

UNIVERSALISM: GLOBAL EVIDENCE^{*}

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Abstract

This paper leverages nationally representative surveys across 60 countries and 64,000 respondents to present novel stylized facts about the relationship-specific nature of altruism. Across individuals, universalist preferences systematically vary with demographics such as age and religiosity, and are predictive of many left-wing political views, albeit in culturally highly heterogeneous ways. Across countries, universalism is strongly linked to a broader radius of trust. Looking at origins, universalism varies with the economic, political and religious organization of societies in ways that are consistent with the idea that the scope of altruism is partly shaped by economic incentives and democracy.

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The circle of altruism has broadened from the family and tribe to the nation and race, and we are beginning to recognize that our obligations extend to all human beings.

Peter Singer, *The Expanding Circle*, 1981

If you believe you are a citizen of the world, you are a citizen of nowhere. [Many] feel the strongest sense of solidarity with those who share their history, language and common culture.

Theresa May, UK Prime Minister, 2016

1 Introduction

Economists have long understood that group membership and identity are important determinants of economic behavior. In prominent economic models (e.g., Tabellini, 2008b), a person's *universalism* reflects to what degree their altruism is invariant to the identity or group membership of the other person. While it is by now clear that people are typically more altruistic (and trusting) towards in-group members, much recent research – and the quotes above – highlight the existence of pronounced *heterogeneity* in people's radius of altruism. Heterogeneity in universalism has attracted considerable interest, partly because an active recent theoretical and empirical literature has linked it to variables such as social cooperation, voting, attitudes towards redistribution, immigration or climate change, the internal organization of firms, hiring processes, friendship networks, donations, and the abolitionist movement (e.g., Tabellini, 2008b,a; Haidt, 2012; Greif and Tabellini, 2017; Enke, 2020, forthcoming; Enke et al., 2023b, 2022, 2023a; Henrich, 2020; Andre et al., 2021; Le Rossignol and Lowes, 2022; Fehr et al., 2022; Landier and Thesmar, 2022; Figueroa and Fouka, 2023). For example, the heated discussions about immigration; transnational redistribution in the framework of the EU; or policies aimed at tackling global climate change are difficult to understand without explicitly recognizing heterogeneity in universalism.

Existing efforts to collect controlled data on universalism only involve a handful of (mostly rich, Western) countries or small convenience samples. The scarcity of controlled representative data is problematic both because it prevents large-scale global analyses, and because of the prominent criticism that stylized facts about preferences, as well as their linkages with behaviors, political views or demographics, may not generalize beyond convenience participant pools (e.g., Henrich et al., 2010b).

To further our understanding of the role of universalism in society, this paper introduces the *Global Universalism Survey (GUS)*, the first large-scale global dataset on the extent to which people make universalistic distributive decisions in monetary tradeoffs between in-group members and strangers. By introducing this data, we (i) present a new set of stylized facts that exposit the variation in universalism within and across coun-

tries; (ii) highlight the relevance of this heterogeneity by reporting correlations with individual-level political views; (iii) document pronounced cultural specificity in how universalism is linked to respondent demographics or political views; (iv) show that universalism is strongly predictive of across-society variation in social capital (the radius of trust); (v) document country-level correlations with “deep” historical, economic and religious variables that are consistent with functional economic origins of the global variation in universalism; and (vi) tentatively identify experience with democracy as a partial driver of heterogeneity in universalism across individuals and cultures. In doing so, the paper is almost entirely descriptive in nature.

Data. Our survey was implemented through the infrastructure of the 2020 Gallup World Poll. The data cover nationally representative samples – based on random sampling techniques and ex post survey weights – in each of 60 countries, with a total sample size of about 64,000 respondents. The countries were selected to be broadly representative of the world population, to move beyond the overrepresentation of Western populations that is endemic to most multinational studies.

The dataset consists of a series of disinterested distributive decisions in which the respondent is tasked with distributing the local currency equivalent of hypothetical \$1,000 between two individuals. We measure both domestic universalism, capturing how people allocate money between different groups in their own country, and foreign universalism, capturing how people split money between compatriots and non-compatriots. For example, in one question, respondents in the U.S. were asked how they would allocate \$1,000 between a friend and a stranger from the U.S.

The survey questions (i) underwent extensive pre-tests in countries of different cultural heritage, (ii) were translated using professional back-and-forth techniques and (iii) involved comparable monetary amounts that were scaled by national income. We provide a within-survey validation of our measure by documenting that both individual-level universalism and country-level average universalism are correlated with the probability of reporting recently having helped a stranger. We discuss in detail potential data quality issues, and find no indication that these differ between economically developed and developing nations. For example, the correlation with our validation variable and with exogenous demographics like age and gender, is essentially identical across rich and poor countries.

Relationship-specific altruism. Our data provide evidence for relationship-specific altruism, with people allocating more to their in-groups than to a stranger in their country. The extent to which people deviate from universalism depends on the nature of the in-group: respondents are substantially less universalistic when the in-group member is a

family member rather than a co-ethnic or a co-religionist. We also find that respondents exhibit relatively large in-group favoritism when making a distributive decision between a compatriot and a global stranger.

Heterogeneity in universalism. There is large individual-level variation in universalism. Around 26% of respondents always act in line with universalism and divide the money equally in all decisions, while 17% of respondents share at most 20% of the money with the stranger across the different situations. In almost all countries, younger people and women are more universalist, and the magnitude of these relationships is very similar in high- and low-income countries. For the more endogenous individual characteristics, we often find pronounced cultural specificity. For example, based on prior evidence in Western samples, we pre-registered the prediction that urbanicity and a college degree would be positively correlated with universalism. Yet in our global data, we see that while well-educated city dwellers are indeed more universalist in Western Europe, the U.S. and Australia, they are actually significantly less universalist outside of this narrow set of countries.

We also find large heterogeneity in average universalism across countries: money shared with the strangers ranges from around 26% in China, India and Israel to 46% in Ethiopia. Perhaps surprisingly, per capita income is slightly negatively correlated with universalism. This result is partly but not entirely driven by many Sub-Saharan populations making relatively universalist decisions. The negative cross-country relationship goes against a folk wisdom in cultural psychology that – based on indirect and small-scale data – views richer nations as particularly universalist (Henrich et al., 2010b; Henrich, 2020).

Political views. To study whether heterogeneity in universalism is consequential for understanding individual behaviors, we first investigate the relationship between universalism and economic and social policy views. Prior work has argued that many canonical left-wing policies have a universalist focus, so that universalism should be predictive of support for these policies (Enke et al., 2023b). For instance, governmental redistribution is a very universalist concept compared to the small-scale group-based redistributive mechanisms that have prevailed for the most part of human history (and still do in many places). A fortiori, policies that aim at supporting immigrants, needy people abroad, or preventing global climate change are highly universalist in nature. In contrast, a strong military is in some ways an antidote to universalism because it serves to defend boundaries between ‘us’ and ‘them’. In line with these ideas, we find that universalists more strongly support (i) governmental programs to reduce economic inequality; (ii) a higher focus on helping the global rather than the local poor; (iii) focusing on protecting the

global rather than the local environment; (iv) higher immigration and (v) a weaker military. While these correlations are almost always quantitatively meaningful and statistically significant in our global sample as a whole, we identify large heterogeneity across cultures. In low- and middle-income countries, universalism explains very little of the variation in political views. The correlations between universalism and political views are also twice as large in rich Western societies than in rich countries outside the West, such as South Korea, Israel or Japan. Our analyses suggest that these patterns are unlikely to be driven by differential measurement error across countries. Rather, we interpret them as genuine cultural specificity that highlights the value of moving beyond Western countries in collecting controlled data on universalism.

The radius of trust. A broad social science literature argues that people’s degree of universalism is relevant for determining whether a society’s social capital is predominantly “local” and personal or more “global” and impersonal in nature (e.g., Putnam et al., 1992; Putnam, 2000; Tabellini, 2008b). Here, social capital is often understood as the radius of trust (the difference between trust in out-group and in-group members), which is believed to be relevant for determining the structure of economic and social cooperation in society. We expect the radius of trust to partly reflect actual trustworthiness towards in-groups and strangers, which, in turn, is plausibly driven by universalist preferences. In line with this idea, we identify a quantitatively large link between country-level universalism and the radius of trust, as measured in the World Values Survey. We view this result as highlighting the relevance of universalism for a society’s social capital.

Potential determinants: Economic incentives and democracy. A prominent hypothesis in the evolutionary social science literature is that people’s degree of universalism is economically functional and partly evolved to support and incentivize cooperation in economic production. According to this idea, cross-cultural heterogeneity in universalism partly reflects that economic systems differ in whether they benefit from a universalist or a relationship-specific social orientation (e.g. Tabellini, 2008b). This broad idea has been put forward in at least two ways. First, historically tight extended kinship systems – and the associated kin-based economic production networks – are said to have fostered a prosociality in which relationship-specific preferences play a prominent role (e.g., Greif and Tabellini, 2017; Enke, 2019; Schulz et al., 2019; Henrich, 2020; Schulz, 2022). Second, historical reliance on irrigation practices is hypothesized to have produced an in-group-focused orientation because large-scale irrigation systems require intensive neighborhood-based cooperation (e.g., Talhelm et al., 2014; Buggle, 2020). However, previous investigations of these hypotheses had to rely on relatively indirect data on universalism. We contribute to this discussion by documenting that – in

line with the aforementioned theories – country-level universalism is strongly negatively correlated with historical and contemporary data on the tightness of kinship networks as well as the intensity of historical irrigation practices. While correlational in nature, these results are consistent with the view that historical economic incentives shaped the distribution of universalism across the globe today.

Prominent theories about the origins of heterogeneity in universalism focus not only on historical (ancestral) economic incentives but also on people’s lifetime experiences. Psychological work has theorized that experience with democracy may induce greater universalism (Henrich, 2020). Similarly, philosophers such as Rawls (1993) have argued that the presence of a democratic system should give rise to universalism. Yet rigorous evidence on this idea is scarce. To make progress, we first document a significant link between universalism and democracy at the country level. Motivated by this correlation, we investigate a potential causal effect of democracy by leveraging two empirical strategies from the political economy and cultural economics literatures. First, we link country-cohort-specific variation in democracy over an individual’s lifetime to universalism. These differences-in-differences analyses always hold the respondent’s country and age fixed, and leverage that different age groups were exposed to democracy for different amounts of time across countries. Second, we conduct cross-migrant analyses that hold the respondent’s current country of residence fixed and leverage variation in democracy in the respondent’s home country (first-generation migrant analysis). In both sets of within-country analyses, experience with democracy is significantly predictive of universalism.

Contribution and related literature. Our paper builds on a recent empirical literature – referenced above – that has documented how heterogeneity in universalism predicts economic or political behaviors and outcomes. We view our paper as making four original contributions relative to this body of work. First, we test long-standing hypotheses about whether universalism is linked to the radius of social capital (yes) and whether it is unusually high in the rich West (no). Second, we provide evidence for strong links between universalism and various left-wing policy views in a large number of countries; we interpret these results as suggesting that heterogeneity in universalism may be a key driver of the polarizing public discussions that are sweeping through much of the developed world, such as those about immigration, climate change and supporting under-privileged people abroad. That said, our third contribution is to document that many individual-level correlates of universalism that are often hypothesized to be universals – those with left-wing policy views, education and urbanicity – are actually highly culturally variable, with known correlations severely attenuating or even reversing outside of the rich West. Fourth, we provide the first rigorous within-country evidence that

experience with democracy may cause universalism, and contribute further evidence on the role of economic incentives as proxied by historical kinship ties.

All of these contributions are facilitated by the scale and cross-cultural nature of our dataset, which allows us to address questions that were largely out of reach for the research community.¹ Appendix A provides an overview of prior cross-cultural work on universalism and parochialism. So far, this work has involved small specialized samples (e.g., Henrich et al., 2010a), decisions in strategic games made by participants in online convenience pools (Romano et al., 2021), or more indirect measures of universalism (Tabellini, 2008a; Enke, 2019; Schulz et al., 2019).

We constructed the *GUS* with a focus on making available to the research community a rich dataset that can potentially be used for a broad set of analyses in behavioral, cultural, political and development economics. Interested researchers with access to Gallup data can merge the *GUS* with the core module of the World Poll, which includes detailed information on demographics, economic and social views, emotions and behaviors. In the data section, we discuss how the *GUS* data facilitates within-country analyses across ethnolinguistic groups, subnational regions, and migrants.

The paper proceeds as follows. Sections 2–3 provide an overview of the *GUS* data and exposit the variation across relationships, individuals and countries. Section 4 studies links with demographics and political views. Sections 5–6 report the results on social capital and origins. Section 7 concludes.

2 Data: The *Global Universalism Survey*

2.1 Sampling and Procedures

We sketch the survey procedures here; Appendix B contains a detailed exposition. As part of the Gallup World Poll 2020, we administered survey items to representative population samples in 60 countries, for a total effective sample size of 63,788 respondents. The sample includes countries from all regions of the world, which allows us to avoid the overrepresentation of Western populations that is endemic to most multinational studies. Our sample includes 10 countries from Western Europe, 8 from Eastern Europe and Central Asia, 7 from the Middle East and North Africa, 11 from Sub-Saharan Africa, 11 from the Americas, 4 from South Asia and 9 from Southeast Asia and the Pacific. For some analyses, we partition the countries into 13 “Western” high income countries (labeled WEIRD by cultural psychologists), 8 non-Western high income countries such

¹Methodologically, we are related to prior work that uses the Gallup World Poll to study the global distribution of economic preferences and beliefs in other domains (Falk et al., 2018; Becker et al., 2020; Sunde et al., 2021; Almås et al., 2022; Bursztyn et al., 2023).

as Israel, Japan and South Korea, and 39 low / middle income countries. Appendix B.3 clarifies the assignment of countries to these three groups. In total, our data represent countries that account for 85% of the world population and 90% of global GDP.

Sampling took place through 530 Gallup sampling units; throughout most of the paper, we compute standard errors and confidence intervals based on clustering at these units, see Appendix B.3.3.

The surveys were conducted by local professional enumerators via telephone between September 2020 and February 2021 (face-to-face interviews were only used in India and Pakistan). Sampling was conducted using random dialing techniques. In addition to the randomness introduced by this technique, Gallup supplies sampling weights that render the sample ex-post representative along the dimensions of age, gender and, where reliable data are available, education or socioeconomic status.

The survey questions were supplied to Gallup in English and then translated by professionals into 70 languages (108 country-language combinations) using standard back-and-forth translation techniques.

2.2 Survey Questions

Our survey questions closely follow the hypothetical disinterested dictator games that were deployed in Enke et al. (2022, 2023b). In these decisions, respondents allocate hypothetical money between a specific in-group member and a random stranger. The decisions are disinterested in the sense that respondents' own payoff is not at stake. The enumerator first introduced the following scenario:

“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.”

Then, the enumerator asked two randomly selected questions (out of five) that only differed in the identity of the in-group member. These five questions measure universalism in the domestic domain:

“How much of your \$1,000 would you give to [IN-GROUP MEMBER], if the rest goes to a random stranger from [COUNTRY NAME]?”

Across the five potential questions, the identities of the in-group members were: “a person in your family,” “a friend of yours,” “a person who lives in your neighborhood,” “a person who shares your religious beliefs” and “a person who shares your ethnic background.” Subsequently, each respondent answered a question that measures foreign universalism:

“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?”

All monetary values used in the study were expressed in local currency, scaled by PPP-adjusted GDP relative to the United States.

In a between-subject design, we randomly assigned respondents to the survey flow explained above or to a variant of these questions that explicitly cues moral reasoning by asking respondents to choose what they consider morally right:

If you were to do what you think is morally right, then how much of your \$1,000 would you give to [IN-GROUP MEMBER], if the rest goes to a random stranger from [COUNTRY NAME]?

In what follows, we refer to this survey question as the *Moral* framing and the first one as *Baseline*. We implemented two different versions of this *Moral* framing, randomized across respondents. One version used the wording above. A second version additionally instructed respondents to “Assume that these two people are equally good people.” As discussed further below, this version is intended to fix respondents’ beliefs about the deservingness of the recipients. The within-survey randomization was designed such that 50% of respondents were randomized into the *Baseline* treatment and 50% into the *Moral* treatment.

Pre-testing of survey questions. Our money allocation tasks are hypothetical in nature. This is in line with a growing line of work that documents that unincentivized measures of preferences are highly predictive of economic behaviors. An attractive approach in this literature – which we also follow here – is to formulate survey questions in close analogy to an incentivized choice context, just without implementing the choice (e.g., Falk et al., 2023, 2018; Stango and Zinman, 2023). This has the advantage that decisions are objectively defined and quantitative in nature. In a large meta-study, Baliet et al. (2014) report that the magnitude of in-group favoritism (in cooperation) is unaffected by whether the stakes in the experiment are hypothetical or real.

The money allocation tasks described above have been tested in three different ways. First, Enke et al. (2022) experimentally validate the survey questions in the U.S. by showing that responses to the hypothetical money allocation games are strongly correlated with analogous incentivized choices.² Second, as a lab-to-field validation, Enke et al.

²The experimental validation involved an amount to be split of \$100, while in our survey respondents split hypothetical \$1,000.

(2022) document that behavior in our hypothetical money allocation games is strongly correlated with donation behavior: universalists donate less to local community organizations but more to national or international organizations. Both of these validation steps were implemented only in the U.S. and, hence, naturally provide only limited evidence for the global sample as a whole. Third, as part of this project, Gallup and our research team pretested our survey items before they went into the field. In particular, we implemented so-called “cognitive interviews,” in which a small set of respondents and enumerators in Brazil, Spain, Tanzania, and Turkey provided detailed feedback on their understanding and interpretation of the survey items. Our general informal take-away from our discussions with the experienced team of Gallup and these cognitive interviews was that respondents showed an encouraging level of engagement with the questions.

2.3 Interpretation

Our survey builds on large literatures on other-regarding preferences in economics, in particular the ones on in-group favoritism, parochial altruism and social identity (e.g., Goette et al., 2006; Lane, 2016; Charness and Chen, 2020; Shayo, 2020). As reviewed in Enke (forthcoming), the traditional focus of this literature has been to document the *existence and magnitude* of (average) in-group favoritism and its economic ramifications. Building on this work, a more recent literature departs from this earlier focus by emphasizing strong heterogeneity in “universalism types.”

Consider a decision-maker i whose overall utility depends on both his own consumption, x_i , and that of others:

$$U_i(x_i, x_{-i}) = u(x_i) + \sum_{j \neq i} \alpha_{i,j} u(x_j), \quad (1)$$

where $u(\cdot)$ is a concave felicity function and $\alpha_{i,j}$ is an altruism weight that depends on the identity (or group membership) of the other individual. The decision-maker is tasked with distributing a fixed pot of \$1 between two other individuals, an in-group member (G) and a stranger (S). The interior optimality condition for the allocation decision is $\frac{u'(x_G^*)}{u'(x_S^*)} = \frac{\alpha_{i,S}}{\alpha_{i,G}}$. Under the stronger assumption of log (Cobb-Douglas) utility, $u(x_i) = \ln(x_i)$, the optimal allocation to the in-group member is given by

$$x_G^* = \frac{\alpha_{i,G}}{\alpha_{i,G} + \alpha_{i,S}}. \quad (2)$$

The optimal allocation directly identifies the relative altruism weight given to the in-group member versus the stranger, and we thus use it as an empirical measure of uni-

versalism. Note that a person’s level of altruism (the average $\alpha_{i,j}$) does not affect this universalism measure. We refer to a person as “universalist” if they assign the same weight to all in-groups and the stranger, which would imply that they always split the money equally. In contrast, if people have relationship-specific preferences, they assign a greater weight to certain groups and thus allocate more of the money to these groups. If they allocate all of the money to the in-group, we refer to them as showing “full in-group favoritism.”

A person’s revealed universalism in our survey may have multiple origins. For instance, non-universalistic decisions may reflect that people perceive that they have relationship-specific moral obligations towards certain in-groups. Rawls (1993) prominently argued that cooperation under a fair basic structure in society creates moral obligations towards compatriots but not towards foreigners, but people may also feel that they have particular moral obligations towards their family, friends, neighbours, and people who share their religion or ethnicity. A second (moral) reason why people may differ in their degree of universalism is that they may have different beliefs about whether in-group members or strangers are “deserving.” For instance, if decision-maker A is more likely to believe that their in-group consists of good people than decision-maker B is, then A may appear more universalist in our survey instruments even if A and B have the same underlying preferences.

To provide evidence on the extent to which moral considerations shape non-universalistic preferences, we implement the *Moral* treatment variation discussed above, in which the respondent is asked to do what is morally right. Furthermore, to study the role of beliefs about deservingness, as noted above, we implement a treatment variation in which we ask respondents to assume that both recipients are equally good people. Given that this treatment has almost no effects on behavior, our preferred interpretation is that revealed universalism in our survey reflects preferences (or beliefs about moral obligations) rather than beliefs about who is more deserving.

Finally, as is the case for essentially any experimental or survey-based elicitation of social behaviors, there is a latent concern about social desirability bias. For example, a potential concern is that some of the universalism that we measure in our survey does not reflect genuine preferences but, rather, virtue signaling (Raux, 2023). Because we are mostly interested in assessing cross-sectional heterogeneity, such virtue signaling, if present, is unproblematic for our purposes as long as it affects all respondents (or at least all countries) equally. It is more problematic if the strength of signaling concerns varies across countries. There is no way for us to definitively rule this out. This said, two considerations appear reassuring in this regard. First, almost all interviews for the Gallup World Poll 2020 were conducted via telephone, which means that signaling concerns are probably less pronounced than in face-to-face interviews. Second, below in Section 5

we document that our cross-country measure of universalism is strongly correlated with an independent measure of the radius of trust in society. This correlation is plausible from the perspective of more universalist societies having a wider radius of trust, but it is difficult for us to imagine how a social signaling confound would produce such a correlation.

2.4 Summary Measures

We compute three pre-registered summary measures of universalism:³ *Composite Universalism*, *Domestic Universalism* and *Foreign Universalism*. Each of these measures is in the range of [0, 100], where 0 means that all money is given to the in-group and 100 that everything is given to the more distant individual in the respective decisions. *Domestic Universalism* corresponds to the average fraction of money shared with the domestic stranger in tradeoffs with in-group members. *Foreign Universalism* corresponds to the fraction of money shared with a global stranger in a tradeoff with a domestic stranger. *Composite Universalism* is the unweighted average of domestic and foreign universalism.⁴

The individual-level correlation between domestic and foreign universalism is $r = 0.32$. The fact that this correlation is very similar (on average) in high and low / middle income countries provides an indication that the quality of the data is comparable across rich and poor countries (if, for example, respondents in poorer countries answered more randomly, the correlation would be more attenuated relative to that in rich countries).

2.5 Recoding of Erroneous Responses

Any multinational survey of this scale is subject to some amount of respondent confusion or misrecordings by enumerators. Appendices B.6 and B.7 detail all data issues that we discovered and corresponding remedies taken.

In our data, 20,338 out of 184,950 allocation decisions (11%) give strictly less to the in-group member. As a result, many of these respondents exhibit universalism of above 50. The occurrence of this pattern is very similar across high and low / middle income countries. There is strong evidence that many of these data points reflect respondent

³We compute country averages using the sample weights provided by Gallup. We construct all country-level measures netting out treatment fixed effects to account for slight imbalances in treatment assignment across countries.

⁴Gallup surveyed a total of 66,233 respondents. However, as discussed in Appendix B, for 11% of respondents at least one allocation decision is missing, usually because the respondent indicated “Don’t know” or refused to answer. For 2,445 respondents, all money allocation decisions are missing, resulting in a final sample size of 63,788. In this sample, 7.5% of respondents have at least one allocation question missing. In those cases, we compute the summary statistics based on the questions answered. When either only domestic or only foreign universalism is available, we use that measure also for composite universalism.

confusion or systematic misrecordings by the enumerator. In particular, the evidence is strongly suggestive that many allocations to the in-group of $x < 50\%$ reflect a “flipped” version of the respondent’s true preference, $100\% - x$.

The Gallup World Poll contains a question that asks whether the respondent helped a stranger in the past month. The editor and reviewers encouraged us to use this question as a “validation variable” for our universalism measure. As shown in Appendix B.7, helping a stranger and allocating money to the stranger in our monetary games are strongly positively correlated in the (much larger) part of the sample in which respondents allocate at most 50% of the budget to the stranger. This is what one would expect. However, in the smaller part of the sample where respondents allocate more money to the stranger than to the in-group, money allocations to the stranger are *strongly negatively* correlated with helping a stranger. This paradoxical pattern suggests that 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 balance the obvious tradeoff between potential concerns over data mining and the need for us to propose the most productive path for the broader research community in using this rich dataset going forward, we implement two strategies. Our main strategy is to recode (“flip”) allocations to the in-group of $x < 50\%$ as $100\% - x$ if and only if the respondent allocates (i) weakly more than 50% to the more socially distant stranger in *all* questions and (ii) strictly more than 50% to the socially more distant stranger in at least half of all decisions (which in practice usually means at least two out of three).⁵ The modal respondent that gets recoded allocates 100% of the endowment to the socially more distant recipient in all decisions. This procedure affects 4,328 respondents (6.8%) and 10,318 allocation decisions (5.6%). A potential concern with this recoding procedure is that it is asymmetric because we only recode decisions that give more to the stranger but not decisions that involve giving more to the in-group member. We, hence, implement an additional (“dropping”) procedure, described in Appendix B.7.3, that is symmetric around 50 and delivers almost identical results.⁶

For transparency, we make available both the raw data and the recoded data. Appendix B.8 replicates all analyses in this paper using the raw data, with very similar

⁵In our view, in future studies, recoding procedures of the type implemented here should only be followed if there is unambiguous evidence that the recoding captures the intended response of participants, as is the case with our validation variable.

⁶In this alternative procedure, (i) we drop all of those respondents that satisfy the criteria above, all of whom exhibit universalism $u > 50\%$; and (ii) for each respondent that we drop, we create a set of “mirror respondents” with universalism $100\% - u$ (from the same country), and randomly select one of them to be dropped. Given the random element implicit in this procedure, we bootstrap it, see Appendix B.7.3 for details. We also make the code for this bootstrapping procedure available. For reasons outlined in the Appendix, we view the recoding procedure as superior and recommend that future users of the GUS data work with this version of the data.

results. The main exception is the democracy exposure analysis in Section 6.2, where large outliers render the OLS estimates insignificant with the uncorrected coding.

2.6 Validation of Universalism Measure

As noted above, the Gallup World Poll contains a question that asks whether the respondent helped a stranger in the past month. Because this question specifically asks about a prosocial act toward a stranger, we view it as an effective way to validate our universalism measure. Of course, we wouldn't expect such a correlation to be perfect, for various reasons: (i) prosocial behavior towards a stranger is not just determined by the *relative* altruism weight (our object of interest) but also by the *absolute level* of altruism; and (ii) there may be various institutional or economic reasons for why people have more opportunities to help strangers in some environments than in others.

Figure 1 reports the results. The left panel shows a binned scatter plot that visualizes the individual-level correlation between universalism and helping a stranger. Here, the bins are constructed to contain the same number of respondents. The plot represents a partial correlation plot that controls for treatment and country fixed effects, such that the plot shows the *within-country* (and within-treatment) link between universalism and helping a stranger. Thus the x-axis captures an individual's universalism, relative to their country average (and analogously for the y-axis).

The right panel shows the *across-country* analog of this result. Here, the x-axis is average universalism in a country and the y-axis shows the fraction of respondents in a country who report having helped a stranger. Note that the across- and within-country correlations are not mechanically related. For instance, in principle, it is possible for a variable to predict helping within a given country, while the country averages are uncorrelated.

Instead, we see that both at the individual level and at the country level, universalism and helping a stranger are significantly correlated. At the individual level, an increase in universalism from 0 to 50 is associated with an increase in the probability of helping a stranger of six percentage points. This quantitative magnitude is almost identical in countries with above- or below-median GDP per capita in our sample, which we interpret as evidence that the quality of the measure is equally high across countries with different levels of development.

At the country level, the correlation is $r = 0.36$ ($p < 0.01$). Overall, we interpret these correlations as encouraging evidence for the validity of our measure.

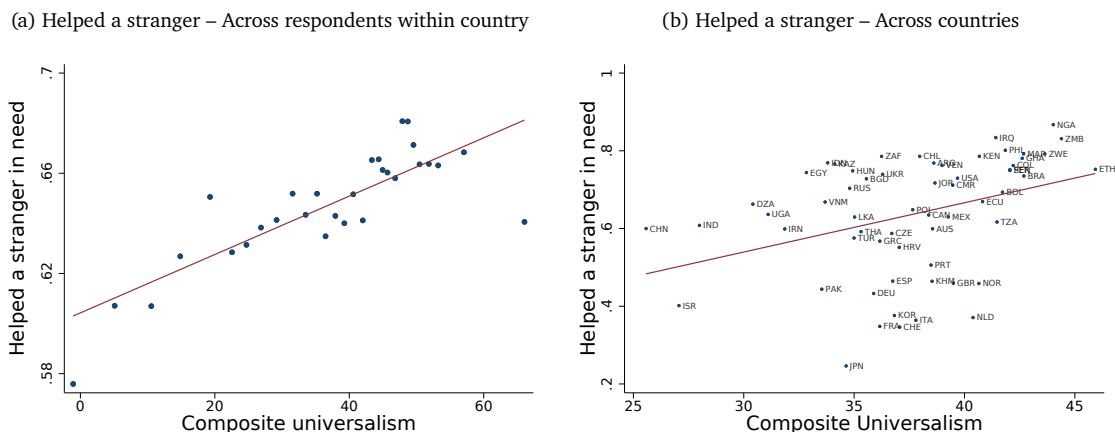


Figure 1: Composite universalism and helping a stranger. The left panel shows a respondent-level binned scatter plot that, for a given level of universalism, computes the average probability of having helped a stranger. The bins are endogenously constructed such that each dot represents the same number 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.

2.7 Additional Variables and Data Linkages

Questions on political views. Our survey module also included six questions about political views, out of which each respondent answered two (randomly selected):

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.

1. *The national government should aim to reduce the economic differences between the rich and the poor in [COUNTRY].*
2. *The national government should focus on helping the poor in [COUNTRY], rather than the poor elsewhere in the world.*
3. *The national government should focus on protecting the environment in [COUNTRY], rather than protecting the global environment.*
4. *There are too many immigrants in the area you live in.*
5. *There are too many immigrants in [COUNTRY].*
6. *The national government should focus on having a strong military.*

Linkages to core module of World Poll and other datasets. The GUS dataset will be made publicly available upon publication of this paper. Because the data contain individual identifiers, interested researchers with a Gallup license can merge our data with the core World Poll data, which contain rich information about respondents' demographics, backgrounds, and economic and social views.

Three background variables deserve being mentioned due to their popularity in the literature and the possibility of using them to create linkages between the *GUS* data and other commonly-used datasets at different levels of aggregation. (i) Respondents' country of birth. Following the "epidemiological approach" in cultural economics, this enables cross-migrant analyses that leverage variation in characteristics of the respondent's home country while holding the current country of residence fixed (Giuliano, 2007). (ii) The interview language is recorded and can plausibly be used as a proxy for ethnolinguistic background and cultural ancestry. With the *GUS* data, we make available a matching of the vast majority of the country-language pairs in the World Poll to the corresponding country-language pair in the *Ethnologue*. (iii) Respondents' subnational region of residence, usually at the state or province level (1,341 distinct subnational regions). We make available a matching of the regions in the World Poll with equivalent level 1 regions in the *Database of Global Administrative Areas*.

2.8 Pre-Analysis Plan

We pre-registered almost all of the analyses in this paper in the AEA RCT registry at <https://www.socialscienceregistry.org/trials/7525>. The pre-registration included: (i) how we aggregate individual allocation decisions into a universalism summary statistic; (ii) a plan for how to analyze treatment effects; (iii) predictions about the link between universalism and demographics; (iv) predictions about correlations between universalism and political views; and (v) predicted cross-country correlations. The main analyses that were not pre-registered are the exposure to democracy analysis in Section 6.2 and the analysis of the radius of trust. The pre-registration was uploaded after Gallup collected the data but before we had access to it.

3 The Global Variation in Universalism

Average allocations. Figure 2 shows average allocations to the in-group (in terms of percentage of the total budget) in each of our six distributive decisions, separately by treatment condition.⁷ The first five groups of bars summarize allocations in the trade-off between in-group members and a domestic stranger. The rightmost bars summarize allocations in the tradeoff between a domestic stranger and a global stranger. That the domestic stranger appears as "out-group" in the first five bars but as "in-group" in the sixth bar reflects our earlier discussion that what matters for us is purely the *relative*

⁷Appendix Figure C.1 shows histograms for each of the allocation decisions. Across all questions, there are large spikes at allocations of 50:50 (full universalism) and 100:0 (full in-group favoritism). In total, 50% of all decisions reflect equal splits and 15% full favoritism.

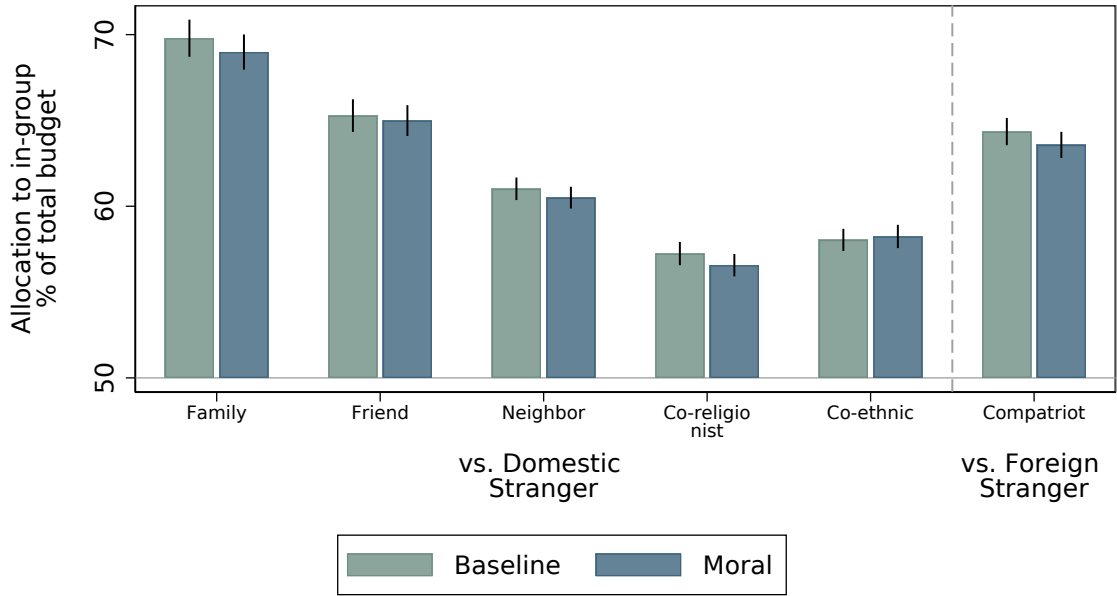


Figure 2: Mean money allocations to the in-group by treatment. Each bar indicates how much of the budget was given to the in-group. Whiskers show 95% confidence intervals, computed based on clustering at the sampling unit level (530 clusters).

distance of the recipients to the decision maker.

We make three main observations. First, our global data robustly show that people are relationship-specific in their altruism and deviate from fully universalistic behavior: people on average consistently allocate more to their in-groups across distributive decisions. Second, the extent to which people deviate from universalistic behavior depends on which in-group is involved in the decision, with people being less universalist when family, friends and neighbors are involved, compared to co-ethnics or co-religionists. For example, respondents on average allocate 22% more of their budget to the family compared to a co-religionist. These patterns are intuitive in that the first three groups usually capture personal relationships, while the latter two groups are best thought of as social identities without strong personal connections to most other in-group members. At the same time, we see that respondents do exhibit relatively large in-group favoritism when making a decision involving a compatriot and a global stranger, even though compatriots are also an impersonal in-group.⁸

Third, we find very similar results regardless of whether the survey question is framed as asking about the respondent's moral views or their hypothetical distributive deci-

⁸Given our global sample, an immediate question is whether countries differ in their implied ranking of different types of in-groups. For instance, it is conceivable that some populations predominantly value neighbors, while others value shared ethnicity. Appendix Figure C.2 instead shows that countries are very similar in which types of in-groups they value more. For example, 55 out of 60 countries exhibit the highest degree of favoritism towards family, and 42 countries exhibit their second-highest degree of favoritism towards friends.

sion. Overall, average allocations to the in-group are 0.6% percentage points higher in *Baseline* than in *Moral*, from a baseline of 63.4%. This difference is small but statistically significant, see Appendix Table D.3. This suggests that deviations from universalism are primarily but not exclusively driven by moral considerations (such as perceived relationship-specific moral obligations): some people believe it is right for them to extend special treatment to in-group members, while others believe it is morally right to treat everyone equally.

Finally (not reported in Figure 2), we do not find a statistically significant difference between the two different versions of the *Moral* treatment that do or do not include a sentence that asks respondents to imagine that both recipients are equally good people ($p = 0.13$). Our preferred interpretation of the absence of a treatment effect across these two versions of the *Moral* framing is that beliefs about deservingness play a relatively small role for distributive behavior.

In the following analysis, we pool the data across treatments, but all results are robust to considering each treatment separately.⁹

Variation across individuals. Figure 3 shows that there is large variation in the composite universalism measure across respondents. About 27% of respondents make universalistic decisions by splitting equally between in-group and stranger, while 6% always give everything to the in-group. 60% of respondents allocate strictly more but not everything to the in-group; the remaining 7% of respondents give slightly more to the stranger.¹⁰

We view this composite measure of the average decision as a meaningful summary statistic of a respondent’s overall universalism “type” because, in our data, all correlations between the different allocation decisions are positive and range between $r = 0.21$ and $r = 0.52$ (Appendix Tables D.1–D.2). This suggests that some individuals are consistently more universalist than others and that analyzing individual-level summary measures of universalism is meaningful.

Variation across countries. Heterogeneity at the country level is also substantial. Figure 4 shows a global map of composite universalism (see also Appendix Figures C.5–C.7 for more disaggregated statistics of domestic and foreign universalism as well as their difference). Figure 5 lists all countries and shows their levels of domestic, foreign and composite universalism. We see that average composite universalism varies between

⁹Appendix E uses the treatment comparison to decompose cross-group differences in universalism into moral views and distributional preferences.

¹⁰Appendix Figure C.4 shows the distributions for domestic and foreign universalism separately. The figure also reports the distribution of the difference between domestic and foreign universalism.

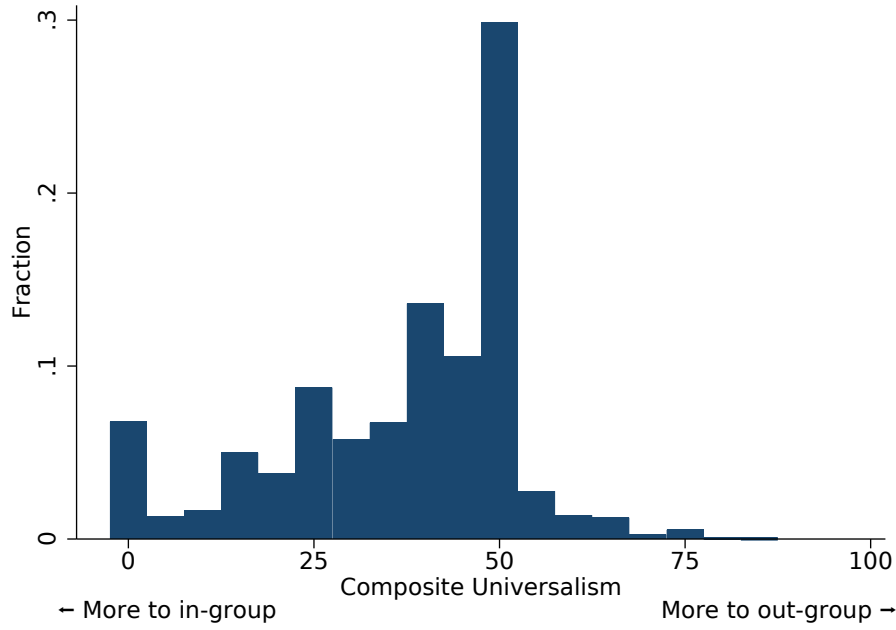


Figure 3: Distribution of composite universalism across individuals, pooled across treatments ($N = 63,788$). 0 means that all money is shared with the in-group, 50 captures equal splits (on average), and 100 that all money is shared with the socially more distant stranger.

roughly 25 and 45, with China, Israel and India exhibiting particularly low universalism, and Ethiopia being the most universalist country in our sample. On average, an Ethiopian respondent shares 20 percentage points more of the monetary endowment with the more socially distant person than a Chinese respondent. Overall, universalism is relatively high in Sub-Saharan Africa, Latin America and to some extent Western Europe and its offshoots. In contrast, universalism is lower in East Asia, South Asia, Eastern Europe and to some extent in the Middle East.

Figure 5 shows notable variation in domestic versus foreign universalism both across regions and across countries within regions. For example, populations in East Asia, North Africa and the Middle East are more universalist in situations involving tradeoffs between domestic in-groups, whereas Western Europe is particularly universalist in domestic-foreign tradeoffs. We see slightly more variation in foreign universalism (cross-country mean 37.5 and s.d. 5.3) than in domestic universalism (cross-country mean 38.1 and s.d. 4.5). Overall, the country-level correlation between domestic and foreign universalism is $r = 0.48$.

An immediate question is whether cross-national variation in universalism is linked to differences in development. As shown in Appendix Figure C.8, the raw correlation of composite universalism with log per capita income is slightly negative ($r = -0.24$, $p = 0.07$). This relationship is entirely driven by domestic ($r = -0.43$) rather than by foreign ($r = -0.01$) universalism, see Appendix Figure C.9. The negative cross-country

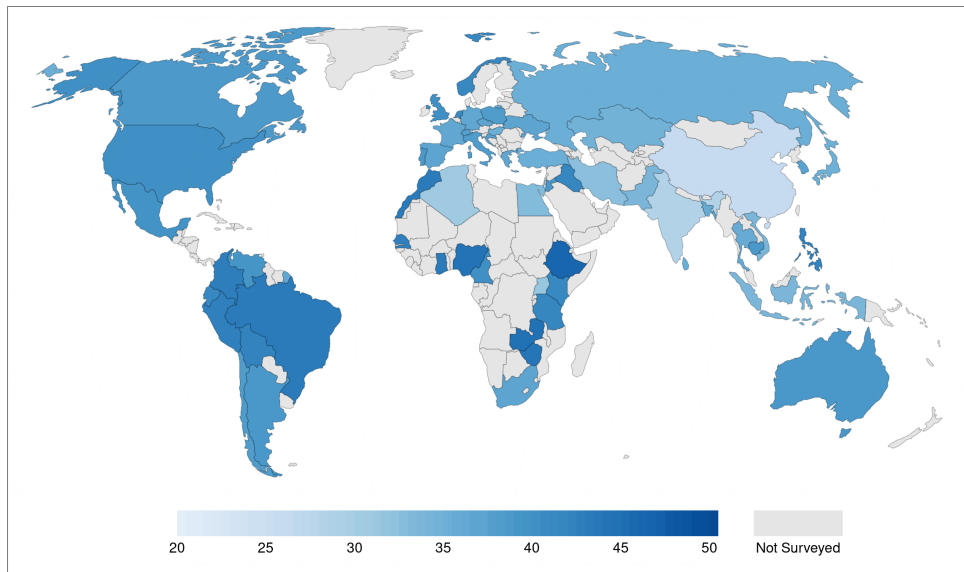


Figure 4: Global variation in composite universalism. The map shows the country level average of composite universalism, pooled across treatments. 0 means that all money is shared with the in-group, 50 captures equal splits (on average), and 100 that all money is shared with the socially more distant stranger.

correlation between universalism and income goes against a popular theory in cultural psychology that – based on more indirect measures – views rich nations as unusually universalist (Henrich et al., 2010b). However, this cross-country result is consistent with the negative individual-level correlation between universalism and income to be documented in Section 4 below.

Variance decomposition. Given the large heterogeneity at both the individual and the country level, a question is which source of variation is dominant in the dataset. The variance explained in a regression of composite universalism on country fixed effects is 8.4%. This suggests that while cross-country variation is quantitatively large (see Figure 5), individual-level heterogeneity is also pronounced. Of course, quantifying the true magnitude of individual-level heterogeneity is difficult because some of the variation in universalism across respondents may reflect measurement error rather than true preference heterogeneity.

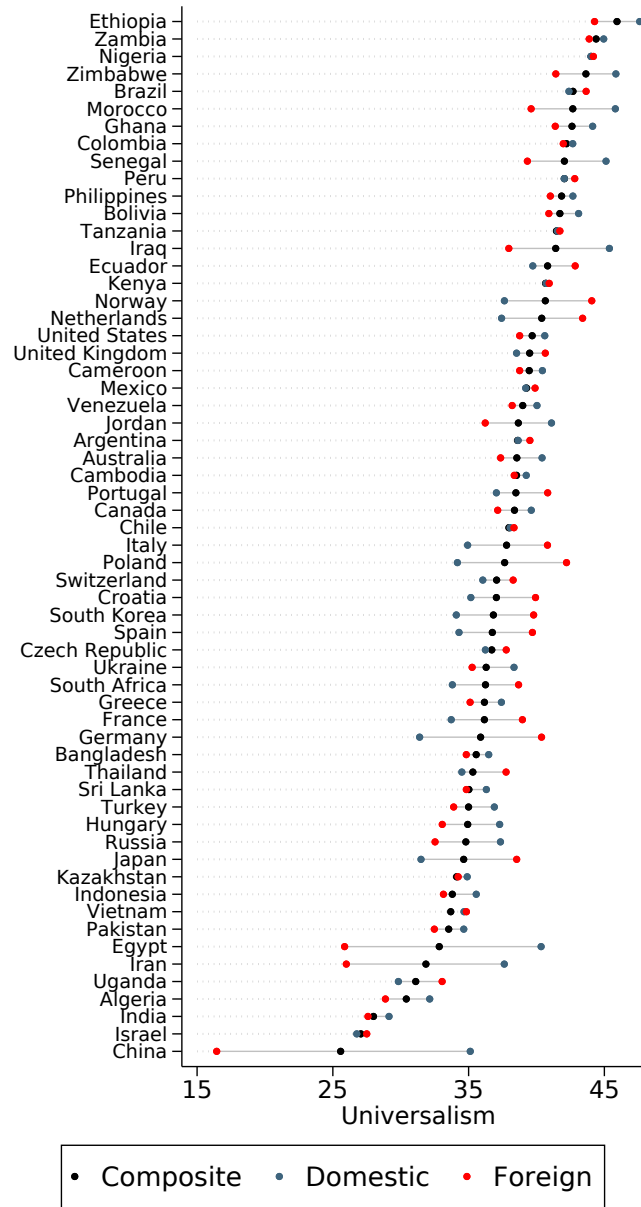


Figure 5: Average composite, domestic and foreign universalism by country. 0 means that all money is shared with the in-group, 50 equal splits, and 100 that all money is shared with the socially more distant stranger. Composite universalism occasionally doesn't equal the average of domestic and foreign universalism because of missing domestic or foreign universalism data (see footnote 4 and Appendix B.6).

4 Individual-Level Correlates and Political Views

4.1 Demographic Correlates

Economists and other social scientists are often interested in the demographic correlates of individual preferences. A main motivation for this line of research is to shed light on the behavioral motivations that underlie across-group differences in economic behaviors and outcomes. The link between demographics and universalism is less well-explored

than is the case for preferences like risk aversion, time preferences, or altruism. We pre-registered an analysis of six demographics to study these differences. The signs indicate the ex-ante hypothesized relationships with universalism: age (-), male (-), income (-), education (+), urban residence (+) and religiosity (-). Our predictions were made based on the available data from rich, Western populations (Enke et al., 2022, 2023b).

Figure 6 shows the results of OLS estimations, in which we separately regress composite universalism on each of the aforementioned variables, controlling for country and treatment fixed effects (Appendix Figures C.10 and C.11 show the patterns for domestic and foreign universalism separately). For ease of comparison, demographic variables are recoded to be binary. The results are essentially identical when we use the underlying continuous variables. To investigate a potential cultural specificity of demographic correlations, we show the results in the full sample and additionally for three sub-samples of countries.

In the first panel, we observe that respondents who are above median age in their country are less universalist and allocate 1.9 percentage points less of the monetary budget to the stranger. This magnitude is very similar across the different groups of countries. When we instead implement an OLS regression of composite universalism on the continuous age variable, the resulting coefficient suggests that moving from age 20 to age 80 is associated with a decrease in the amount shared with the stranger of 4.7 percentage points. To put this magnitude in perspective, the sample mean of composite universalism is 37%.

The second panel documents that men are less universalist than women, on average, by 2.1% of the budget. This gender difference is similar across rich WEIRD, rich non-WEIRD and poorer countries.

The third through fifth panel show the results for more endogenous demographics: whether the respondent falls into the top two out of five income buckets in Gallup's data, whether they have completed a college degree, and whether they reside in a city. Regarding income, we see that richer people tend to be less universalist in all groups of countries, though this relationship is considerably smaller in magnitude than is the case for age and gender differences.

In the full sample, college-educated respondents are *less* universalist, yet the patterns differ across the different groups of countries. As we hypothesized, the correlation is positive and statistically significant in rich, Western countries. In contrast, in low / middle income countries, college-educated respondents tend to be less universalist. Even in rich-but-not-WEIRD countries (such as South Korea, Japan or Israel), the college coefficient is statistically indistinguishable from zero.

Similar patterns hold for residing in a big city. While in the full sample there is

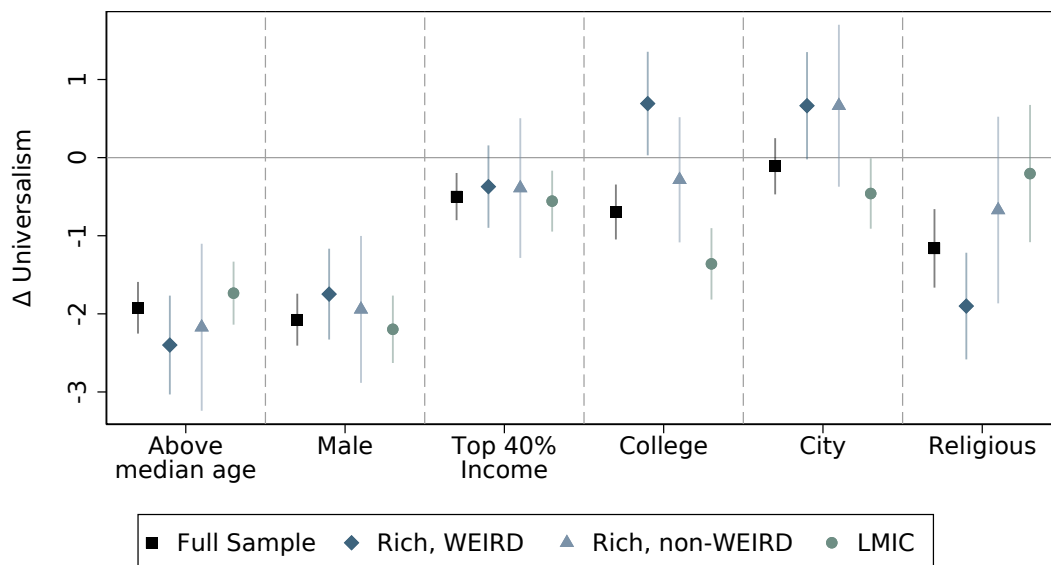


Figure 6: Universalism and demographics. OLS coefficients from regressions of composite universalism on each demographic, controlling for country and treatment fixed effects. Each coefficient reflects the results of a separate regression on a different sub-sample and can be interpreted as the percentage 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 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. The estimates are virtually identical without treatment fixed effects.

no discernible link, for the high income countries we see that living in a big city is significantly positively correlated with universalism. However, opposite results hold in poorer countries. In all, these results on education and living in a city suggest that either self-selection into cities and educated environments operates fundamentally differently in rich and poor countries (as far as universalism is concerned), or that potential causal effects of education or cities on universalism are culturally specific.

The sixth panel documents that religious people allocate 1.2% less of the budget to the socially more distant recipient, on average. This pattern is present in all groups of countries but more pronounced in the rich cultural West (WEIRD countries) than in other parts of the world.

In all, we view this set of results as illustrating the danger of generalizing from WEIRD data. For the more exogenous variables age and gender, the findings are in line with the predictions based on evidence from rich Western countries, with older people and males being less universalist. However, for the more endogenous demographics, the empirical evidence often goes against our pre-registered predictions. In line with a large body of work on the cultural specificity of psychological findings (Henrich et al., 2010b), this highlights that researcher expectations and intuitions need to be disciplined by representative data from various cultures. For example, based on correla-

tions between universalism and education, researchers commonly express the intuition that education causes universalism and therefore produces certain political views (e.g., Gethin et al., 2022). Yet if these correlations are entirely absent outside of the rich West, then either such causal claims are misguided, or more nuance is required in teasing out what makes Western education “special.”

4.2 Linking Universalism and Political Views

To study the link between universalism and economic and social policy views, we make use of the second part of our survey module, described in Section 2.7. We elicited people’s views on different types of redistribution, environmental protection, immigration and the military. In our pre-analysis plan, and building on prior literature (Enke et al., 2023b), we hypothesized that universalism would be predictive of policy views that are often considered “left-wing”: (i) support for reducing inequality; (ii) support for helping the global vs. domestic poor; (iii) support for protecting the global vs. domestic environment; (iv) support for immigrants in the respondent’s area and country; and (v) lower support for a strong military. The broad idea behind all of these hypotheses is that policies such as federal, impersonal redistribution, global redistribution, climate change prevention, and supporting immigrants are very universalistic in nature because they typically benefit strangers. For example, we hypothesize that universalists desire *more* domestic redistribution because they care about all members of society. Yet we also hypothesize that universalists would focus as much on helping poor people elsewhere in the world relative to poor people in their own country. Similarly, supporting immigrants, the global environment and a weak military arguably all reflect weaker “us vs. them” thinking and should therefore be positively linked to universalism.

Figure 7 summarizes the results by providing binned scatter plots of political views against composite universalism, where each dot captures the same number of underlying respondents. The plot shows average levels of the y-variable (e.g., views on redistribution) across all individuals in a given universalism bucket. We code all political views such that our pre-registration predicts a positive correlation with universalism. These figures control for country and treatment fixed effects. Thus the figures purely show the within-country link between universalism and political views.

We see that all relationships go in the predicted direction. Universalism is positively correlated with support for reducing economic inequality; focusing on helping the global vs. domestic poor; focusing on protecting the global vs. local environment; being open to immigrants in one’s area and country; and being opposed to a strong military. The patterns are visually clear and statistically significant for all dependent variables ($p < 0.01$), except for support for immigrants in one’s own area, where the correlation is

positive but not statistically significant ($p = 0.48$).¹¹

Many of the policy views that we consider largely concern either domestic people (such as reducing domestic inequality) or a combination of domestic and international people (such as a strong military). If our measures of domestic and foreign universalism pick up meaningful independent variation (their correlation is $\rho = 0.32$), then they should be differentially predictive of policy views across the different questions. To assess this, Table 1 reports multivariate regressions (partial correlations). Here, we link policy views to both domestic and foreign universalism, controlling for income and education as well as age, gender and urban residence.

The broad picture that emerges from this analysis is that the correlations of policy views with domestic and foreign universalism are usually significantly different from each other, and are always consistent with a domain-specific role. For example, consistent with the view that reducing inequality largely concerns questions related to domestic universalism, we find in column (1) of Table 1 that support for reducing economic inequality is significantly positively correlated with domestic universalism, but uncorrelated with foreign universalism. Similarly, as shown in column (4), support for immigrants in one's local area is only significantly positively associated with domestic universalism, perhaps because respondents interpreted this question as asking about within-country migrants. Conversely, the foreign universalism component turns out to be more strongly positively linked to those policy views that involve tradeoffs between compatriots and foreigners, such as for whether the global or domestic poor should be prioritized (column (2)), for whether environmental protection efforts should focus on the global or local environment (column (3)), and for views on the military (column (6)). Of course, given that foreign and domestic universalism are positively correlated, it is unsurprising to see that often both measures are statistically significant – but the relative magnitudes are always consistent with domain-specific universalism considerations.

Overall, the quantitative magnitude of the universalism coefficients suggests that an increase in universalism from zero to 50 is associated with an increase in support for the left-wing policies of between 0.06 and 0.17 points on a four-point scale. For comparison, consider explanatory variables that have attracted interest in traditional political economy analyses, such as income or education. The universalism coefficient is considerably larger (sometimes by a factor of 10) than the effect implied by moving a respondent from the lowest to the highest income quintile. Likewise, interpreted causally, the implied effect size of moving a respondent's universalism from zero to 50 is often as

¹¹The simultaneous (i) absence of a correlation with support for immigrants in one's area and (ii) presence of a correlation with support for immigrants in one's country is consistent with a role for "NIMBY-ism": that universalists support immigrants (even if it comes at the expense of domestic people), as long as it doesn't harm themselves. This highlights the need to differentiate between the group-specific nature and the overall level of altruism.

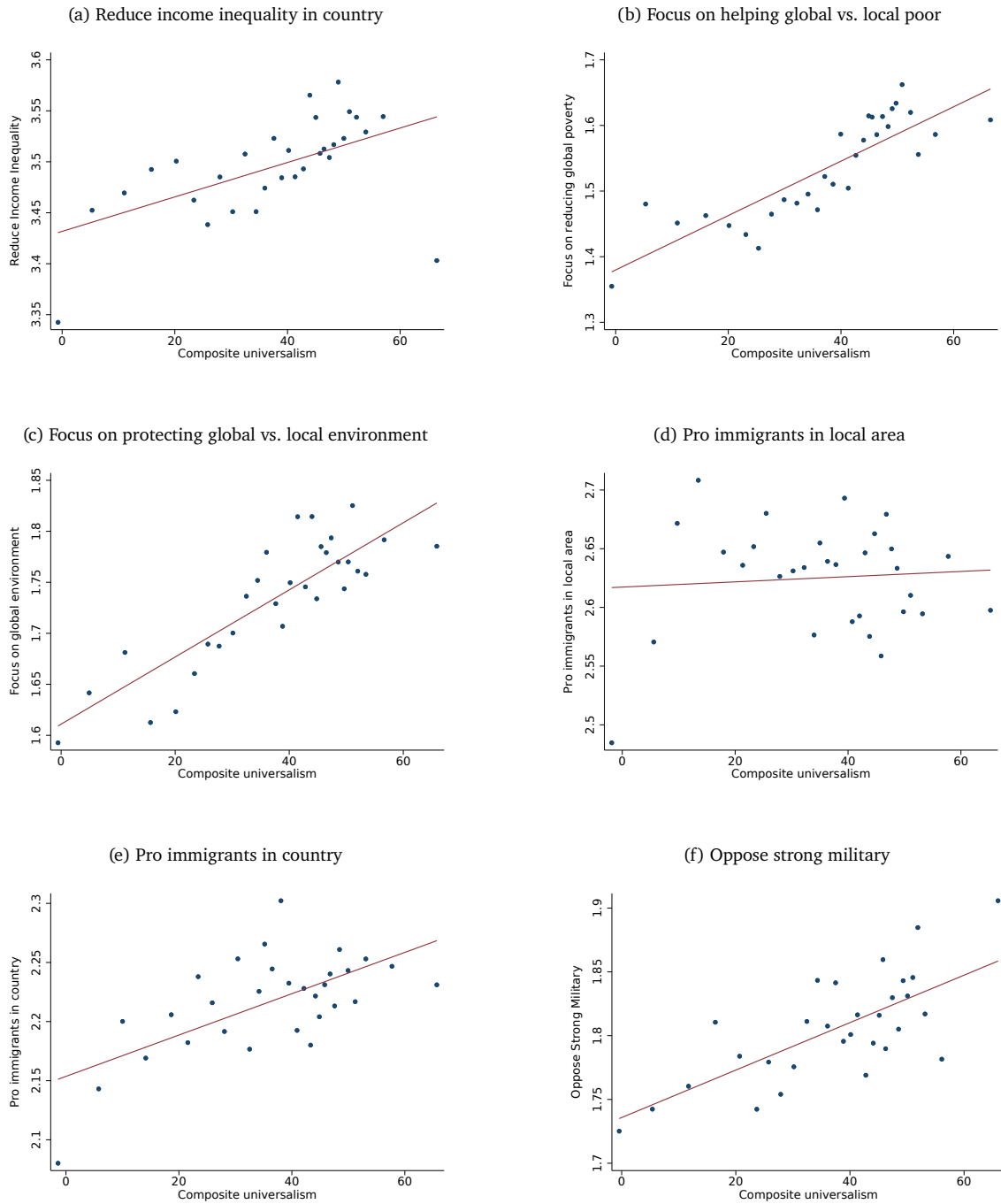


Figure 7: Composite universalism and political views at the individual level. The figures show binned scatter plots that average agreement with a policy view for a given level of universalism. The bins are constructed by Stata such that each dot represents the same number of respondents. The figures 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.” See Section 2.7 for the wording of the political questions. The sample size varies between $N = 19,628$ and $N = 22,219$ across panels.

large as the effect associated with a college degree.

Table 1: 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	0.18*** (0.04)	0.07* (0.04)	0.09** (0.04)	0.17*** (0.04)	0.09** (0.04)	-0.05 (0.04)
Foreign universalism / 100	-0.01 (0.03)	0.34*** (0.03)	0.23*** (0.03)	-0.06 (0.04)	0.13*** (0.05)	0.22*** (0.04)
Age	0.00* (0.00)	-0.00 (0.00)	-0.00*** (0.00)	0.00** (0.00)	-0.00 (0.00)	-0.00*** (0.00)
Male	-0.05*** (0.01)	-0.00 (0.01)	-0.02 (0.01)	-0.00 (0.01)	0.05*** (0.01)	-0.00 (0.02)
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)
City dweller	0.03** (0.01)	-0.00 (0.01)	0.02 (0.02)	-0.16*** (0.02)	-0.01 (0.02)	0.05*** (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
Adjusted R^2	0.066	0.155	0.163	0.149	0.205	0.257
Observations	18528	18676	18478	21248	20951	18430

Notes. Estimates from OLS with robust standard errors, clustered at the sampling unit level (530 clusters). Universalism is divided by 100 for expositional ease. Each observation is an individual. See Section 2.7 for the wording of the political questions. 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. Appendix Table D.4 presents estimates controlling for religiosity (not included in the main analysis because it wasn’t elicited in five countries). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Heterogeneity across countries. To investigate a potential cultural specificity of these patterns, we look at the relationship between universalism and political views across rich WEIRD, rich non-WEIRD and low / middle income countries. Figure 8 summarizes the results. There are two main takeaways. First, the relationships between universalism and policy views are largely driven by relatively rich countries. In the low and middle income countries, only two out of seven coefficients are statistically significantly different from zero in the predicted direction. Second, even within the set of high income countries, the regression coefficients tend to be roughly twice as large in the WEIRD compared to the non-WEIRD countries.¹²

These results highlight the cultural specificity of the link between universalism and

¹²Enke et al. (2023b) study the link between universalism and policy views in a smaller, seven-countries study. They also find that universalism is less predictive of policy views in the two non-WEIRD countries in their sample (Brazil and South Korea).

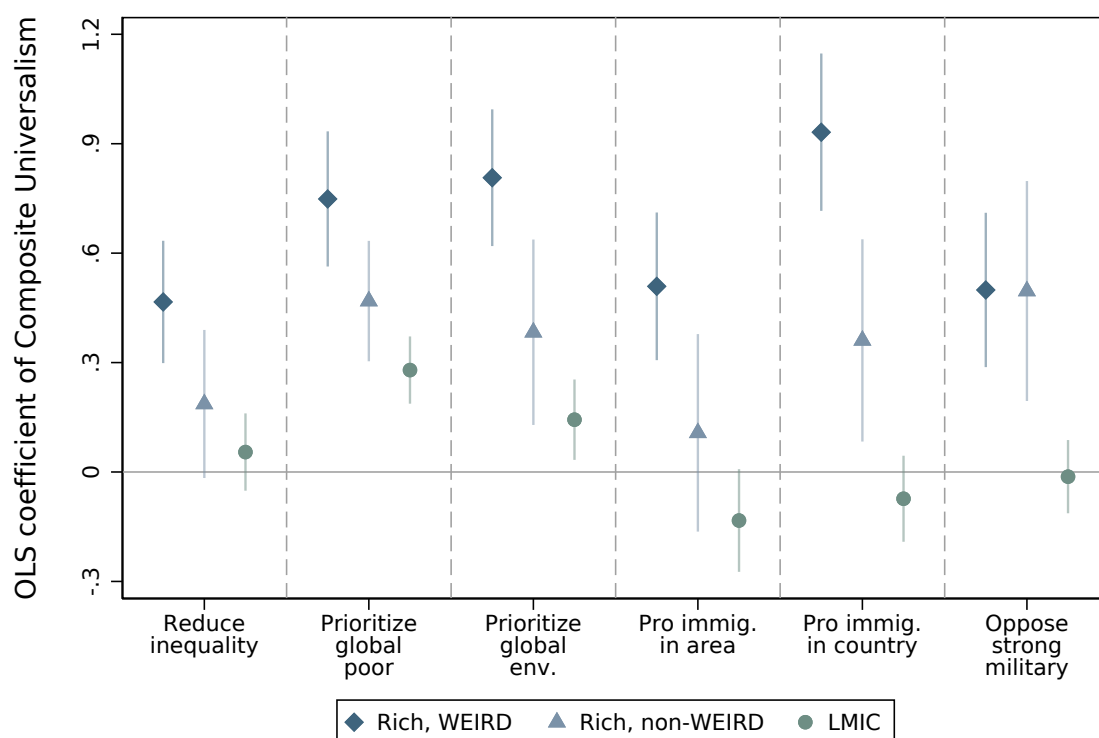


Figure 8: Individual-level composite universalism and political views in different sub-samples. OLS coefficients from regressions of political attitudes on composite universalism, controlling for country and treatment fixed effects. Each coefficient reflects the results of a separate regression on a different sub-sample and can be interpreted as the change in agreement with a policy view (on a scale 1–4) in response to moving universalism from 0 to 100. 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. The estimates used in creating this figure are displayed in Appendix Tables D.5– D.10

support for left-wing policies. One potential reason is that people outside the rich West form their policy views based on considerations other than universalism. Another possibility is that political elites in rich Western nations emphasize themes related to universalism vs. favoring in-groups to a greater degree than politicians outside the West. Our study was not designed to tease these potential mechanisms apart.¹³ Further research is needed to disentangle the role of political parties and voters in driving heterogeneity

¹³There are two potential mechanical (statistical) reasons for the difference in coefficient estimates across country groups, both of which we test and rule out. First, we do not find evidence that the stronger relationship in rich, Western countries is driven by larger individual-level variation in universalism – the correlation between a country’s per capita income and the magnitude of the regression coefficient in the policy views regressions is unaffected by whether or not we control for the within-country variance of universalism. Second, the results could be driven by higher measurement error in universalism in poorer countries and resulting attenuation bias. There are various pieces of evidence that speak against such an account. First, as discussed in Section 4, the correlations between universalism, age and gender (the most exogenous individual characteristics in our data) are very similar across countries. Second, as discussed in Section 2.6, the link between universalism and having helped a stranger is very similar across the different groups of countries. Third, as discussed in Section B.6, various other indicators of data quality are very similar across countries with different income levels.

in the importance of universalism across countries.

5 Universalism and the Radius of Trust

A broad social science literature argues that people's degree of universalism is essential for the structure of a society's social capital (e.g., Putnam et al., 1992; Putnam, 2000; Henrich, 2020). While early research on cultural variation in social capital and trust studied how much people trust "other people in general," more recent work has focused on understanding the more specific radius of trust in society: *which* social groups individuals trust or distrust (e.g., Delhey et al., 2011; Enke, 2019; Schulz et al., 2019; Le Rossignol and Lowes, 2022). Such an analysis seems crucial because social and economic relationships in society are plausibly different if – holding fixed a certain level of average trust – people trust everyone to the same degree versus exhibit high trust in in-group members but low trust in out-group members. This relates to the key distinction between social capital that is more "local" (or personal) and social capital that is more "global" (impersonal) in nature.

The radius of trust captures people's beliefs about who is trustworthy. Yet these beliefs probably at least in part reflect people's actual trustworthiness – whom they do or do not treat well. This, in turn, is partly shaped by whether people have universalist preferences. We, hence, hypothesize that more universalist societies have a broader radius of trust.

On the other hand, one could imagine various reasons that would attenuate or even eliminate a potential link between universalistic preferences and radius of trust beliefs. First, people's actual trustworthiness with respect to specific social groups is not just determined by their universalism but potentially also by other preferences, institutional factors, the social cost of not being trustworthy, or historical accidents. Second, there is now a large economics literature that emphasizes the importance of misperceptions in people's beliefs about others, such that people's beliefs about the trustworthiness of different groups in society need not be well-calibrated (Bursztyn and Yang, 2022).

The radius of trust is typically measured using a series of questions from the World Values Survey that elicit respondents' trust in six specific groups: family, neighbors, people one knows, people one meets for the first time, people of another religion and foreigners. Following Delhey et al. (2011), the literature has converged on a standard summary statistic to aggregate these questions into an index of in-group vs. out-group trust, which is computed as average trust in the first three groups minus average trust in the latter three groups. Note that this index does not capture how much people trust others, but how much more they trust in-groups.

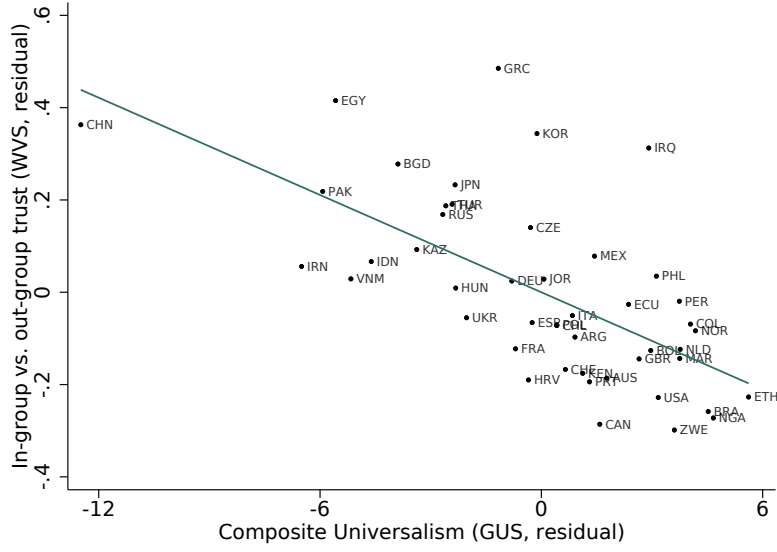


Figure 9: Country-level partial correlation plot of the difference between in-group and out-group trust against composite universalism, controlling for log per capita income. Both axes show residuals after residualizing from log per capita income. The difference between in-group and out-group trust is 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). Some of the countries in the *GUS* dataset are missing because the WVS radius of trust variable is not available for all countries.

Figure 9 shows a partial correlation plot between in-group minus out-group trust and our composite universalism index, controlling for log per capita income. As we hypothesized, the correlation is negative, such that societies with more universalistic preferences exhibit a broader radius of trust. The partial correlation conditional on log per capita income is $r = -0.64$ and the raw correlation is $r = -0.41$. The difference between partial and raw correlation reflects that per capita income is correlated with both universalism and the WVS trust variable.

6 Origins of Variation in Universalism

6.1 Economic Incentives: Cross-Country Correlations

Recent research has devoted considerable attention to understanding the origins of variation in people's in-group versus out-group behavior (see Enke, forthcoming, for a review). A prominent idea in the literature is that people's degree of universalism is economically functional: that it partly evolved to support and incentivize cooperation in economic production. According to this idea, different economic systems produce different degrees of universalism depending on whether the economic system primarily incentivizes local or more impersonal cooperation (see Tabellini, 2008b, for a theoretical

exposition of this idea). Below, we first summarize these arguments that were developed by other reserachers in prior work, and then investigate whether the cross-country variation in our universalism data can descriptively be explained by some of these accounts.

A first hypothesis is that strong relationship-specific preferences have been fostered by tight kinship ties (Enke, 2019; Henrich, 2020; Greif and Tabellini, 2017; Schulz et al., 2019; Schulz, 2022). The argument is that societies with tight kinship (extended family) systems inculcate preferences of low universalism because this is economically functional when economic production and exchange networks largely involve kith and kin. Relatedly, Schulz et al. (2019) and Henrich (2020) argue that Christianity induced higher universalism because the Western European Church was actively involved in dissolving the tight extended kinship systems that may have fostered strong parochial altruism. According to this body of theories, kinship tightness and Christianity should be related to universalism in opposite directions.

A second argument likewise asserts that the historical subsistence mode had an effect on people's universalism. Compared to rainfed agriculture, irrigation-intensive crops such as wetland rice are theorized to produce more interdependent and less universalist societies because building and maintaining large-scale irrigation systems requires extensive cooperation and collaboration with neighbors. Because irrigation could not be efficiently practiced by individual farmers, people had to rely on the group for economic production and survival, hence potentially fostering a prosociality that is focused on the in-group. In contrast, rainfed agriculture does not require extensive local cooperation, which may induce more universalist altruism. Accordingly, the literature has studied the effects of irrigation practices on a group-based psychology (e.g., Talhelm et al., 2014; Buggle, 2020).

To test whether these accounts can shed light on cross-country variation in the *GUS* data, we study correlations with the tightness of historical kinship networks (from Enke, 2019), data on contemporary cousin marriage (which has been argued to be a contemporary proxy for tight kin networks, Schulz, 2022), the share of Christians in society (from Barro and McCleary, 2003) and the intensity of ancestral irrigation practices (taken from Buggle, 2020). Figure B.13 shows partial correlation plots for each of these variables. Each panel is constructed controlling for log per capita income. All of the variables are conditionally correlated with composite universalism in ways hypothesized by prior literature: societies with tight ancestral kinship ties, higher rates of cousin marriage, a smaller share of Christians, and those with more intensive irrigation practices are less universalist. The raw (partial) correlations with composite universalism are -0.18 (-0.42) for kinship tightness, -0.32 (-0.44) for log cousin marriage rates, 0.45 (0.55) for share of Christians, and -0.33 (-0.38) for ancestral irrigation. The partial correlations are all statistically significant at the 1% level. The raw correlations are all statistically signif-

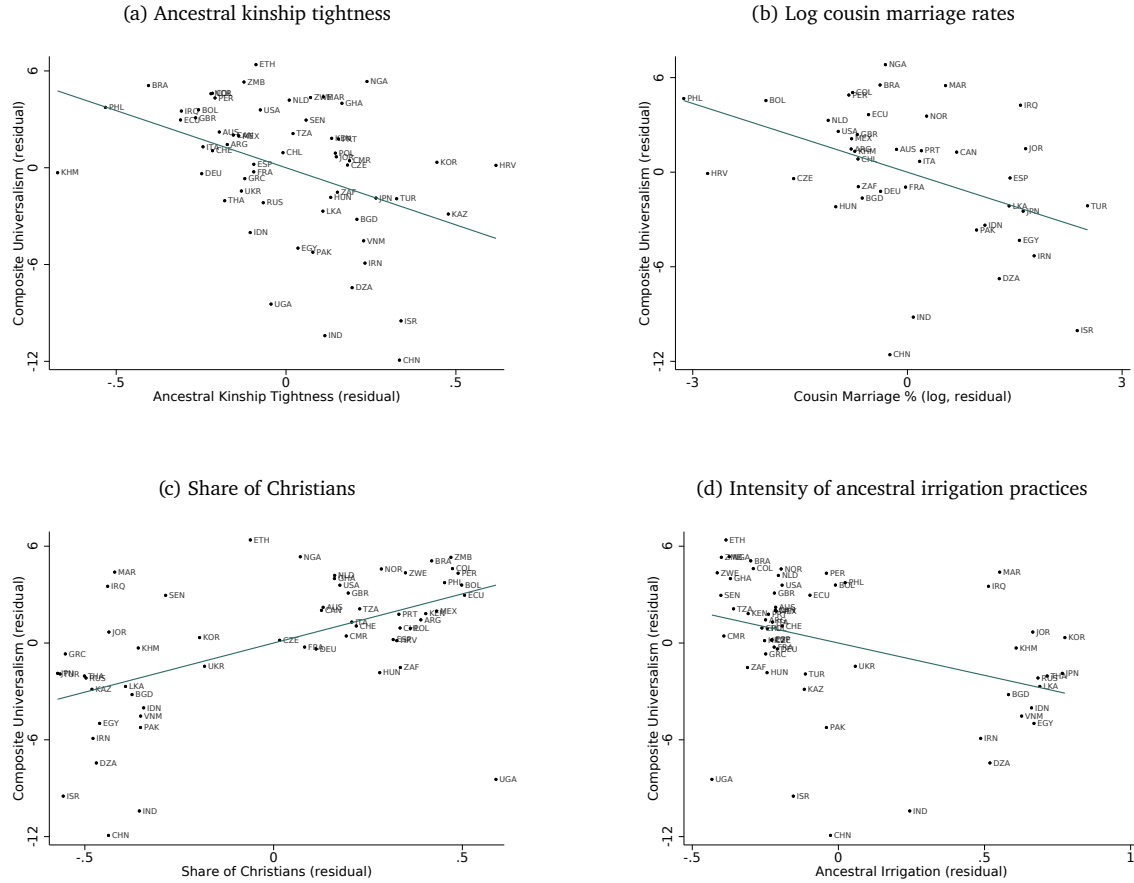


Figure 10: Country-level partial correlation plots of the cross-country relationships between composite universalism and ancestral kinship tightness (top left panel), the log of contemporary cousin marriage rates (top right panel), Christian share (bottom left panel) and the intensity of ancestral irrigation 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 (Buggle, 2020). 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 (2003). Some of the countries in the *GUS* dataset are missing because the respective correlate is not available for all countries.

icant at the 5% level, except for kinship tightness ($p = 0.17$). Naturally, these partial correlations do not shed light on which (if any) of these variables *cause* universalism.¹⁴

¹⁴In our pre-analysis plan, we specified that we would additionally study the correlations between composite universalism and other country-level outcomes, including property rights, education, federal redistribution, income inequality, foreign aid and environmental protection. The correlations are usually statistically insignificant, see Appendix Figure C.12. We also intended to look at the prevalence of family firms but were unable to locate a dataset on family firms that had sufficient coverage for a meaningful analysis.

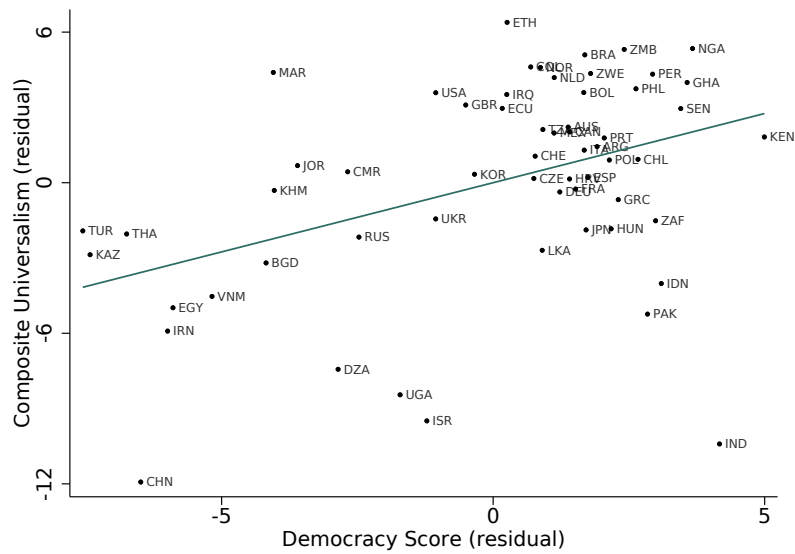


Figure 11: Country-level partial correlation plot of composite universalism against democracy, controlling for log per capita GDP. The democracy score ranges from 0 to 10 and is taken from the Polity V dataset. It includes information on competitiveness of executive recruitment, openness of executive recruitment, constraint on chief executive and competitiveness of political participation.

6.2 Experience with Democracy and Universalism

A prominent narrative among social scientists is that exposure to democracy fosters universalist preferences: if all people in society engage in collective decision-making to elect a joint set of leaders, then this may weaken group-based divisions and induce people to treat all others alike. Philosophers such as Rawls (1993) have argued that a fair basic structure in society (including democracy) creates moral obligations towards compatriots. Similarly, democracy is frequently highlighted in discussions of potential drivers of prosociality by psychologists and cultural evolution researchers (the “D” in the widely-used WEIRD acronym).

The *GUS* dataset facilitates an investigation of this hypothesis. As a first step, Figure 11 shows the partial cross-country correlation between the Polity V democracy index and composite universalism. The plot is constructed controlling for log per capita income, such that, for example, the y-axis shows average universalism in a country, after log per capita income has been partialled out. The raw correlation is $r = 0.22$ ($p = 0.09$) and the partial correlation $r = 0.42$ ($p < 0.01$).

To move beyond this purely descriptive evidence, we now make use of the fact that the degree of democracy varies widely not just across countries but also across age cohorts.

6.2.1 Variation Across Country-Age-Cohorts

Recent research has leveraged country-cohort-specific variation in lifetime experience with democracy to study the determinants of support for democracy (Fuchs-Schündeln and Schündeln, 2015; Acemoglu et al., 2021). Here, we use the same difference-in-differences strategy to provide evidence on whether experience with democracy shapes universalism.

For each respondent in the *GUS*, we construct an index of the strength of experience with democracy. We work with the democracy score in the Polity V dataset, which is a summary index ranging from zero to ten that captures different institutional aspects such as the degree of constraints on the executive and the competitiveness of political recruitment and participation. For most countries in our sample, this variable is available for each year. For each individual in our data, we compute the average democracy score over a respondent's lifetime in their current country of residence.

Two remarks on the sample are in order. First, because the Polity V democracy score is missing for some countries and years, we restrict attention to respondents for whom the democracy score is available for at least 75% of their lifetime, since otherwise we cannot credibly proxy an individual's experience with democracy. Our results are robust to using cutoffs such as 70%, 80% or 90%. Second, given that we separately look at migrants below, and given that we don't know when exactly an individual migrated to their current country of residence (based on which we compute experience with democracy), we exclude migrants from this first analysis, though we have verified that the results are quantitatively identical when we include them.

The regression analysis follows a difference-in-differences strategy that relates differential changes in universalism across cohorts in different countries to changes in cohort-level experience with democracy.¹⁵ Intuitively, the hypothesis is that if in a given country the young were exposed to democracy for a longer fraction of their lifetime than the old, then universalism should be higher among the young. However, if in another country the young were exposed to democracy for a shorter fraction of their lifetime than the old, then universalism should be higher among the old. Importantly, there is sizable variation across countries in which cohorts lived in a democratic regime for a longer share of their lifetime because different countries transition into and out of democracy at different points in time, see Appendix Figure C.13.

¹⁵Formally, the estimating equation is given by:

$$univ_{i,a,c} = \alpha + \beta d_{a,c} + \sum_c \gamma_c \mathbb{1}_c + \sum_a \gamma_a \mathbb{1}_a + \epsilon_{i,a,c} \quad (3)$$

Here, $univ_{i,a,c}$ is universalism of individual i of age a from country c , $d_{a,c}$ is experience to democracy in a country-age cell, and the two summands capture fixed effects for age and country, respectively.

Table 2: 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	0.38*** (0.10)	0.38*** (0.10)	0.34*** (0.10)	0.42*** (0.11)	0.41*** (0.11)	0.30** (0.13)	0.21 (0.13)
Lifetime average log GDP p/c		1.46 (0.90)	1.11 (0.94)	-0.07 (0.93)	-0.38 (0.97)	2.56** (1.21)	2.07* (1.26)
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	0.09	0.09	0.09	0.06	0.06	0.09	0.10
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 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 2 summarizes the results. The regression reported in column (1) shows that, holding fixed an individual's age and their country of residence, longer experience with more democratic institutions is associated with higher universalism. The standardized beta in this regression (not reported in the regression table) is 8%, suggesting that a one standard deviation increase in experience with democracy is associated with an increase in 8% of a standard deviation in universalism. Column (2) shows that these patterns are specific to democracy and do not hold similarly for average lifetime (log) GDP per capita. Column (3) controls for demographics. The results are very similar.

Columns (4)–(7) break these patterns down into domestic and foreign universalism. While the point estimate of lifetime exposure to democracy is positive in both cases, it is 30–70% larger for domestic universalism (though the difference between the regression coefficients is not statistically significant).

6.2.2 Variation Across First-Generation Migrants

The Gallup World Poll contains information about respondents' country of birth, which allows us to study a potential impact of democracy on universalism through cross-migrant analyses that hold the current country of residence fixed. The idea is that if two migrants currently reside in the same country, they may still have had differential experience with

democracy in the past if they descend from different home countries. This is the so-called epidemiological approach in cultural economics (Giuliano, 2007), though we here work with first- rather than second-generation migrants. To facilitate this, we assign each migrant in the *GUS* data the democracy score in their country of origin, and link it to universalism, controlling for country of residence fixed effects. All non-migrants in the data are excluded from the analysis. We note that this migrant analysis has less power than the cohort analysis above because of a lower number of observations (2,741 migrants vs. 53,639 respondents).

Table 3 summarizes the results, which are broadly similar to those from the analysis across age cohorts: exposure to democracy is positively linked to universalism. We find a strong positive relationship between domestic universalism and democracy, while there is no significant effect for foreign universalism. Our analysis controls for individual-level demographics (column (3)) and biogeographical features of the country of origin in column (4): average temperature, precipitation, percentage of the population living in tropical or subtropical zones, and percentage of the population at risk of contracting malaria.

Overall, we view these combined results from the cross-country, cross-cohort and cross-migrant analyses as tentative evidence that experience with democracy leads to higher universalism. One interpretation of the slightly stronger results in the domestic domain is that democracy may shape more strongly how people think about domestic group divisions because living in a democracy versus autocracy arguably primarily matters for interactions with fellow citizens rather than foreigners. Indeed, Rawls (1993) argued that a fair basic structure in society creates moral obligations towards compatriots but not towards foreigners.

7 Discussion and Outlook

This paper provides a comprehensive analysis of the global variation in universalism. By introducing a new large-scale dataset, the *Global Universalism Survey*, we document how universalism varies across societies and individuals, whether moral considerations underlie observed behavior, how universalism helps understand heterogeneity in political views and the social fabric of societies, and how experience with democracy may shape universalist attitudes.

A main takeaway from the analysis is that universalism and politico-economic outcomes appear to be deeply intertwined. On the one hand, we provide evidence across countries, across age cohorts and across migrants that experience with democracy may shape universalism. On the other hand, we also show that universalism is strongly pre-

Table 3: 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	0.20* (0.10)	0.28** (0.13)	0.26* (0.14)	0.27* (0.15)	0.45*** (0.14)	0.44*** (0.14)	0.10 (0.18)	0.05 (0.18)
Log GDP p/c in home country		-0.20 (0.54)	-0.30 (0.55)	-0.69 (0.73)	-0.40 (0.51)	-0.59 (0.52)	-0.38 (0.73)	-0.37 (0.75)
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	0.07	0.07	0.07	0.07	0.05	0.06	0.06	0.06
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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

dictive of people's social and economic policy views as well as the structure of a society's social fabric. These two sets of results suggest that politico-economic outcomes and universalism co-evolve.

While this paper has made some first attempts to illuminate demographic and cultural differences in universalism, we believe that the existence of the *GUS* dataset opens up the possibility for an entire research agenda on the correlates, determinants and consequences of variation in universalism. Many research questions that were previously out of reach due to data limitations can now be tackled, including a broader investigation of how the prevalence of universalism interacts with political and economic institutions, how it is shaped by ecological and climatic conditions, and how it shapes individual-level behaviors and outcomes.

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ONLINE APPENDIX

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., 2019), 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 translations 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 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

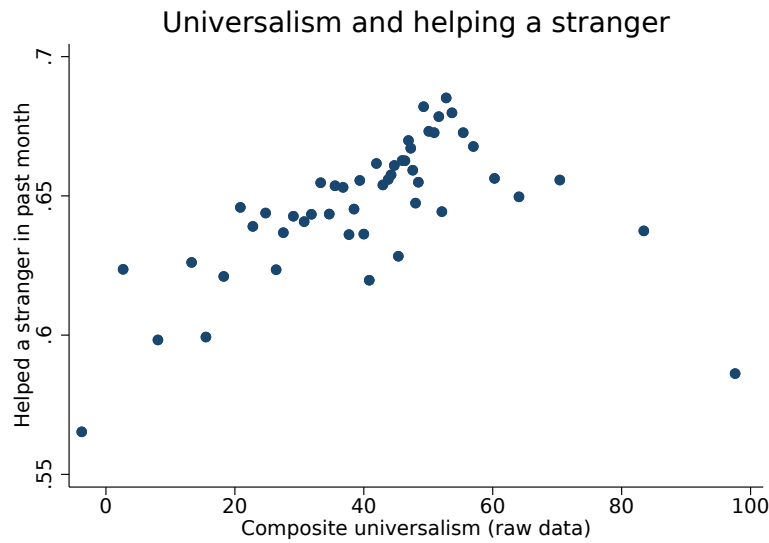


Figure B.1: Binned scatter plot of response to Gallup question “In the last 12 months, did you help a stranger in need?” as a function of a respondent’s composite universalism, controlling for country fixed effects. Universalism is computed based on the raw data in the money allocation decisions.

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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

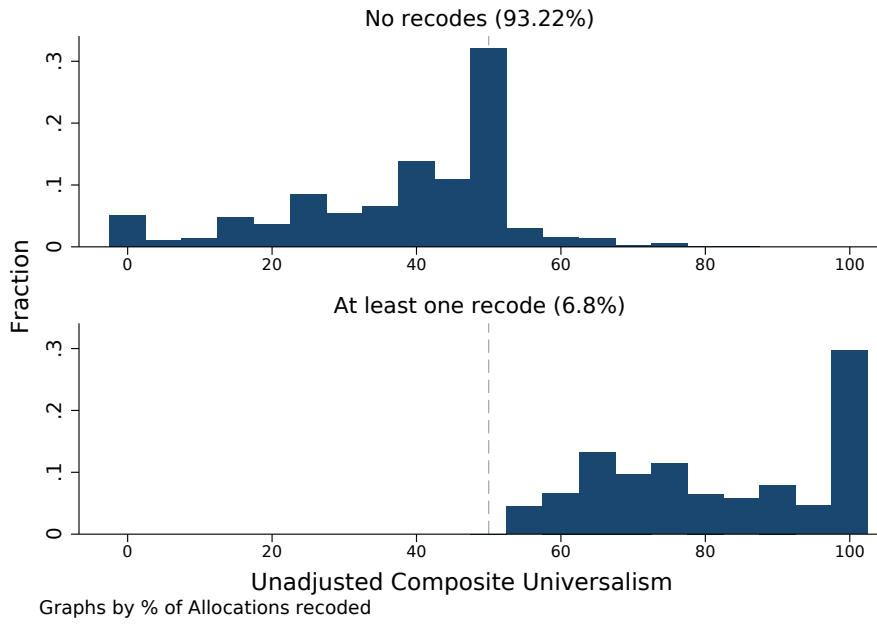


Figure B.2: Universalism patterns in recoded observations. Each panel shows the distribution of the *unadjusted* composite universalism statistic, as a function of whether or not we eventually recode at least one of the respondent’s allocation decisions.

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:¹⁶

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.

¹⁶We 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.

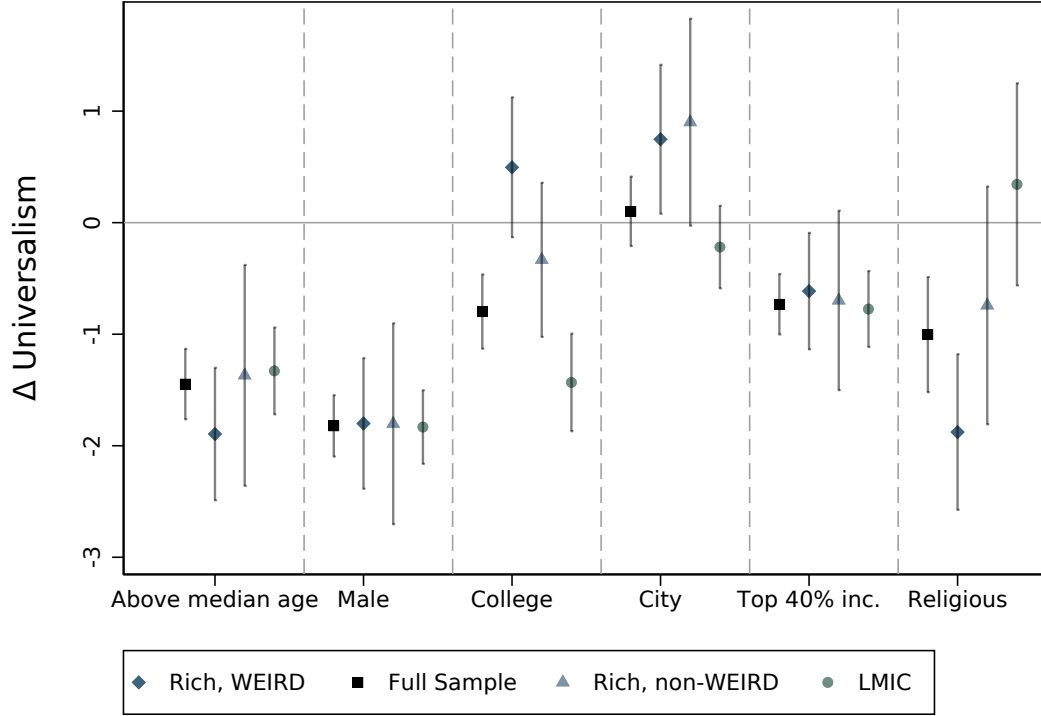


Figure B.3: Universalism and demographics. OLS coefficients and standard errors from the symmetric drop procedure, based on regressions of composite universalism on each demographic, controlling for country and treatment fixed effects. We report the average regression coefficient from $M = 500$ iterations 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. See Section 2.7 for the wording of the political questions. 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.

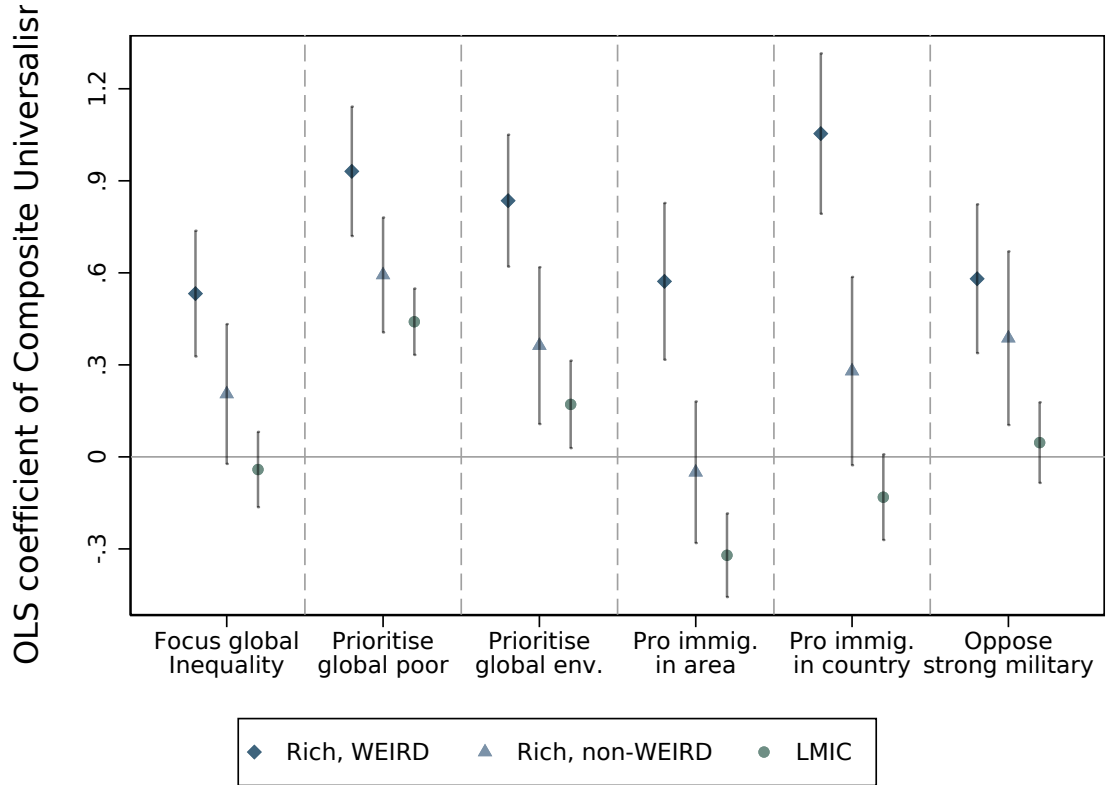


Figure B.4: Individual-level composite universalism and political views in different sub-samples, using the symmetric drop procedure. OLS coefficients from regressions of political attitudes on composite universalism, controlling for country and treatment fixed effects. We report the average regression 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 2.5.

B.8.1 Replication of Figures with Raw Data

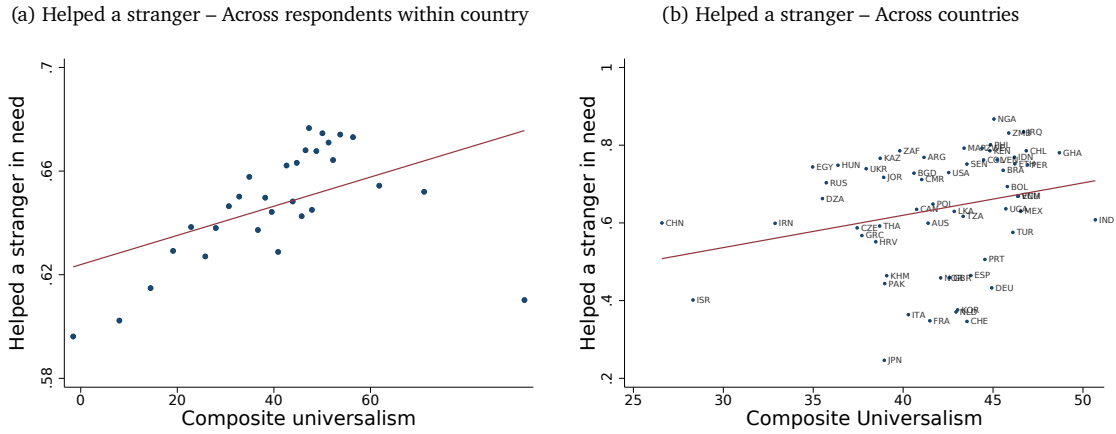


Figure B.5: Composite universalism and helping a stranger. The left panel shows a respondent-level binned scatter plot that, for a given level of universalism, computes the average probability of having helped a stranger. The bins are endogenously constructed such that each dot represents the same number 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.

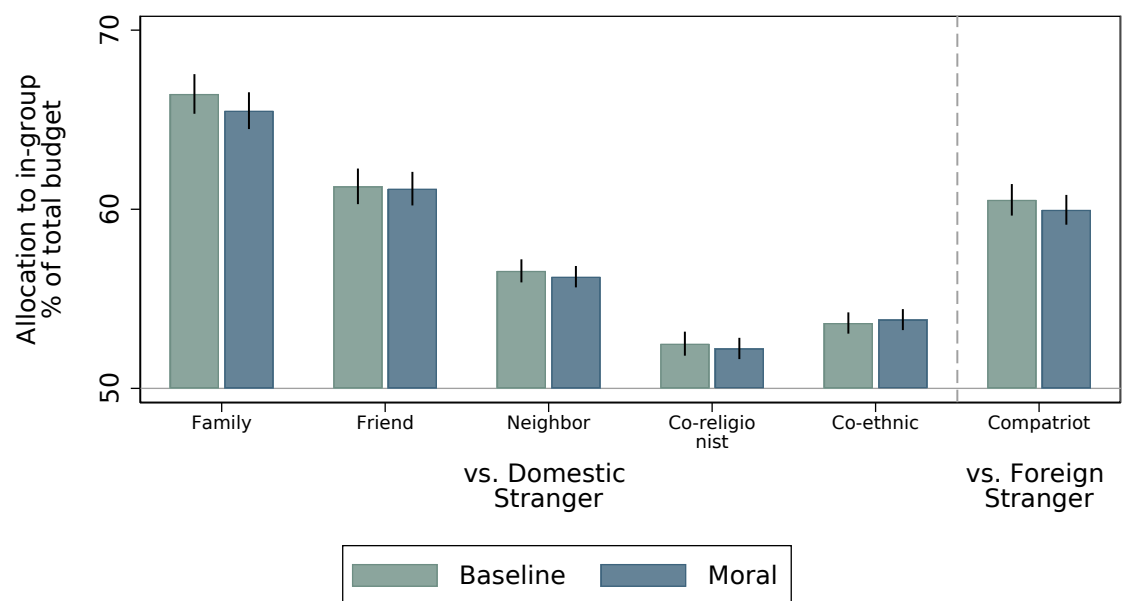


Figure B.6: Mean money allocations to the in-group by treatment. Each bar indicates how much of the budget was given to the in-group. Whiskers show 95% confidence intervals, computed based on clustering at the sampling unit level.

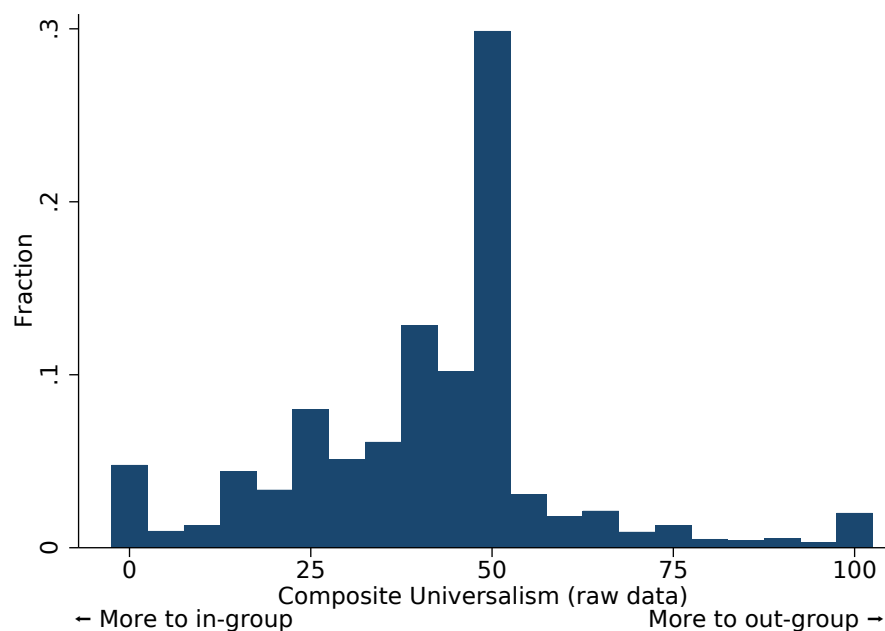


Figure B.7: Distribution of composite universalism across individuals, pooled across treatments ($N = 63,788$). 0 means that all money is shared with the in-group, 50 captures equal splits (on average), and 100 that all money is shared with the socially more distant stranger.

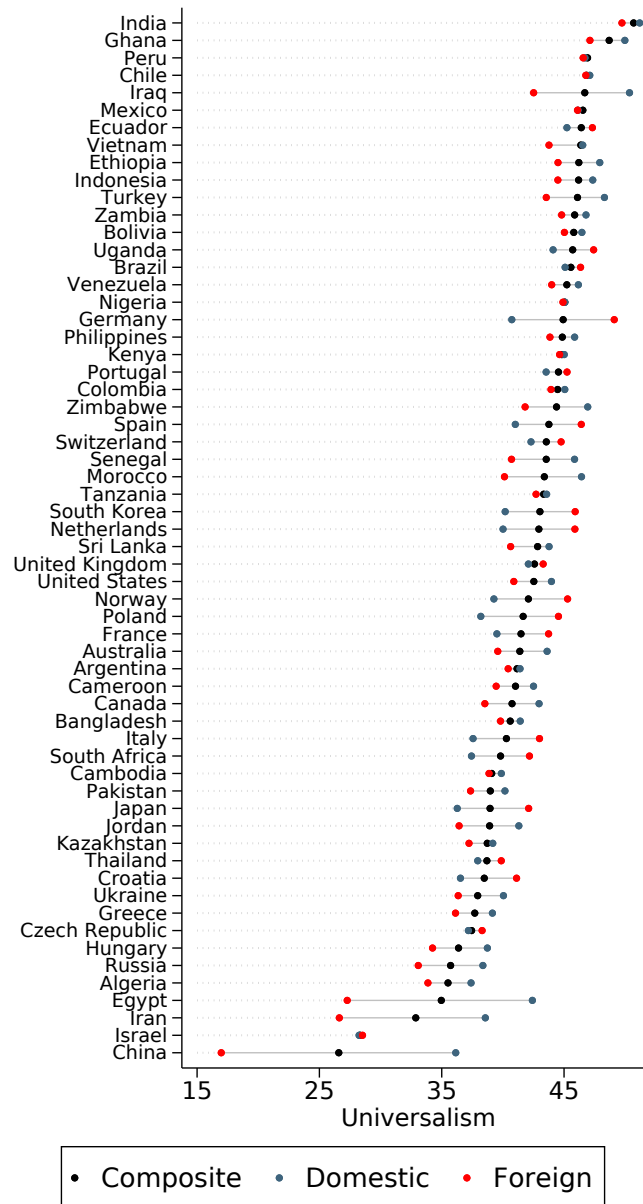


Figure B.8: Average composite, domestic and foreign universalism by country. 0 means that all money is shared with the in-group, 50 equal splits, and 100 that all money is shared with the socially more distant stranger. Composite universalism occasionally doesn't equal the average of domestic and foreign universalism because of missing domestic or foreign universalism data (see footnote 4 and Appendix B.6).

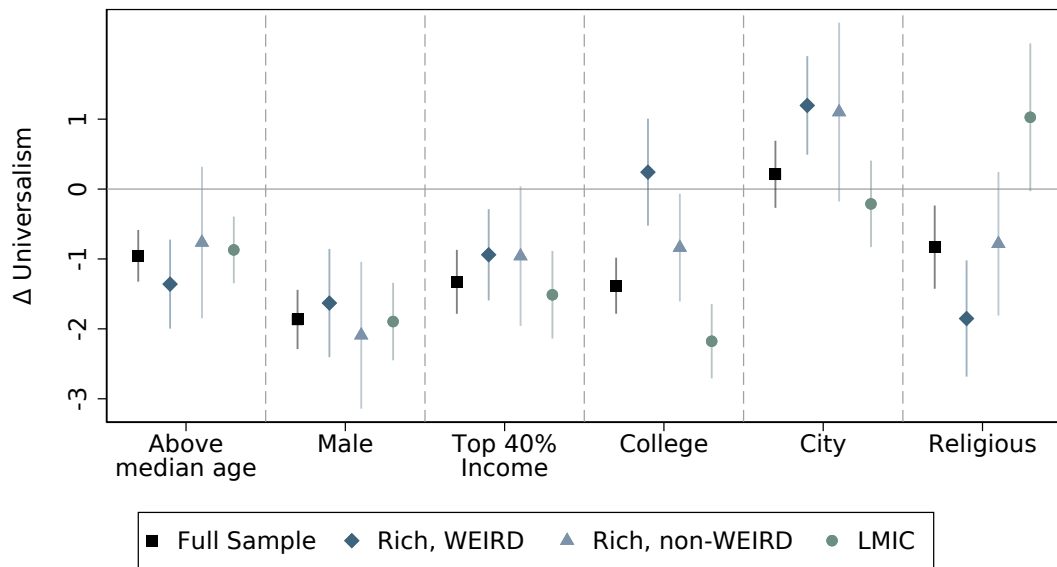


Figure B.9: Universalism and demographics. OLS coefficients from regressions of composite universalism on each demographic, controlling for country and treatment fixed effects. Each coefficient reflects the results of a separate regression on a different sub-sample and can be interpreted as the percentage 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.

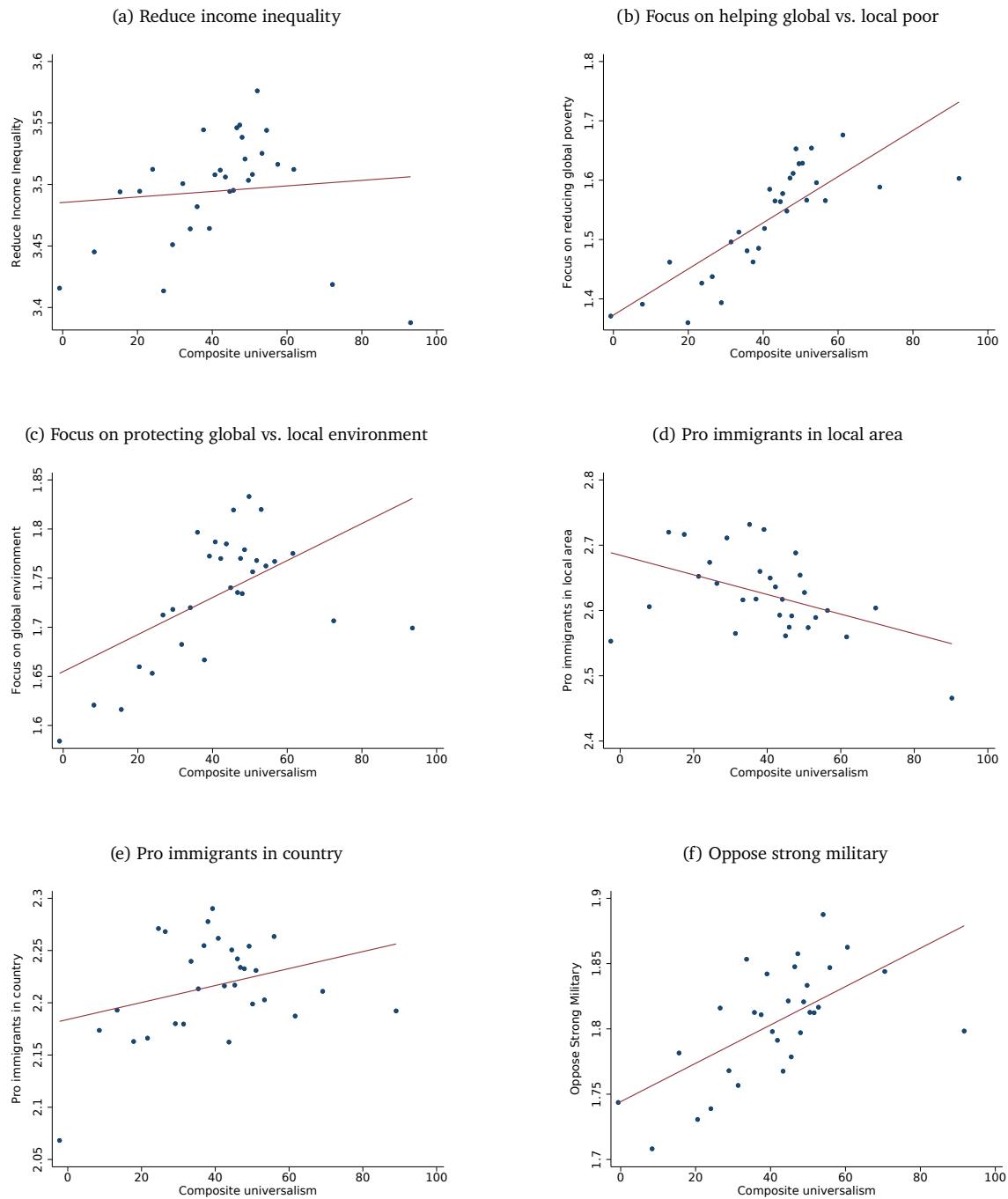


Figure B.10: Composite universalism and political views at the individual level. The figures show binned scatter plots that average agreement with a policy priority for a given level of universalism. The figures are constructed controlling for country and treatment FE. Political views are coded as 1–4, based on responses of “Strongly disagree”, “Disagree”, “Agree” and “Strongly agree.” See Section 2.7 for the wording of the political questions.

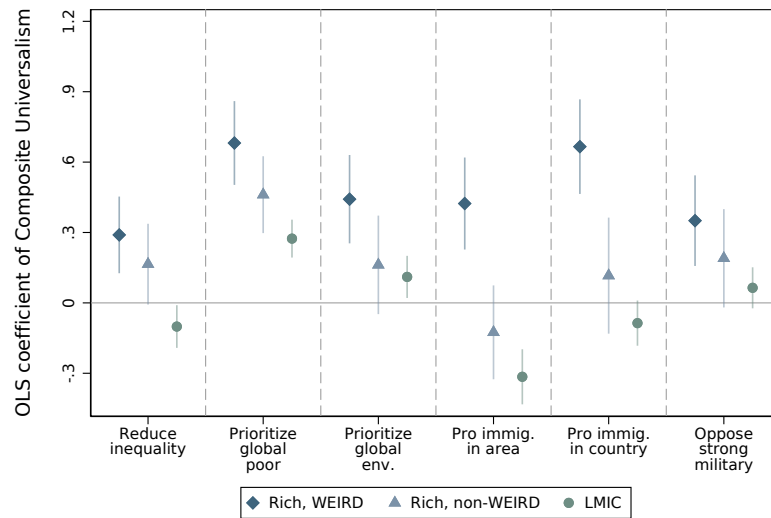


Figure B.11: Individual-level composite universalism and political views in different sub-samples. OLS coefficients from regressions of political attitudes on composite universalism, controlling for country and treatment fixed effects. Each coefficient reflects the results of a separate regression on 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.

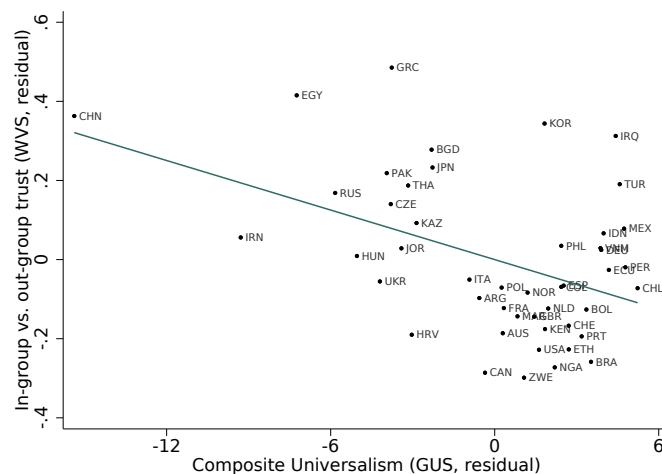


Figure B.12: Country-level added variable plot of the difference between in-group and out-group trust against composite universalism, controlling for log per capita income. The difference between in-group and out-group trust is computed based on World Values Survey questions that ask about trust in three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

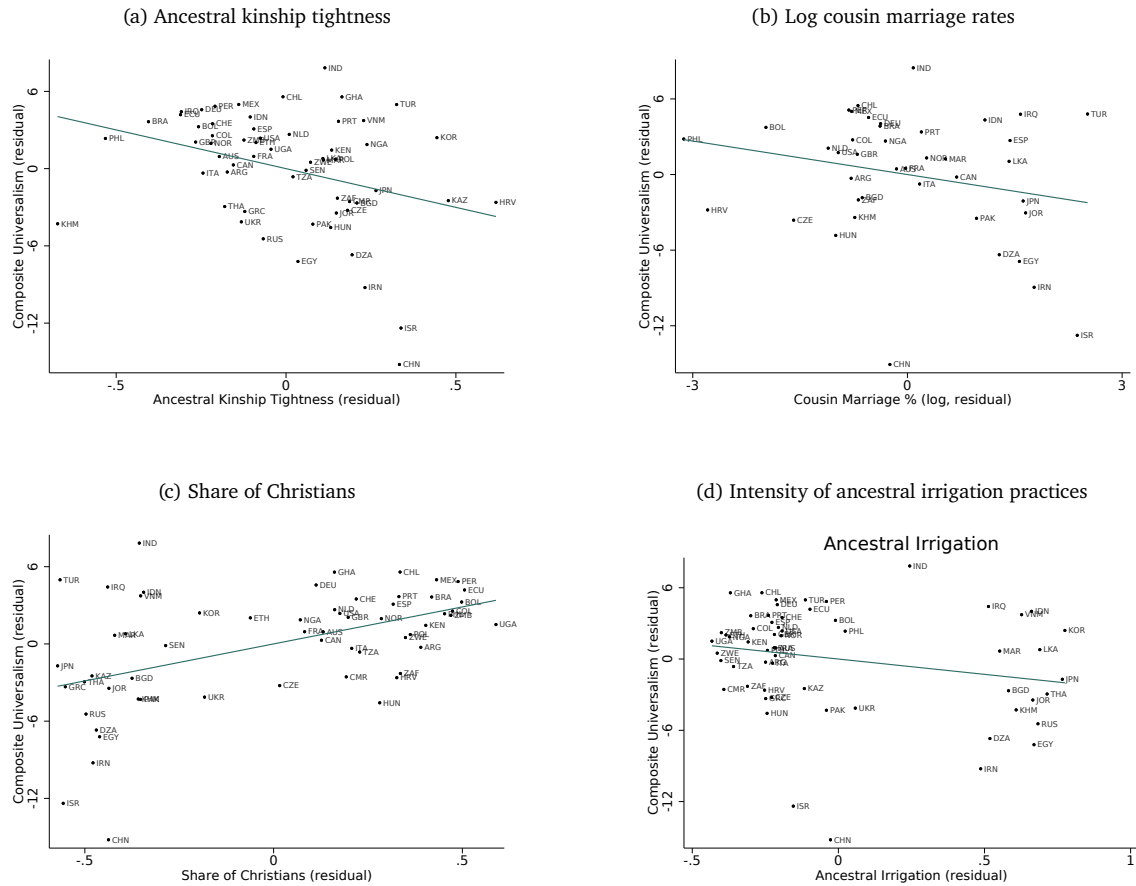


Figure B.13: Country-level partial correlation plots of the cross-country relationships between composite universalism and ancestral kinship tightness (top left panel), the log of contemporary cousin marriage rates (top right panel), Christian share (bottom left panel) and the intensity of ancestral 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 (Buggle, 2020). 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 (2003). Some of the countries in the *GUS* dataset are missing because the respective correlate is not available for all countries.

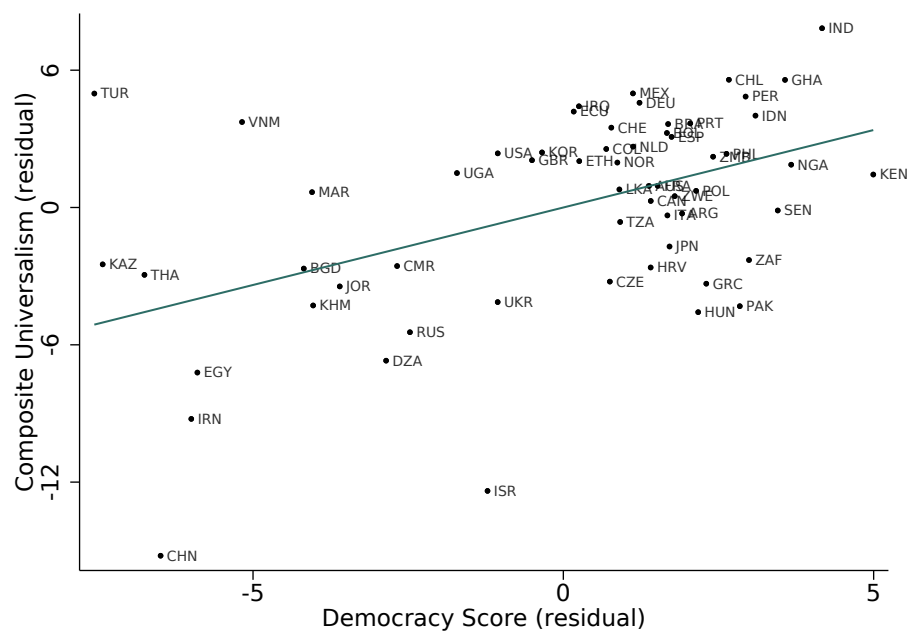


Figure B.14: Country-level added variable plot of composite universalism against democracy, controlling for log per capita GDP. The democracy score ranges from 0 to 10 and is taken from the Polity V dataset. It includes information on competitiveness of executive recruitment, openness of executive recruitment, 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. See Section 2.7 for the wording of the political questions. 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. 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. OOLS 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

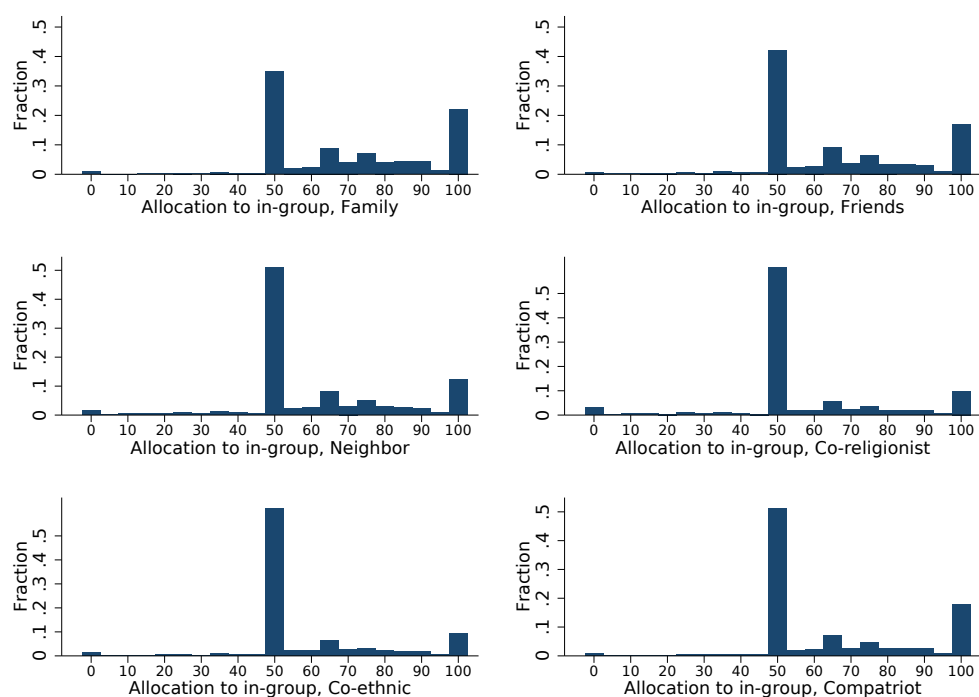


Figure C.1: Histograms of allocations to the in-group across the six survey questions, pooled across treatments. The number of allocations ranges from 23,073 to 25,360 in the first 5 panels. The last panel shows the histogram of 61,753 allocations.

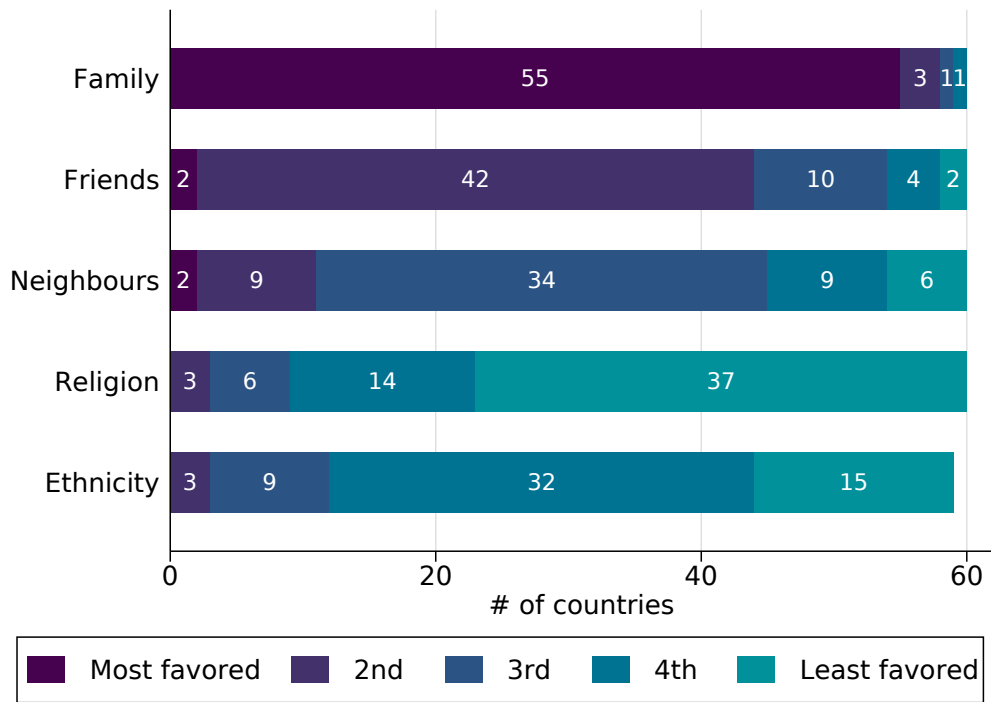


Figure C.2: “Ranking” of in-groups across countries implied by the allocation decisions, pooled across treatments, excluding the Compatriot-Foreigner allocation question. Each section of a bar represents a rank (1 to 5, 1 being the most favored) for that in-group. The size of each section is proportional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

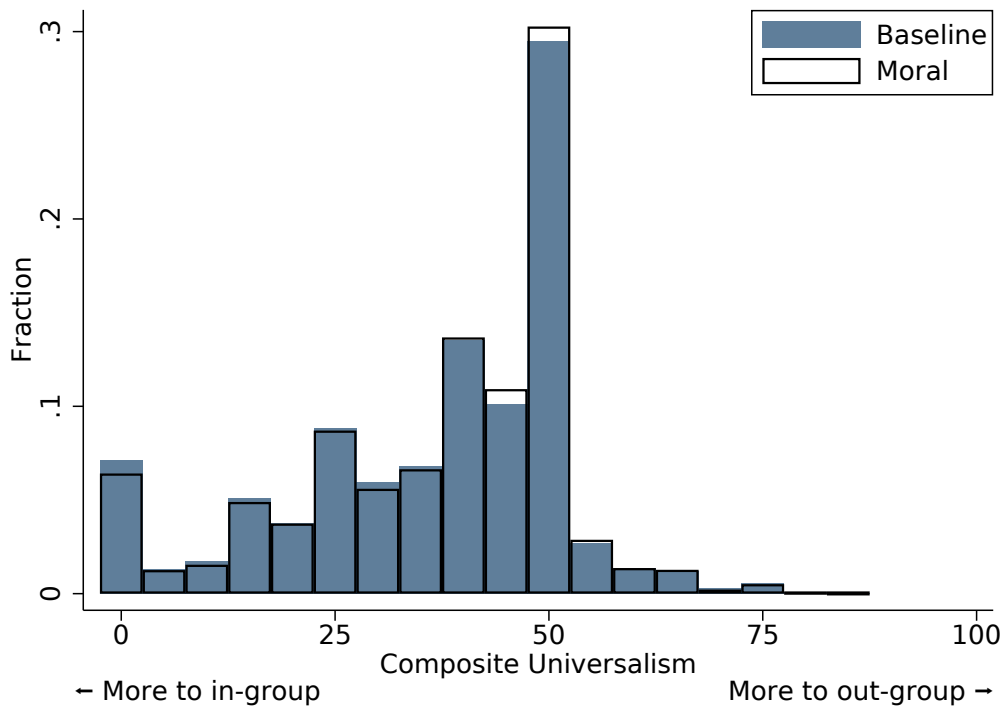


Figure C.3: Distribution of composite universalism across individuals, split by treatment (*Baseline* and *Moral*), $N = 31,670$ and $32,118$ respectively.

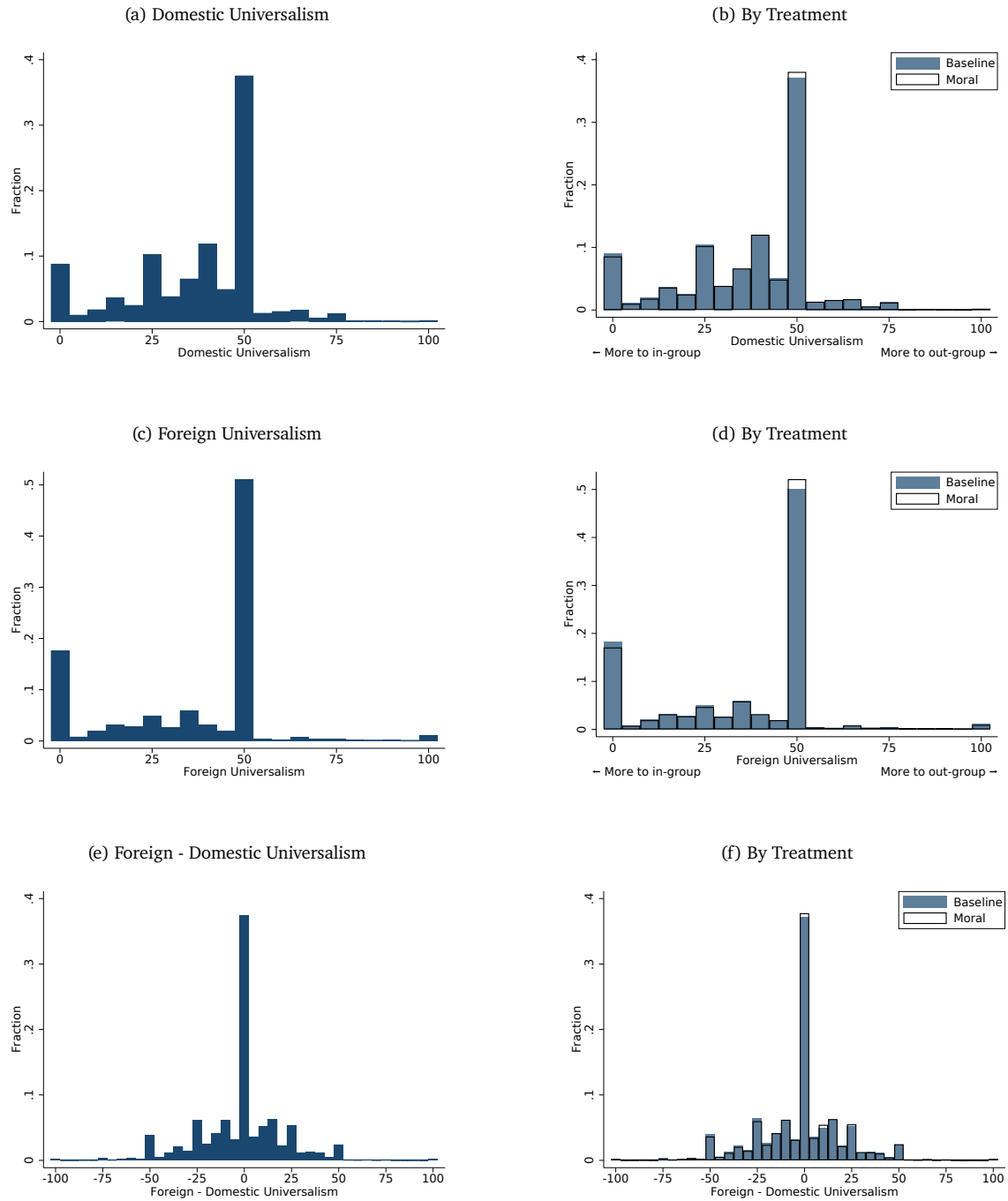


Figure C.4: Distribution of domestic, foreign and foreign - domestic universalism across individuals, separately by treatment (Baseline and Moral).

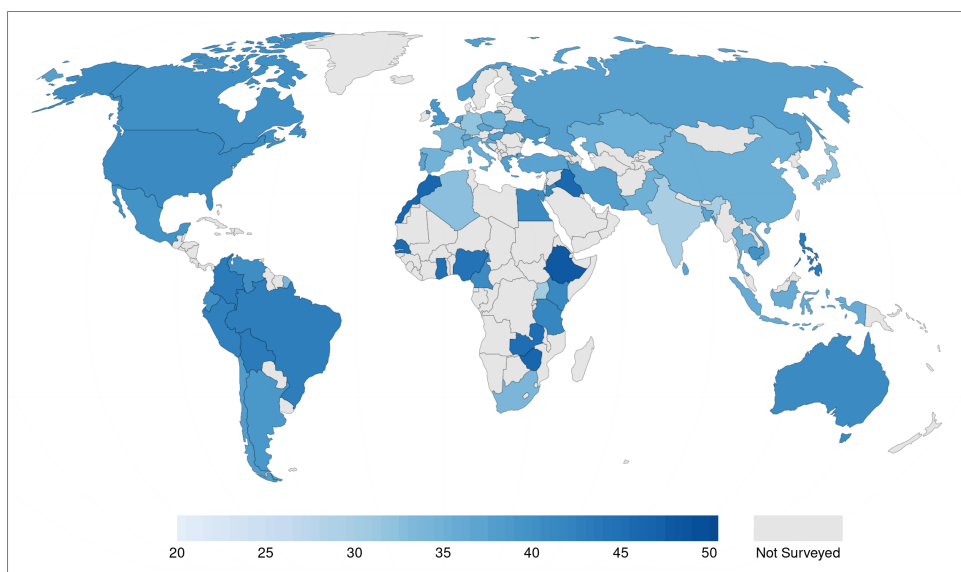


Figure C.5: Global variation in domestic universalism

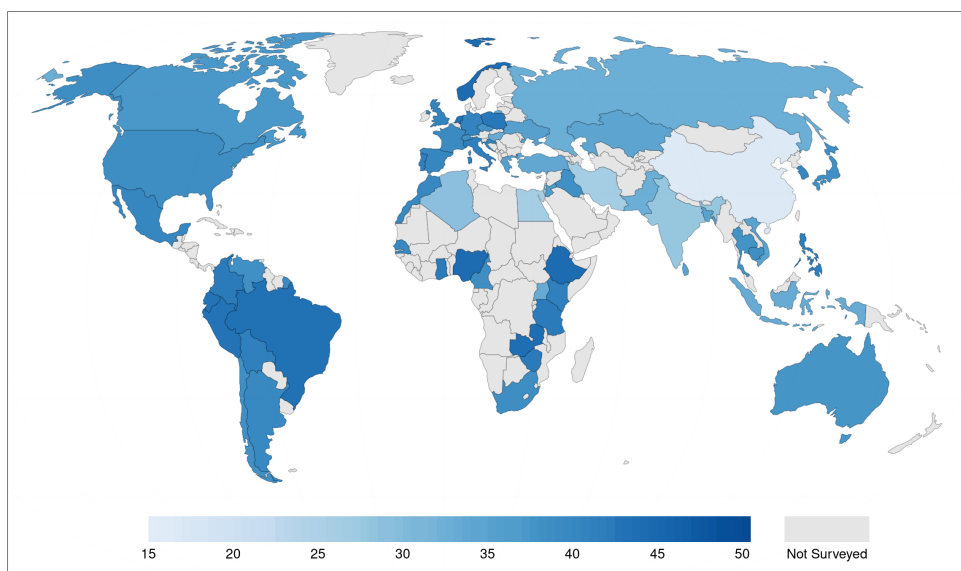


Figure C.6: Global variation in foreign universalism

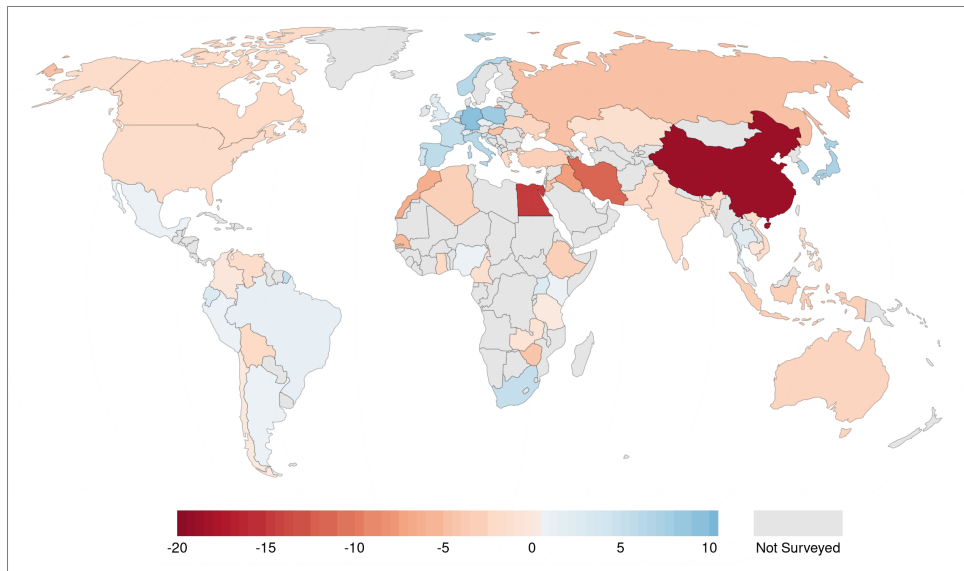


Figure C.7: Global variation in difference between domestic and foreign universalism

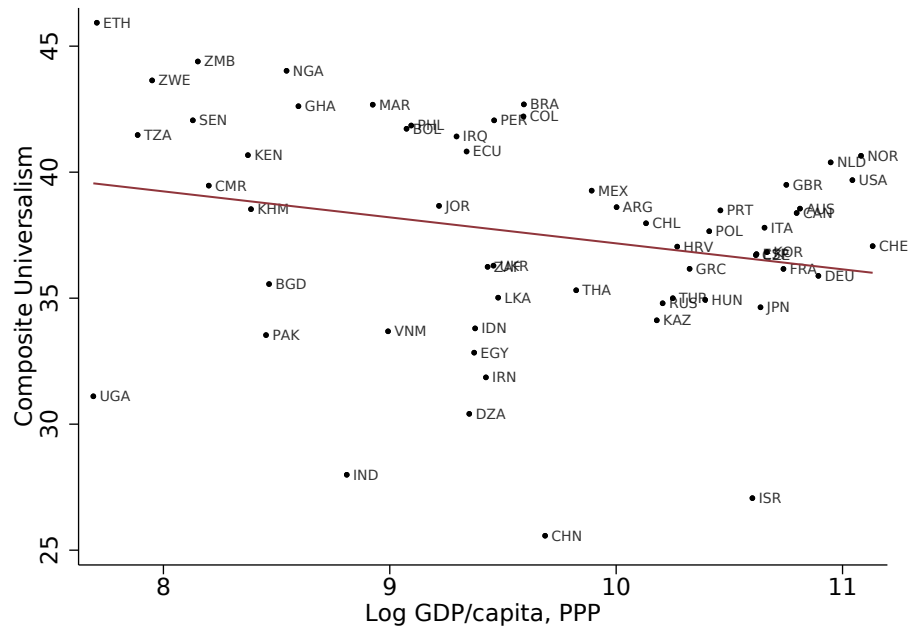


Figure C.8: Correlation between country-level composite universalism and log GDP per capita.

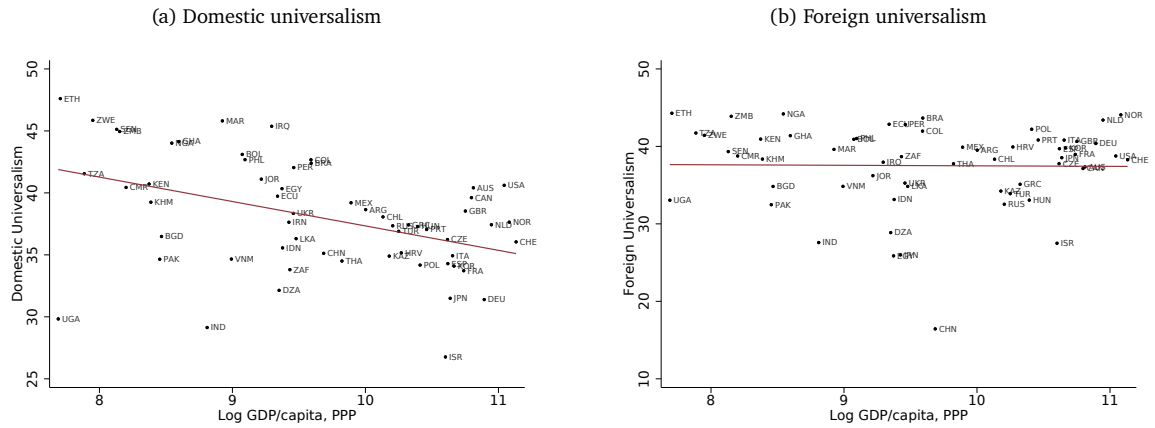


Figure C.9: Domestic / foreign universalism and log GDP per capita. 0 means that all money is shared with the in-group, 50 equal splits, and 100 that all money is shared with the socially more distant stranger.

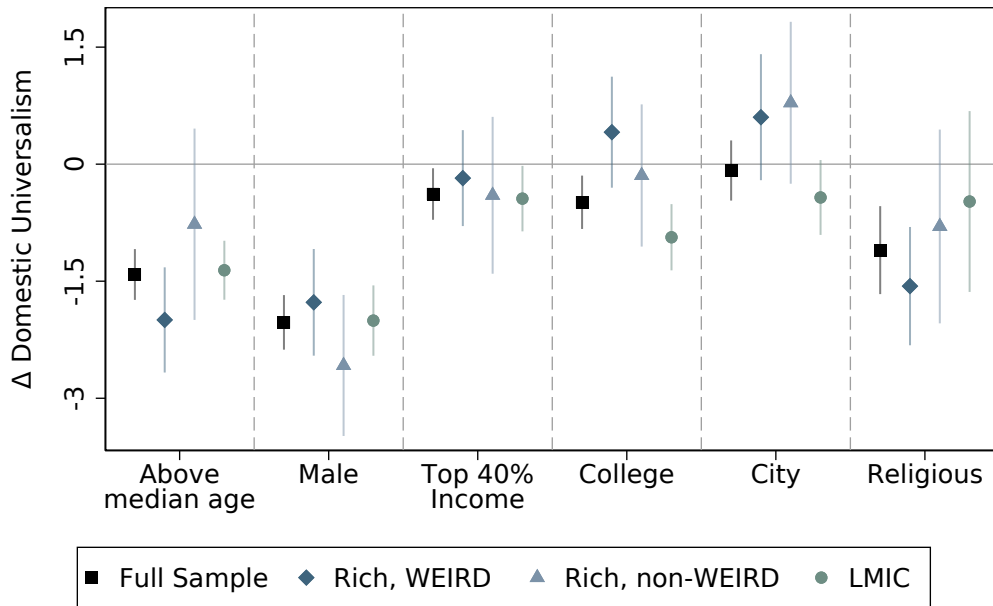


Figure C.10: Domestic universalism and demographics. OLS coefficients from regressions of domestic universalism on demographics, controlling for country and treatment fixed effects. Each coefficient reflects the results of a separate regression and can be interpreted as the percentage point change in 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.

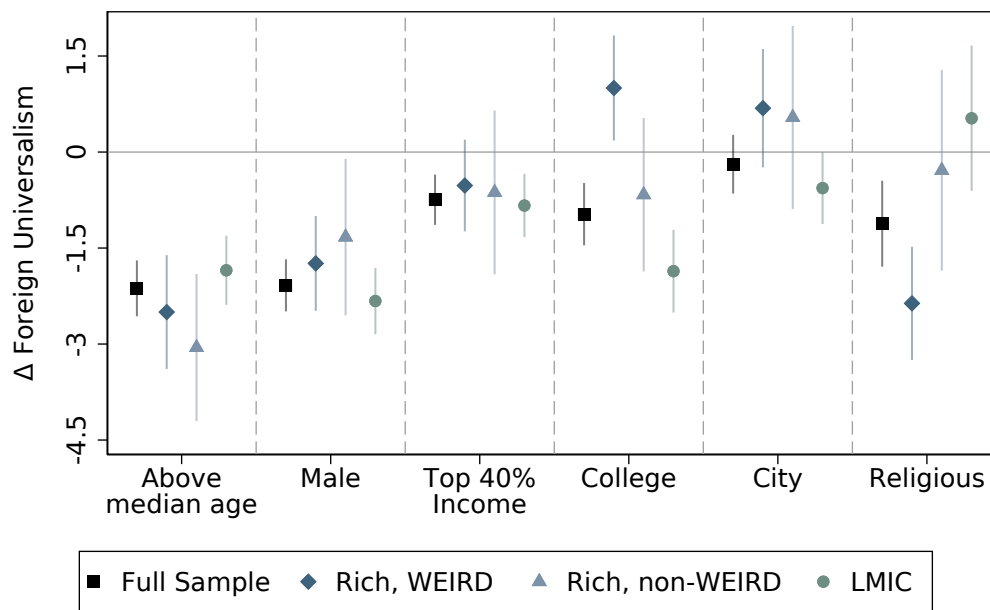


Figure C.11: Foreign universalism and demographics. OLS coefficients from regressions of foreign universalism on demographics, controlling for country and treatment fixed effects. Each coefficient reflects the results of a separate regression and can be interpreted as the percentage point change in universalism. 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.

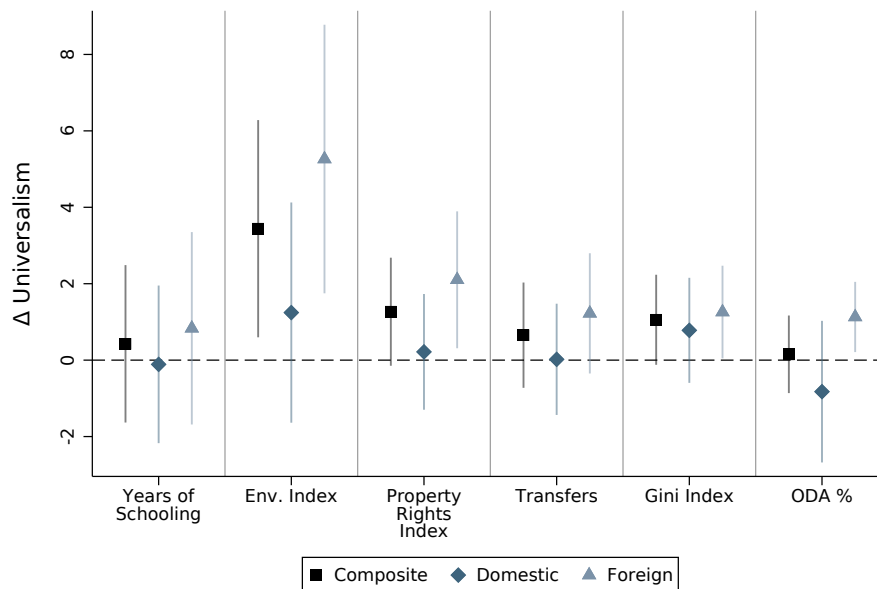


Figure C.12: Additional country-level correlations: Results from OLS regressions of composite universalism on each of various country-level variables, controlling for log GDP p/c. All country characteristics other than universalism are standardized into z-scores. As a result, the coefficients show by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

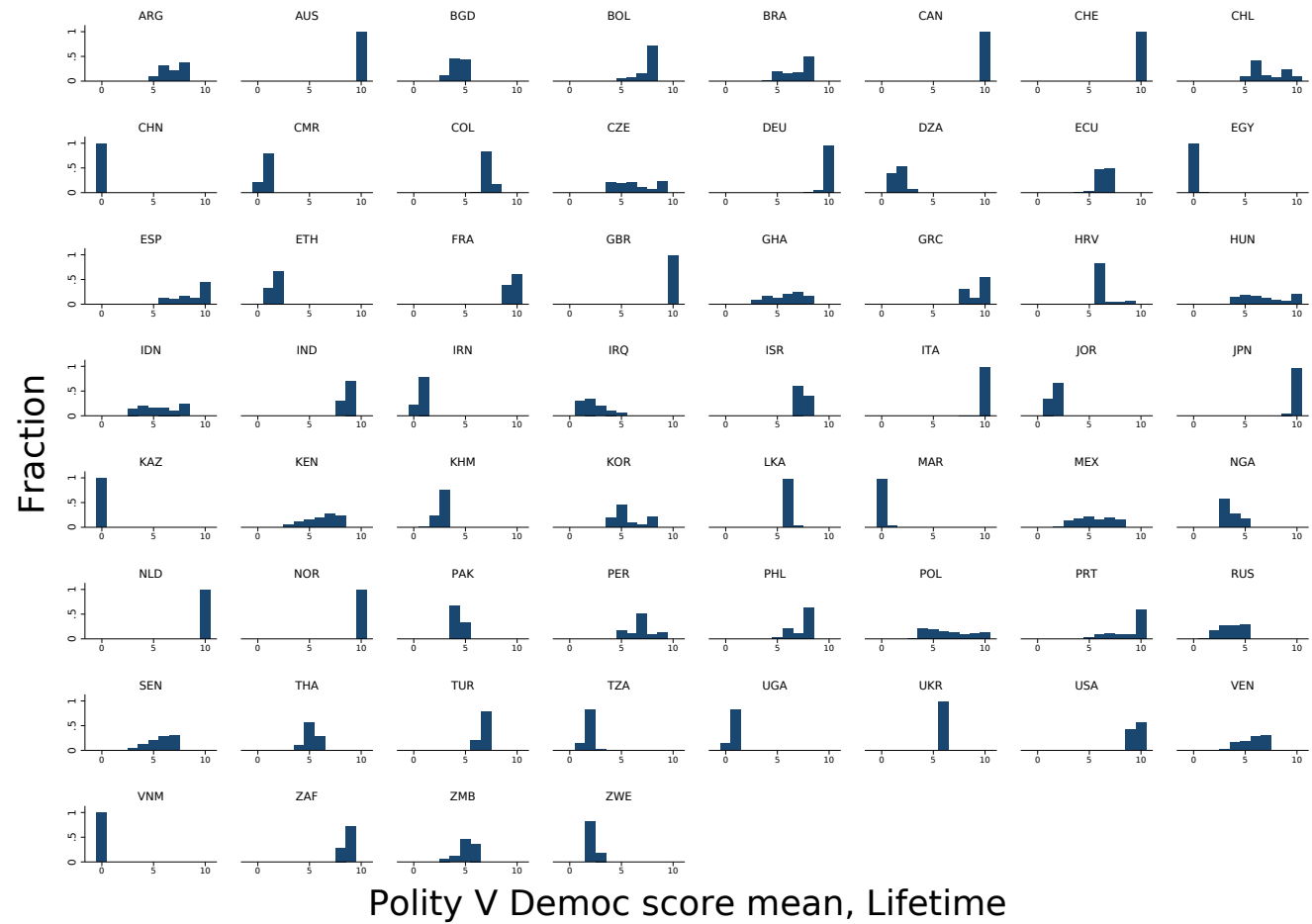


Figure C.13: Country-wise variation in exposure to democracy. Each plot shows the distribution of the average democracy (score from 0 to 10) experienced by an individual in our sample from that country.

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). The coefficients and standard errors here are used in plotting the values in figure 8. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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). The coefficients and standard errors here are used in plotting the values in figure 8. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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). The coefficients and standard errors here are used in plotting the values in figure 8. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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). The coefficients and standard errors here are used in plotting the values in figure 8. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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). The coefficients and standard errors here are used in plotting the values in figure 8. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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). The coefficients and standard errors here are used in plotting the values in figure 8. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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. As discussed in Section 3, 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 Section 4, 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 interaction

Table E.1: Decomposition of demographic differences in universalism

	<i>Dependent variable: Composite Universalism</i>					
	<i>Baseline</i>		<i>Moral</i>		<i>Full sample</i>	
	(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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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

In-group minus out-group trust: Computed based on World Values Survey Data. First compute average trust in in-groups (family, neighbors and people one knows) and average trust in out-groups (strangers, foreigners, people of another religion). Then compute difference.

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 (2020), captures how much the ancestors of today's populations relied on irrigation for subsistence.

Family ties: Constructed using the methodology described in Alesina and Giuliano (2013). Using data from the 3rd and 4th waves of the World Values Survey, we focus on the three questions represented by the variables V4, V13, V14. The final variable is the first principal component of these three variables, averaged at the country level.

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.

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

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

Development Aid: Official Development Assistance as a percent of Gross National Income. Data from OECD/WDI. 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.

Property Rights: We use measures of property rights and other governance indicators from the Quality of Governance data set (2021 release).

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

Family Ties: We construct a measure of family ties by collating various waves of the World Values Survey. Following the procedure in Alesina and Giuliano (2013), our measure is the first principal component of V4 (Importance of family), V13 (Respect and love for parents) and V14 (Parents responsibilities to their children).

Government transfers: We use the transfers series from the World Development Indicators.

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

Relative Trust: The variable is constructed using the trust questions in the World Values Survey. 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). 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 (2013).