsidies for renewable energy. In contrast, we find a null or even slightly negative effect on preferences to increase taxes on fossil fuels, a result that attenuates our policy index despite two of the three components being strongly positive. It is plausible that respondents either fail to see the equivalence between taxes and subsidies or that policy preferences about taxation are generally more strongly held and are less malleable.

[Table 3 here]

V Conclusion

Climate change poses existential risks to the planet and generates trillions of dollars in annual costs to society. While changing pro-climate beliefs, behaviors, and policy preferences is difficult, one approach that can move the needle is additional education. This paper provides strong causal evidence that education can impact a range of pro-climate outcomes. We find that an additional year of education is linked with increases in pro-climate beliefs, behaviors, and most policy preferences – all highly consequential pro-climate outcomes which are notoriously difficult to change. At the same time, we find heterogeneous effects on voting for green parties, with large effects in some countries and small effects in others.

While education is often a footnote in climate change agendas, this paper reveals the promise

of education as an additional tool to influence a set of key climate change outcomes. This is not to suggest education should replace other potent climate change tools; rather it can be added to the arsenal. Europe in particular is a context where climate change is receiving substantial attention, including efforts such as the European Green New Deal, yet education remains an underutilized lever. Moreover, while educational attainment has expanded dramatically in recent decades, the median school reform law in 2020 in Europe guaranteed only 10 years of schooling, a full two years below what is often considered a complete primary and secondary education of 12 years.[15] These gaps are even more dramatic in low- and middle-income countries; in sub-Saharan Africa educational reform laws only guarantee 8 years of schooling on average. Expanding access to education has traditionally been believed to play a transformative role in the economic and social well-being of societies – it now also appears to play an important role in the battle against climate change.

[15] 12 years of high-quality education is often cited as a global goal by many international and development agencies during Sustainable Development Goal conversations.

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Tables and Figures

Table 1: **Climate Outcomes – Beliefs, Behaviors, Policy Preferences, and Voting**

Question	Beliefs	Behaviors	Policy	Voting
Do you think the world's climate is changing	✓			
Climate change good or bad impact across world	✓			
How worried about climate change	✓			
How much electricity should be generated from coal/hydro/solar	✓			
How worried too dependent on fossil fuels	✓			
How much thought about climate change before today		✓		
How likely to buy most energy efficient home appliance		✓		
How often do things to reduce energy use		✓		
Favor increase taxes on fossil fuels to reduce climate change			✓	
Favor subsidize renewable energy to reduce climate change			✓	
Favor ban of inefficient household appliances to reduce CC			✓	
Voted for green party in last national election				✓

Notes. Each outcome is grouped by index category. Each index is computed as an average for each individual across the indicated questions. The final outcome, green voting, is a stand-alone binary outcome not aggregated with others into an index. For beliefs about the source of electricity, we create a sub-index: the ESS has questions about individuals' opinions on electricity generation from coal, gas, hydroelectric, nuclear, solar, wind, and biofuel. Given these outcomes are highly inter-related, we average pro-hydroelectric, pro-solar, and anti-coal beliefs. We exclude indicators

which might be collinear with renewables captured by solar and hydro-electric, such as wind, as well as indicators with more ambiguous climate interpretations, such as nuclear.

Table 2: **The effect of education on pro-climate outcomes**.

	(1)	(2)	(3)	(4)
	Pro-climate beliefs	Pro-climate behaviors	Pro-climate policy preferences	Green voting
Panel A: indicators for above-median climate stance				
Years of education	0.019	0.030	0.008	0.003
	(0.005)	(0.004)	(0.003)	(0.008)
	[0.000]	[0.000]	[0.001]	[0.670]
Country-Avg Treatment Effect	0.023	0.043	0.010	0.005
Mean	0.652	0.703	0.632	0.080
Percent Change	2.9 %	4.3 %	1.3 %	4.3 %
Panel B: continuous pro-climate variables				
Years of education	0.012	0.017	0.007	0.003
	(0.003)	(0.003)	(0.002)	(0.008)
	[0.000]	[0.000]	[0.000]	[0.670]
Country-Avg Treatment Effect	0.010	0.024	0.005	0.005
Observations	33238	33238	32698	100474
Clusters	66	66	66	71
Mean	0.642	0.645	0.603	0.080
Percent Change	1.9 %	2.7 %	1.1 %	4.3 %

Notes: This table shows the causal effect of a year of education on each pro-climate outcome index, as in Equation (2). The outcome in Panel A denotes effects on being pro-climate defined in binary terms (relative to the median). Panel B shows averages of the continuous outcomes, where 1 is the most pro-climate response to each question and 0 is the least. Standard errors clustered by country$\times$CSL in parentheses. Standard errors are in parentheses and p-values are in brackets. Sample sizes vary due to variation in the availability of outcomes across survey questions. Green voting is available in multiple rounds of the survey, but only for select countries with green parties. 'Country-Avg Treatment Effect' refers to Callaway and Sant'Anna (2021)-inspired estimates.

Table 3: **Effect of education on each element of pro-climate outcome indices**.

	Climate Outcomes				
	(1)	(2)	(3)	(4)	(5)
Panel A: pro-climate beliefs	Think the world's climate is changing	CC has bad (not good) impact across world	Worried about CC	Pro-clean energy beliefs	Too dependent on fossil fuels
Years of education	0.028	0.021	0.016	0.005	0.016
	(0.008)	(0.011)	(0.007)	(0.003)	(0.006)
	[0.000]	[0.049]	[0.022]	[0.196]	[0.008]
Country-Avg TE	0.026	0.024	0.030	0.012	0.025
Observations	32631	31139	32059	32460	31971
Clusters	66	65	66	66	66
Mean	0.559	0.580	0.747	0.729	0.676
Percent Change	4.9 %	3.6 %	2.2 %	0.6 %	2.4 %

Panel B: pro-climate behaviors	Thought about CC before today	Likely to buy most efficient appliance	How often do things to reduce energy use
Years of education	0.033	0.029	0.027
	(0.007)	(0.009)	(0.007)
	[0.000]	[0.002]	[0.000]
Country-Avg TE	0.049	0.046	0.030
Observations	33093	32496	32869
Clusters	66	66	66
Mean	0.717	0.676	0.724
Percent Change	4.6 %	4.2 %	3.7 %

Panel C: pro-climate policy preferences	Favor increase taxes on fossil fuels to reduce CC	Favor subsidize renewable energy	Favor ban sale of inefficient household appliances
Years of education	-0.006	0.021	0.011
	(0.006)	(0.005)	(0.006)
	[0.314]	[0.000]	[0.079]
Country-Avg TE	-0.018	0.024	0.024
Observations	31913	32308	32089
Clusters	66	66	66
Mean	0.546	0.761	0.591
Percent Change	-1.1 %	2.8 %	1.8 %

Notes: This table shows point estimates for each of the elements of the indices. Panel A shows beliefs, Panel B shows behaviors, and Panel C shows policy preferences. Outcomes are binary, so multiplying the point estimate by 100 yields the percentage point increase in the likelihood of having a pro-climate outcome from a year of education. Standard errors are clustered at the country$\times$law level in parentheses and p-values are in brackets. Estimates include country fixed effects and time trends. "CC" means "climate change". 'Country-Avg TE' refers to Callaway and Sant'Anna (2021)-inspired estimates.

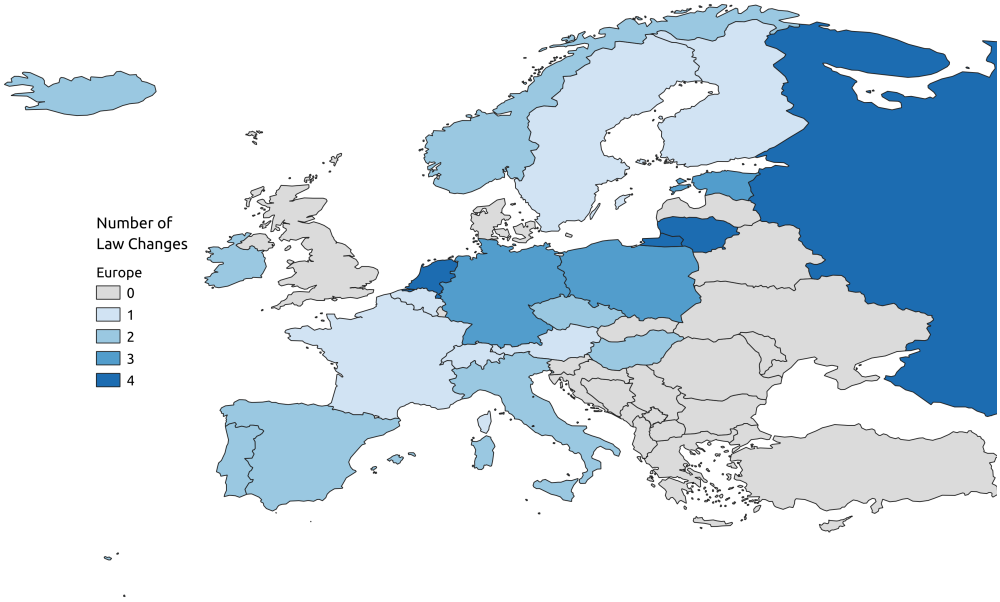

Figure 1: **Compulsory schooling law changes by country**. This figure shows the number of compulsory schooling law changes by country. Countries in the main analysis include Austria, Belgium, Switzerland, Czechia, Germany, Estonia, Spain, Finland, France, Hungary, Iceland, Ireland, Italy, Lithuania, Netherlands, Norway, Poland, Portugal, Russia and Sweden.

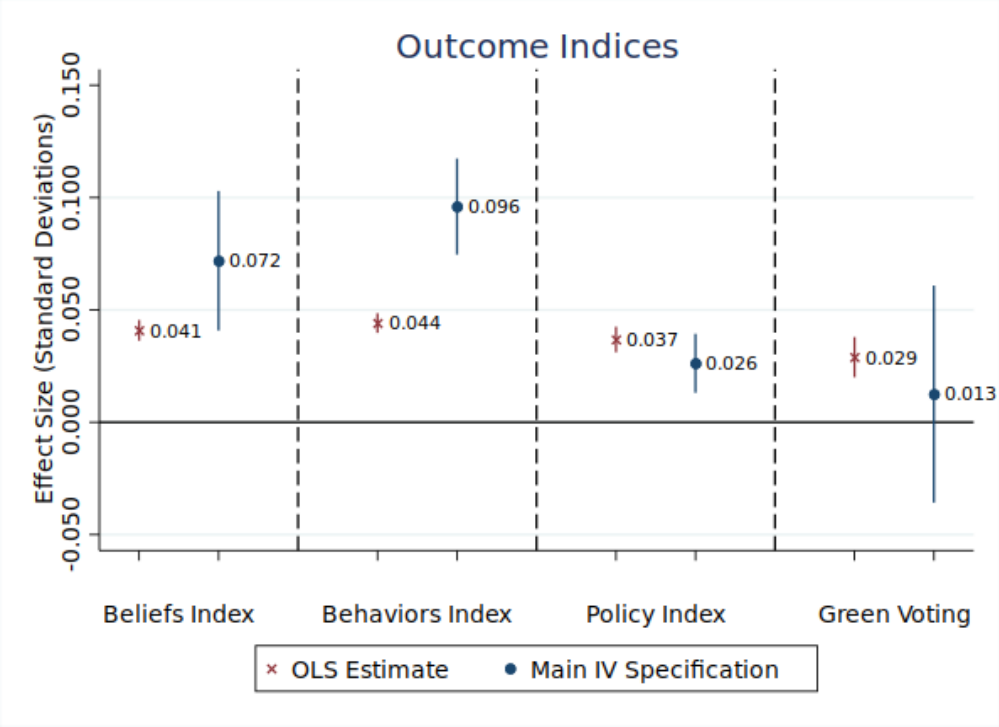

Figure 2: **Effects of Education on Pro-Climate Outcomes - Standardized causal estimates vs. correlations**. This figure plots estimates from our main IV specification which captures causal estimates compared to the OLS estimate which shows correlational estimates, both with a pooled linear time trend and country fixed effects. The OLS regression is restricted to the same sample as the IV. The indices are standardized and expressed in terms of standard deviations. 90% confidence intervals shown from standard errors clustered at the country×law level.

A Appendix

A.A Correlations between Education, Income, and Conservatism

Table A1 shows the correlations between education, income, and conservatism. As expected, income and schooling are positively correlated, as are income and conservatism. This might explain why causal IV estimates are higher than OLS correlations for belief and behavioural outcomes. OLS correlations between education and climate outcomes could potentially biased down since more educated individuals are wealthier and more conservative, which could attenuate the correlational relationship between education and climate outcomes.

A.B Green Party Coding and Results by Country

Table A2 shows the the parties identified as green across European countries. Additionally, Table A3 highlights heterogeneous effects of education on green voting by country.

Table A1: **Correlations between education, income, and conservatism**.

	Raw		Residualized	
	Schooling	Conservatism	Schooling	Conservatism
Income	0.388	0.071	0.313	0.077

Notes: This table shows correlation coefficients between income, schooling, and conservatism. Conservatism reflects where respondents self-report falling on a 0-1 scale where 1 is most right-leaning and 0 is most left-leaning on the political spectrum. Years of schooling is the Winsorized years of education attained, as in the main text. Lastly, income is the self-reported household income decile, normalized to fall on the 0-1 range. Raw correlations are simply the correlation coefficients in our main analysis sample. Residualized coefficients are the result of first residualizing income, schooling, and conservatism on country fixed effects and the pooled linear time trend as in the main analysis.

Table A2: **Green party coding**.

Country	Abbr.	Green Parties
Austria	AT	Grüne
Belgium	BE	Groen!, Ecolo
Switzerland	CH	Green Party
Cyprus	CY	The Cyprus Green Party
Czechia	CZ	Česká pirátská strana
Germany	DE	Alliance 90/The Greens
Denmark	DK	SF Socialistisk Folkeparti, Alternativet
Estonia	EE	Erakond Eestimaa Rohelised
Spain	ES	En Comú Podem, Iniciativa per Catalunya-Verds
Finland	FI	Green League
France	FR	EELV (Europe Ecologie Les Verts)
Hungary	HU	LMP (Lehet Más A Politika)
Ireland	IE	Green Party
Iceland	IS	Vinstri hreyfinguna - grænt framboõ
Lithuania	LT	Lithuanian Green Party (LZP)
Latvia	LV	Zaļo un Zemnieku savienība
Netherlands	NL	Green Left
Norway	NO	Miljøpartiet De Grønne
Portugal	PT	PAN - Pessoas-Animais-Natureza
Sweden	SE	Miljöpartiet de gröna

Notes: An individual is coded as voting green if they reported voting for one of the listed parties in the last election. Missing responses and those from countries with no green parties in the relevant election are coded as missing. Those who voted for a different party in countries with green parties at the time are coded as not voting green.

Table A3: **Green party voting results by country**.

	(1)		
	Vote Green		
	b	se	p
AT × Years of education	0.049	(0.012)	[0.000]
BE × Years of education	0.020	(0.009)	[0.029]
CH × Years of education	0.048	(0.017)	[0.005]
CY × Years of education	0.006	(0.005)	[0.238]
CZ × Years of education	0.015	(0.051)	[0.769]
DE × Years of education	0.048	(0.009)	[0.000]
DK × Years of education	0.025	(0.010)	[0.010]
EE × Years of education	0.007	(0.010)	[0.526]
ES × Years of education	0.002	(0.005)	[0.633]

FI × Years of education	0.037	(0.006)	[0.000]
FR × Years of education	0.020	(0.007)	[0.003]
HU × Years of education	0.027	(0.015)	[0.067]
IE × Years of education	0.008	(0.008)	[0.311]
IS × Years of education	0.001	(0.008)	[0.899]
LT × Years of education	0.006	(0.007)	[0.383]
LV × Years of education	-0.055	(0.015)	[0.000]
NL × Years of education	0.013	(0.008)	[0.106]
NO × Years of education	0.011	(0.009)	[0.208]
PT × Years of education	0.005	(0.004)	[0.268]
SE × Years of education	0.029	(0.009)	[0.001]
Observations	100474		

Notes: Effects of education on green voting by country. Results shown are coefficients from an IV regression with a pooled time trend, country fixed effects, and country by educational attainment interactions to recover country-specific treatment effects.

A.C First Stage Estimates

In this paper, we leverage a new dataset on compulsory schooling laws in Europe from the World Bank, which is one of the largest databases on CSLs to date. Figure A1 shows the number of compulsory schooling law reforms by country. Table A4 shows the share of students achieving minimum required schooling as well as the average years of schooling attained per country. Table A5 shows the first stage for each country and reform.

Table A4: **Share of CSL Compliance and Average Education by Country**.

Country	Share Achieving Min. CSL	Avg Education (Years)
AT	0.969	12.639
BE	0.934	13.816
CH	0.940	11.393
CZ	0.909	12.548
DE	0.960	14.479
EE	0.978	13.246
ES	0.907	12.675
FI	0.990	13.946
FR	0.848	12.428
HU	0.880	11.929
IE	0.995	14.550

IS	0.968	15.128
IT	0.941	11.322
LT	0.970	12.917
NL	0.927	13.790
NO	0.983	14.313
PL	0.989	12.553
PT	0.933	10.196
RU	0.989	13.138
SE	0.976	13.441

Notes: This table shows the share of respondents in the sample who report having attained at least the minimum level of schooling required of the CSL assigned by their year of birth and survey country. 95% of respondents in our sample achieve at least the minimum schooling required by their assigned CSL. The remaining 5% may have truly not achieved the legally mandated level of schooling despite the requirement. Alternatively, they may have been incorrectly assigned to a CSL, potentially because of migration out of their country of birth, exceptions to the compulsory schooling laws, or mischaracterization of the CSL rules. Average Winsorized educational attainment is also shown.

Table A5: **CSL changes with any education effect**[1]

Reform & Year	Estimate	Positive	Positive+Significant
AL8	2.079	✓	✓
1963	(0.271) [0.000]		
AT9	0.595	✓	✓
1966	(0.201) [0.004]		
BE8	1.217	✓	✓
1919	(0.122) [0.000]		
BG8	0.718	✓	✓
1960	(0.349) [0.042]		
CH9	0.069	✓	
1970	(0.209) [0.741]		
CY6	0.194	✓	

1962	(0.480) [0.687]		
CZ9	0.363	✓	✓
1948	(0.013) [0.000]		
DE13	0.502	✓	✓
1992	(0.044) [0.000]		
DE4	0.315	✓	✓
1920	(0.015) [0.000]		
DE8	0.981	✓	✓
1946	(0.026) [0.000]		
DK7	0.893	✓	✓
1958	(0.247) [0.000]		
DK9	0.109	✓	
1972	(0.310) [0.726]		

EE6	0.850	✓	
1920	(0.776) [0.276]		
EE8	0.729	✓	
1958	(0.666) [0.276]		
ES8	0.331	✓	
1970	(0.355) [0.353]		
FI6	1.136	✓	
1921	(1.399) [0.419]		
FR10	0.116	✓	
1967	(0.061) [0.059]		
HU10	0.568	✓	✓
1961	(0.173) [0.001]		
HU8	1.077	✓	✓

1945	(0.095) [0.000]		
IE9	0.182	✓	
1972	(0.203) [0.371]		
IS7	1.361	✓	✓
1936	(0.555) [0.016]		
IT8	1.040	✓	✓
1963	(0.516) [0.046]		
LT11	0.023	✓	
1980	(0.045) [0.607]		
LT5	1.894	✓	✓
1937	(0.051) [0.000]		
LT7	0.766	✓	✓
1953	(0.030) [0.000]		

LT8	1.477	✓	✓
1958	(0.082) [0.000]		
LT9	0.135	✓	
1980	(0.124) [0.276]		
LU10	0.717	✓	✓
1977	(0.098) [0.000]		
LU11	0.785	✓	✓
1993	(0.019) [0.000]		
LV5	1.176	✓	✓
1937	(0.055) [0.000]		
LV7	0.188	✓	✓
1953	(0.032) [0.000]		
LV8	0.736	✓	✓

1958	(0.088) [0.000]	
NL10	0.142	✓
1973	(0.162) [0.382]	
NL7	0.169	✓
1928	(0.292) [0.565]	
NL8	0.570	✓
1950	(0.289) [0.051]	
NL9	0.220	✓
1969	(0.173) [0.207]	
NO7	0.761	✓
1936	(0.772) [0.327]	
NO9	0.105	✓
1969	(0.809) [0.897]	

PL8	0.309	✓	
1966	(0.463) [0.506]		
PT6	1.170	✓	
1964	(0.804) [0.148]		
PT9	0.756	✓	
1986	(0.750) [0.316]		
RU5	1.482	✓	✓
1937	(0.039) [0.000]		
RU7	0.855	✓	✓
1953	(0.024) [0.000]		
RU8	0.651	✓	✓
1958	(0.086) [0.000]		
RU9	0.069	✓	

2004	(0.096) [0.472]		
SE9	0.357	✓	
1963	(0.864) [0.680]		
SK8	1.338	✓	✓
1948	(0.153) [0.000]		
SK9	0.709	✓	✓
1948	(0.145) [0.000]		
UA12	0.714	✓	✓
2002	(0.024) [0.000]		
UA5	1.575	✓	✓
1937	(0.077) [0.000]		
UA7	1.144	✓	✓
1953	(0.043) [0.000]		

UA8	0.474	✓	✓
1958	(0.141) [0.001]		
UA9	0.102	✓	
1996	(0.156) [0.517]		
Observations	315927		
F-statistic	147.8		

[1]Notes: This table shows first stage estimates for Equation (1) for each CSL that positively affects educational attainment. The point estimate is the effect on educational attainment following each CSL's implementation, controlling for country-specific linear time trends and country fixed effects. The numbers following each country code indicate the years of schooling required by each law (AL8 requires 8 years of schooling in Albania). The listed year is the first birthyear affected by the law. CSL changes not included in this table have nonpositive first stage estimates. Standard errors are in parentheses.

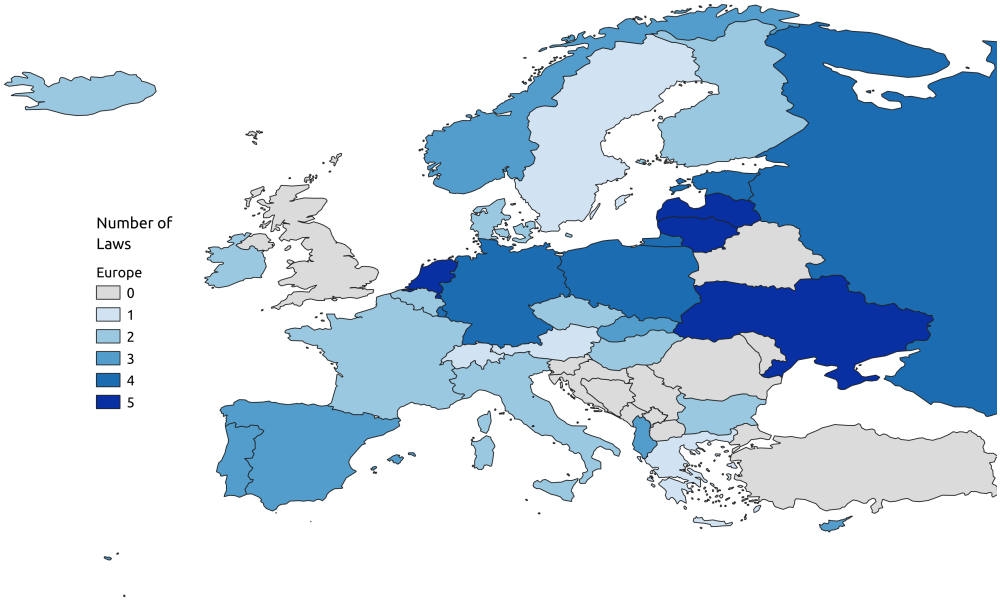

Figure A1: **Number of compulsory schooling laws (CSL) by country**. The map shows all CSLs that can be mapped to the ESS data. Note that a British reform commonly used in literature is excluded from our analysis, because this reform is region-specific and the ESS data does not have enough geographic granularity to accurately assign regional laws to respondent's individual level climate outcomes.

A.D CSL Validity Test

Table A6 shows a validity test providing evidence that the instrument (being born after a given reform) is not predictive of pre-determined outcomes like sex or birth country, consistent with a key validity assumption of instrumental variables analysis.

A.E Placebo Robustness Tests

To demonstrate instrument reliability, we conduct placebo tests in Table A7 – each observation is assigned the country and accompanying CSL assignments of another randomly drawn respondent in the sample. Then, the main results regressions are rerun using these placebo instruments and country assignment. Each outcome has a highly insignificant placebo point estimate under one percentage point, suggesting that the original identifying variation in our identification strategy is indeed coming from the correctly assigned CSL instruments.

[3] *Notes:* Regressions of indicators for male and being born in the same country as being surveyed in on the indicators for each CSL in the presence of country fixed effects and country-specific linear time trends.

Table A6: **Validity test**

	(1)	(2)
	Male	Born in Country
	b/se/p	b/se/p
any_reform	-0.003	0.013
	(0.012)	(0.014)
	[0.820]	[0.370]
Observations	314017	313907

Standard errors in parentheses. P-values in brackets.

Notes: This table shows the coefficient on the indicator for being after the first CSL change in a country while additionally controlling for country fixed effects and linear pooled time trends. "any reform" is zero for respondents in the sample under the first compulsory schooling law in the analysis window and one for all others. The outcomes are (1) an indicator for the respondent being male, (2) an indicator for being born in the country they are surveyed in. The small and nonsignificant estimates in Columns (1) and (2) suggest that the instrument is not predictive of other factors like gender and whether the respondent was born in the country in which they are surveyed,supporting the validity of the instrument, as CSL changes have no discernible effect on predetermined outcomes like gender and birth country. Note that while the ESS has plenty of other outcomes that could be tested in this manner, gender and birth location are the primary ones that we do not expect to be influenced by education, as these are determined before the amount of schooling is realized.

Table A7: **Placebo Results for Main Indices**.

	(1) Pro-climate beliefs	(2) Pro-climate behaviors	(3) Pro-climate policy preferences	(4) Green voting
Years of Education	0.007	0.005	0.001	0.006
	(0.007)	(0.009)	(0.007)	(0.007)
	[0.363]	[0.595]	[0.907]	[0.393]
Observations	33238	33238	32698	100474
Clusters	104	104	104	107
Mean	0.652	0.703	0.632	0.080

Notes: This table shows the IV estimates when assigning placebo instruments and countries for the four main outcomes.

A.F Robustness and Alternate Specifications

In this section, we consider the robustness of our estimates to several modeling decisions. We analyze results with all positive first stages and all positive and significant first stages. To establish the first stage, we estimate Equation 1 on all rounds of the ESS with standard errors clustered by country$\times$law[16]. In addition, we analyze results with alternative time trends such as quadratic and cubic time trends as well as completely flexible birth cohort fixed effects. Finally, rather than using indicators for compulsory schooling laws as the instrument for educational attainment, we use the current level of the minimum schooling requirement rather than a binary indicator, controlling for the upward time trends and country fixed effects. Figure A2 shows a plot of estimates across these robustness tests, showing broadly similar patterns and robustness. We see broadly consistent results across specifications; the positive and large effects of education on pro-climate outcomes persist. Figure A3 includes a set of additional robustness tests, assessing robustness to leaving out any one country or reform, also showing highly consistent results. Finally, Figure A4 shows the correlation between the standard IV estimates and the Callaway and Sant'Anna (2021)-inspired country-averaged treatment effect estimates. The correlation is 0.87, with both estimators closely aligned on all outcomes, suggesting that cross-country treatment timing is not a significant sources of bias in the main analysis.

[16]By using all rounds of the ESS to determine strong first stages, we have more power to estimate the true effect of compulsory schooling laws beyond the country's time trend.

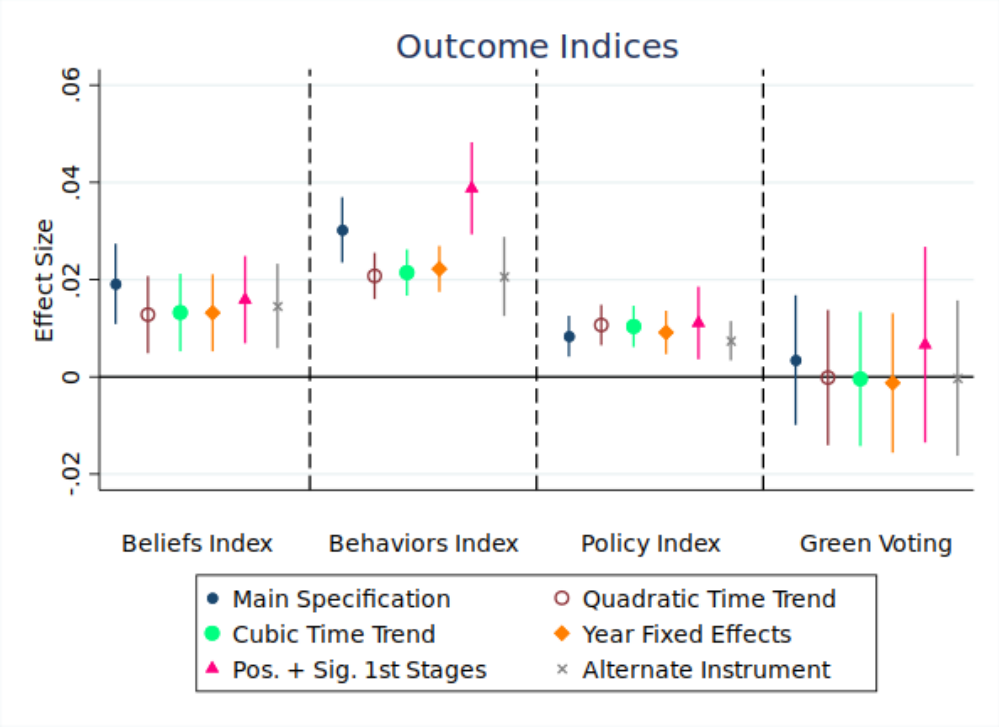

Figure A2: **Robustness checks**. This figure shows IV estimates for the four outcome indices under alternative time trend specifications and inclusion criteria for the first stage. The main specification is as in Section III (linear pooled time trend and country fixed effects for all CSLs in the sample). *Quadratic Time Trend* is the same as *Main* adds in a squared birth year term, while *Cubic Time Trend* also adds a cubic term. *Year Fixed Effects* uses year fixed effects as the time trend along with country fixed effects, pooling birth years earlier than 1920 with the 1920 cohort (less than 0.5% of the sample). *Pos. + Sig. 1st Stages* restricts the analysis to reforms with a positive and statistically significant first stage. *Alternate* is the secondary IV specification where the instrument is the number of years of schooling interacted with country. 90% confidence intervals shown from standard errors clustered at the country$\times$law.

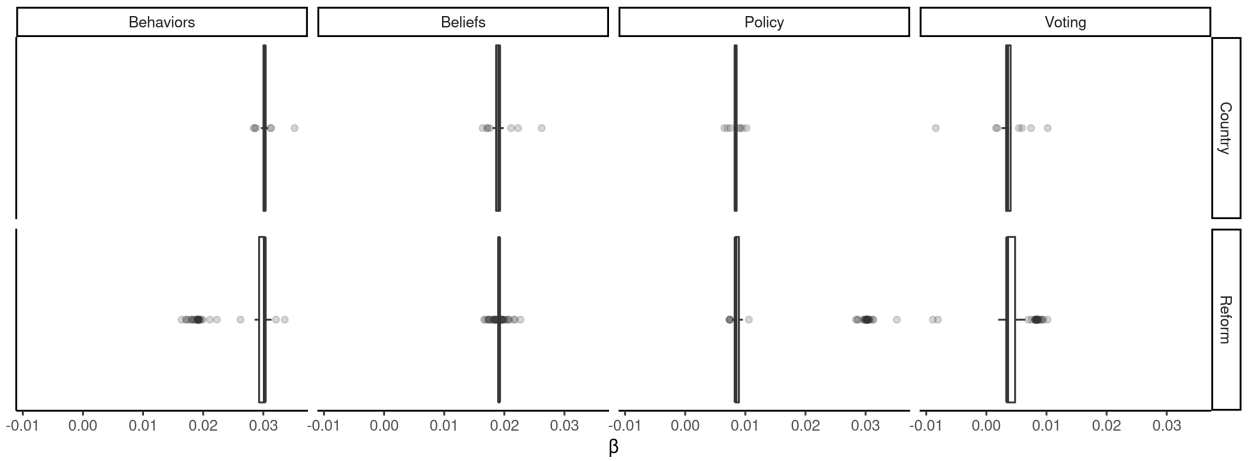

Figure A3: **Robustness to Leaving Out Countries or Reforms**. This figure shows boxplots of the distributions of β estimates for each of the four main indices when leaving out one country (top row) or all reforms from one country (bottom row). Nearly all estimates from this jackknife procedure remain positive, with the 25th-75th percentile clustered tightly around the main estimate, indicating that these results are not sensitive to the inclusion of any particular reform or country.

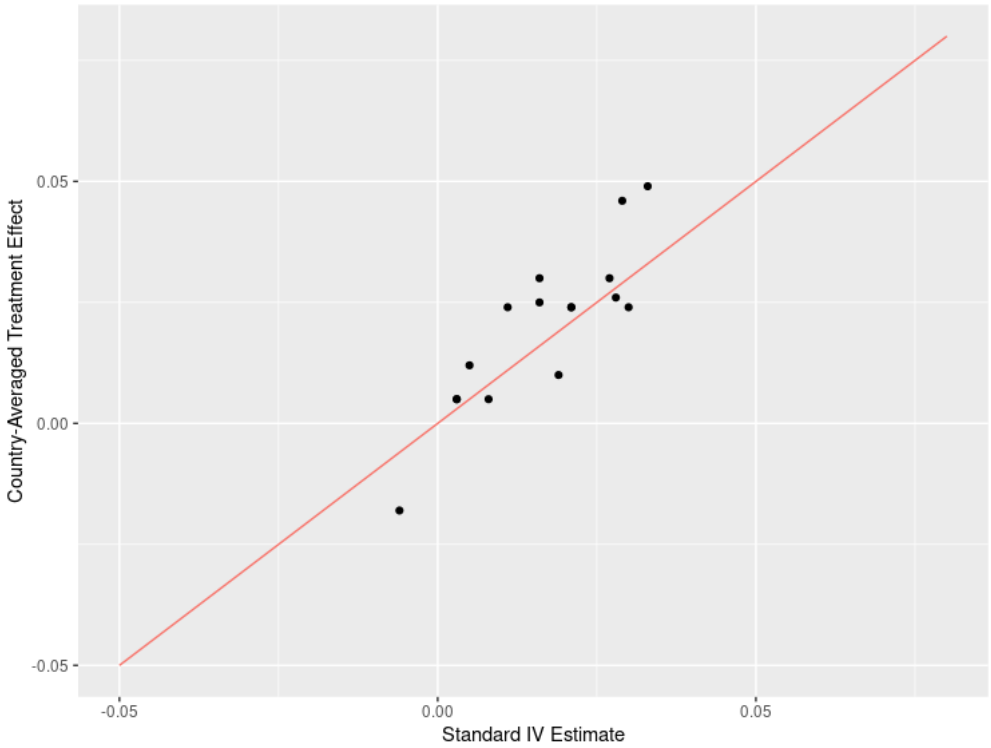

Figure A4: **Correlation between Estimators**. This figure demonstrates the correlation across all outcomes between the two main estimators used in this analysis: the standard IV estimate and the country-averaged treatment effect estimate. The correlation is 0.87. Each point represents one outcome, either one of the main indices or one of the constituent questions. The 45°line through the origin is shown for reference; the country-averaged treatment effect estimates closely match the IV estimates as noted by points very close to the 45°line, which represents a perfect match.

A.G ESS Question Text and Pro Environmental Beliefs Definitions

We include exact question wording and coding for our main pro-climate outcomes.

- **How likely to buy most energy efficient home appliance**: If you were to buy a large electrical appliance for your home, how likely is it that you would buy one of the most energy efficient ones?
  0 Not at all likely - 10 Extremely likely

- **How often do things to reduce energy use**: There are some things that can be done to reduce energy use, such as switching off appliances that are not being used, walking for short journeys, or only using the heating or air conditioning when really needed. In your daily life, how often do you do things to reduce your energy use?

- **How much electricity should be generated from [energy source]**: The highlighted box at the top of this card shows a number of energy sources that can be used to generate electricity. Please take a moment to look over them. How much of the electricity used in [country] should be generated from each energy source? First, how much of the electricity used in [country] should be generated from [energy source]?
  Note: pro-clean energy beliefs outcome is an average of being pro-hydro and solar, and anti-coal.

- **How worried too dependent on fossil fuels**: How worried are you about [country] being too dependent on using energy generated by fossil fuels such as oil, gas and coal?

- **Do you think the world's climate is changing**: You may have heard the idea that the world's climate is changing due to increases in temperature over the past 100 years. What is your personal opinion on this? Do you think the world's climate is changing?

- **How much thought about climate change before today**: How much have you thought about climate change before today?

- **How worried about climate change**: How worried are you about climate change?

- **Climate change good or bad impact across world**: How good or bad do you think the impact of climate change will be on people across the world? Please choose a number from 0 to 10, where 0 is extremely bad and 10 is extremely good.
  0 Extremely bad - 10 Extremely good

- **Favour increase taxes on fossil fuels to reduce climate change**: To what extent are you in favour or against the following policies in [country] to reduce climate change? Increasing taxes on fossil fuels, such as oil, gas and coal.

- **Favour subsidise renewable energy to reduce climate change**: To what extent are you in favour or against the following policies in [country] to reduce climate change? Using public money to subsidise renewable energy such as wind and solar power.

- **Favour ban of least energy efficient household appliances to reduce climate change**: To what extent are you in favour or against the following policies in [country] to reduce climate change? A law banning the sale of the least energy efficient household appliances.