Universalism: Global Evidence[†]

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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. (JEL D12, D64, D72, Z12, Z13)

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

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) toward 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 toward 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, 2024; Enke, Rodríguez-Padilla, and Zimmermannn 2023, 2022; Enke, Fisman et al. 2023; Henrich 2020; Andre et al. 2021; Le Rossignol and Lowes 2022; Fehr, Mollerstrom, and Perez-Truglia 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, Heine, and Norenzayan 2010).

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 trade-offs between in-group members and strangers. By introducing these data, we (i) present a new set of stylized facts that exposit the variation in universalism within and across countries; (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 United States were asked how they would allocate $1,000 between a friend and a stranger from the United States.

The survey questions (i) underwent extensive pretests 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 (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 percent of respondents always act in line with universalism and divide the money equally in all decisions, while 17 percent of respondents share at most 20 percent 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 preregistered 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 United States, 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 percent in China, India, and Israel to 46 percent 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, Heine, and Norenzayan 2010; 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, Rodríguez-Padilla, and Zimmermann 2023). 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, Leonardi, and Nanetti 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 people's actual behavior toward 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 (WVS). 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 underprivileged 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] Supplemental 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. 2010), 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 I–II provide an overview of the *GUS* data and exposit the variation across relationships, individuals, and countries. Section III studies links with demographics and political views. Sections IV–V report the results on social capital and origins. Section VI concludes.

I. Data: The *Global Universalism Survey*

A. *Sampling and Procedures*

We sketch the survey procedures here; Supplemental 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 as Israel, Japan, and South Korea; and 39 low-/middle-income countries. Supplemental Appendix B.3.2 clarifies the assignment of countries to these three groups. In total, our data represent countries that account for 85 percent of the world population and 90 percent 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 Supplemental 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

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[1] Methodologically, 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, Enke, and Falk 2020; Sunde et al. 2022; Almås et al. 2022; Bursztyn et al. 2023).

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.

B. *Survey Questions*

Our survey questions closely follow the hypothetical disinterested dictator games that were deployed in Enke, Rodríguez-Padilla, and Zimmermann (2022, 2023). 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 (in what follows we equate "goodness" with "deservingness"). The within-survey randomization was designed such that 50 percent of respondents were randomized into the *Baseline* treatment and 50 percent into the *Moral* treatment.

Pretesting 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, Balliet, Wu, and De Dreu (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, Rodríguez-Padilla, and Zimmermann (2022) experimentally validate the survey questions in the United States 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, Rodríguez-Padilla, and Zimmermann (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 United States 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 takeaway 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.

C. *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, Huffman, and Meier 2006; Lane 2016; Charness and Chen 2020; Shayo 2020). As reviewed in Enke (2024), the traditional focus of this literature has

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

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:

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

(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 $u'(x_G^*)/u'(x_S^*) = \alpha_{i,S}/\alpha_{i,G}$. 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

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

(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 universalism. 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 toward certain in-groups. Rawls (1993) prominently argued that cooperation under a fair basic structure in society creates moral obligations toward compatriots but not toward foreigners, but people may also feel that they have particular moral obligations toward their family, friends, neighbors, 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 less 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, in Section IV 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.

D. *Summary Measures*

We compute three preregistered 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 trade-offs with in-group members. *Foreign Universalism* corresponds to the fraction of money shared with a global stranger in a trade-off 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).

[3]We 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.

[4]Gallup surveyed a total of 66,233 respondents. However, as discussed in Supplemental Appendix B, for 11 percent 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 percent 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.

E. *Recoding of Erroneous Responses*

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

In our data, 20,338 out of 184,950 allocation decisions (11 percent) 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 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 Supplemental Appendix B.6, 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 percent 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 percent 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 trade-off 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 percent to the more socially distant stranger in *all* questions and (ii) strictly more than 50 percent to the socially more distant stranger in at least half of all decisions (which in practice usually means at least 2 out of 3).[5] The modal respondent who gets recoded allocates 100 percent of the endowment to the socially more distant recipient in all decisions. This procedure affects 4,328 respondents (6.8 percent) and 10,318 allocation decisions (5.6 percent). 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 Supplemental Appendix B.7.2, that is symmetric around 50 and delivers almost identical results.[6]

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[5] In 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.

[6] In this alternative procedure, (i) we drop all of those respondents who satisfy the criteria above, all of whom exhibit universalism $u > 50\%$, and (ii) for each respondent 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

For transparency, we make available both the raw data and the recoded data. Supplemental Appendix B.8 replicates all analyses in this paper using the raw data, with very similar results. The main exception is the democracy exposure analysis in Section VB, where large outliers render the OLS estimates insignificant with the uncorrected coding.

F. *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 toward 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 scatterplot 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's 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 by 6 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.

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element implicit in this procedure, we bootstrap it; see Supplemental Appendix B.7.2 for details. We also make the code for this bootstrapping procedure available. For reasons outlined in the Supplemental Appendix, we view the recoding procedure as superior and recommend that future users of the *GUS* data work with this version of the data.
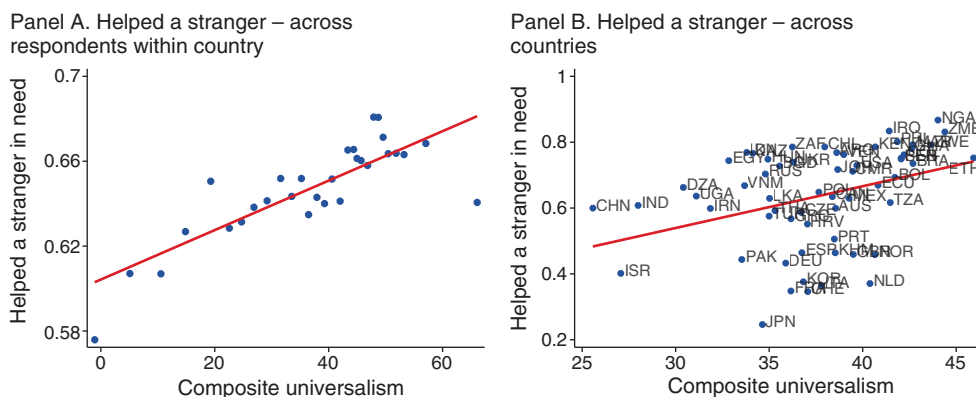
FIGURE 1. COMPOSITE UNIVERSALISM AND HELPING A STRANGER

Notes: The left panel shows a respondent-level binned scatterplot 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 fixed effects (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.

G. *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*.

H. *Pre-analysis Plan*

We preregistered almost all of the analyses in this paper in the AEA RCT registry at https://www.socialscienceregistry.org/trials/7525. The preregistration 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 preregistered are the exposure to democracy analysis in Section VB and the analysis of the radius of trust. The preregistration was uploaded after Gallup collected the data but before we had access to it.

II. 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 trade-off 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* 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

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[7] Supplemental Appendix 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 percent of all decisions reflect equal splits, and 15 percent full favoritism.
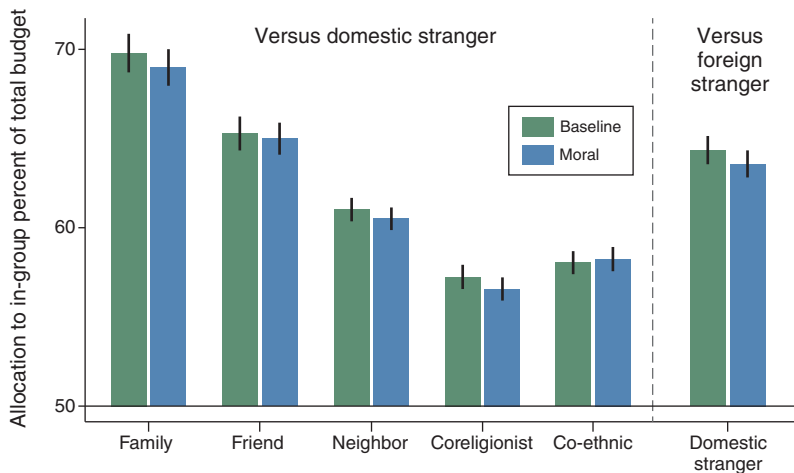
FIGURE 2. MEAN MONEY ALLOCATIONS TO THE IN-GROUP BY TREATMENT

Notes: Each bar indicates how much of the budget was given to the in-group. Whiskers show 95 percent confidence intervals, computed based on clustering at the sampling unit level (530 clusters).

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 percent 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 decision. Overall, average allocations to the in-group are 0.6 percentage points higher in *Baseline* than in *Moral*, from a baseline of 63.4 percent. This difference is small but statistically significant; see Supplemental 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

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[8] Given 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. Supplemental 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 toward family, and 42 countries exhibit their second-highest degree of favoritism toward friends.
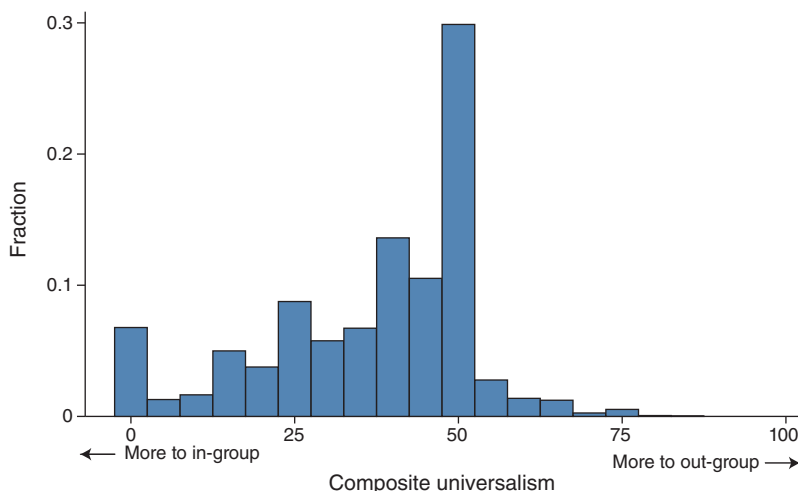
FIGURE 3. DISTRIBUTION OF COMPOSITE UNIVERSALISM ACROSS INDIVIDUALS,
POOLED ACROSS TREATMENTS (OBSERVATIONS = 63,788)

Notes: Zero 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.

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 percent of respondents make universalistic decisions by splitting equally between in-group and stranger, while 6 percent always give everything to the in-group. Sixty percent of respondents allocate strictly more but not everything to the in-group; the remaining 7 percent 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 $r = 0.55$ (Supplemental 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 Supplemental
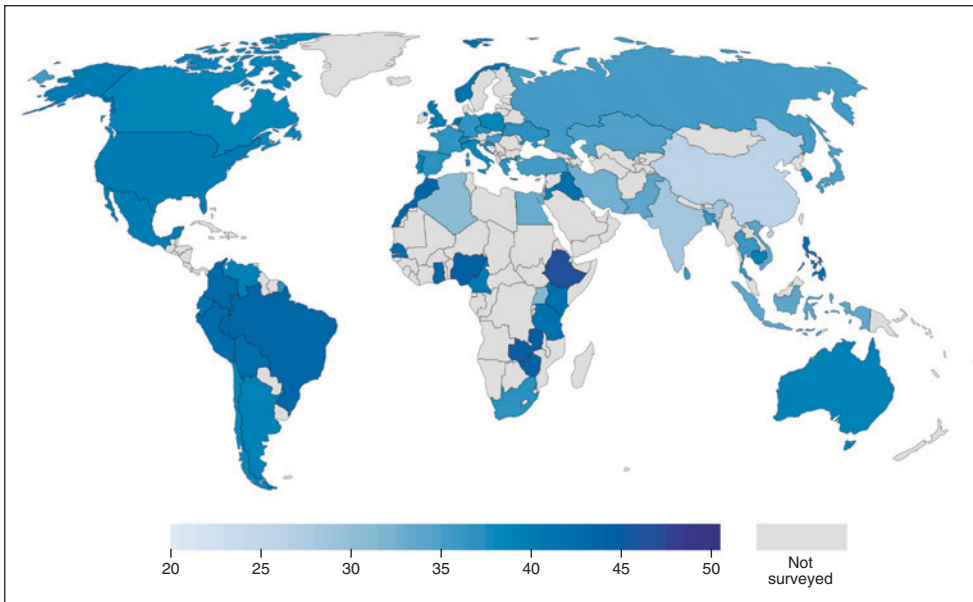
FIGURE 4. GLOBAL VARIATION IN COMPOSITE UNIVERSALISM

Notes: The map shows the country-level average of composite universalism, pooled across treatments. Zero 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.

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 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 trade-offs between domestic in-groups, whereas Western Europe is particularly universalist in domestic-foreign trade-offs. We see slightly more variation in foreign universalism (cross-country mean 37.5 and SD 5.3) than in domestic universalism (cross-country mean 38.1 and SD 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 Supplemental 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
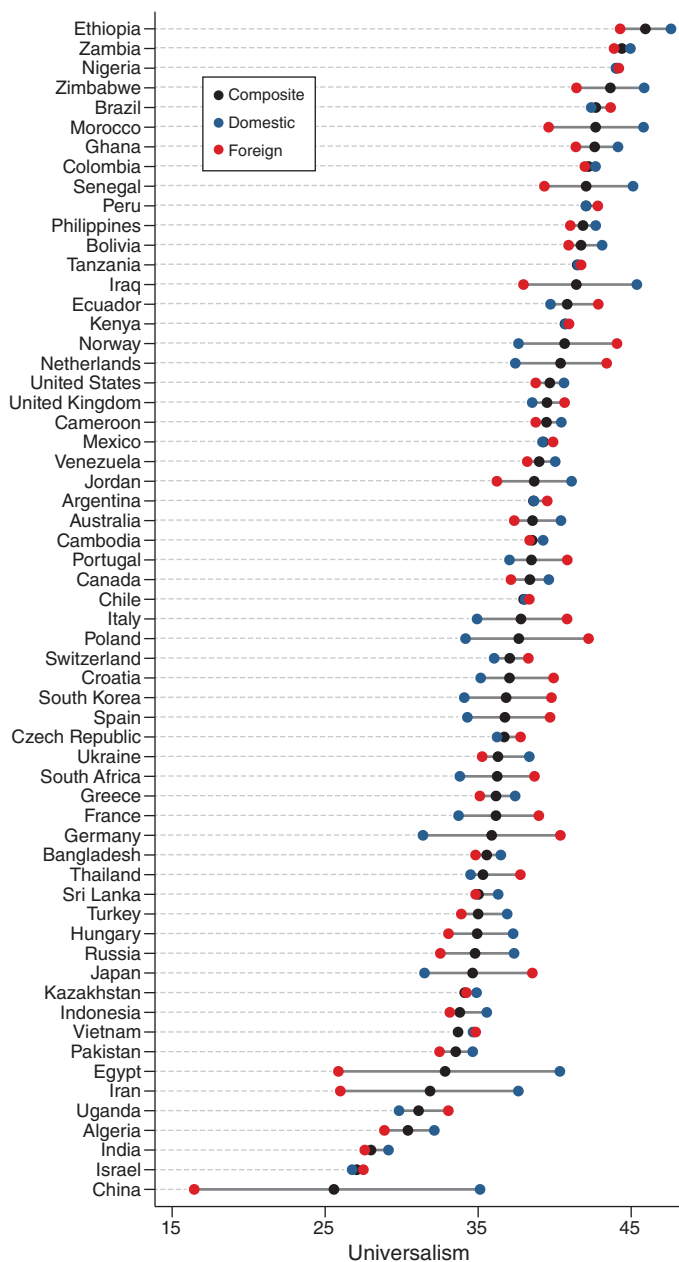
FIGURE 5. AVERAGE COMPOSITE, DOMESTIC, AND FOREIGN UNIVERSALISM BY COUNTRY

Notes: Zero 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 Supplemental Appendix B.6).

$(r = -0.43)$ rather than by foreign $(r = -0.01)$ universalism; see Supplemental Appendix Figure C.9. The negative cross-country 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, Heine, and

Norenzayan 2010). However, this cross-country result is consistent with the negative individual-level correlation between universalism and income to be documented in Section III.

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 percent. 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.

III. Individual-Level Correlates and Political Views

A. *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 preregistered 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, Rodríguez-Padilla, and Zimmermann 2022, 2023).

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 (Supplemental 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 subsamples 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 percent.

The second panel documents that men are less universalist than women, on average, by 2.1 percent of the budget. This gender difference is similar across rich WEIRD, rich non-WEIRD, and poorer countries.
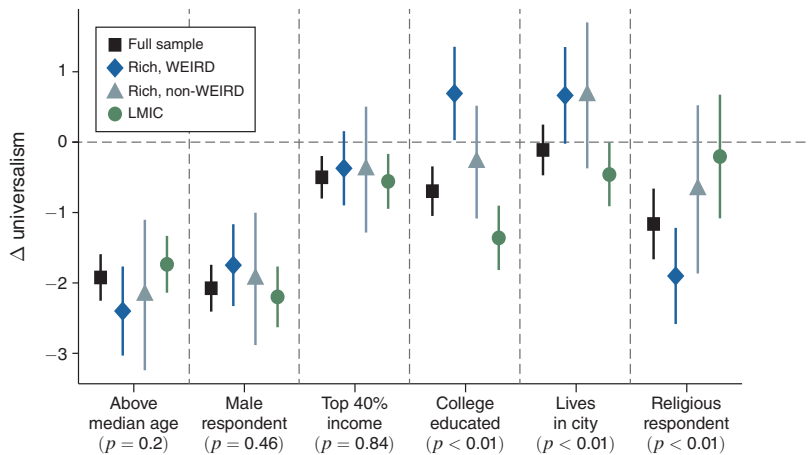
FIGURE 6. UNIVERSALISM AND DEMOGRAPHICS

Notes: 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 subsample 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 percent confidence intervals, computed based on robust standard errors, clustered at the sampling unit level (530 clusters). *p*-values refer to tests of the null hypothesis that the correlations are identical in the three groups of countries. LMIC = low- and middle-income countries. WEIRD = rich Western countries. The estimates are virtually identical without treatment fixed effects.

The third through fifth panels show the results for more endogenous demographics: whether the respondent falls into the top two out of five within-country 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 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 percent 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 preregistered predictions. In line with a large body of work on the cultural specificity of psychological findings (Henrich, Heine, and Norenzayan 2010), this highlights that researcher expectations and intuitions need to be disciplined by representative data from various cultures. For example, based on correlations between universalism and education, researchers commonly express the intuition that education causes universalism and therefore produces certain political views (e.g., Gethin, Martínez-Toledano, and Piketty 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."

B. *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 IG. 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, Rodríguez-Padilla, and Zimmermann 2023), 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 versus domestic poor, (iii) support for protecting the global versus 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 versus them" thinking and should therefore be positively linked to universalism.

Figure 7 summarizes the results by providing binned scatterplots 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 preregistration 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 versus domestic poor, focusing on protecting the global versus local
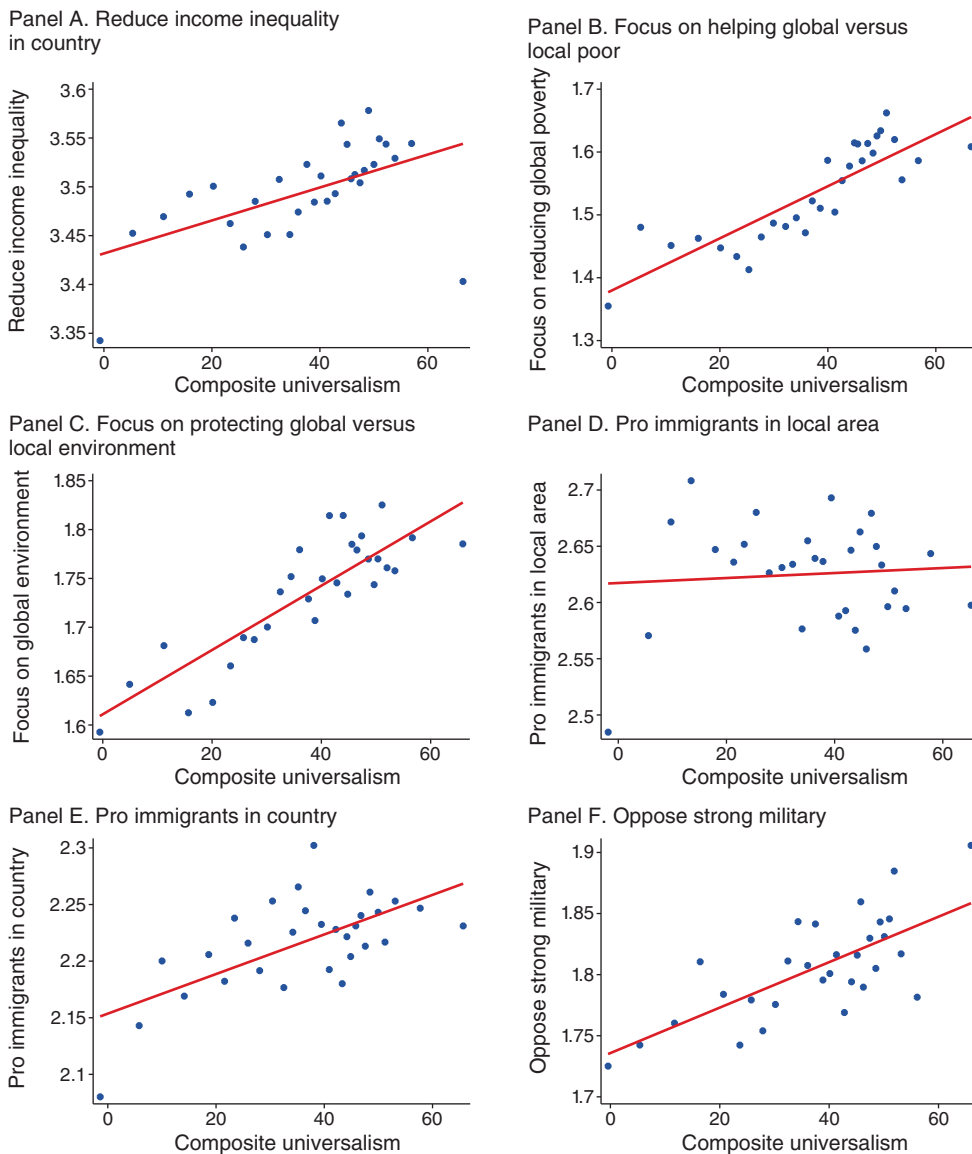
FIGURE 7. COMPOSITE UNIVERSALISM AND POLITICAL VIEWS AT THE INDIVIDUAL LEVEL

Notes: The figures show binned scatterplots 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 fixed effects. Political views are coded as 1–4, based on responses of "Strongly disagree," "Disagree," "Agree" and "Strongly agree." See Section IG for the wording of the political questions. The sample size varies between *Observations* $=$ 19,628 and *Observations* $=$ 22,219 across panels.

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]

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[11] The 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

TABLE 1—UNIVERSALISM AND POLITICAL VIEWS AT THE INDIVIDUAL LEVEL

| | Dependent variable: Support for... | | | | | |
| | | Prioritize global versus domestic | | Pro immigrants | | |
	Reduce inequality (1)	Poor (2)	Environment (3)	In area (4)	In country (5)	Weak military (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 fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Treatment fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.066	0.155	0.163	0.149	0.205	0.257
Observations	18,528	18,676	18,478	21,248	20,951	18,430

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 IG 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 preregistration 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. Supplemental Appendix Table D.4 presents estimates controlling for religiosity (not included in the main analysis because it wasn't elicited in five countries).

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

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.

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 trade-offs 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 0 to 50 is associated with an increase in support for the left-wing policies of between 0.06 and 0.17 points on a 4-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 0 to 50 is often as large as the effect associated with a college degree.

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 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 versus 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 in the importance of universalism across countries.

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[12] Enke, Rodríguez-Padilla, and Zimmermann (2023) study the link between universalism and policy views in a smaller, seven-country 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).

[13] There 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
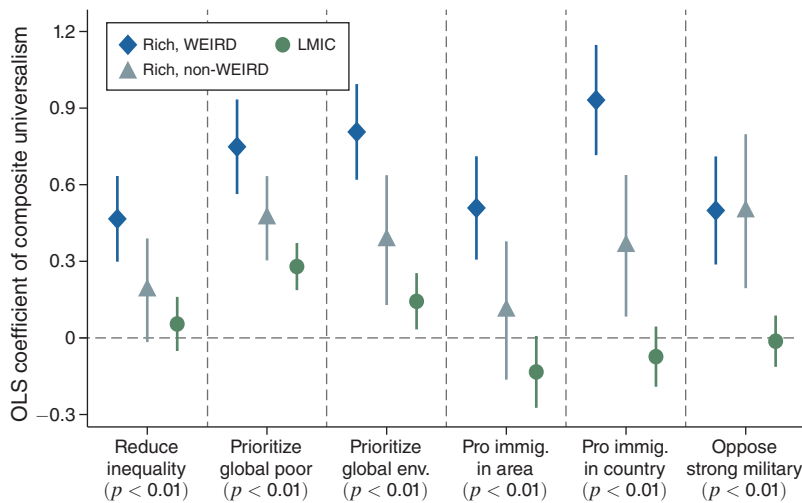
FIGURE 8. INDIVIDUAL-LEVEL COMPOSITE UNIVERSALISM AND POLITICAL VIEWS IN DIFFERENT SUBSAMPLES

Notes: 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 subsample 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 percent confidence intervals, computed based on robust standard errors, clustered at the sampling unit level (530 clusters). *p*-values refer to tests of the null hypothesis that the correlations are identical in the three groups of countries. LMIC = low- and middle-income countries. WEIRD = rich Western countries. The estimates used in creating this figure are displayed in Supplemental Appendix Tables D.5–D.10.

IV. 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, Leonardi, and Nanetti 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, Newton, and Welzel 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 rather than 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

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pieces of evidence that speak against such an account. First, as discussed in Section III, 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 IF, the link between universalism and having helped a stranger is very similar across the different groups of countries. Third, as discussed in Supplemental Appendix B.6, various other indicators of data quality are very similar across countries with different income levels.

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 (Haerpfer et al. 2022) 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, Newton, and Welzel (2011), the literature has converged on a standard summary statistic to aggregate these questions into an index of in-group versus 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 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.

V. Origins of Variation in Universalism

A. *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 2024 for a review). A prominent idea in the literature is that people's degree of universalism is economically functional, meaning 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 researchers 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
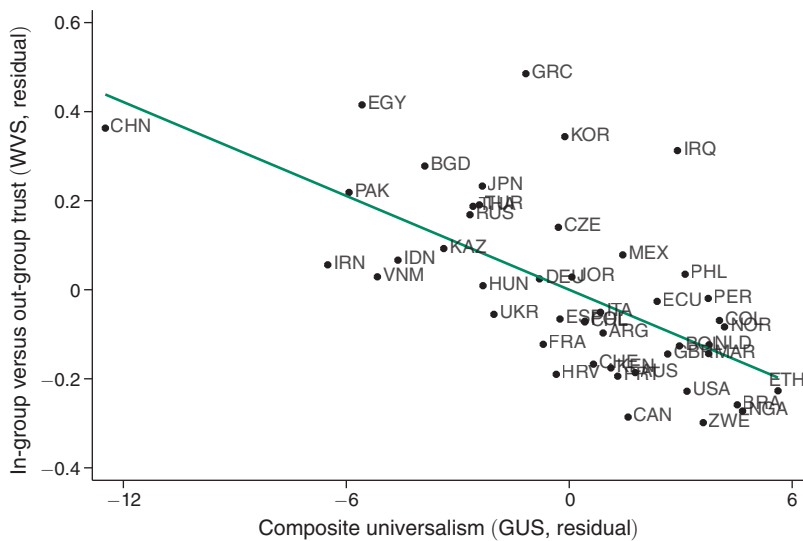
FIGURE 9. COUNTRY-LEVEL PARTIAL CORRELATION PLOT OF THE DIFFERENCE BETWEEN IN-GROUP AND OUT-GROUP TRUST AGAINST COMPOSITE UNIVERSALISM, CONTROLLING FOR LOG PER CAPITA INCOME

Notes: 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.

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 rain-fed 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, rain-fed 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; see Schulz 2022), the share of
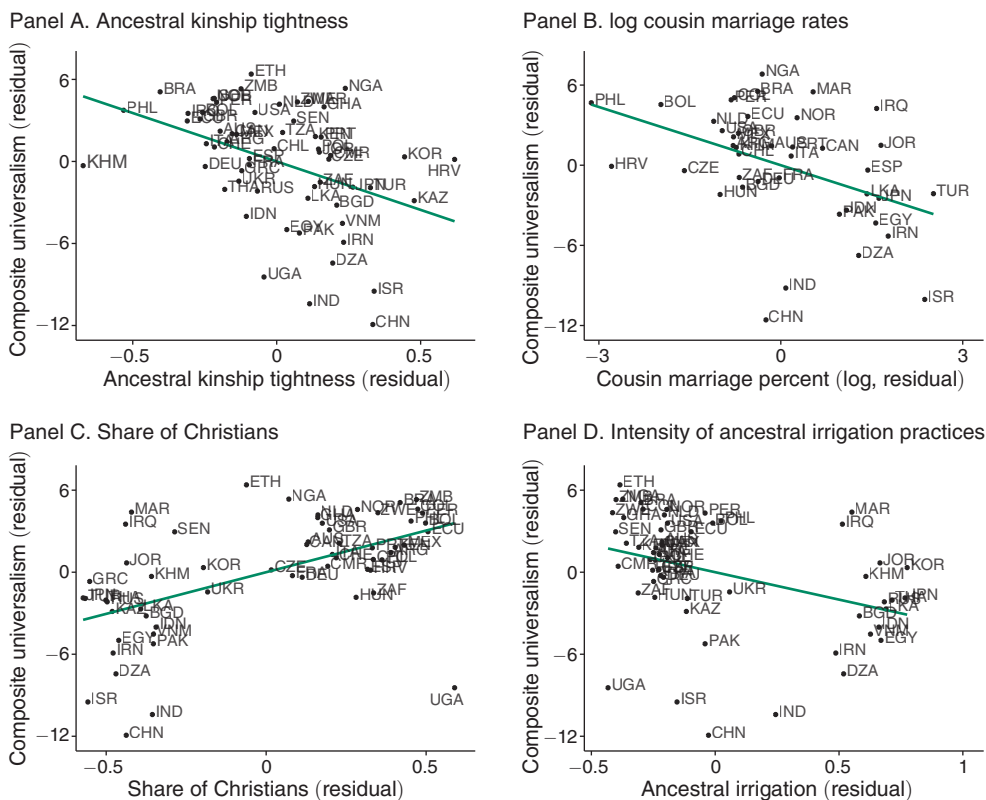
FIGURE 10

Notes: 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.

Christians in society (from Barro and McCleary 2003), and the intensity of ancestral irrigation practices (taken from Buggle 2020). Figure 10 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 percent level. The raw correlations are all statistically significant at the 5 percent 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]

B. *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 toward 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 (Marshall and Gurr 2020) 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.

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 (Marshall and Gurr 2020), which is a summary index ranging from 0 to 10 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 percent 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 percent, 80 percent, or 90 percent.

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[14] In 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 Supplemental 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

Notes: 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.

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 very similar 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 Supplemental Appendix Figure C.13.

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

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[15] Formally, the estimating equation is given by

$$(3) \qquad univ_{i,a,c} = \alpha + \beta d_{a,c} + \sum_c \gamma_c \mathbf{1}\{c\} + \sum_a \gamma_a \mathbf{1}\{a\} + \epsilon_{i,a,c}.$$

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.
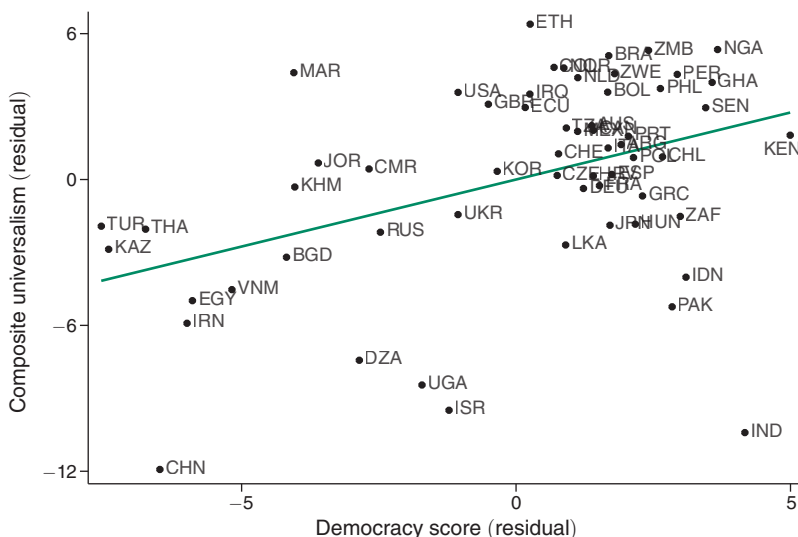
Table 2—Exposure to Democracy and Universalism: Variation across Country-Age-Cohorts

| | Dependent variable: *Universalism* | | | | | | |
| | Composite | | | Domestic | | Foreign | |
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Lifetime average democracy score	0.38	0.38	0.34	0.42	0.41	0.30	0.21
	(0.10)	(0.10)	(0.10)	(0.11)	(0.11)	(0.13)	(0.13)
Lifetime average log GDP p/c		1.46	1.11	−0.07	−0.38	2.56	2.07
		(0.90)	(0.94)	(0.93)	(0.97)	(1.21)	(1.26)
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Treatment fixed effects	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	55,323	55,323	53,826	54,867	53,391	53,765	52,332

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.

with more democratic institutions is associated with higher universalism. The standardized beta in this regression (not reported in the regression table) is 8 percent, suggesting that a 1 standard deviation increase in experience with democracy is associated with an increase in 8 percent 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 percent larger for domestic universalism (though the difference between the regression coefficients is not statistically significant).

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 nonmigrants 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 versus 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

TABLE 3—EXPOSURE TO DEMOCRACY AND UNIVERSALISM: VARIATION ACROSS FIRST-GENERATION MIGRANTS

	Dependent variable: *Universalism*							
	Composite				*Domestic*		*Foreign*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Democracy score in country of origin	0.20 (0.10)	0.28 (0.13)	0.26 (0.14)	0.27 (0.15)	0.45 (0.14)	0.46 (0.15)	0.10 (0.18)	0.06 (0.20)
log GDP p/c *in country of origin*		−0.20 (0.54)	−0.30 (0.55)	−0.69 (0.73)	−0.40 (0.51)	−1.45 (0.71)	−0.38 (0.73)	−0.33 (1.00)
Destination country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Treatment fixed effects	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	Yes	No	Yes
Adjusted R^2	0.07	0.07	0.07	0.07	0.05	0.06	0.06	0.06
Observations	2,741	2,451	2,412	2,391	2,424	2,367	2,398	2,342

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*.

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 toward compatriots but not toward foreigners.

VI. 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

predictive 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 coevolve.

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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