

ONLINE APPENDIX

UNIVERSALISM: GLOBAL EVIDENCE*

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Abstract

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A Related Literature

Table A.1 collects the most closely related previous cross-cultural studies on the topics of universalism, parochialism and in-group favoritism. Our criteria for inclusion are (i) that the study has an explicit in-group vs. stranger (or in-group vs. out-group) component; (ii) that it was conducted across multiple countries; and (iii) that both individual- and country-level data was gathered. Note that this excludes some of the measures used by cultural psychologists (e.g., Schulz et al., 2019b), because these are typically only available at the country level (essentially all of these measures are based on non-representative samples).

None of the available papers measures universalism in distributive decisions. Most closely related, Romano et al. (2021) consider (hypothetical) money allocations, yet these are made in a strategic interaction format (two-player prisoner’s dilemma), such that observed decisions reflect not only universalism but also reciprocal preferences and beliefs about the other person’s strategy. Their sample is also considerably less broad than ours, given that it is drawn from an online convenience pool.

The measure used by Enke (2019) is based on the Moral Foundations Questionnaire that elicits agreement or disagreement with morally charged statements. Because many of these statements involve deontological motivations, this questionnaire cannot be directly translated into universalism in distributive situations. This said, the questionnaire elicits what we interpret as a more general form of universalist values. The sample is highly non-representative because it skews young and well-educated.

Table A.1: Overview of cross-cultural datasets

Study	Sample	# Individuals	# Countries	Main measure
Romano et al. (2021)	Online convenience pool (Harris)	18,411	42	Parochial cooperation (hypothetical prisoner's dilemma with compatriot or foreigner)
Enke (2019)	Online convenience pool (www.yourmorals.org)	338,875	66	Moral Foundations Questionnaire
Delhey et al. (2011)	World Values Survey	Evolving	Evolving	In-group and out-group trust
Lang et al. (2019)	Small-scale societies	2,228	12	Parochial altruism in random allocation game

B Details on *Global Universalism Survey*

B.1 Background on Gallup World Poll and Sampling Procedures

B.1.1 Data collection – Infrastructure and Selection of Countries

We implemented the “Global Universalism Survey” module as part of the 2020 wave of the Gallup World Poll. The Gallup World Poll is a probability based and nationally representative sample of the resident adult (aged 15 and older) population. The World Poll has been conducted nearly every year since 2005 through a global survey infrastructure that consists of a network of vendors.

Our survey module was implemented in 60 countries, with a median of 1,000 respondents in each country and a total of 66,233 respondents in all (63,788 of which answered at least one of our universalism questions). The countries were chosen to maximize the global representativeness of our sample. Interviews were mostly conducted over the phone, except in India and Pakistan where the interviews were face-to-face. The surveys were conducted between October 2020 and February 2021. Appendix Table B.1 contains the details of how and when the survey was conducted in each country.

B.1.2 Sampling

In countries where interviews are conducted by telephone, Gallup uses random-digit-dialing (RDD) or a nationally representative list of phone numbers. Gallup typically uses a dual sampling frame based on landline and mobile telephone numbers. In some countries, the sampling frame is mobile telephone only (for example, Libya and Finland). The split between landline and mobile is based on country-specific information from past surveys or other secondary data. One person, drawn at random, was interviewed in each sampled household through Gallup’s network of survey providers. For respondents contacted by landline telephone, the interviewee was selected (among eligible respondents aged 15 and older) either by identifying the household member with the next upcoming birthday, or by using the interviewing program to randomly select an eligible household member. Mobile phone users were directly interviewed. According to the protocol, interviewers make several attempts to contact someone from a randomly identified household before moving on to another household.

Sampling in face-to-face interview countries occurs in three stages. First, depending on the granularity of the available population data, sampling units are constructed by either stratifying along population weights (if population information is available), or by random sampling at the ward/village level. Next, the local survey vendors use a “random route procedure” to select a candidate household. Finally, an interviewee is selected from

a list of household members by the computer program used to conduct and record the interviews. Similar to the telephone protocols, interviewers make several attempts to contact a selected household member before moving on to another household.

B.1.3 Sample Weights

Gallup provides probabilistic weights to make the survey data ex-post nationally representative. The weights are constructed to account for multiple sources of bias such as different household sizes, selection of primary sampling units, individuals owning both a landline and a mobile phone, and selection of telephone numbers from the respective frames. We use these weights to calculate the country-level averages of the universalism statistic and use this weighted mean in our country-level analyses.

B.1.4 Translation and Piloting

After the final survey instrument for the Universalism module was finalized in English, translations were made and tested in the field in four countries - Turkey, Brazil, Spain, and Kenya. These cognitive interviews tested the survey on a small sample of 10-20 persons with different income and education levels. The results from the field testing were used to refine the English version to improve comprehension for respondents. The final survey instrument was then translated into all the languages needed, and each translation was reviewed by native speakers of each language to ensure that the translation was comprehensible and that it matched the English version. Each translation was modified based on the research team's feedback. Interviewers were instructed to follow the interview script without deviations. For some languages that are in use in more than one country, multiple translations into localized versions were made (such as Arabic, French, and Spanish).

Table B.1: Survey Details

Country	Dates	Respondents	Mode	Languages	Exclusions
Algeria	Nov 20 - Dec 9, 2020	1062	Landline and Mobile	Arabic	
Argentina	Dec 2, 2020 - Feb 23, 2021	1003	Landline and Mobile	Spanish	
Australia	Sep 21 - Nov 1, 2019	1000	Landline and Mobile	English	
Bangladesh	Dec 8 - Dec 20, 2020	1054	Mobile	Bengali	
Bolivia	Nov 24 - Dec 24, 2020	1000	Mobile	Spanish	
Brazil	Dec 2, 2020 - Jan 27, 2021	1003	Landline and Mobile	Portuguese	
Cambodia	Dec 25, 2020 - Jan 15, 2021	1000	Mobile	Khmer	
Cameroon	Nov 23 - Dec 19, 2020	1024	Mobile	French, English, Fulfulde	
Canada	Oct 13 - Nov 24, 2020	1010	Landline and Mobile	English, French	Yukon, Northwest Territories and Nunavut were excluded from the sample.
Chile	Dec 9, 2020 - Feb 24, 2021	1000	Landline and Mobile	Spanish	
China	Oct 28 - Dec 13, 2020	3502	Mobile	Chinese	Tibet was excluded from the sample. The excluded areas represent less than 1% of the population of China
Colombia	Nov 30, 2020 - Jan 27, 2021	1002	Landline and Mobile	Spanish	
Croatia	Nov 6 - Dec 2, 2020	1000	Landline and Mobile	Croatian	
Czech Republic	Dec 22, 2020 - Jan 25, 2021	1004	Landline and Mobile	Czech	
Ecuador	Dec 7, 2020 - Feb 11, 2021	1002	Landline and Mobile	Spanish	
Egypt	Dec 19 - Dec 30, 2020	1002	Landline and Mobile	Arabic	
Ethiopia	Dec 7 - Dec 31, 2020	1022	Mobile	Amharic, English, Oromo, Tigrinya	
France	Oct 19 - Nov 14, 2020	1000	Landline and Mobile	French	
Germany	Oct 19 - Nov 14, 2020	1000	Landline and Mobile	German	

Table B.1: Survey Details

Country	Dates	Respondents	Mode	Languages	Exclusions
Ghana	Dec 11 - Dec 31, 2020	1000	Mobile	English, Ewe, Twi, Hausa	
Greece	Dec 2 - Dec 30, 2020	1003	Landline and Mobile	Greek	
Hungary	Nov 25 - Dec 21, 2020	1002	Landline and Mobile	Hungarian	
India	Dec 26, 2020 - Feb 24, 2021	3300	Face-to-Face (HH)*	Assamese, Bengali, Gujarati, Hindi, Kannada, Malayalam, Marathi, Odia, Punjabi, Tamil, Telugu	Excluded population living in Northeast states and remote islands, and Jammu and Kashmir. The excluded areas represent less than 10% of the population.
Indonesia	Dec 4, 2020 - Jan 10, 2021	1011	Mobile	Bahasa Indonesia	
Iran	Nov 2 - Nov 8, 2020	1007	Landline and Mobile	Farsi	
Iraq	Jan 3 - Feb 28, 2021	1006	Mobile	Arabic, Kurdish	
Israel	Dec 19, 2020 - Jan 7, 2021	1059	Landline and Mobile	Hebrew, Arabic	
Italy	Nov 2 - Nov 25, 2020	1000	Landline and Mobile	Italian	
Japan	Oct 2 - Dec 3, 2020	1012	Landline and Mobile	Japanese	For landline RDD, excluded 12 municipalities near the nuclear power plant in Fukushima. These areas were designated as not-to-call districts due to the devastation from the 2011 disasters. The exclusion represents less than 1% of the population of Japan.
Jordan	Dec 21 - Dec 31, 2020	1005	Mobile	Arabic	
Kazakhstan	Dec 11 - Dec 25, 2020	1000	Mobile	Russian, Kazakh	

Table B.1: Survey Details

Country	Dates	Respondents	Mode	Languages	Exclusions
Kenya	Nov 13 - Nov 22, 2020	1000	Mobile	English, Swahili/Kishwahili	
Mexico	Nov 10 - Dec 20, 2020	1006	Landline and Mobile	Spanish	
Morocco	Dec 17, 2020 - Jan 6, 2021	1010	Landline and Mobile	Moroccan Arabic	
Netherlands	Oct 27 - Dec 19, 2020	1000	Landline and Mobile	Dutch	
Nigeria	Dec 9 - Dec 21, 2020	1019	Mobile	English, Hausa, Igbo, Pidgin English, Yoruba	
Norway	Oct 9 - Nov 9, 2020	1018	Landline and Mobile	Norwegian	
Pakistan	Jan 9 - Feb 5, 2021	1001	Face-to-Face (HH)*	Urdu	Did not include AJK, Gilgit-Baltistan. The excluded area represents approximately 5% of the population. Gender-matched sampling was used during the final stage of selection.
Peru	Nov 27, 2020 - Feb 4, 2021	1003	Landline and Mobile	Spanish	
Philippines	Nov 16 - Dec 19, 2020	1000	Landline and Mobile	Filipino, Iluko, Cebuano, Waray, Bicol	
Poland	Nov 20 - Dec 17, 2020	1002	Landline and Mobile	Polish	
Portugal	Nov 9 - Dec 10, 2020	1000	Landline and Mobile	Portuguese	
Russia	Dec 2, - Dec 28, 2020	2002	Mobile	Russian	
Senegal	Nov 4 - Nov 26, 2020	1017	Mobile	French, Wolof	
South Africa	Dec 14 - Dec 23, 2020	1001	Mobile	Afrikaans, English, Sotho, Xhosa, Zulu	

Table B.1: Survey Details

Country	Dates	Respondents	Mode	Languages	Exclusions
South Korea	Dec 12, 2020 - Jan 11, 2021	1005	Landline and Mobile	Korean	
Spain	Oct 19 - Nov 12, 2020	1000	Landline and Mobile	Spanish	
Sri Lanka	Dec 7, 2020 - Jan 31, 2021	1013	Mobile	Sinhala, Tamil	
Switzerland	Oct 19 - Nov 17, 2020	1000	Landline and Mobile	French, German, Italian	
Tanzania	Dec 15 - Dec 24, 2020	1000	Mobile	Swahili, Kishwahili	
Thailand	Dec 13, 2020 - Jan 25, 2021	1000	Mobile	Thai	
Turkey	Dec 18 - Dec 29, 2020	1000	Landline and Mobile	Turkish	
Uganda	Dec 18 - Dec 28, 2020	1000	Mobile	Ateso, English, Luganda, Runyankole	
Ukraine	Nov 26 - Dec 6, 2020	1006	Landline and Mobile	Russian, Ukrainian	
United Kingdom	Nov 2 - Nov 27, 2020	1000	Landline and Mobile	English	
United States	Oct 14 - Dec 8, 2020	1008	Landline and Mobile	English, Spanish	
Venezuela	Dec 10, 2020 - Jan 24, 2021	1020	Landline and Mobile	Spanish	
Vietnam	Dec 6 - Dec 20, 2020	1000	Mobile	Vietnamese	
Zambia	Dec 14, 2020 - Jan 20, 2021	1005	Mobile	Bemba, English, Lozi, Nyanja, Tonga	
Zimbabwe	Dec 14 - Dec 26, 2020	1002	Mobile	English, Shona, Ndebele	

B.2 Survey Questions

Each respondent is randomized into treatment *Baseline* or *Moral*, where the latter treatment was split evenly between two different sub-treatments. Each respondent in *Baseline* answers two randomly selected questions out of A-1 through A-5. Each respondent in *Moral* answers two randomly selected questions out of B-1 through B-5. Each respondent in a sub-treatment of *Moral*, which we will here call *Deserving*, answers two randomly selected questions out of C-1 through C-5. In addition, each respondent answers A-6 / B-6 / C-6. Responses to these questions are either A. a currency value, B. "Do not know", or C. "Refused to answer". After each question, the interviewer repeats the response and asks for confirmation from the respondent.

Finally, all respondents answer two randomly selected questions out of D-1 through D-6. Responses to these questions are coded as either A. a value from 1 to 4 (with 1 indicating "Strongly agree" and 4 "Strongly disagree"), B. "Do not Know", or C. "Refused".

Treatment *Baseline*. Suppose you have earned \$1,000, but you have to give away the money to two other people. You can't keep any of the money for yourself. Assume that these two people have the same standard of living.

A-1. How much of your \$1,000 would you give to a person in your family, if the rest goes to a random stranger from (COUNTRY NAME)?

This means that you would give [VALUE FROM A-1] to a person in your family and [1,000 MINUS VALUE FROM A-1] to a random stranger from (COUNTRY NAME). Is this correct? → if No, repeat A-1.

All subsequent questions follow this same logic, whereby the interviewer verifies participant responses through a follow-up question.

A-2. How much of your \$1,000 would you give to a friend of yours, if the rest goes to a random stranger from (COUNTRY NAME)?

A-3. How much of your \$1,000 would you give to a person who lives in your neighborhood, if the rest goes to a random stranger from (COUNTRY NAME)?

A-4. How much of your \$1,000 would you give to a person who shares your religious beliefs, if the rest goes to a random stranger from (COUNTRY NAME)?

A-5. How much of your \$1,000 would you give to a person who shares your ethnic background, if the rest goes to a random stranger from (COUNTRY NAME)?

A-6. Suppose now that the two people are someone from (COUNTRY NAME) and someone from anywhere in the world. Again, assume that these two people have the same living standard. How much of your \$1,000 would you give to a random stranger from (COUNTRY NAME), if the rest goes to a random stranger from anywhere in the world?

Treatment *Moral*. Suppose you have earned \$1,000, but you have to give away the money to two other people. You can't keep any of the money for yourself. Assume that these two people have the same living standard.

B-1. If you were to do what you think is morally right, then how much of your \$1,000 would you give to a person in your family, if the rest goes to a random stranger from (COUNTRY NAME)?

B-2. If you were to do what you think is morally right, then how much of your \$1,000 would you give to a friend of yours, if the rest goes to a random stranger from (COUNTRY NAME)?

B-3. If you were to do what you think is morally right, then how much of your \$1,000 would you give to a person who lives in your neighborhood, if the rest goes to a random stranger from (COUNTRY NAME)?

B-4. If you were to do what you think is morally right, then how much of your \$1,000 would you give to a person who shares your religious beliefs, if the rest goes to a random stranger from (COUNTRY NAME)?

B-5. If you were to do what you think is morally right, then how much of your \$1,000 would you give to a person who shares your ethnic background, if the rest goes to a random stranger from (COUNTRY NAME)?

B-6. Suppose now that the two people are someone from (COUNTRY NAME) and someone from anywhere in the world. Again, assume that these two people have the same living standard. If you were to do what you think is morally right, then how much of your \$1,000 would you give to a random stranger from (COUNTRY NAME), if the rest goes to a random stranger from anywhere in the world?

Treatment *Deserving* (sub-treatment of *Moral* and pooled with *Moral* in all analyses). Suppose you have earned \$1,000, but you have to give away the money to two other people. You can't keep any of the money for yourself. Assume that these two people are equally good people and have the same living standard.

C-1 through C-6: Same questions as B-1 through B-6.

Political Questions. We are now going to read a number of statements. In each case, we want you to say whether you Strongly Agree, Somewhat Agree, Somewhat Disagree, Strongly Disagree.

D-1. There are too many immigrants in the area you live in.

D-2. There are too many immigrants in (country).

D-3. The national government should focus on helping the poor in (country), rather than the poor elsewhere in the world.

D-4. The national government should focus on protecting the environment in (country), rather than protecting the global environment.

D-5. The national government should focus on having a strong military.

D-6. The national government should aim to reduce the economic differences between the rich and the poor in (country).

B.3 Main Covariates, Country Classifications and SE Clustering

B.3.1 Demographic Variables

Many of the analyses in the paper use demographic information to study heterogeneity or simply to control for variation driven by demographic characteristics. Below, we document how these variables are constructed.

Age: Continuous variable, recorded at the individual level in the survey.

Above Median Age: An indicator which is 1 if an individual is older than the country's median age.

Religiosity: An indicator which is 1 if the respondent answers the question "What is your religion" with anything other than "Secular/Atheist/Non-religious/Agnostic".

Income: The income quintile relative to other respondents from the same country. In some of our analyses, we use an indicator that is 1 if the individual is in the top 2 income quintiles (i.e. top 40%).

City: An indicator that is 1 if the respondent indicates that they live in a large city. Other levels in the base variable are: rural area, small town/village, and suburbs.

Education: An indicator that is 1 if the respondent indicates that they have "Completed 4 years of education beyond high school and/or received a 4-year college degree".

Gender: Gallup codes all respondents as either male or female. We use an indicator = 1 if the respondent is male in our analyses.

B.3.2 Country Classification

Income levels. We use the World Bank's income classification schemes as one way of dividing countries into economically meaningful groups. The World Bank classifies countries as "High Income", "Middle Income" and "Low Income". We code an indicator *highincome* = 1 if a country is highincome, and 0 otherwise. The countries are: Australia, Canada, Switzerland, Spain, Germany, France, United Kingdom, Greece, USA, Netherlands, Norway, Portugal, Italy, Czech Republic, Croatia, Hungary, Israel, Japan, South Korea and Poland.

WEIRD countries. In many of our analyses, we study differences between "Western, Educated, Industrialized, Rich, and Democratic", i.e. WEIRD and non-WEIRD countries. We use the Maddison Project Database (MPD) to create an indicator $weird = 1$ if a country is in the "Western Europe" or "Western Offshoots" country groups in the MPD. The countries are: Australia, Canada, Switzerland, Spain, Germany, France, United Kingdom, Greece, USA, Netherlands, Norway, Portugal, and Italy.

Non-WEIRD and High Income countries. These are: Chile, Czech Republic, Croatia, Hungary, Israel, Japan, South Korea, Poland.

Low and Middle Income countries. These are: Argentina, Bangladesh, Bolivia, Brazil, China, Cameroon, Colombia, Algeria, Ecuador, Egypt, Ethiopia, Ghana, Indonesia, India, Iran, Iraq, Jordan, Kazakhstan, Kenya, Cambodia, SriLanka, Morocco, Mexico, Nigeria, Pakistan, Peru, Philippines, Russia, Senegal, Thailand, Turkey, Tanzania, Uganda, Ukraine, Venezuela, Vietnam, South Africa, Zambia, Zimbabwe.

B.3.3 Standard Errors

The individual level analyses presented in this paper compute standard errors that are clustered by 530 "primary sampling units". These are essentially survey strata. In countries where telephonic interviews are conducted, these represent mobile providers (countries where landlines are a part of the sampling frame may have an additional regional component to the phone provider). In countries where face-to-face interviews are conducted, these represent administrative regions, split by urbanicity.

B.4 Sample Overview

Table B.2 provides an overview of the samples in each country. We provide the number of observations, fraction female, fraction religious, fraction living in a city, fraction having a college degree, median age and number of interview languages used.

B.5 Monetary Amounts Used in the Survey

We calculate the ratio of the PPP-adjusted GDP per capita of each country to the GDP of the United States using the latest available data from the World Bank WDI Database. We use the latest available exchange rate before the cutoff dates for the finalisation of the survey instruments. In the interest of simplicity consistency, we round down the amount from this conversion process to the first digit. Table B.3 lists the local currency amounts used in each country.

Table B.2: Sample overview

	Obs.	Female %	Religious %	City %	College %	Median age	Languages
Algeria	1048	.43	1	.51	.26	36	1
Argentina	978	.46	.83	.45	.22	43	1
Australia	965	.52	.68	.069	.42	63	1
Bangladesh	920	.39	1	.4	.15	26	1
Bolivia	922	.49	.98	.36	.25	31	1
Brazil	972	.5	.88	.5	.28	38	1
Cambodia	969	.43	1	.26	.28	32	1
Cameroon	1013	.51	.99	.48	.18	27	3
Canada	992	.54	.67	.34	.42	55	2
Chile	984	.59	.91	.52	.37	43	1
China	3410	.34	.	.34	.3	31	1
Colombia	997	.56	.88	.42	.19	34	1
Croatia	935	.62	.88	.38	.44	46	1
Czech Republic	982	.59	.32	.38	.29	46	1
Ecuador	921	.55	.94	.33	.19	32	1
Egypt	991	.45	1	.52	.31	33	1
Ethiopia	1021	.43	1	.61	.34	30	4
France	995	.51	.68	.24	.29	49	1
Germany	991	.48	.67	.35	.26	55	1
Ghana	989	.38	.99	.36	.26	27	3
Greece	1001	.45	.87	.45	.57	45	1
Hungary	968	.53	.82	.39	.45	48	1
India	2801	.47	1	.12	.089	32	11
Indonesia	904	.43	1	.21	.23	31	1
Iran	995	.49	.99	.53	.28	38	1
Iraq	988	.38	1	.53	.18	30	2
Israel	1055	.47	.99	.77	.36	45	2
Italy	997	.49	.84	.29	.27	53	1
Japan	983	.41	.34	.2	.35	59	1
Jordan	1001	.46	1	.48	.25	33	1
Kazakhstan	848	.52	.98	.57	.4	35	2
Kenya	996	.47	1	.14	.23	27	2
Mexico	941	.47	.89	.38	.35	38	1
Morocco	986	.43	.	.55	.13	32	1
Netherlands	980	.44	.48	.22	.54	57	1
Nigeria	1014	.43	1	.48	.05	30	5
Norway	1004	.48	.71	.21	.59	57	1
Pakistan	983	.5	.99	.26	.053	32	1
Peru	982	.48	.92	.46	.27	34	1
Philippines	1000	.57	1	.25	.19	31	5
Poland	972	.51	.85	.46	.6	46	1
Portugal	947	.55	.79	.23	.36	43	1
Russia	1980	.53	.81	.44	.5	40	1
Senegal	1011	.52	1	.42	.058	28	2
South Africa	1000	.58	.98	.2	.067	29	5
South Korea	999	.39	.47	.45	.4	53	1
Spain	996	.54	.75	.32	.13	48	1
Sri Lanka	930	.44	1	.084	.024	37	2
Switzerland	994	.53	.8	.15	.56	51	3
Tanzania	996	.45	1	.38	.15	29	1
Thailand	925	.55	.97	.36	.6	38	1
Turkey	952	.54	.99	.74	.29	28	1
Uganda	958	.42	1	.16	.039	26	3
Ukraine	955	.53	.89	.45	.57	38	2
United Kingdom	988	.46	.71	.21	.59	51	1
United States	1002	.47	.82	.2	.48	56	2
Venezuela	993	.55	.94	.32	.32	36	1
Vietnam	764	.41	.49	.58	.58	31	1
Zambia	1005	.48	1	.2	.3	26	5
Zimbabwe	999	.52	.98	.094	.14	31	3

Notes. Descriptive statistics for the respondent pool in each country.

Table B.3: Local currency amounts

Countries	Country code	Currency code	Local Currency Amount
Algeria	DZA	DZD	30,000
Argentina	ARG	ARS	20,000
Australia	AUS	AUD	1,000
Bangladesh	BGD	BDT	6,000
Bolivia	BOL	BOB	900
Brazil	BRA	BRL	1,000
Cambodia	KHM	KHR	300,000
Cameroon	CMR	XAF	40,000
Canada	CAN	CAD	1,000
Chile	CHL	CLP	300,000
China	CHN	CNY	2,000
Colombia	COL	COP	800,000
Croatia	HRV	HRK	3,000
Czech Republic	CZE	CZK	10,000
Ecuador	ECU	USD	200
Egypt	EGY	EGP	3,000
Ethiopia	ETH	ETB	900
France	FRA	EUR	700
Germany	DEU	EUR	800
Ghana	GHA	GHS	400
Greece	GRC	EUR	400
Hungary	HUN	HUF	100,000
India	IND	INR	9,000
Indonesia	IDN	IDR	3,000,000
Iran	IRN	IRR (toman)	1,000,000
Iraq	IRQ	IRD	300000
Israel	ISR	ILS	2,000
Italy	ITA	EUR	600
Japan	JPN	JPY	70,000
Jordan	JOR	JOD	100
Kazakhstan	KAZ	KZT	200,000
Kenya	KEN	KES	6,000
Mexico	MEX	MXN	6,000
Morocco	MAR	MAD	1,000
Netherlands	NLD	EUR	800
Nigeria	NGA	NGN	30,000
Norway	NOR	NOK	10,000
Pakistan	PAK	PKR	10,000
Peru	PER	PEN	800
Philippines	PHL	PHP	7,000
Poland	POL	PLN	2000
Portugal	PRT	EUR	500
Russia	RUS	RUB	30,000
Senegal	SEN	XOF	40000
South Africa	ZAF	ZAR	3,000
South Korea	KOR	KRW	700,000
Spain	ESP	EUR	600
Sri Lanka	LKA	LKR	40,000
Switzerland	CHE	CHF	1,000
Tanzania	TZA	TZS	100,000
Thailand	THA	THB	9,000
Turkey	TUR	TRY	3,000
Uganda	UGA	UGX	100,000
Ukraine	UKR	UAH	4,000
United Kingdom	GBR	GBP	600
USA	USA	USD	1,000
Venezuela	VEN	VES	50,000,000
Vietnam	VNM	VND	3,000,000
Zambia	ZMB	ZMW	900
Zimbabwe	ZWE	USD	50

Notes. Local currency amounts used in each country. The amounts are generated by scaling 1000 USD in the ratio of each country's GDP (PPP per capita) to US GDP, multiplying by the exchange rate and then rounding down to the first digit.

B.6 Data Considerations

Number of observations and questions. The data from Gallup contains 66,233 respondents from which we are able to use 63,788 respondents.

The biggest cause of lost observations are cases where responses to allocation questions are coded as "Don't Know" or "Refused to answer" (2,427 observations). These instances are not randomly distributed and are more frequent in some country-language combinations than in others. For example, nearly 50% of the respondents in Vietnam do not answer one or more allocation question.

Next, the survey protocol has a confirmation step in each allocation decision question. 647 allocations are "unconfirmed", of which more than half are from India. We have chosen to ignore this step of the protocol and include the "unconfirmed" allocations in the interest of maximizing the number of observations. Finally, we drop 18 respondents from Pakistan because none of the questions in our module were recorded.

In our final sample of 63,788, 7.5% of respondents have at least one allocation question missing. In those cases, we compute the summary statistics based on fewer questions. When either only domestic or only foreign universalism is available, we use this measure also for composite universalism.

Recording Errors. The raw data set contains 1,828 allocation decisions which we suspect have been incorrectly recorded and have attempted to correct. These fall into two categories. First, some allocation decisions recorded in Bangladesh, Uganda, Cambodia and Iraq clearly have allocation amounts with an incorrect number of zeroes – for example, an allocation decision of 6000:4000 was recorded as 6:4 in many Bangladeshi records. In these cases, we preserve the base information and adjust our universalism calculations to account for this problem.

Second, some observations in France, Germany, Italy, Spain, Switzerland and the United Kingdom were incorrect in that the sum of the recorded allocations for the in- and out-groups did not add up to the allocation budget. For example, if the total amount to be allocated was 1000, then in some cases the sum of the in- and out-group allocations was less than 1000. We attributed this to a recording error as enumerators make a manual calculation when they record the amount allocated to the stranger (after soliciting the in-group allocation from respondents). We apply a correction to these allocations by preserving the in-group allocation as-is, and scaling the out-group allocation to match the total amount.

Missing questions. The survey questions on allocation to co-ethnics and attitudes on government policies were not asked in China due to local restrictions on data collection

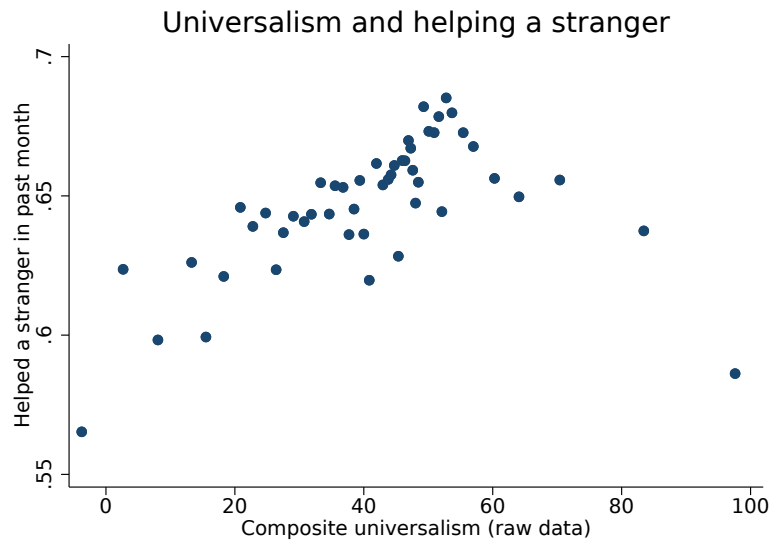


Figure B.1: Binned scatter plot of response to Gallup question “In the last 12 months, did you help a stranger in need?” as a function of a respondent’s composite universalism, controlling for country fixed effects. Universalism is computed based on the raw data in the money allocation decisions.

or other issues beyond the control of the survey collection agency. These questions were replaced with other equivalent allocation decision or political views questions.

B.7 Dealing With Apparent Erroneous Responses

B.7.1 Recoding of Some Allocation Decisions

20,338 of 184,950 (11%) allocations in our data give strictly more to the stranger than the in-group member. This could reflect a combination of (i) genuine preferences; (ii) respondent confusion or lack of engagement; and (iii) misrecordings by the enumerator.

The Gallup World Poll (in which our survey was embedded) contains a question that asks respondents whether they helped a stranger in the past month. Both the Editor and the reviewers encouraged us to use this variable as a “validation variable” for the universalism measure, meaning that this variable allows us to study to what degree the raw data reflect (i) or to a significant degree (ii) and (iii).

Figure B.1 shows a binned scatter plot that illustrates the respondent-level link between helping a stranger (y-axis) and the summary statistic of universalism that we construct based on the multiple questions each respondent answers (x-axis). The figure is constructed based on the raw data (no recoding or dropping). The bins in the scatter plot are endogenously generated by Stata to contain the same number of observations per bin.

The figure shows a strongly non-monotonic relationship between universalism and the validation variable. Specifically, in the part of the sample in which respondents allo-

cate at most 50% of the budget to the stranger (the much larger, “unproblematic” part of the sample), helping a stranger and allocating money to the stranger are significantly positively correlated. This is what one would expect. However, in the smaller, “problematic” part of the sample where respondents allocate more money to the stranger than to the in-group, money allocations are *strongly negatively* correlated with helping a stranger. This paradoxical result suggests to us that, indeed, many of the allocations of more than 50% to the stranger (i) do not reflect genuine preferences and (ii) neither do they reflect unsystematic noise; but rather (iii) these allocations reflect genuine expressions of underlying preferences, albeit in a “flipped” manner.

To see this even more clearly, consider Table B.4. It summarizes the results of OLS regressions that correspond to Figure B.1. As we can see, the sign of the regression coefficient does not only *flip* when we consider respondents above or below universalism of 50 – the estimated coefficients even have a *very similar magnitude*, compare columns (1) and (2), and also columns (3) and (4). The main implication of this “flipping” pattern is that, when we run the regression on the full sample (column (5)), the coefficient is considerably smaller, which we interpret as heavy bias. Indeed, as column (6) shows, the coefficient doubles in size when we, instead, use the universalism variable that is based on the recoding procedure that we report in the main text.

These patterns suggest a tradeoff. On the one hand, we do not desire to leave the reader with the impression that we arbitrarily recode individual observations to “manufacture” certain results, in particular because our pre-analysis plan did not foreshadow such a procedure. On the other hand, we anticipate that this rich dataset may be used more widely by the research community going forward, and we feel it is incumbent upon us to suggest the most productive way to interpret and code the data. As a result, we opt for a balanced strategy. We recode allocations to the in-group of $x < 50\%$ as $100\% - x$ if and only if two arguably conservative criteria were satisfied: the respondent in question allocates (i) weakly more than 50% to the stranger in *all* questions and (ii) strictly more than 50% to the stranger in at least half of all decisions (which in practice usually means at least two out of three).

This procedure affects 4,328 respondents and 10,318 allocation decisions. To illustrate how conservative this recoding procedure is, consider the distribution of universalism (before recoding) for the observations that we recode (Appendix Figure B.2). For example, the top right panel shows the distribution of the unadjusted composite universalism measure in the subset of respondents for which at least one of the respondent’s allocation decisions get recoded. The main takeaway is that the universalism scores that we recode are often *very* extreme. In fact, the modal individual has an unadjusted composite universalism score of 100, meaning that the modal individual for whom we recode at least one decision allocates the entire budget to the socially more distant individual

Table B.4: Universalism and “validation variable” of having helped stranger

Sample:	Dependent variable:						
	Helped stranger in last month (in %)						
	Univ ≤ 50 (1)	Univ ≥ 50 (2)	Univ < 50 (3)	Univ > 50 (4)	All (5)	All (6)	Symmetric drop (7)
Composite universalism (no recoding)	0.140*** (0.017)	-0.109*** (0.029)	0.153*** (0.022)	-0.160*** (0.043)	0.056*** (0.014)		0.108 (.015)
Composite universalism (current recoding)						0.116*** (0.014)	
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.103	0.119	0.096	0.122	0.104	0.105	–
Observations	54971	26003	37447	8479	63450	63450	54851

Notes. OLS estimates, standard errors (clustered by sampling strata) in parentheses. Samples split by level of universalism (larger than 50 means more money allocated to socially more distant stranger). The difference between columns (1)–(2) and (3)–(4) is that the former include universalism of 50. Column (7) - Average of the regression coefficients from $M = 500$ iterations of the symmetric dropping procedure explained in the main text. Standard error of average coefficient computed using Cauchy-Schwartz inequality, $[SE(\frac{1}{M} \sum_i \beta^i)]^2 \leq \frac{1}{M^2} \sum_i \sum_j \sqrt{[SE(\beta^i)]^2 \cdot [SE(\beta^j)]^2}$, where $SE(\beta^i)$ is the standard error of the coefficient estimate in the i -th iteration. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

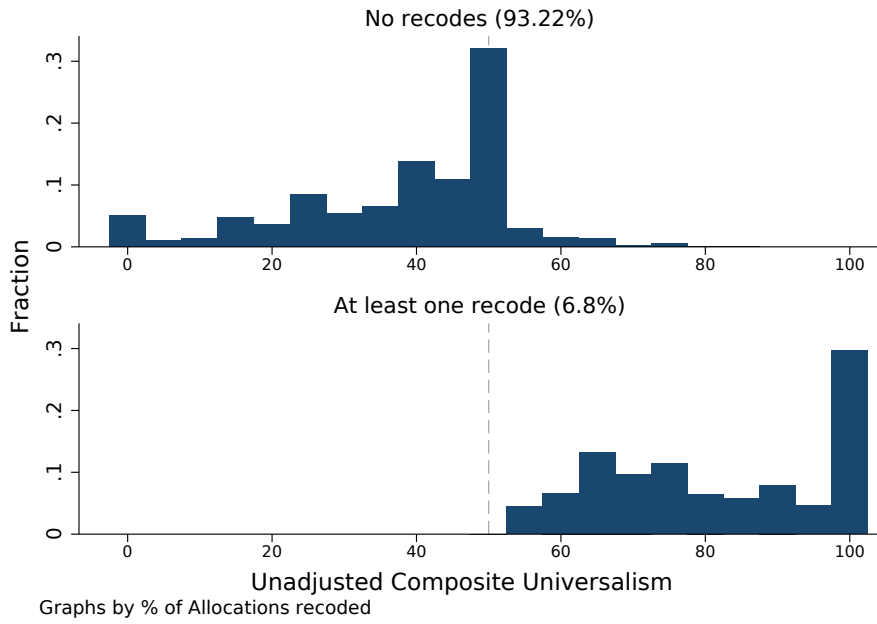


Figure B.2: Universalism patterns in recoded observations. Each panel shows the distribution of the *unadjusted* composite universalism statistic, as a function of whether or not we eventually recode at least one of the respondent’s allocation decisions.

in all decisions.

For transparency, Appendix B.8 replicates all results in this paper using the original coding, with similar results. Also for transparency, our published dataset will include both the recoded and the original allocation decisions.

B.7.2 Symmetric Dropping Procedure

A potential concern with the recoding procedure outlined in the previous subsection is that it is not symmetric around 50 because we do not recode any respondents who allocate strictly more to the in-group. We, hence, propose and implement a sensitivity check that is symmetric:¹

1. We drop all of those respondents that we recode according to the procedure outlined above. As explained above, this is a subset of those respondents who have universalism of $x > 50$.
2. For each respondent that we drop, we construct a set of “mirror respondents” with universalism of (approximately) $100 - x$ from the same country. We randomly select one respondent from this set and drop them. Thus, for each respondent to the right of 50 that we drop, we also drop a respondent to the left of 50.

¹We are indebted to Larry Katz for proposing this symmetric dropping procedure as a sensitivity check.

3. Given the random element implicit in this procedure, we implement it 500 times and compute the average coefficient across iterations. See the notes of Table B.4 for details on the calculation of the standard error.

To illustrate the symmetric dropping approach, column (7) of Table B.4 shows the results for the variable of having helped a stranger in the last month. Reassuringly, the symmetric dropping procedure produces almost the same average coefficient estimate as the recoding procedure.

Potential problems with any symmetric procedure: “base rate neglect”. The procedure outlined above is symmetric with respect to 50. While this seems attractive, this symmetry may also entail a problem. In our data (and in all earlier papers that use this type of money allocation task), the vast majority of respondents give less than 50 to the stranger, suggesting that a large fraction $y > 0.5$ of respondents have a true preference in favor of the in-group. Denote by p the probability that any given respondent will not indicate their true preference but instead its “flipped” version. It then follows that the share of respondents who incorrectly make a decision of > 50 is given by yp , while the share of respondents who incorrectly make a decision of < 50 is given $(1 - y)p$. Then, intuitively speaking, any symmetric procedure neglects the “base rate” y of how many people have a true preference above or below 50.

To illustrate, suppose that in a population of 100 people, $y = 90\%$ had a true preference below 50, and that the probability of giving a “flipped” answer is $p = 10\%$. Then, a researcher with full information about true preferences would like to recode (or drop) 9 respondents to the right of 50, but only 1 respondent to the left of 50 (even though, in total, 82 people will be to the left of 50 and 18 to the right). Notably, in this hypothetical world, it is easy to show that the “symmetric” procedure would introduce a bias that is actually larger than the bias resulting from our “asymmetric” recoding procedure.

Because we don’t know the true quantity y , we cannot definitively tell which strategy is superior. However, we strongly suspect that y is larger than 50% (probably much larger), which leads us to favor the recoding procedure. Nonetheless, the following subsection replicates all individual-level analyses with the symmetric dropping procedure.

B.7.3 Replication of Results with Symmetric Dropping Procedure

Tables B.5–B.7 and Figures B.3–B.4 replicate all individual-level regressions using the symmetric dropping procedure outlined above.

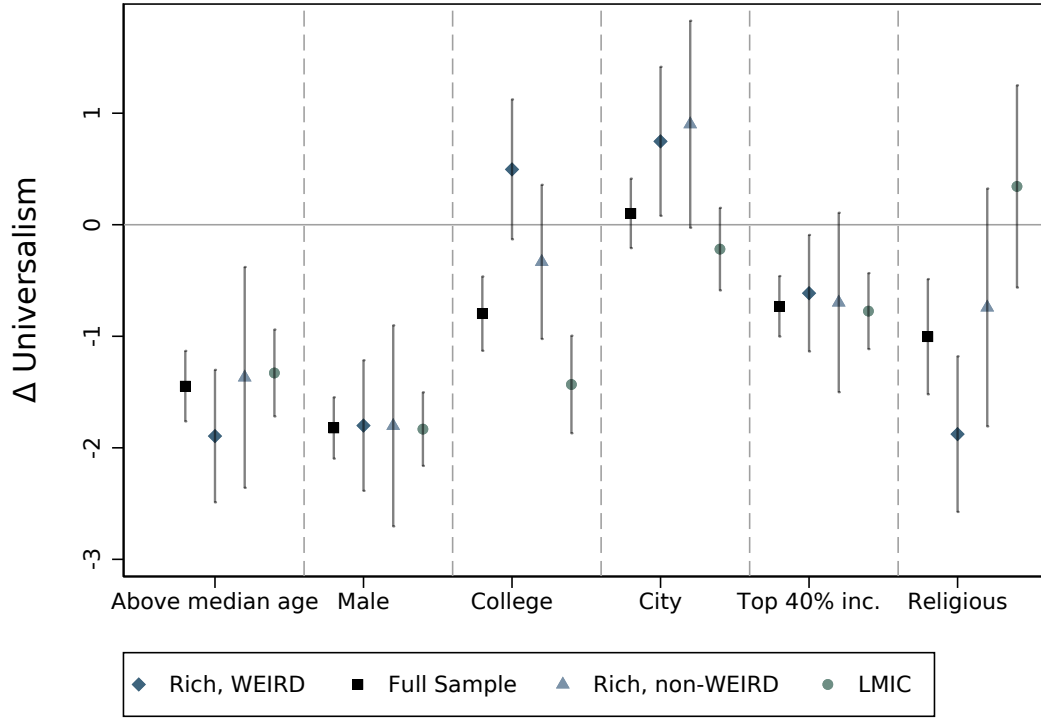


Figure B.3: Universalism and demographics. OLS coefficients and standard errors from the symmetric drop procedure, based on regressions of composite universalism on each demographic, controlling for country and treatment fixed effects. We report the average regression coefficient from $M = 500$ iterations of the symmetric dropping procedure explained in the main text. Standard error of average coefficient computed using Cauchy-Schwartz inequality, $[SE(\frac{1}{M} \sum_i \beta^i)]^2 \leq \frac{1}{M^2} \sum_i \sum_j \sqrt{[SE(\beta^i)]^2 \cdot [SE(\beta^j)]^2}$, where $SE(\beta^i)$ is the standard error of the coefficient estimate in the i -th iteration. All demographics are coded to be binary. Median age and income percentiles are computed separately for each country based on the sample. College captures a college degree, city whether the respondent lives in a large city (self-report), and religious whether the respondent reports belonging to a religious denomination. Whiskers show 95% confidence intervals, computed based on robust standard errors, clustered at the sampling unit level (530 clusters). LMIC = low- and middle-income countries. WEIRD = rich Western countries.

Table B.5: Universalism and political views at the individual level

	<i>Dependent variable:</i>					
	Reduce	Prioritize global vs. domestic		Pro immigrants		Weak
	Inequality	poor	environment	in area	in country	military
	(1)	(2)	(3)	(4)	(5)	(6)
Domestic universalism / 100	0.20 (0.05)	0.16 (0.04)	0.11 (0.05)	0.12 (0.05)	0.07 (0.05)	-0.02 (0.05)
Foreign universalism / 100	-0.01 (0.04)	0.41 (0.04)	0.22 (0.04)	-0.14 (0.05)	0.10 (0.05)	0.24 (0.04)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Treatment FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16075	16231	16025	18701	18468	16018

Notes. OLS coefficients and standard errors following the symmetric drop procedure. Universalism is divided by 100 for expositional ease. Each observation is an individual. We report the average regression coefficient from $M = 500$ iterations of the symmetric dropping procedure explained in the main text. Standard error of average coefficient computed using Cauchy-Schwartz inequality, $[SE(\frac{1}{M} \sum_i \beta^i)]^2 \leq \frac{1}{M^2} \sum_i \sum_j \sqrt{[SE(\beta^i)]^2 \cdot [SE(\beta^j)]^2}$, where $SE(\beta^i)$ is the standard error of the coefficient estimate in the i -th iteration. Responses are coded as “Strongly agree”, “Somewhat agree”, “Somewhat disagree” and “Strongly disagree”. We transform these into values 1, 2, 3 and 4. We code all political variables such that our pre-registration predicts a positive correlation with universalism. Ordered probit regressions show very similar results. College education is an indicator. Income quintile is a variable with values 1–5.

Table B.6: Exposure to democracy and universalism: Variation across country-age-cohorts

	<i>Dependent variable:</i>						
	Universalism						
	Composite			Domestic		Foreign	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Lifetime average democracy score	0.19 (0.10)	0.21 (0.09)	0.16 (0.10)	0.24 (0.10)	0.24 (0.11)	0.17 (0.13)	0.07 (0.14)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Treatment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lifetime average log GDP p/c	No	Yes	Yes	Yes	Yes	Yes	Yes
Demographic controls	No	No	Yes	No	Yes	No	Yes
Observations	48834	48834	47577	48493	47248	47742	46525

Notes. Coefficients and standard errors following the symmetric drop procedure. We report the average regression coefficient from $M = 500$ iterations of the symmetric dropping procedure explained in the main text. Standard error of average coefficient computed using Cauchy-Schwartz inequality, $[SE(\frac{1}{M} \sum_i \beta^i)]^2 \leq \frac{1}{M^2} \sum_i \sum_j \sqrt{[SE(\beta^i)]^2 \cdot [SE(\beta^j)]^2}$, where $SE(\beta^i)$ is the standard error of the coefficient estimate in the i -th iteration. Exposure to democracy is constructed by taking the mean of the Democracy score time series in the Polity V database over the respondent’s lifetime. Demographic controls include gender, income quintile fixed effects, college degree and an indicator for whether an individual lives in a big city.

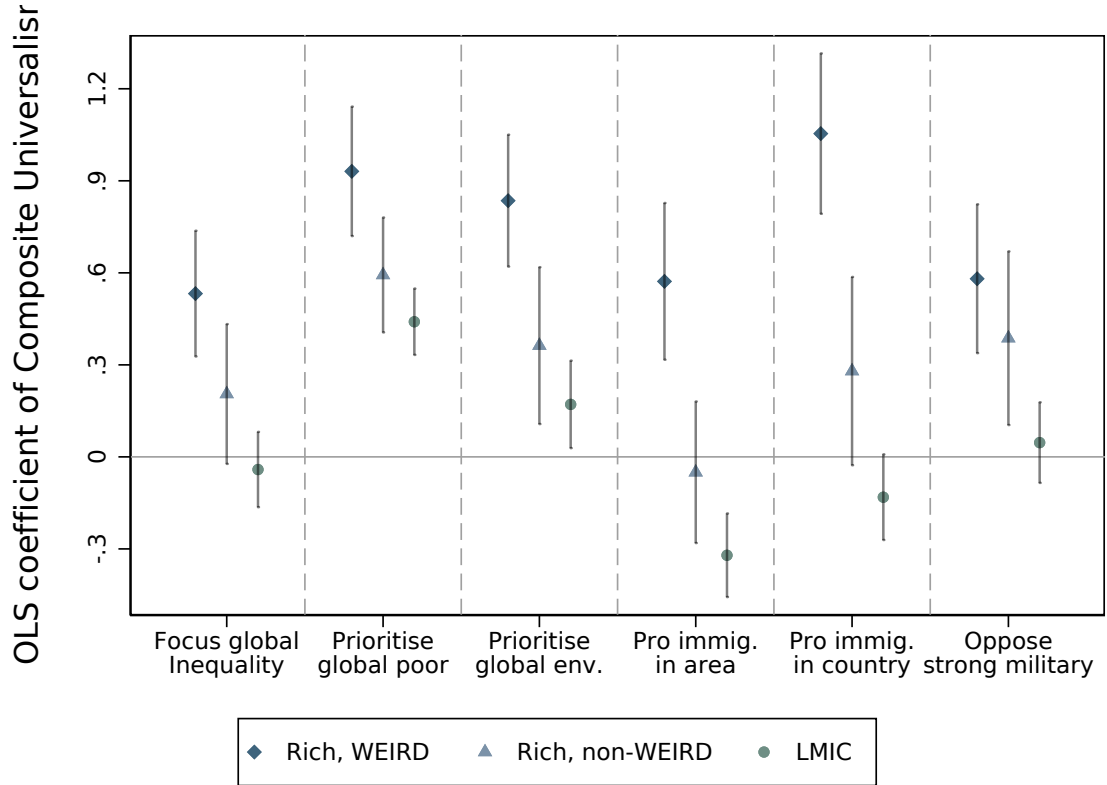


Figure B.4: Individual-level composite universalism and political views in different sub-samples, using the symmetric drop procedure. OLS coefficients from regressions of political attitudes on composite universalism, controlling for country and treatment fixed effects. We report the average regression coefficient from $M = 500$ iterations of the symmetric dropping procedure explained in the main text. Standard error of average coefficient computed using Cauchy-Schwartz inequality, $[SE(\frac{1}{M} \sum_i \beta^i)]^2 \leq \frac{1}{M^2} \sum_i \sum_j \sqrt{[SE(\beta^i)]^2 \cdot [SE(\beta^j)]^2}$, where $SE(\beta^i)$ is the standard error of the coefficient estimate in the i -th iteration. Whiskers show 95% confidence intervals, computed based on standard errors from the symmetric drop procedure. LMIC = low- and middle-income countries. WEIRD = rich Western countries.

Table B.7: Exposure to democracy and universalism: Variation across first-generation migrants

	<i>Dependent variable:</i>							
	Universalism							
	Composite				Domestic		Foreign	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Democracy score in home country	0.22 (0.11)	0.30 (0.13)	0.28 (0.13)	0.29 (0.15)	0.44 (0.14)	0.44 (0.14)	0.12 (0.18)	0.08 (0.19)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Treatment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Home country log GDP p/c	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic controls	No	No	Yes	Yes	No	Yes	No	Yes
Country of origin controls	No	No	No	Yes	No	No	No	No
Observations	2460	2196	2165	2147	2174	2143	2157	2129

Notes. Coefficients and standard errors following the symmetric drop procedure. We report the average regression coefficient from $M = 500$ iterations of the symmetric dropping procedure explained in the main text. Standard error of average coefficient computed using Cauchy-Schwartz inequality, $[SE(\frac{1}{M} \sum_i \beta^i)]^2 \leq \frac{1}{M^2} \sum_i \sum_j \sqrt{[SE(\beta^i)]^2 \cdot [SE(\beta^j)]^2}$, where $SE(\beta^i)$ is the standard error of the coefficient estimate in the i -th iteration. Demographic controls include gender, income quintile fixed effects, college degree and an indicator for whether an individual lives in a big city. Country of origin controls include average temperature, precipitation, percentage of the population living in tropical or subtropical zones, and percentage of the population at risk of contracting malaria.

B.8 Replication of Results with Raw Data

This appendix replicates all figures and tables from the main paper using the raw universalism data that are not corrected as described in Section 1.5 of the main paper.

B.8.1 Replication of Figures with Raw Data

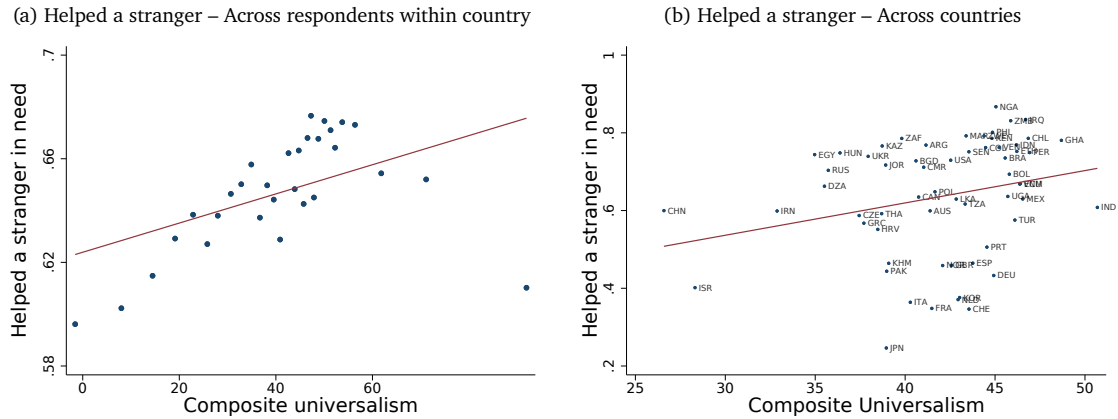


Figure B.5: Composite universalism and helping a stranger. The left panel shows a respondent-level binned scatter plot that, for a given level of universalism, computes the average probability of having helped a stranger. The bins are endogenously constructed such that each dot represents the same number of observations. This is a partial correlation plot, controlling for country and treatment FE (constructed based on 63,450 respondents). The right panel shows the raw correlation between country-level average universalism and the fraction of respondents who report having helped a stranger.

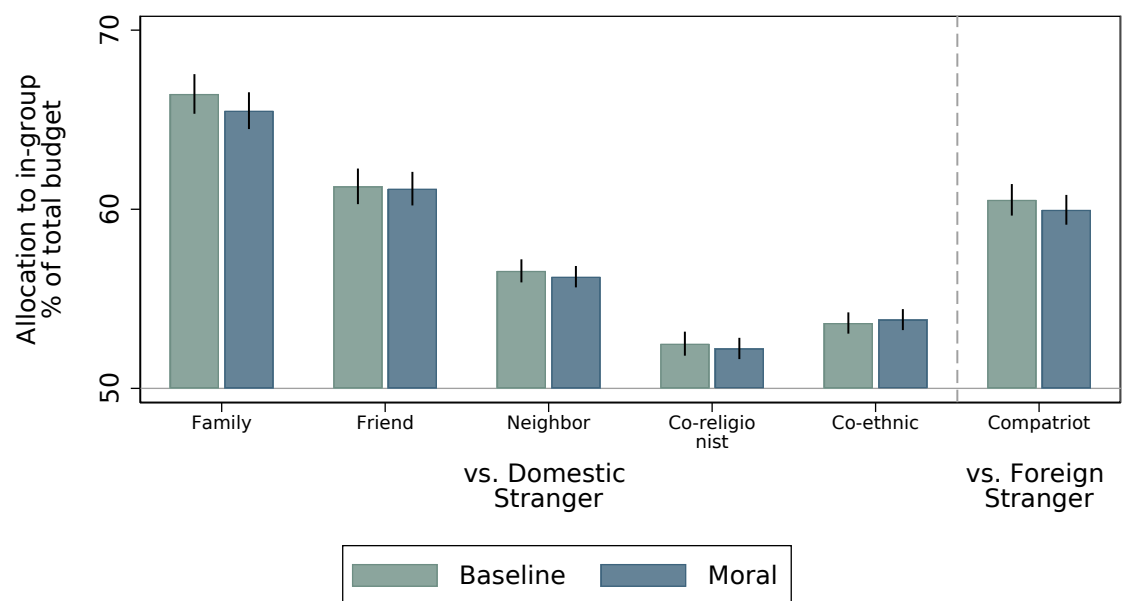


Figure B.6: Mean money allocations to the in-group by treatment. Each bar indicates how much of the budget was given to the in-group. Whiskers show 95% confidence intervals, computed based on clustering at the sampling unit level.

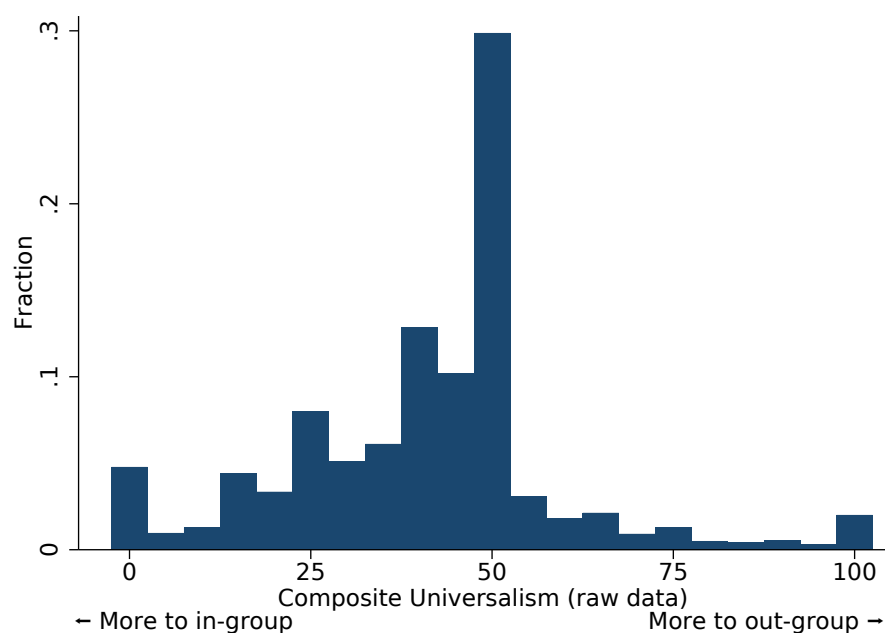


Figure B.7: Distribution of composite universalism across individuals, pooled across treatments ($N = 63,788$). 0 means that all money is shared with the in-group, 50 captures equal splits (on average), and 100 that all money is shared with the socially more distant stranger.

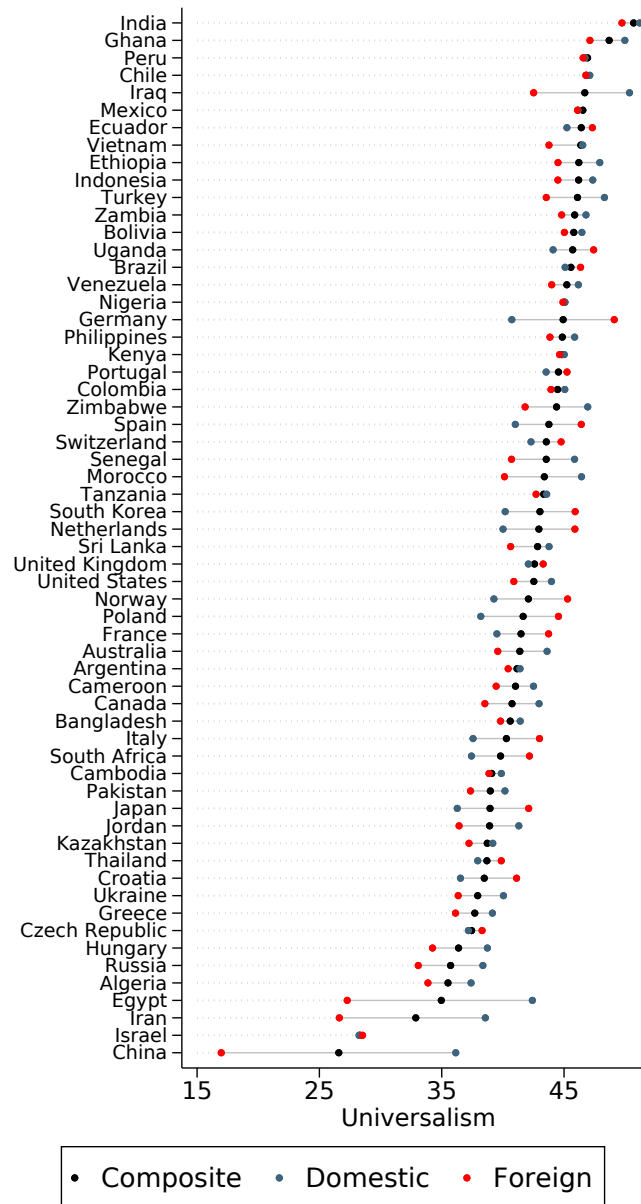


Figure B.8: Average composite, domestic and foreign universalism by country. 0 means that all money is shared with the in-group, 50 equal splits, and 100 that all money is shared with the socially more distant stranger. Composite universalism occasionally doesn't equal the average of domestic and foreign universalism because of missing domestic or foreign universalism data (see Appendix B.6).

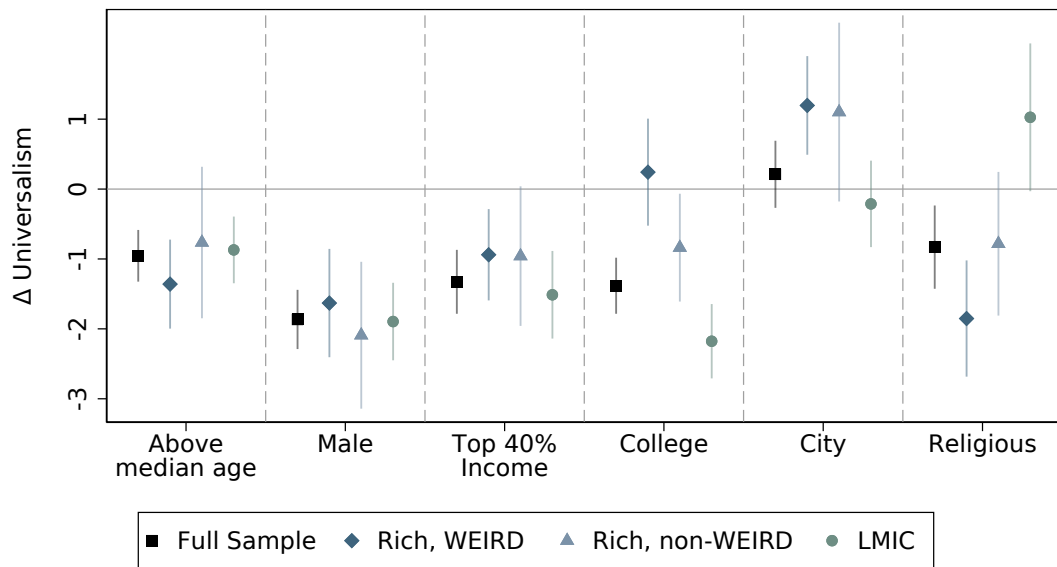


Figure B.9: Universalism and demographics. OLS coefficients from regressions of composite universalism on each demographic, controlling for country and treatment fixed effects. Each coefficient reflects the results of a separate regression on a different sub-sample and can be interpreted as the percentage point change in universalism. All demographics are coded to be binary. Median age and income percentiles are computed separately for each country based on the sample. College captures a college degree, city whether the respondent lives in a big city, and religious whether the respondent reports belonging to a religious denomination. Whiskers show 95% confidence intervals, computed based on robust standard errors, clustered at the sampling unit level. LMIC = low- and middle-income countries. WEIRD = rich Western countries.

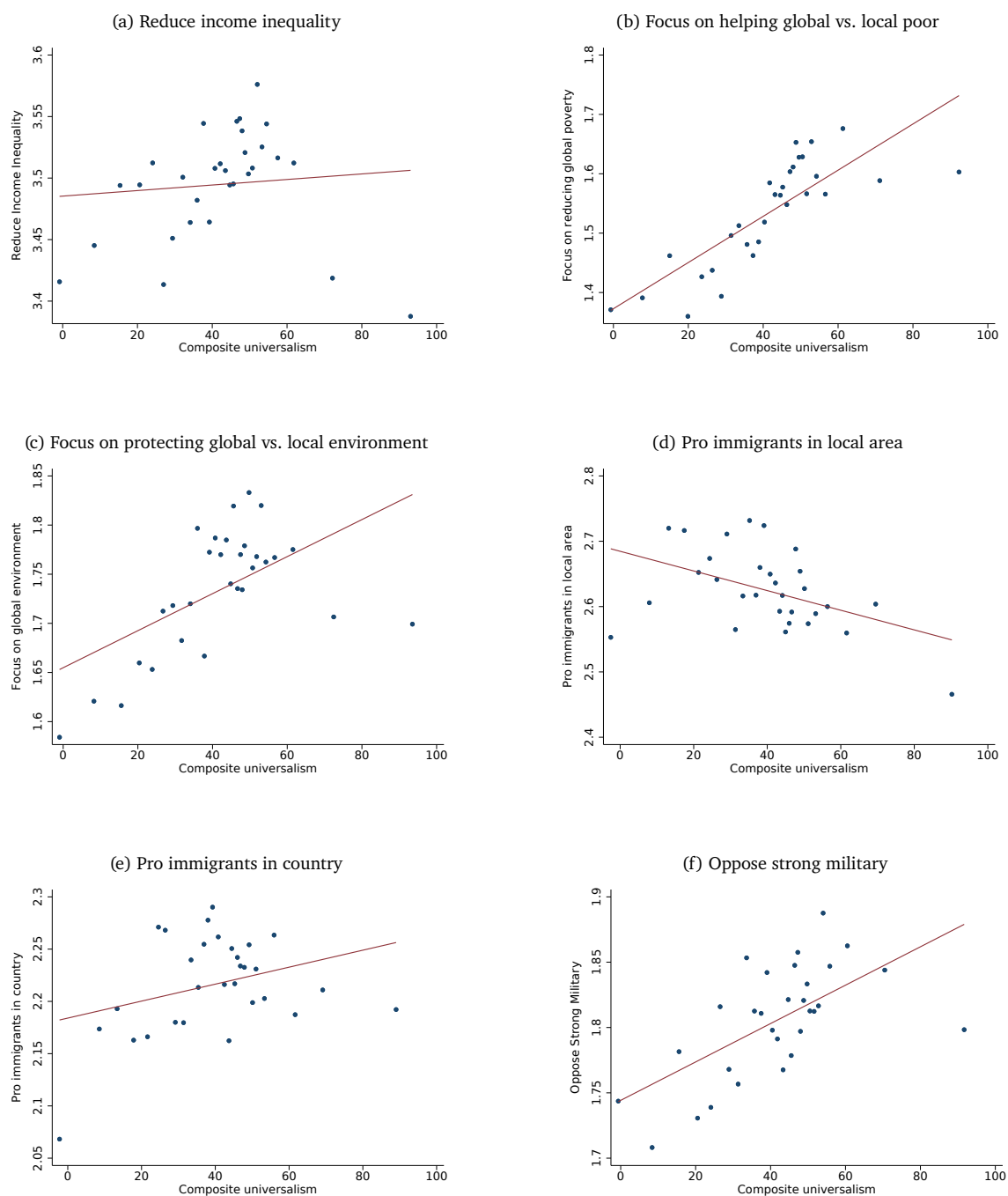


Figure B.10: Composite universalism and political views at the individual level. The figures show binned scatter plots that average agreement with a policy priority for a given level of universalism. The figures are constructed controlling for country and treatment FE. Political views are coded as 1–4, based on responses of “Strongly disagree”, “Disagree”, “Agree” and “Strongly agree.”

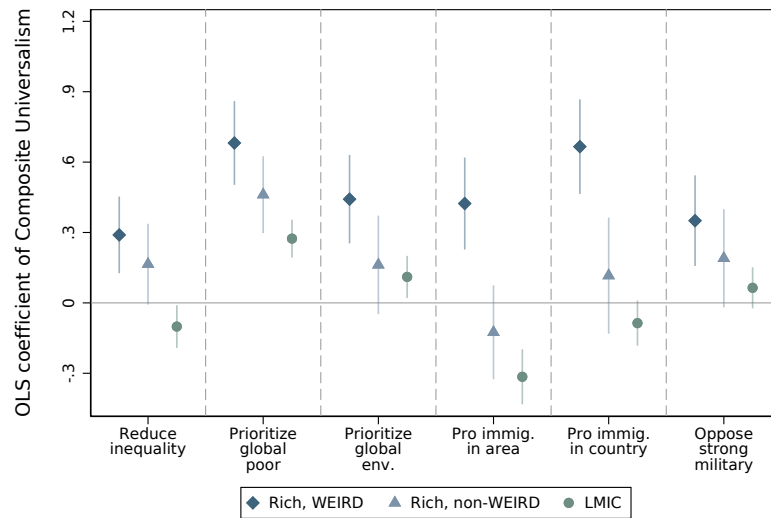


Figure B.11: Individual-level composite universalism and political views in different sub-samples. OLS coefficients from regressions of political attitudes on composite universalism, controlling for country and treatment fixed effects. Each coefficient reflects the results of a separate regression on a different sub-sample and can be interpreted as the change in agreement with a policy priority (on a scale 1–4) in response to a one unit change in universalism. Whiskers show 95% confidence intervals, computed based on robust standard errors, clustered at the sampling unit level. LMIC = low- and middle-income countries. WEIRD = rich Western countries.

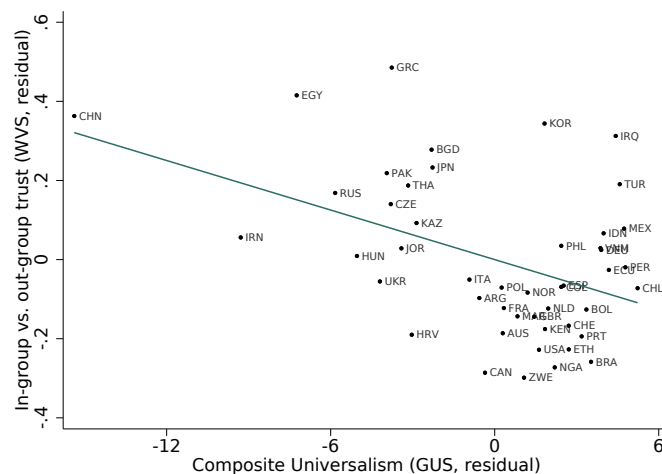


Figure B.12: Country-level added variable plot of the difference between in-group and out-group trust against composite universalism, controlling for log per capita income. The difference between in-group and out-group trust is computed based on World Values Survey questions that ask about trust in three in-groups (family, neighbors, people one knows) and three out-groups (strangers, foreigners, people of another religion).

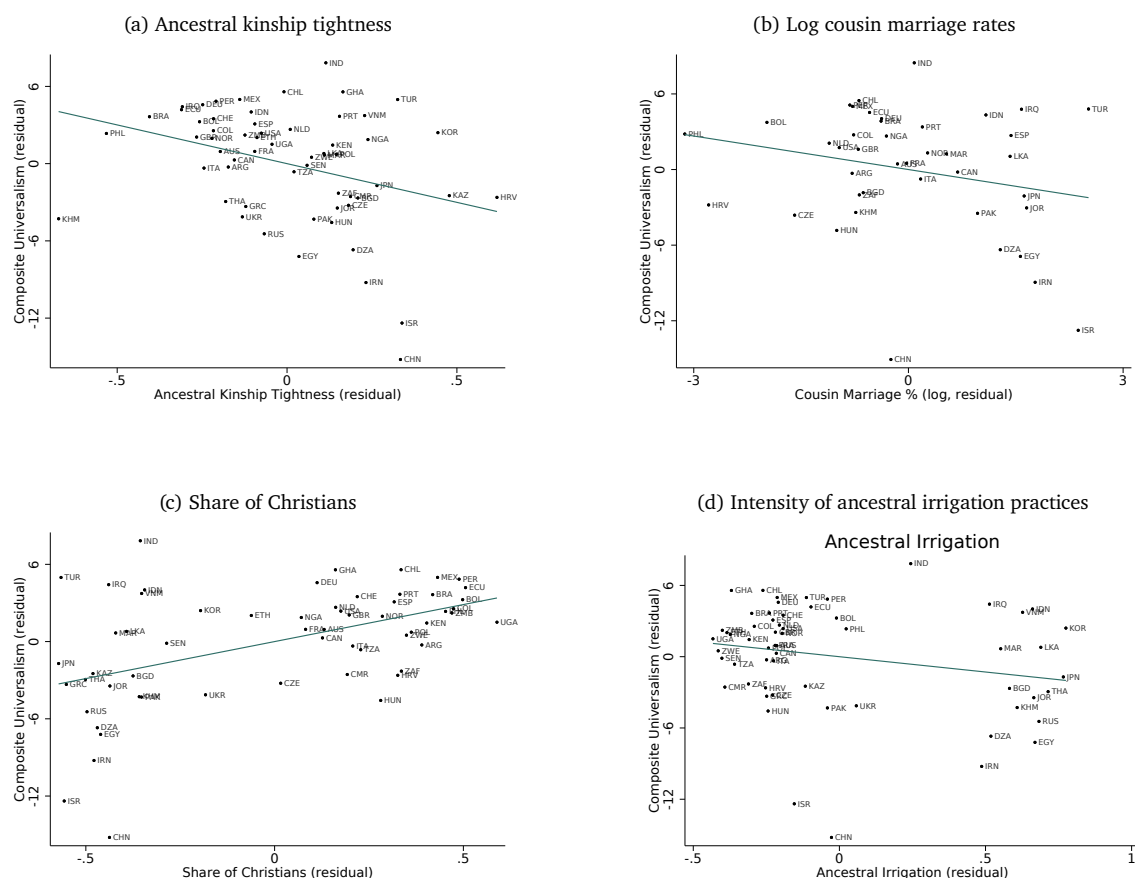


Figure B.13: Country-level partial correlation plots of the cross-country relationships between composite universalism and ancestral kinship tightness (top left panel), the log of contemporary cousin marriage rates (top right panel), Christian share (bottom left panel) and the intensity of ancestral irrigation practices (bottom right panel). Each panel is constructed controlling for log per capita income. Kinship tightness measures the tightness of extended family relationships of the ancestors of today's populations (Enke, 2019). Ancestral irrigation captures how much the ancestors of today's populations relied on irrigation for economic subsistence (Bugge, 2020b). Both kinship tightness and ancestral irrigation practices are measured at the ethnicity-level in the Ethnographic Atlas (Murdock, 1967) and then mapped to contemporary country populations. Country-level cousin marriage rates are from Schulz (2022). The share of Christians is from Barro and McCleary (2003b). Some of the countries in the *GUS* dataset are missing because the respective correlate is not available for all countries.

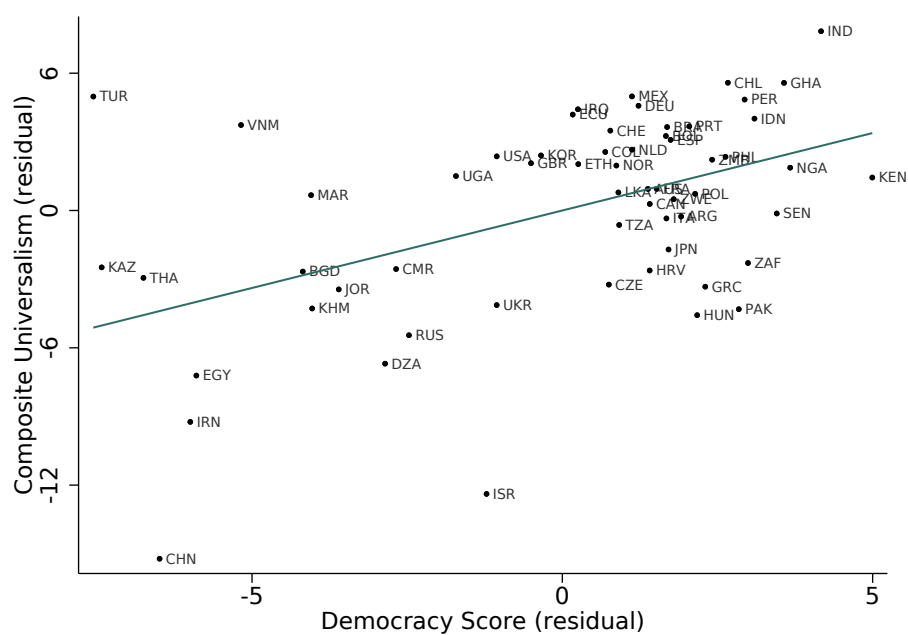


Figure B.14: Country-level added variable plot of composite universalism against democracy, controlling for log per capita GDP. The democracy score ranges from 0 to 10 and is taken from the Polity V dataset. It includes information on competitiveness of executive recruitment, openness of executive recruitment, constraint on chief executive and competitiveness of political participation.

B.8.2 Replication of Tables With Raw Data

Table B.8: Universalism and political views

	<i>Dependent variable:</i>					
	Reduce	Prioritize global vs. domestic		Pro immigrants		Weak
	Inequality	poor	environment	in area	in country	military
	(1)	(2)	(3)	(4)	(5)	(6)
Domestic universalism / 100	0.13*** (0.04)	0.07** (0.03)	0.03 (0.04)	0.06 (0.04)	0.03 (0.04)	-0.06 (0.04)
Foreign universalism / 100	-0.05 (0.03)	0.33*** (0.03)	0.16*** (0.03)	-0.18*** (0.04)	0.08* (0.04)	0.22*** (0.04)
College education	-0.00 (0.02)	0.08*** (0.02)	0.12*** (0.02)	0.15*** (0.02)	0.15*** (0.02)	0.16*** (0.02)
Income quintile	-0.01 (0.00)	0.01*** (0.00)	0.01** (0.01)	0.01 (0.01)	0.01* (0.01)	0.02*** (0.01)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Treatment FE	Yes	Yes	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.065	0.156	0.162	0.149	0.204	0.258
Observations	18528	18676	18478	21248	20951	18430

Notes. Estimates from OLS with robust standard errors, clustered at the sampling unit level. Universalism is divided by 100 for expositional ease. Each observation is an individual. Responses to the political questions are coded as “Strongly agree”, “Somewhat agree”, “Somewhat disagree” and “Strongly disagree”. We transform these into values 1, 2, 3 and 4. We code all political variables such that our pre-registration predicts a positive correlation with universalism. Ordered probit regressions show very similar results. College education is an indicator. Income quintile is a variable with values 1–5. Appendix Table D.4 presents estimates controlling for religiosity (not included in the main analysis because it wasn’t elicited in five countries). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.9: Exposure to democracy and universalism: Variation across country-age-cohorts

	<i>Dependent variable:</i>						
	Universalism						
	Composite			Domestic		Foreign	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Lifetime average democracy score	0.11 (0.11)	0.12 (0.11)	0.04 (0.12)	0.15 (0.12)	0.13 (0.12)	0.10 (0.14)	-0.04 (0.14)
Lifetime average log GDP p/c		1.64 (1.09)	1.24 (1.14)	-0.15 (1.08)	-0.42 (1.12)	2.94** (1.34)	2.41* (1.41)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Treatment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic controls	No	No	Yes	No	Yes	No	Yes
Adjusted R^2	0.08	0.08	0.09	0.04	0.05	0.10	0.11
Observations	55323	55323	53826	54867	53391	53765	52332

Notes. OLS estimates of universalism on democracy exposure with robust standard errors, clustered at the level of 3,468 country-age cells. Exposure to democracy is constructed by taking the mean of the Democracy score time series in the Polity V database over the respondent's lifetime. Demographic controls include gender, income quintile fixed effects, college degree and an indicator for whether an individual lives in a big city. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.10: Exposure to democracy and universalism: Variation across migrants

	<i>Dependent variable:</i>							
	Universalism							
	Composite				Domestic		Foreign	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Democracy score in home country	0.23* (0.12)	0.33** (0.14)	0.30** (0.14)	0.32** (0.15)	0.48*** (0.14)	0.49*** (0.14)	0.15 (0.19)	0.09 (0.19)
Log GDP p/c in home country		-0.20 (0.46)	-0.06 (0.49)	-0.05 (0.70)	0.00 (0.50)	-0.07 (0.51)	-0.40 (0.63)	-0.15 (0.67)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Treatment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic controls	No	No	Yes	Yes	No	Yes	No	Yes
Country of origin controls	No	No	No	Yes	No	No	No	No
Adjusted R^2	0.08	0.08	0.08	0.08	0.06	0.06	0.08	0.08
Observations	2741	2451	2412	2391	2424	2387	2398	2363

Notes. OLS estimates of universalism on democracy in a migrant's country of origin. Standard errors are clustered at the level of 151 countries of origin. Demographic controls include gender, income quintile fixed effects, college degree and an indicator for whether an individual lives in a big city. Country of origin controls include average temperature, precipitation, percentage of the population living in tropical or subtropical zones, and percentage of the population at risk of contracting malaria. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C Additional Figures

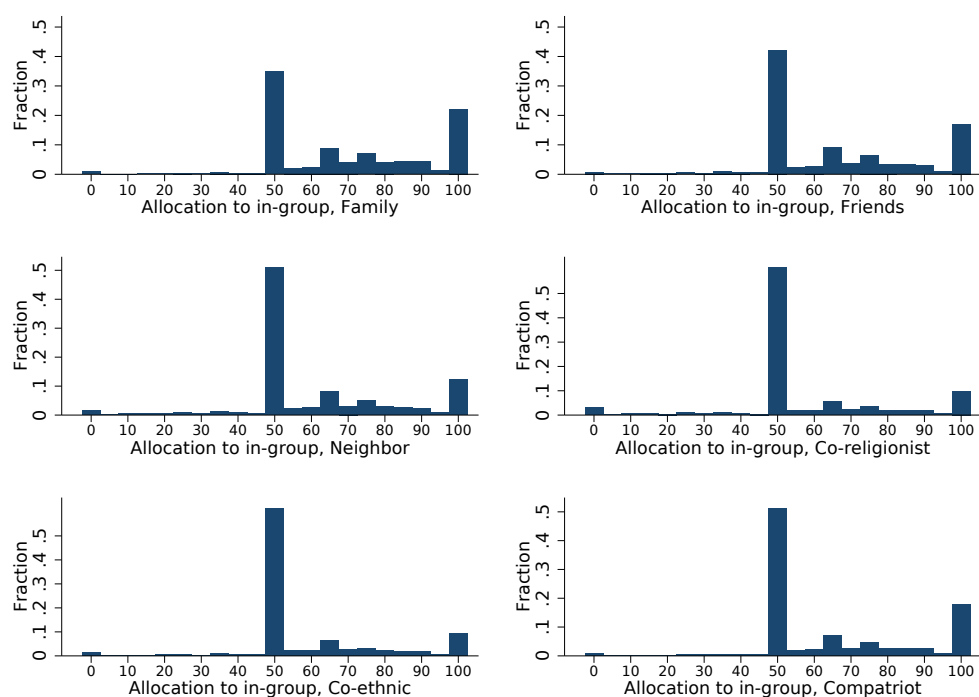


Figure C.1: Histograms of allocations to the in-group across the six survey questions, pooled across treatments. The number of allocations ranges from 23,073 to 25,360 in the first 5 panels. The last panel shows the histogram of 61,753 allocations.

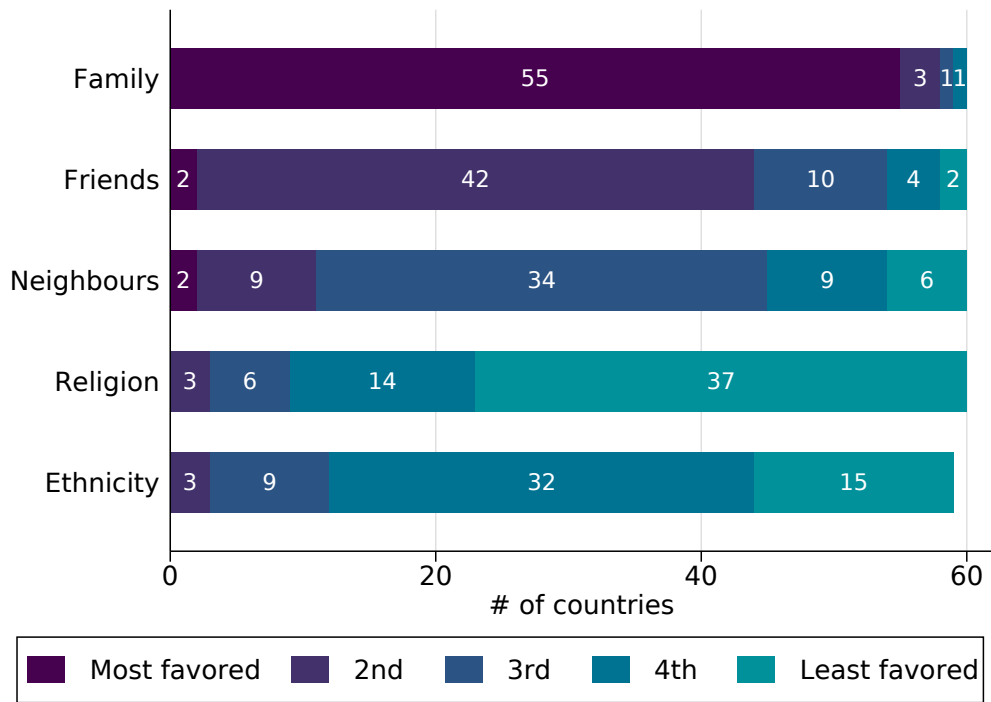


Figure C.2: “Ranking” of in-groups across countries implied by the allocation decisions, pooled across treatments, excluding the Compatriot-Foreigner allocation question. Each section of a bar represents a rank (1 to 5, 1 being the most favored) for that in-group. The size of each section is proportional to the number of countries that assign that rank to the in-group.

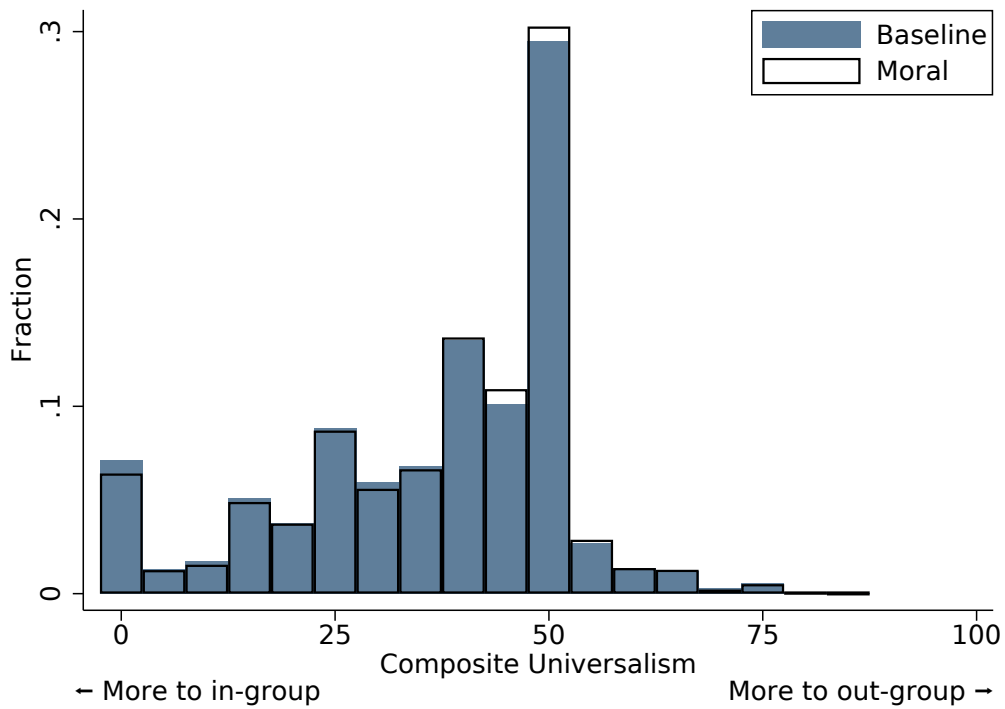


Figure C.3: Distribution of composite universalism across individuals, split by treatment (*Baseline* and *Moral*), $N = 31,670$ and $32,118$ respectively.

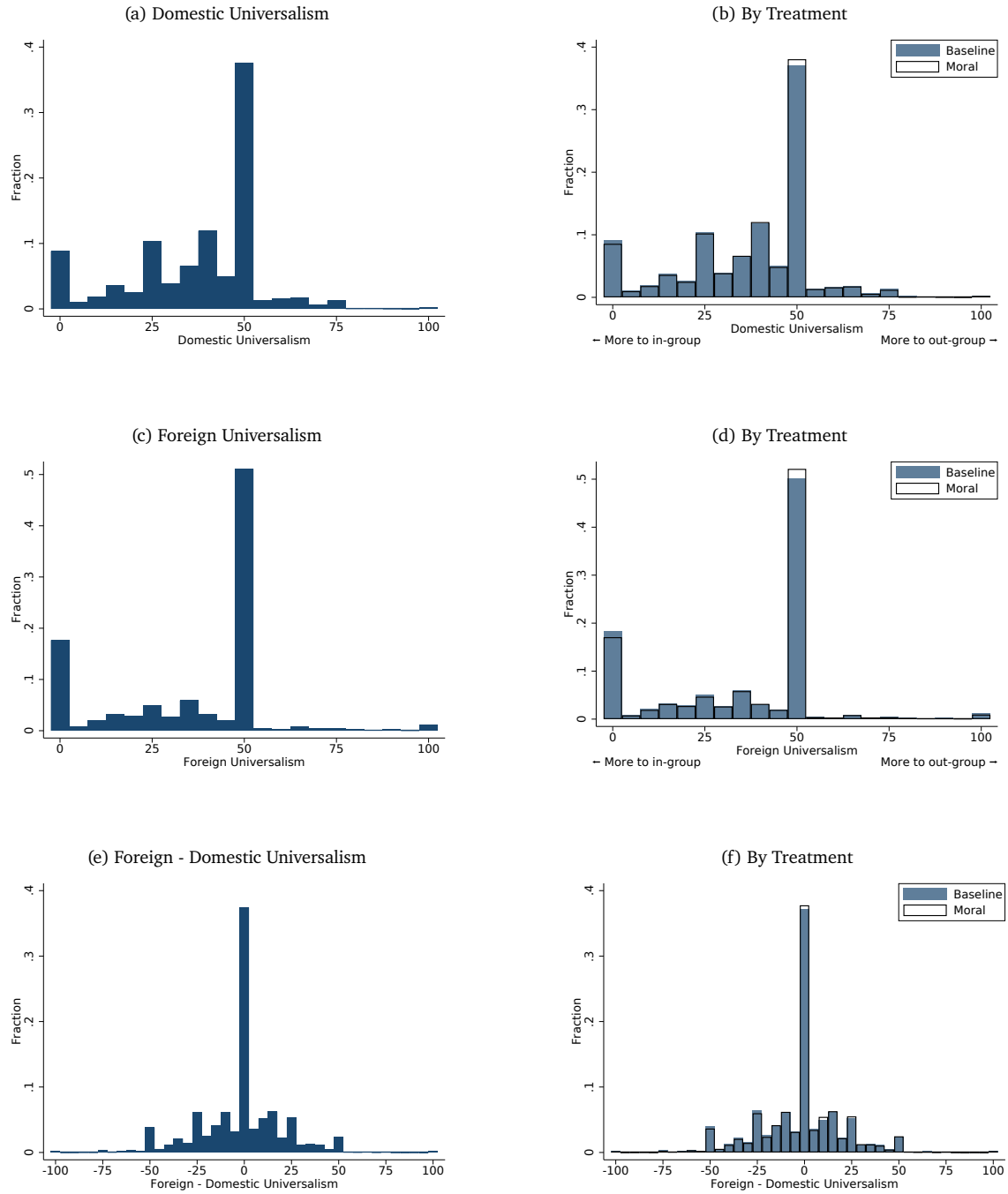


Figure C.4: Distribution of domestic, foreign and foreign - domestic universalism across individuals, separately by treatment (Baseline and Moral).

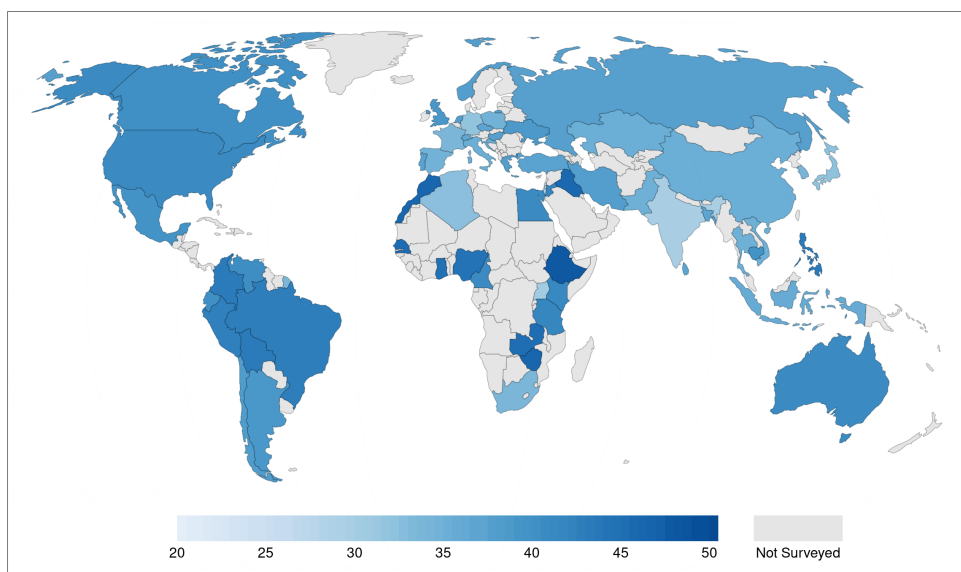


Figure C.5: Global variation in domestic universalism

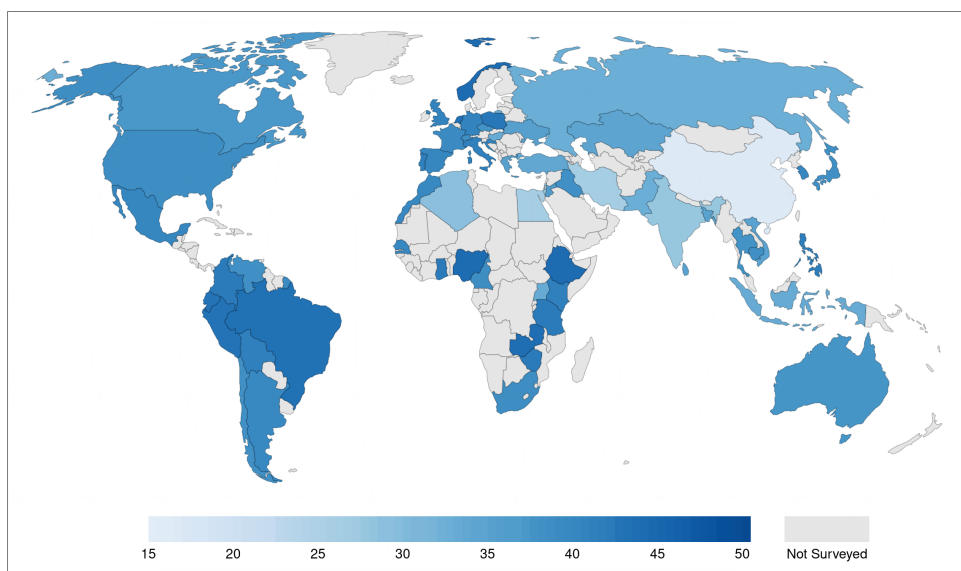


Figure C.6: Global variation in foreign universalism

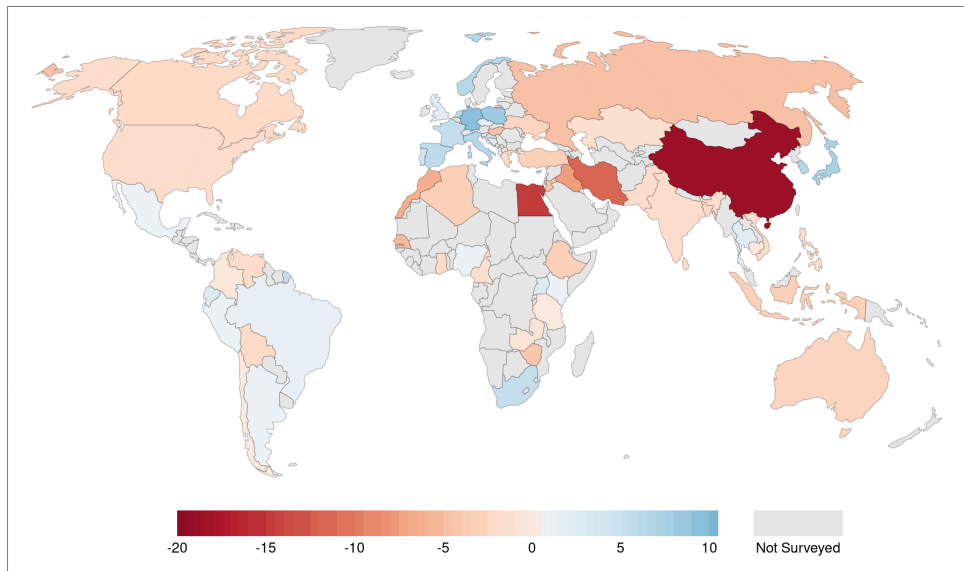


Figure C.7: Global variation in difference between domestic and foreign universalism

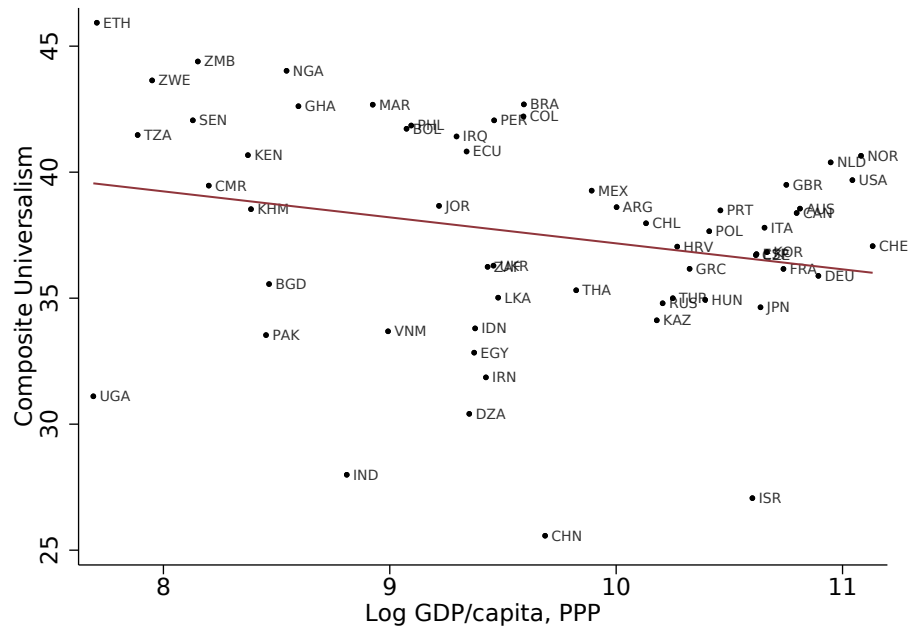


Figure C.8: Correlation between country-level composite universalism and log GDP per capita.

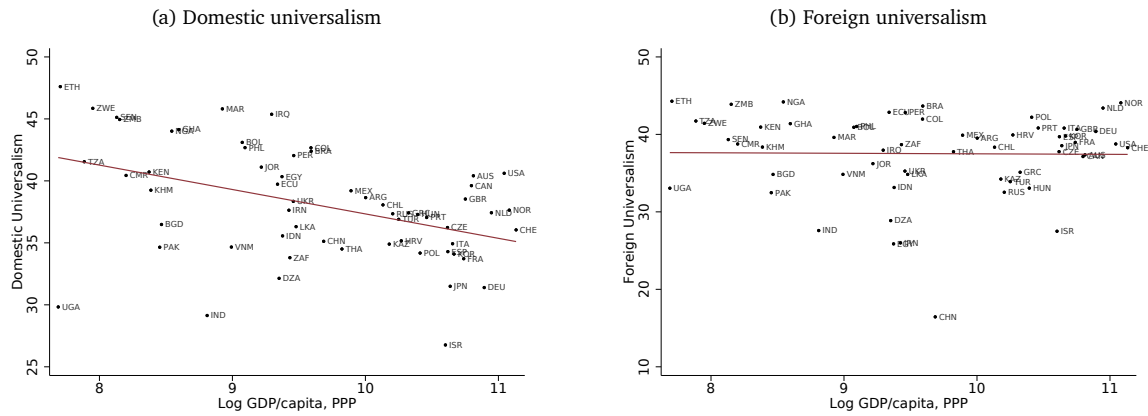


Figure C.9: Domestic / foreign universalism and log GDP per capita. 0 means that all money is shared with the in-group, 50 equal splits, and 100 that all money is shared with the socially more distant stranger.

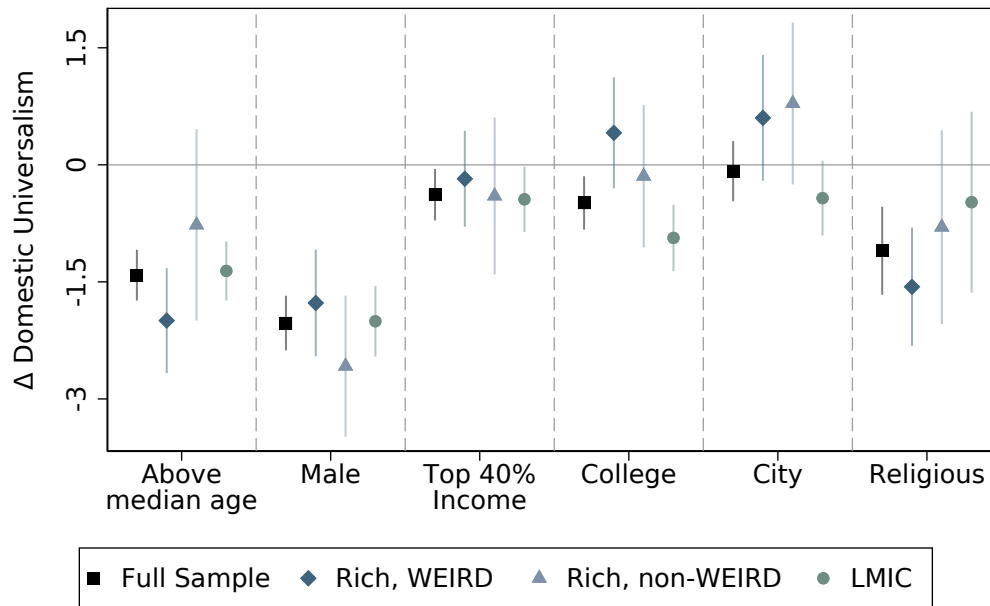


Figure C.10: Domestic universalism and demographics. OLS coefficients from regressions of domestic universalism on demographics, controlling for country and treatment fixed effects. Each coefficient reflects the results of a separate regression and can be interpreted as the percentage point change in universalism. The demographic variables here are indicators; Median age and income quintiles are computed for each country separately. Whiskers show 95% confidence intervals, computed based on robust standard errors, clustered at the sampling unit level.

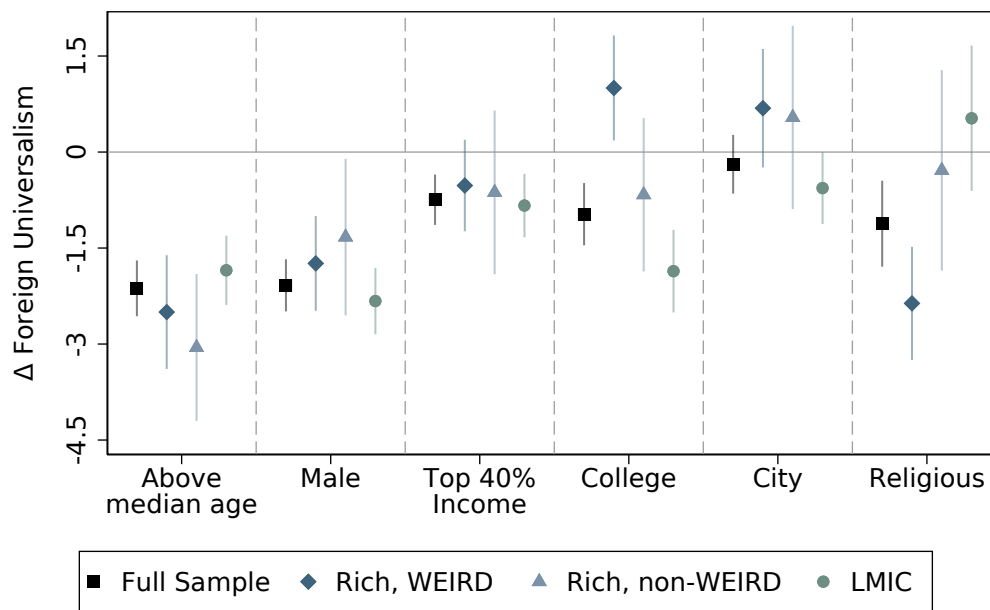


Figure C.11: Foreign universalism and demographics. OLS coefficients from regressions of foreign universalism on demographics, controlling for country and treatment fixed effects. Each coefficient reflects the results of a separate regression and can be interpreted as the percentage point change in universalism. The demographic variables here are indicators; Median age and income quintiles are computed for each country separately. Whiskers show 95% confidence intervals, computed based on robust standard errors, clustered at the sampling unit level.

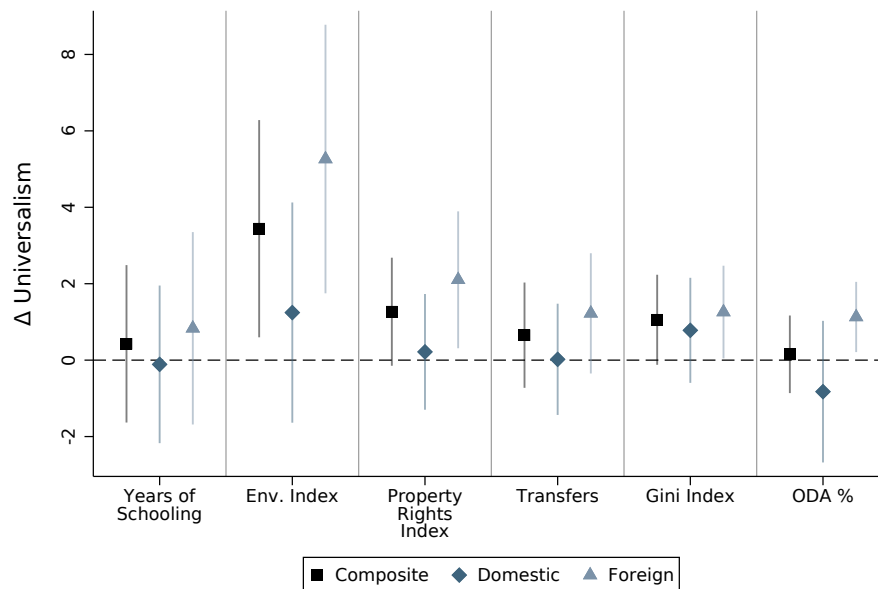


Figure C.12: Additional country-level correlations: Results from OLS regressions of composite universalism on each of various country-level variables, controlling for log GDP p/c. All country characteristics other than universalism are standardized into z-scores. As a result, the coefficients show by how much universalism changes (descriptively) when a country characteristic increases by one standard deviation conditional on log GDP p/c.

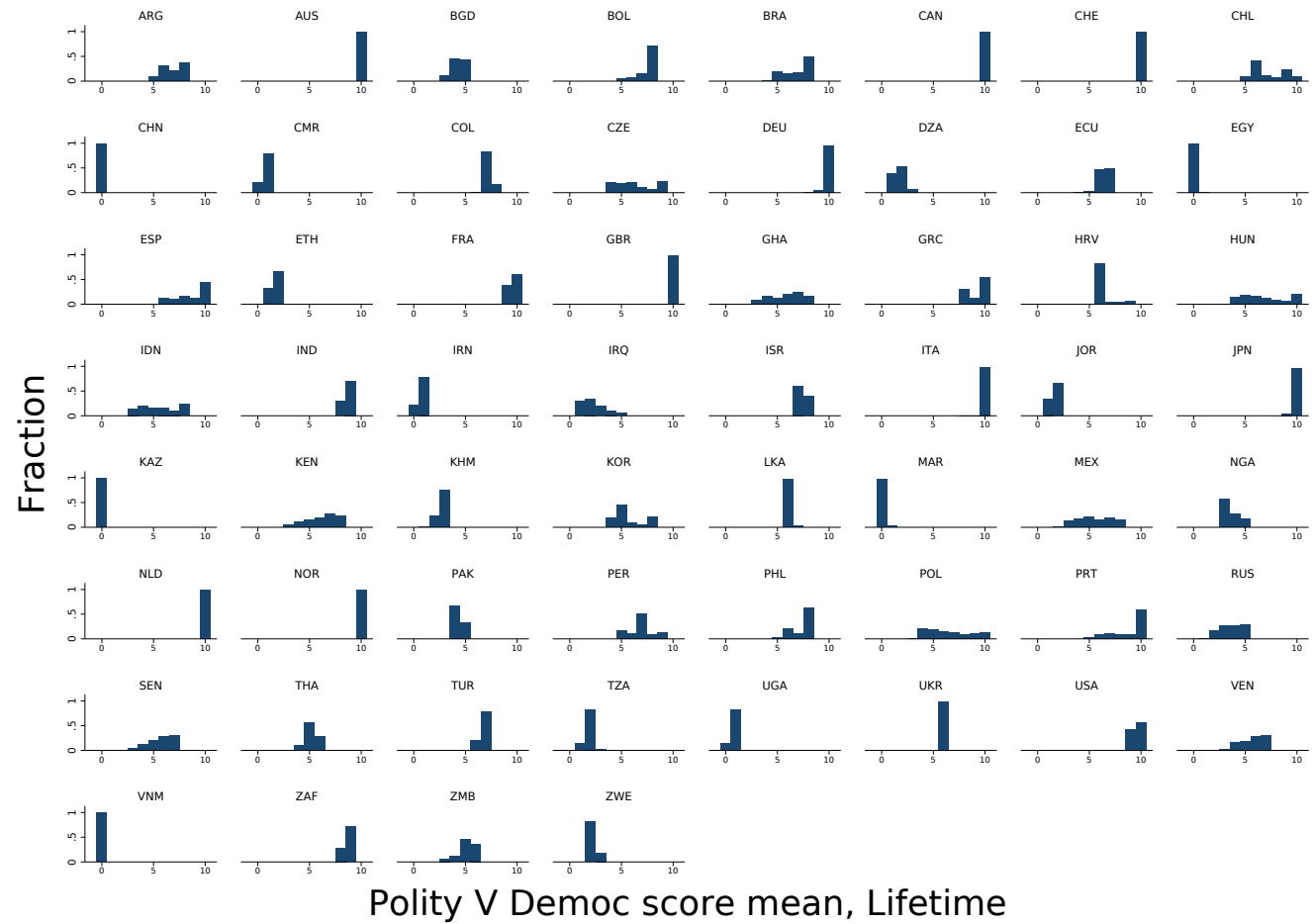


Figure C.13: Country-wise variation in exposure to democracy. Each plot shows the distribution of the average democracy (score from 0 to 10) experienced by an individual in our sample from that country.

D Additional Tables

Table D.1: Pearson correlations among allocation decisions

	Family	Friend	Neighbor	Co-religionist	Co-ethnic	Compatriot
Family	1.00	0.52	0.33	0.21	0.28	0.25
Friend	0.52	1.00	0.45	0.30	0.35	0.28
Neighbor	0.33	0.45	1.00	0.35	0.41	0.27
Co-religionist	0.21	0.30	0.35	1.00	0.45	0.26
Co-ethnic	0.28	0.35	0.41	0.45	1.00	0.30
Compatriot	0.25	0.28	0.27	0.26	0.30	1.00

Notes. Pairwise correlations among individual-level allocation decisions, pooled across treatments.

Table D.2: Spearman rank correlations among allocation decisions

	Family	Friend	Neighbor	Co-religionist	Co-ethnic	Compatriot
Family	1.00	0.55	0.37	0.23	0.28	0.26
Friend	0.55	1.00	0.46	0.31	0.34	0.28
Neighbor	0.37	0.46	1.00	0.36	0.41	0.27
Co-religionist	0.23	0.31	0.36	1.00	0.44	0.25
Co-ethnic	0.28	0.34	0.41	0.44	1.00	0.30
Compatriot	0.26	0.28	0.27	0.25	0.30	1.00

Notes. Spearman rank order pairwise correlations among individual-level allocation decisions, pooled across treatments.

Table D.3: Treatment effects on universalism

	Composite Universalism		Domestic		Foreign	
	(1)	(2)	(3)	(4)	(5)	(6)
Moral	0.604*** (0.135)	0.518*** (0.134)	0.394*** (0.147)	0.374** (0.148)	0.773*** (0.185)	0.617*** (0.183)
Constant	36.280*** (0.338)	36.742*** (0.541)	37.369*** (0.343)	37.166*** (0.558)	35.650*** (0.403)	36.205*** (0.739)
Country FE	No	Yes	No	Yes	No	Yes
Demog. controls	No	Yes	No	Yes	No	Yes
Adjusted R^2	0.000	0.073	0.000	0.066	0.000	0.056
Observations	63788	57769	63230	57353	61753	56016

Notes. OLS results from regressing universalism on an indicator for the *Moral* treatment, controlling for demographic and country characteristics. Standard errors (in parentheses) clustered at the sampling unit level. Controls are a person's age, square of the age and indicators for the country of residence and whether the person is male, college educated, religious, lives in a city, is in the top 40% of the income distribution in the country sample. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.4: Composite universalism and political views: Controlling for Religiosity

	<i>Dependent variable:</i>					
	Reduce	Prioritize global vs. domestic		Pro immigrants		Weak
	Inequality	poor	environment	in area	in country	military
	(1)	(2)	(3)	(4)	(5)	(6)
Domestic universalism / 100	0.18*** (0.04)	0.08** (0.04)	0.09** (0.04)	0.23*** (0.05)	0.07 (0.05)	-0.06 (0.04)
Foreign universalism / 100	-0.00 (0.03)	0.34*** (0.03)	0.23*** (0.03)	-0.01 (0.05)	0.23*** (0.05)	0.22*** (0.04)
College education	0.00 (0.02)	0.07*** (0.02)	0.12*** (0.02)	0.14*** (0.02)	0.15*** (0.02)	0.15*** (0.02)
Income quintile	-0.01 (0.00)	0.01** (0.01)	0.01* (0.01)	0.01* (0.01)	0.01** (0.01)	0.01** (0.01)
Religious	-0.04* (0.02)	-0.04 (0.02)	-0.12*** (0.03)	-0.12*** (0.03)	-0.18*** (0.03)	-0.28*** (0.03)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Treatment FE	Yes	Yes	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.066	0.156	0.166	0.163	0.220	0.267
Observations	17985	18074	17875	17682	17471	17897

Notes. Estimates from OLS with robust standard errors, clustered at the sampling unit level. Controls are a person's age, square of the age, indicators for the country of residence, treatment and whether the person is male, lives in a city. Universalism is divided by 100 for expositional ease. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.5: Political Views and Composite Universalism in sub-samples

	<i>Dependent variable: Focus on reducing inequality</i>							
	Full sample (1)	WEIRD (2)	HIC, Non-WEIRD (3)	LMIC (4)	Full sample (5)	WEIRD (6)	HIC, Non-WEIRD (7)	LMIC (8)
Universalism / 100	0.169*** (0.043)	0.466*** (0.085)	0.186* (0.101)	0.055 (0.054)	0.162*** (0.043)	0.406*** (0.084)	0.215** (0.097)	0.053 (0.055)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Treatment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demog. controls	No	No	No	No	Yes	Yes	Yes	Yes
Adjusted R^2	0.063	0.046	0.102	0.060	0.065	0.048	0.116	0.062
Observations	19753	4335	2621	12797	18990	4248	2550	12192

Notes. Estimates from OLS of responses to survey questions on political views (coded from 1 to 4, coded such that 4 is the predicted correlation with universalism) on universalism, controlling for treatment and country effects with robust standard errors, clustered at the sampling unit level. Columns (5) – (8) show estimates controlling for individual demographic characteristics (age, square of the age and indicators for the country of residence and whether the person is male, college educated, religious, lives in a city, is in the top 40% of the income distribution in the country sample). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.6: Political Views and Composite Universalism in sub-samples

	<i>Dependent variable: Focus global vs local poor</i>							
	Full sample (1)	WEIRD (2)	HIC, Non-WEIRD (3)	LMIC (4)	Full sample (5)	WEIRD (6)	HIC, Non-WEIRD (7)	LMIC (8)
Universalism / 100	0.414*** (0.040)	0.749*** (0.094)	0.469*** (0.082)	0.279*** (0.047)	0.424*** (0.041)	0.699*** (0.095)	0.463*** (0.087)	0.295*** (0.048)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Treatment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demog. controls	No	No	No	No	Yes	Yes	Yes	Yes
Adjusted R^2	0.146	0.071	0.110	0.041	0.149	0.082	0.126	0.042
Observations	19942	4364	2570	13008	19092	4265	2482	12345

Notes. Estimates from OLS of responses to survey questions on political views (coded from 1 to 4, coded such that 4 is the predicted correlation with universalism) on universalism, controlling for treatment and country effects with robust standard errors, clustered at the sampling unit level. Columns (5) – (8) show estimates controlling for individual demographic characteristics (age, square of the age and indicators for the country of residence and whether the person is male, college educated, religious, lives in a city, is in the top 40% of the income distribution in the country sample). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.7: Political Views and Composite Universalism in sub-samples

	<i>Dependent variable: Focus global vs local environment</i>							
	Full sample (1)	WEIRD (2)	HIC, Non-WEIRD (3)	LMIC (4)	Full sample (5)	WEIRD (6)	HIC, Non-WEIRD (7)	LMIC (8)
Universalism / 100	0.329*** (0.047)	0.807*** (0.095)	0.383*** (0.127)	0.143** (0.056)	0.303*** (0.047)	0.701*** (0.096)	0.409*** (0.131)	0.126** (0.058)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Treatment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demog. controls	No	No	No	No	Yes	Yes	Yes	Yes
Adjusted R^2	0.150	0.036	0.117	0.052	0.161	0.053	0.133	0.057
Observations	19700	4229	2529	12942	18861	4136	2456	12269

Notes. Estimates from OLS of responses to survey questions on political views (coded from 1 to 4, coded such that 4 is the predicted correlation with universalism) on universalism, controlling for treatment and country effects with robust standard errors, clustered at the sampling unit level. Columns (5) – (8) show estimates controlling for individual demographic characteristics (age, square of the age and indicators for the country of residence and whether the person is male, college educated, religious, lives in a city, is in the top 40% of the income distribution in the country sample). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.8: Political Views and Composite Universalism in sub-samples

	<i>Dependent variable: Pro immigrants in area</i>							
	Full sample (1)	WEIRD (2)	HIC, Non-WEIRD (3)	LMIC (4)	Full sample (5)	WEIRD (6)	HIC, Non-WEIRD (7)	LMIC (8)
Universalism / 100	0.022 (0.057)	0.509*** (0.102)	0.107 (0.135)	-0.133* (0.072)	0.135** (0.063)	0.467*** (0.102)	0.138 (0.134)	-0.003 (0.086)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Treatment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demog. controls	No	No	No	No	Yes	Yes	Yes	Yes
Adjusted R^2	0.137	0.103	0.220	0.075	0.157	0.121	0.244	0.083
Observations	22619	4144	2586	15889	18640	4052	2521	12067

Notes. Estimates from OLS of responses to survey questions on political views (coded from 1 to 4, coded such that 4 is the predicted correlation with universalism) on universalism, controlling for treatment and country effects with robust standard errors, clustered at the sampling unit level. Columns (5) – (8) show estimates controlling for individual demographic characteristics (age, square of the age and indicators for the country of residence and whether the person is male, college educated, religious, lives in a city, is in the top 40% of the income distribution in the country sample). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.9: Political Views and Composite Universalism in sub-samples

	<i>Dependent variable: Pro immigrants in country</i>							
	Full sample (1)	WEIRD (2)	HIC, Non-WEIRD (3)	LMIC (4)	Full sample (5)	WEIRD (6)	HIC, Non-WEIRD (7)	LMIC (8)
Universalism / 100	0.175*** (0.053)	0.932*** (0.109)	0.361** (0.138)	-0.073 (0.060)	0.269*** (0.056)	0.813*** (0.107)	0.343** (0.135)	0.024 (0.071)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Treatment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demog. controls	No	No	No	No	Yes	Yes	Yes	Yes
Adjusted R^2	0.192	0.137	0.270	0.121	0.211	0.167	0.299	0.109
Observations	22225	4215	2482	15528	18376	4113	2418	11845

Notes. Estimates from OLS of responses to survey questions on political views (coded from 1 to 4, coded such that 4 is the predicted correlation with universalism) on universalism, controlling for treatment and country effects with robust standard errors, clustered at the sampling unit level. Columns (5) – (8) show estimates controlling for individual demographic characteristics (age, square of the age and indicators for the country of residence and whether the person is male, college educated, religious, lives in a city, is in the top 40% of the income distribution in the country sample). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.10: Political Views and Composite Universalism in sub-samples

	<i>Dependent variable: Oppose strong military</i>							
	Full sample (1)	WEIRD (2)	HIC, Non-WEIRD (3)	LMIC (4)	Full sample (5)	WEIRD (6)	HIC, Non-WEIRD (7)	LMIC (8)
Universalism / 100	0.186*** (0.049)	0.499*** (0.107)	0.496*** (0.150)	-0.013 (0.051)	0.183*** (0.048)	0.420*** (0.102)	0.471*** (0.150)	0.002 (0.051)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Treatment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demog. controls	No	No	No	No	Yes	Yes	Yes	Yes
Adjusted R^2	0.240	0.157	0.165	0.131	0.257	0.191	0.180	0.145
Observations	19628	4205	2593	12830	18866	4111	2525	12230

Notes. Estimates from OLS of responses to survey questions on political views (coded from 1 to 4, coded such that 4 is the predicted correlation with universalism) on universalism, controlling for treatment and country effects with robust standard errors, clustered at the sampling unit level. Columns (5) – (8) show estimates controlling for individual demographic characteristics (age, square of the age and indicators for the country of residence and whether the person is male, college educated, religious, lives in a city, is in the top 40% of the income distribution in the country sample). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

E Decomposing Cross-Group Differences

The extant literature on universalism vs. in-group favoritism focuses on people’s distributive *behavior*. Yet, as is well-known in the literature on distributive preferences, variation in observed behavior can often be decomposed into what people deem the morally right thing to do (their *moral views*) and what they personally prefer. To take a simple example, it is conceivable that in treatment *Baseline*, people believe that the morally right thing to do would be to split the money equally, but that they actually share more with their in-group members because they care more about them. As a result, it is a priori unclear whether cross-group differences (across demographic groups or countries) reflect differences in moral views or group-specific altruism weights.

To study this, we also implemented treatment *Moral*, in which respondents were asked to do what they consider morally right. Behavior in the two treatments is very similar to each other in the global sample as a whole. This means that (i) universalistic behavior largely reflects moral universalism and (ii) that deviations from universalistic behavior largely reflect that people have the moral view that they have specific obligations to their in-group. Yet, while a large majority of the heterogeneity in our dataset reflects moral views, this is not true for all cross-group differences. To document this, we here present a decomposition exercise that decomposes differences across different groups (such as demographic groups or countries) into differences in moral views and differences in altruism weights.

The idea is that if demographic differences in treatment *Moral* are exactly as large as those in *Baseline*, then the entirety of cross-group differences is attributable to moral views. If, on the other hand, differences in *Moral* are smaller, then a part of the demographic differences reflects differences in group-specific altruism weights, rather than in moral views.

Decomposing differences across demographic groups. As shown in the main text, the most pronounced and consistent demographic differences are that older people and men are less universalist. Do these differences reflect that men vs. women and young vs. old people have different moral views or that they have different altruism weights for different social groups? Table E.1 summarizes the results of regressions that link composite universalism to (i) demographics; (ii) a treatment indicator and (iii) their interaction. Columns (1) and (2) show the age and gender difference in universalism in treatment *Baseline*. Columns (3) and (4) show that these differences are also statistically highly significant and quantitatively large in treatment *Moral*, which provides evidence for an important role of moral views in explaining these cross-group differences. In columns (5) and (6), we report the regressions for the pooled sample allowing for an

Table E.1: Decomposition of demographic differences in universalism

	<i>Dependent variable: Composite Universalism</i>					
	<i>Baseline</i>		<i>Moral</i>		Full sample	
	(1)	(2)	(3)	(4)	(5)	(6)
Above median age	-2.30*** (0.21)	-2.31*** (0.22)	-1.55*** (0.22)	-1.58*** (0.22)	-2.30*** (0.21)	-2.31*** (0.22)
Male	-2.44*** (0.22)	-2.46*** (0.23)	-1.72*** (0.21)	-1.62*** (0.21)	-2.39*** (0.22)	-2.39*** (0.23)
<i>Moral</i>					-0.14 (0.22)	-0.18 (0.22)
Above median age $\times$ <i>Moral</i>					0.75*** (0.26)	0.72*** (0.27)
Male $\times$ <i>Moral</i>					0.62** (0.26)	0.71*** (0.27)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Demographic controls	No	Yes	No	Yes	No	Yes
Adjusted R^2	0.09	0.09	0.09	0.10	0.09	0.09
Observations	31670	30863	32118	31317	63788	62180

Notes. Individual-level OLS estimations of composite universalism on demographic variables and their interactions with an indicator for the *Moral* treatment. Standard errors (in parentheses) are clustered at the sampling unit level. Controls include college degree, urban residence, and income quintile fixed effects. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

interaction effect for the treatment. The estimated interaction effect identifies whether cross-group differences partly reflect differences in altruism weights or only differences in moral views. We observe in column (1) that the difference in the behavior of people below and above median age is -2.3, and from (3) that the difference in moral views between these groups is -1.55. This implies that the difference in the treatment effect is 0.75, as shown in column (5). Hence, the regression estimates suggest that about 70% of the age difference in universalistic behavior reflects that older people have less universalistic moral views, and that about 30% of the difference reflects that older people have less universalistic altruism weights. This decomposition is quantitatively almost identical for the gender difference.

F Variable Descriptions for Country-Level Variables

Kinship tightness score: The Kinship tightness variable from Enke (2019) which captures the strength of ancestral kinship ties at the country level.

Ancestral irrigation: Variable taken from Buggle (2020b,a), captures how much the ancestors of today's populations relied on irrigation for subsistence.

Democracy: This is a score from 0 to 10 with 10 being the most democratic (insofar as that can be defined and indexed). The elements of this index are: Competitiveness of Executive Recruitment, Openness of Executive Recruitment, Constraint on Chief Executive and Competitiveness of Political Participation. Taken from Polity V dataset (Marshall and Gurr, 2020).

Gini index: This is the Gini index, using the most recent value that is available for each country in the World Bank WDI database (World Bank, 2021).

Share Christians: This is the share of Christians in a country. Data taken from Barro and McCleary (2003b).

Development Aid: Official Development Assistance as a percent of Gross National Income. Data from OECD/WDI (World Bank, 2021). This variable is only available for a few countries in the sample.

Environmental Indices: Environmental health, Ecological Vitality and the Environmental Performance Index are drawn from the 2020 release of the Environmental Performance Index (Wolf et al., 2022).

Property Rights: We use measures of property rights and other governance indicators from the Quality of Governance data set (2021 release) (Bank, 2016; Dahlberg and Nilsson, 2024).

Years of Schooling: We use the data collected by Barro and Lee (2013).

Government transfers: We use the transfers series from the World Development Indicators (World Bank, 2021).

Religion: We use the country shares of each religion from the Barro and McCleary (2003a) data set.

Relative Trust: The variable is constructed using the trust questions in the World Values Survey (Haerpfer et al., 2022; EVS/WVS, 2024). We take difference between the average of trust towards family/neighbors/personal acquaintances and the average of trust towards foreigners/people belonging to other religions/people met for the first time.

Cousin Marriage: This is taken from the country level data set used in Schulz (2022); Schulz et al. (2019a). We use the log of cousin marriage percentage at the country level.

Temperature, precipitation, population in tropical zones and population at risk of contracting malaria: Taken from Ashraf and Galor (2013a,b).

GDP time series: Taken from Bolt and van Zanden (2020)

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