Moral Universalism: Measurement and Economic Relevance^{*}

Benjamin Enke Ricardo Rodríguez-Padilla

Florian Zimmermann

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Abstract

Many applied economic settings involve tradeoffs between in-group members and strangers. To better understand decision-making in these contexts, this paper measures and investigates the economic relevance of heterogeneity in moral universalism: the extent to which people exhibit the same level of altruism and trust towards strangers as towards in-group members. We first introduce a new experimentally-validated survey-based measure of moral universalism that is simple and easily scalable. We then deploy this tool in a large, representative sample of the U.S. population to study heterogeneity and economic relevance. We find that universalism is a relatively stable trait at the individual level. In exploratory analyses, heterogeneity in universalism is significantly related to observables: older people, men, the rich, the rural, and the religious exhibit less universalist preferences and beliefs. Linking variation in universalism to self-reports of economic and social behaviors, we document the following correlations. Universalists donate less money locally but more globally, and are less likely to exhibit home bias in equity and educational investments. In terms of social networks, universalists have fewer friends, spend less time with them, and feel more lonely. These results provide a blueprint for measuring moral universalism in applied settings, and suggest that variation in universalism is relevant for understanding a myriad of economic behaviors.

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1 Introduction

Many economic, managerial and policy decisions involve tradeoffs between those that are socially close to us and distant strangers. When we invest into equity, do we equally trust foreign and domestic managers to refrain from rent-seeking? When designing policy, how do policymakers evaluate and internalize the differential effects of a given policy for different societal groups? Within firms, how does group membership impact hiring and promotion decisions, and the allocation of tasks and rewards? These and many related questions concern *moral universalism*: the extent to which people exhibit the same level of altruism and trust towards strangers as towards in-group members. Universalism is not about being more or less altruistic or trusting, but rather about a more uniform allocation of a given altruism or trust "budget." Here, full universalism corresponds to the extreme case that one's neighbor, friend or cousin are treated in the same way (and trusted by the same amount) as a random stranger.

Both psychologists and economists have long been aware that people are often not fully universalist but expend more altruism towards, and are more likely to trust, members of their own social groups. In lab and lab-in-the-field experiments, participants often allocate more money to in-group members, both when group membership is artificially induced using minimal group paradigms (Tajfel and Turner, 1986; Chen and Li, 2009; Kranton et al., 2020) and when it derives from real social identities (Goette et al., 2006; Bernhard et al., 2006; Sutter and Kocher, 2004; Lane, 2016; Bauer et al., 2020).¹

While this rich body of experimental work has been very influential, it has largely focused on documenting the existence of in-group favoritism in lab or lab-in-the-field settings. As a result of this focus, a number of open questions have potentially hampered a more widespread investigation of the role of moral universalism for applied economic decision contexts. To preview, these open questions concern (i) the appropriate measurement of universalism in large heterogeneous samples under time and cost constraints; (ii) the nature of individual heterogeneity in universalism; and (iii) the ecological relevance of universalism for understanding economic behavior. First, because existing contributions to the literature almost exclusively focus on one or two specific social groups, it is ex ante unclear how universalism should be conceptualized and measured in applied work. While some theoretical work has entertained the possibility that individuals possess a domain-general trait of moral universalism (e.g., Tabellini, 2008), an alternative reading of the existing evidence is that different people are just parochial with respect to different types of in-groups. The empirical distinction between these competing interpretations is not only important from a theoretical perspective, but becomes all the more

¹Of course, exceptions exist. For example, Fershtman and Gneezy (2001) and Berge et al. (2018) do not find strong in-group favoritism along ethnic lines.

relevant for practitioners and applied researchers when they would like to measure universalism under tight time and cost constraints. A second open question concerns the nature of individual heterogeneity in universalism. This is problematic because our understanding of universalism will ultimately depend on gathering data from large and heterogeneous organizations and populations. Finally, a third open question concerns the potential ecological relevance of individual-level heterogeneity in universalism. For example, the economics literatures on heterogeneity in risk aversion, patience, and altruism partly gained prominence because controlled measures of these preferences were shown to be predictably related to important economic behaviors outside of the lab. A comparable body of work on the ecological relevance of universalism is missing.

Addressing these open questions amounts to building a bridge between the influential experimental work on in-group favoritism and empirically scalable analyses of applied economic settings. To build such a bridge, in this paper we (i) develop a simple, efficient and easily scalable survey-based measure of universalism in both altruism and trust with respect to a wide range of potential in-groups; (ii) experimentally validate this tool by showing that tightly structured survey questions contain as much informational content about an individual's universalism as financially incentivized choices; (iii) show that an individual's degree of universalism is a relatively domain-general trait that is fairly insensitive to the choice of specific in-groups or the decision-making domain (altruism or trust beliefs); (iv) document through exploratory analyses that, in a representative sample of the U.S. population, individuals exhibit large heterogeneity in universalism, which correlates with a range of sociodemographic variables, most notably religiosity and age; and (v) show that heterogeneity in universalism is systematically associated with a pre-registered set of self-reported economic and social behaviors and outcomes, including donations, investment behavior, and the structure of people's social networks.

The defining characteristic of universalists is that they expend a given altruism (or trust) budget more uniformly across people that are close or more socially distant from them. Our empirical measure of universalism in altruism directly builds on this reduced-form definition. In a series of money allocation games in a spectator design, survey participants split the hypothetical sum of \$100 between two individuals: a member of an in-group and a randomly-selected stranger. This spectator design has the attractive feature that it does not rely on the detour of self-other tradeoffs and hence holds the overall level of a respondent's altruism constant by construction.

Ideally, we would like to capture universalism for the universe of potential in-groups, yet this is infeasible in practice. To avoid baking our own biases into the choice of ingroups, we select a broad range of groups based on an ex-ante crowdsourcing exercise. We then vary the specific identity of the recipients in the bystander money allocation games to construct measures of domestic, foreign, and global universalism in altruism. First, domestic universalism measures tradeoffs between a domestic in-group member and a random domestic person. Here, the list of in-groups includes the respondent's extended family, neighbors, friends of family, colleagues, organization (e.g., club) and people who share the respondent's hobbies, religious beliefs, age, political views, or race. For instance, in one question, a participant is asked to split hypothetical \$100 between a member of their extended family and a randomly-selected person from the United States. Second, foreign universalism is measured by asking respondents to split \$100 between a random person who lives in the U.S. and a random world citizen. Third, global universalism is derived from allocation games in which the potential recipients are either random world citizens or global in-group members, such as someone who speaks the respondent's language or shares their religious beliefs. In total, respondents complete a set of 16 money allocation games. We estimate an individual's degree of universalism in trust using similar techniques. To this end, respondents complete the same 16 allocation games as described above, yet split 100 trust points (rather than \$100) to indicate which of two individuals they trust more.

In principle, it is conceivable that individuals who are very universalist in one domain– such as exclusively domestic tradeoffs–exhibit more in-group favoritism once it comes to another domain–such as domestic-foreign tradeoffs. Instead, we find that the domestic, foreign, and global universalism components are highly correlated, both for trust and altruism. We therefore combine them into two summary statistics, one for universalism in altruism and one for universalism in trust.

A potential problem in using survey questions to estimate preference parameters and beliefs is that survey responses are not strictly incentive compatible. As proposed by Falk et al. (2015, 2018), a solution to this problem that maintains the advantage of low-cost and scalable survey questions is an experimental validation procedure, in which the responses to survey questions are shown to be highly correlated with financially incentivized experimental decisions. We validate the hypothetical money allocation tasks by correlating participants' decisions with their choices in identical, yet financially incentivized, tasks over a one-week horizon. After accounting for measurement error, the correlation between hypothetical and incentivized choices is in the range of $\rho \approx 0.50$, which is as high as a financially incentivized test-retest correlation benchmark.

To shed light on the internal structure of universalism and its relevance for economic behaviors and outcomes, we deploy our instruments in a large-scale, pre-registered and nationally-representative internet survey of the U.S. population ($N \approx 6,600$). Our analysis begins by decomposing the variation in behavior in the 32 money and trust point allocation games that measure universalism. From an ex-ante perspective, variation in this data could plausibly be generated by heterogeneity along one of two orthogonal dimensions: (i) across-respondent heterogeneity in the average level of universalism (where

each individual applies their level of universalism to all in-groups) and (ii) across-ingroup heterogeneity, such that all individuals are parochial towards some in-group but universalist with respect to another in-group. We find that 31% of the variation in the data is due to respondent fixed effects; in contrast, only 8% of the variation in the data are due to fixed effects for specific in-groups. These patterns imply that individual-level heterogeneity in universalism is much more important in the data than what exactly the identity of the in-group is: to a first approximation, people who are parochial when their extended family is involved, are also parochial when co-ethnics are involved. Individuallevel heterogeneity in universalism is substantial: while a large majority of participants favor their in-groups to varying degrees, others essentially never discriminate based on group membership.

Our findings suggest that universalism is a reasonably general trait that is partly invariant to the specific domain (beliefs or preferences) and choice of in-group. Indeed, our summary measures of universalism in altruism and trust are highly correlated with each other ($\rho = 0.82$ after correcting for measurement error). This is even though there is nothing mechanical that prescribes that an individual who is universalist in altruism space also trusts socially distant strangers a great deal. The fact that we do find such a strong relationship again suggests that these different dimensions reflect the same underlying psychology. Further analyses show that participants' consistency across tasks is not mechanically driven by "laziness," but indeed reflects deliberate decisions that vary in meaningful and internally consistent ways across social groups.

Given that individual-level heterogeneity is the main source of variation in our data, we present the first nationally representative analysis of the sociodemographic correlates of universalism. In exploratory analyses, we find that older people, men, whites, the rich, people with lower cognitive skills, the rural, and the religious exhibit less universalist preferences and beliefs, on average. These results illustrate the large variation in universalism in diverse samples, and shed light on the different moral priorities of different segments of the population, in particular substantial moral divides along generational, income and religious lines.

Ultimately, we are interested in variation in universalism because it may help explain heterogeneity in economically-relevant behaviors and outcomes. This is largely uncharted territory in the literature. To argue our case that universalism is of potentially broad relevance, we look at a set of pre-registered self-reported outcomes and behaviors, all of which are intuitively linked to tradeoffs between in-groups and strangers: donation decisions, home bias in equity and educational investments, and the structure of people's social networks. First, we document that more universalist people donate less money locally (to local community organizations or churches); at the same time, universalists donate more money to nationwide and global charities. This result further highlights that universalism is fundamentally different from the *level* of altruism and trust: while universalism is only weakly (if at all) negatively correlated with overall donation amounts, it is strongly related *towards whom* people give.

Second, we elicit measures of home bias in equity and educational investments. It is well-known in the finance literature that people tend to exhibit so-called equity home bias, whereby they invest considerably less money into foreign companies relative to domestic ones than seems warranted given actual risk-return profiles and transaction cost differences. We measure equity home bias by eliciting the value of national and international stocks participants own. In addition, we asked participants how they would invest a hypothetical budget between a manufacturer in the U.S. and one outside the U.S. For both measures, we find that universalists exhibit less home bias. For educational investments, it is conceivable that non-universalists are less open to move away for educational purposes, because they might value and trust their local community more. We hence ask respondents whether they would advise their child to attend a local college, or a college that is geographically distant but ranked slightly higher. We find that universalists are more likely to advise their children to study at a faraway college.

Finally, we elicit a range of pre-registered measures related to social ties. The rationale for doing so is that universalists – by virtue of expending their altruism and trust budget more uniformly – might invest less into forging close relationships. Indeed, moral philosophers in the communitarian tradition have long argued that universalism produces atomized and socially isolated individuals (Sandel, 2005; Etzioni, 1994). However, we are not aware of prior empirical work that has considered the link between universalism and social networks. We measure how many friends and acquaintances our respondents have, how much time they spend with them, how often participants give and receive help within their community, and whether they feel that they live a sociallyrewarding or lonely life. In line with our hypothesis, universalists have fewer friends and acquaintances, spend less time with their social contacts, and report being more lonely. While these analyses are all correlational and built off of respondents' self-reports, they point to a potentially important role of moral universalism across different domains of economic and social decision-making.

Given these encouraging results on the potential relevance of universalism for economic settings, we conclude the paper by paving a way forward for applied researchers who are interested in measuring universalism in their own work. To this effect, we first discuss the interpretation of our survey-based measure in detail. Here, we also relate universalism to existing concepts in the literature, such as taste-based discrimination and inequality aversion. Second, we propose short measurement modules that capture a very large fraction of the true variation in universalism, but can be deployed under tight time constraints.

2 Conceptualizing and Measuring Universalism

2.1 Universalism

A key hypothesis behind this paper is that people have a reasonably stable trait of moral universalism: the extent to which people exhibit the same level of altruism (preferences) and trust (beliefs) towards everyone, holding fixed the overall level of altruism and trust. An alternative – and ex-ante plausible – way of thinking of in-group favoritism is that different individuals are parochial with respect to different in-groups: some towards their neighbors, others towards co-ethnics, etc. Figure 1 provides a stylized illustration of how we think about universalism in altruism, adapting the idea behind the models in Tabellini (2008) and Enke (2019). In the figure, we depict a decision-maker's level of altruism towards others, as a function of social distance (group membership). Altruism will usually be higher towards those that are socially close, yet the extent to which people favor in-group members potentially varies across individuals. For some, altruism declines relatively quickly as we move from in-group members to strangers, while for others this relationship may be entirely flat. Importantly, in our framework, variation in universalism leaves the overall level of altruism unaffected. This clarifies that universalists are not "more" or "less" moral. Universalism in trust can be conceptualized in an analogous fashion. Here, the y-axis in Figure 1 would show the decision-maker's level of trust in someone else. Thus, a full universalist trusts their friends to the same extent as a random stranger, while less universalist beliefs reflect high trust in in-group members but low trust in out-group members. Under this formulation, the overall level of trust again does not vary as universalism changes.

A key issue is the types of in-groups we consider. Throughout, we define the following types of social identities: (1) domestic in-group members, such as someone in the U.S. who shares your religious beliefs; (2) domestic strangers; (3) global in-group members, such as someone anywhere in the world who shares your religious beliefs; and (4) global strangers.² We will empirically measure (i) domestic universalism as tradeoff between (1) and (2); (ii) foreign universalism as tradeoff between (2) and (4); and (iii) global universalism as tradeoff between (3) and (4).

2.2 Measurement

We designed our measurement instrument with several goals in mind. We wanted to (i) capture moral universalism in both trust and altruism, (ii) cover a broad set of social groups, (iii) measure domestic, foreign and global components of universalism, and

²As illustrated in Figure 1, we only impose the (arguably very weak) assumptions that, in terms of social distance from a decision maker, (1) < (2), (2) < (4) and (3) < (4).



Figure 1: Illustration of heterogeneity in universalism with respect to altruism.

(iv) provide an instrument that can be easily implemented at scale and low cost in surveys and experiments.³

Universalism in altruism. To measure universalism in altruism, we use a "bystander" money allocation game.⁴ In any given task, participants were asked to allocate hypothetical \$100 between two recipients: a member of an in-group and a "randomly-selected person." Subjects could allocate the \$100 in any way they saw fit, but to ensure that the overall level of altruism is held constant, they could not keep money for themselves. Participants were asked to assume (i) that both recipients are equally rich (addressing income effects) and (ii) that neither of these recipients would find out who sent them the money (ruling out reciprocity considerations). Figure 6 in Appendix A.1 provides a screenshot of an example decision screen.

Respondents completed a total of 16 hypothetical money allocation tasks that fall into three categories, based on the discussion in Section 2.1: domestic, foreign, and global universalism. First, to estimate *domestic* universalism, respondents made a total of ten decisions, the order of which was randomized across respondents. In each of them,

³Recent work in political economy and cultural economics has measured universalism using psychological questionnaires that include a broad swath of both utilitarian and deontological moral concepts (Enke, 2020, 2019; Haidt, 2012).

⁴Bystander or spectator designs have been employed in controlled lab studies and surveys before. For recent examples, see Chen and Li (2009); Almås et al. (2019); Cappelen et al. (2019); Cohn et al. (2019).

respondents were asked to split hypothetical \$100 between (i) a randomly-selected person who lives in the U.S. and (ii) a randomly-selected member of one of their social groups, who also resides in the U.S. To tie our hands in the selection of in-groups as much as possible, we based the selection on an ex-ante crowd-sourcing exercise. On Amazon Mechanical Turk (AMT), we asked a set of N = 400 respondents which social groups they believe people most identify with. We then used those social groups that were mentioned most often (see Appendix A.2.1 for details).⁵ The final set of social groups includes extended family, friends of family, neighbors, colleagues at work or school, same organization (e.g., club), same age, same ethnic background or race, same political views, same hobbies, and same religious beliefs.⁶ The average allocation to the randomly-selected person across the ten questions makes up the domestic universalism measure.

Second, to estimate *foreign* universalism, respondents were asked to split hypothetical \$100 between (i) a randomly-selected person from the U.S. and (ii) a randomlyselected person who lives anywhere in the world. Foreign universalism then corresponds to the monetary amount sent to the global stranger.

Third, to estimate *global* universalism, respondents made five decisions, in each of which they were asked to split hypothetical \$100 between (i) a randomly-selected person who lives anywhere in the world and (ii) a randomly-selected person who lives anywhere in the world and is a member of the respondent's social groups. Across the five questions, the social groups included same language, same religious beliefs, same ethnic background, same values, and same occupation. Again, the selection of these five groups is based on the crowd-sourcing exercise described in Appendix A.2.1. The average amount of money sent to the randomly-selected world citizen makes up the global universalism measure.

To reduce the dimensionality of the data, and because these measures are highly correlated, we compute a summary statistic of universalism in altruism, which averages domestic, foreign, and global universalism. See Appendix D.1 for a correlation matrix.

Universalism in trust. The paradigm to measure universalism in trust is identical to the one described for altruism, except that in a given task respondents were asked to allocate 100 "trust points" (rather than hypothetical \$100) between two individuals. This was explained to participants as allocating points to indicate whom they trust more. This measure is meant to capture trust beliefs, i.e., beliefs about the trustworthiness of

⁵We excluded "close family" and "close friends" because pilot data revealed very little variation in respondents' parochialism with respect to these groups.

⁶In Appendix A.2.1, we report on a validation exercise in which we measure universalism with respect to a much larger set of 40 social groups. The measures that are derived from this larger set of questions are highly correlated with the measure we use here.

others. Figure 8 in Appendix A.1 provides a screenshot of an example decision screen. Respondents again completed a total of 16 tasks, based on the same social groups as above. This again yields domestic, foreign, and global universalism components, which we again average into a summary statistic of universalism in trust.

2.3 Ex-Ante Experimental Validation of Survey Questions

Universalism in Altruism. All of the decisions in our survey are hypothetical in nature. This is in line with an emerging large-scale survey literature that relies on unincentivized preference measures (Falk et al., 2018; Sunde et al., 2019). Indeed, recent evidence suggests that in the absence of strong external defaults there are no differences in responses between incentivized and hypothetical survey questions (Baillon et al., 2020). As shown in prior experimental validation work (Falk et al., 2015), a particularly successful strategy in devising survey questions – which we also follow here – is to implement standard structured experimental games, just without real financial incentives.

To add further credence to the validity of our measures, we follow Falk et al. (2015) in conducting an experimental validation exercise. We validate our measure of universalism in altruism by financially-incentivizing our bystander dictator games. On Amazon MTurk, subjects completed two rounds of a condensed version of our survey that included only money allocation tasks, hypothetical or financially-incentivized. The goal was to compare decisions within-subject and across hypothetical and incentivized rounds. To obtain a bechmark value, a subset of subjects conducted the incentivized tasks twice, delivering an incentivized test-retest correlation. We followed Falk et al. (2015) in implementing a one-week time lag between the two survey rounds.⁷

We find that, over a one-week horizon, the correlation between average unincentivized universalism and average incentivized universalism is $\rho = 0.50$ after applying the Obviously Related Instrumental Variables (ORIV) technique of Gillen et al. (2019) to reduce measurement error (N = 273). The benchmark test-retest ORIV correlation in another sample of N = 79 subjects is $\rho = 0.45$, and hence very close to our incentivizedhypothetical correlation. We conclude from this validation exercise that our hypothetical money allocation games are as good a measure of universalism in altruism as financially incentivized experiments. This insight is related to the recent validation study by Bauer et al. (2020) who show that ethnic favoritism is strongly correlated in incentivized and

⁷In order to make financially incentivizing subjects feasible, we had to restrict the set of groups. Money allocations in both rounds were played only with the following set of five groups: (i) same hobby, (ii) same age/ generation, (iii) same race or ethnicity, (iv) same political views, and (v) same religious views. In order to incentivize the allocation tasks, subjects completing the incentivized round were informed that 2.5% of study participants would be selected for payout. We sampled AMT workers with the appropriate characteristics to serve as recipients in the financially-incentivized money allocation games. See Appendix A.2.2 for further details.

hypothetical versions of a dictator game.8

Universalism in Trust. The decision context we use to measure universalism in trust is less precisely defined than the money allocation games. This is because there is no objective analogue to "allocating 100 trust points." We elected to implement this more qualitative question because it is intuitive for participants, and much simpler to implement than structured experimental cheating games. At the same time, ideally one would like to know that trust beliefs as measured in our questions correspond to beliefs about others' actions in more quantitative, structured choice contexts. To this effect, Appendix A.2.3 reports on an additional validation exercise in which we show a strong correlation (ORIV $\rho = 0.75$) between responses to our trust questions and hypothetical beliefs about others' cheating in a structured cheating game that is modeled after the die-in-a-cup task developed by Fischbacher and Föllmi-Heusi (2013).

3 Survey Design and Logistics

In 2019, we implemented a pre-registered survey of U.S. citizens born in 2001 or earlier through *Dynata*, a market research internet panel. The survey consisted of five components: (i) an initial screen that screened respondents in or out of the survey depending on whether their sociodemographic characteristics satisfied our sample quotas; (ii) decision screens to measure universalism and additional social preferences; (iii) a questionnaire; (iv) six survey blocks to elicit our outcome variables of interest; and (v) a Raven matrices IQ test. We took two measures to ensure quality control. First, every respondent who completed the survey in less than 400 seconds was immediately dropped and replaced by *Dynata*. Second, the survey contained two attention check questions. Whenever a respondent answered an attention check incorrectly, they were immediately routed out of the survey and replaced by *Dynata*. The median response time was 18 minutes.

We contracted with Dynata for a nationally representative sample of N = 1,000 respondents. However, because constructing a sample that matches the census on the dimensions of age, gender, educational attainment, income, race, and employment status is logistically challenging, *Dynata* eventually supplied a much larger sample to us (total N = 6,591), a subset of which makes up the more representative sample. The

⁸These correlations result from a well-powered validation exercise, in particular because we designed the study with an eye toward reducing measurement error. First, because we focus on validating a summary statistic of universalism, measurement error is reduced by averaging money allocations across social groups. Second, measurement error is further reduced through the ORIV technique. As a result, given the ORIV correlations we find and the corresponding sample sizes, with 90% power we would be able to detect a minimum correlation of $\rho = 0.34$ between unincentivized and incentivized universalism in altruism. See Appendix A.2.4 for additional power analyses.

physical process was that *Dynata* kept sampling respondents until our pre-specified quotas were satisfied. Since we view throwing away data as scientifically questionable, we report analyses based on the full sample in the main text. In Appendix G, we replicate all analyses using the pre-registered representative sample. The results are always very similar. The sample characteristics for both the full and representative samples are described in Appendix C.1.⁹

4 The Structure of Universalism

4.1 Variance Decomposition

We begin our analysis by laying out the internal structure of universalism. Do people strongly differ in how universalist they are? Are some people more universalist vis-à-vis some in-groups, but less universalist vis-à-vis other in-groups? To address these questions, we decompose the variation in our full set of allocation game decisions, pooled across domains (universalism in altruism and trust) and types of in-groups, for a total of 210,912 decisions by 6,591 respondents.¹⁰

Figure 2 shows that 31% of the variation in the data is due to respondent fixed effects, or fixed "universalism types." On the other hand, only about 8% of the variation is due to in-group fixed effects. This suggests that it matters much more for observed allocation decisions whether the respondent is overall universalist or not, than whether the particular in-group is given by, for example, domestic neighbors or global co-ethnics. In addition, a substantial portion of variation–36%–is explained by the *interaction* between respondent and social group fixed effects.¹¹

⁹The target sample size, specifications of universalism measures employed, and outcome variables were included in a pre-registration on EGAP, see http://egap.org/registration/5810. We pre-specified that we would conduct separate analyses for universalism in altruism and in trust. However, to reduce the dimensionality of our analyses in the main text, we work with a summary statistic of universalism that averages decisions across altruism and trust. Appendices E and F report robustness checks for universalism in altruism and trust separately. The results are always very similar. Moreover, we report two additional correlational analyses that were left out of the pre-registration but included in the sociodemographic questionnaire: race / ethnicity and wealth.

¹⁰See Appendix D.2 for histograms of these decisions.

¹¹We can compare the incremental R-squared for the different types of variation with benchmark values that would obtain under different plausible decision modes. (i) If in each decision in our survey, respondents implemented their idiosyncratic level of universalism plus noise that is uncorrelated with the identity of the social group, then the entire variation would be explained by respondent-specific and residual variation. (ii) If all respondents exhibited the same average level of universalism but differed in towards which specific groups they are parochial, the entire variation would be explained by social group-specific variation (and residual variation if decisions are noisy). (iii) If all respondents exhibited the same level of average universalism, and average parochialism was identical across all social groups, yet different respondents are parochial with respect to different groups, in a consistent way for both altruism and trust tasks, then the entire variation would be explained by respondent-times-social group variation. The results show that a combination of (i) and (iii) explains our data best.

Variance Decomposition



Figure 2: This figure presents the incremental R^2 for a series of stacked regressions of all 32 allocation decisions made by all 6,591 respondents in our survey on various fixed effects. That is, we stack all allocation decisions across both choice domains into a single column, regressing this column sequentially on respondent, social group, and respondent-times-social-group fixed effects. It shows that 31% of variation in the data can be explained by heterogeneity in respondents' average level of universalism, while 8% of the variance in the data can be attributed to heterogeneity in average universalism across social groups. Finally, 36% of the variation in the data can be explained by respondent-times-in-group fixed effects, i.e., by how individual respondents choose allocations specific to the given in-group, beyond what would be predicted by their average universalism level and the specific social group.

A different way to see the high degree of consistency in universalism is to take each respondent's set of 32 money allocation and trust point allocations and construct a full correlation matrix. As illustrated in Figure 10 in Appendix D.1, we find that *every single one* of these correlation coefficients is positive and usually sizable in magnitude. This again suggests that researchers interested in measuring universalism can deploy such tools in fairly domain-general ways that do not strongly depend on the specific in-group.

A final perspective on within-respondent consistency is to look across choice domains and link a respondent's overall universalism in altruism and their overall universalism in trust. Even though there is no mechanical reason for these two universalism measures to be correlated, we find that their ORIV correlation is $\rho = 0.82$ (standard $\rho = 0.56$). This strongly suggests that there appears to be a deeper psychology – which we call moral universalism – that generates both universalism in altruism and universalism in trust. These results are related to Kranton et al. (2020) who show a correlation between in-group favoritism in a political setting and in-group favoritism in an artificial minimalgroup paradigm.

A potential concern is that these strong correlations reflect heuristic decision-making

by respondents, in the sense that they mentally settle on an allocation decision and then mechanically implement this rule, without paying much attention to the particular in-group in a question. However, that 36% of the variation in the data are due to respondent-times-social-group fixed effects indicates otherwise. These interactions identify the *attitudes of a specific respondent towards a specific in-group*. If respondents had decided heuristically without paying attention to the specific in-groups, then – conditional on respondent and in-group fixed effects – the respondent-times-social-group interactions would explain none of the variation in the data. Instead, the large fraction of the variation explained by respondent-times-social-group fixed effects indicates that respondents who are relatively universalist vis-a-vis their extended family in altruism are also relatively universalist vis-a-vis their extended family in universalism reflects deliberate and internally consistent decisions, rather than heuristics.

4.2 Across-Group Heterogeneity

Having established the relative importance of across-respondent and across-social-group heterogeneity, in Figure 3 we visualize the variation across these two dimensions. Panel A shows average allocation decisions, separately for each in-group. While there is large heterogeneity in universalism across different in-groups, perhaps the most striking feature of the figure is the close correspondence between allocation decisions in altruism and trust games. On average, people allocate about 62 dollars (or trust points) to their respective in-group members.

4.3 Across-Respondent Heterogeneity

To visualize across-respondent heterogeneity, we compute an overall composite measure of universalism, which is given by the average of the summary statistic of universalism in altruism and the summary statistic of universalism in trust. Panel B of Figure 3 shows a histogram of this composite measure, which has a simple interpretation. Zero means that the respondent allocated all money and all trust points to the respective in-group member in each of the 32 decisions. 50, on the other hand, means that the respondent split the money and the trust points equally, on average. Thus, values between zero and 50 correspond to intermediate, parochial decisions, while values above 50 reflect respondents who allocated more money or trust points to the strangers than the respective in-group members, on average.¹² We see that a large majority of respondents is partially parochial (very few data points are to the right of 50), yet there appears to be

¹²Figure 12 in Appendix E reproduces this distribution for universalism in altruism and trust separately.

large heterogeneity in universalism across respondents.

5 Sociodemographic Correlates of Universalism

Having identified individual-level heterogeneity as main source of variation in our data, we now turn to an exploratory analysis examining sociodemographic correlates. We are not aware of prior contributions that have attempted to understand the distribution of universalism in the population. Such insights may be important, however, to understand the main lines of division in the population when it comes to moral priorities.

Figure 4 displays OLS estimates from regressions of our composite universalism measure on a set of covariates. See Appendix C.2 for detailed descriptions of the construction of each of these variables, and Appendix E.2 for tabular results. Here, among others, we consider (i) a measure of cognitive skills that corresponds to the score on a five-item Raven matrices IQ test; (ii) an income and wealth index that aggregates measures of income and net worth; (iii) an urbanicity index that aggregates information on local population density and the respondent's self-reported neighborhood size; and (iv) a religiosity index that aggregates self-described religiosity, frequency of church attendance, and an indicator for no religious affiliation.

We find that older people, men, whites, people with lower cognitive skills, the rich, the rural, and the religious exhibit less universalist preferences and beliefs. The strongest correlations are found with age ($\rho = -0.21$) and religiosity ($\rho = -0.20$). In terms of quantitative magnitudes, the results suggest that an additional ten years in age is associated with an additional 1.1 dollars or trust points allocated to a member of one's in-groups relative to a random stranger. Similarly, a one-standard-deviation increase in religiosity is associated with an additional 2.4 dollars or trust points allocated to a member of a member of one's in-groups relative to a random stranger.

6 Universalism and Economic Behaviors and Outcomes

Ultimately, we are interested in variation in universalism because it may help explain heterogeneity in economically-relevant behaviors and outcomes. As a proof-of-concept, we present pre-registered correlational analyses of the relationship between universalism and four types of self-reported behaviors and outcomes that are unified by a common theme: they relate to how an individual makes trade-offs involving options that are more or less "local."¹³

¹³Appendix C.2 contains details on the elicitation procedure for all variables discussed in this section. Appendix F.2 presents histograms of the outcome variables for all respondents, and separately for belowand above-average universalism. Appendix F.1 presents tabular results of the figures in this section.







Figure 4: This figure presents OLS estimates from regressions of the composite measure of moral universalism on each of the given sociodemographic correlates, along with 95% confidence intervals. All sociodemographics are standardized into z-scores. Since moral universalism increases with allocation amounts to a stranger vis-à-vis an in-group member, negative coefficients in this figure represent a *decrease* in moral universalism given a one-standard-deviation increase in the given sociodemographic correlate. In our sample, one standard deviation in age is approximately 16.6 years. For the male indicator, one standard deviation is 0.4, and for the white indicator, one standard deviation is equal to 0.8. Appendix E.4 adjusts p-values using the False Discovery Rate procedure (Anderson, 2008).

6.1 Results

Donations. We begin by looking at the structure of people's charitable giving: the perhaps most straightforward implication of universalism is that it determines whom people donate to. Here, an important distinction is between donations that go to local community organizations such as schools or local churches, and donations to nationwide or even global charities. To assess this, our survey asked respondents to report the dollar amount that they donated over the past 12 months in each of four categories: local community organizations, local church, nationwide charities, and global charities.

The top panel of Figure 5 studies the relationship between universalism and log donations in each of these four categories. Throughout, we standardize dependent variables into z-scores for ease of interpretation. For each dependent variable, we present the OLS coefficient of universalism for each of three different regression specifications: (i) a univariate regression (blue); (ii) a regression that conditions on age, gender, race, cognitive skills, and the income and wealth index; and (iii) a specification that additionally controls for religiosity, urbanicity, and college education. We emphasize that some of these covariates (such as age and religiosity) potentially induce variation in universalism in the first place. We hence view conditional regressions more as sensitivity checks than as attempts to get closer to causal identification.

The results show that universalist people donate less locally than less universalist people, yet they donate more at more global levels. In fact, going from left to right, the regression coefficients become uniformly more positive looking across the different donation domains. In terms of quantitative magnitude, an increase in moral universalism equivalent to an additional one dollar or trust point allocated to a random stranger is associated with a decrease in donations to local religious organizations of 4.9% and to local communities of 0.9%, and an increase in donations to global non-profits of 0.9%.

In contrast, the relationship between universalism and total donation amounts (which we did not pre-register) is relatively weak. If anything, universalists donate less money overall, but this correlation becomes insignificant once controls are accounted for.

We view this set of results as suggesting two implications. First, the defining feature of universalists and non-universalists is indeed not who is "more" or "less" moral but instead towards whom one acts prosocially. Second, the correlations between universalism and donation decisions further underline the meaningfulness of our measure.

Equity Investments. A long line of literature originating with French and Poterba (1991) has documented "home bias" across a wide variety of financial assets. We hypothesize that some part of such home bias in equity investment decisions might be driven by low universalism. For example, it is conceivable that people with low universalism believe that managers of foreign companies engage in rent-seeking activities. To investigate the relationship between universalism and home bias, our survey included two questions. First, we asked respondents how much money they have currently invested in national and foreign stocks. Second, because many people do not themselves own financial assets, we included a hypothetical investment question in which respondents were asked how they would invest hypothetical \$100 between stocks of a U.S. manufacturer and those of a manufacturer based abroad.

The middle panel of Figure 5 summarizes the results. We find that higher universalism is consistently correlated with lower home bias in equity. In terms of quantitative magnitudes, our data suggests that a one-dollar or trust point increase in moral universalism is associated with a 0.2% decrease in the portion of an investor's actual equity portfolio made up of domestic stocks, and similarly a 0.5% decrease in the domestic portion of an investor's hypothetical portfolio.

Education Choices. A potentially important dimension of educational decisions is geographic distance. Some people might prefer to study close to their place of upbringing, while others might be willing to move further away from home. It is conceivable that less universalist people are less willing to study further from home, simply because they value and trust their local community more. To study the relationship between univer-



Figure 5: This figure presents OLS coefficients for the regression of a given outcome variable on the composite measure of moral universalism, along with 95% confidence intervals. All dependent variables are standardized into z-scores. Other than the first panel, the following outcome variables are in logs: (1) number of friends, (2) number of acquaintances, (3) interactions with friends, and (4) times given and received help. The sparse set of controls consists of age, gender, race, cognitive skill, and our composite economic index (of log net worth and log income). To these, the full set of controls adds an indicator for college education, urbanicity, and religiosity.

salism and educational investment, our survey included a question on how likely (on a scale 0–10) respondents would be to recommend to their child that they attend a local college, relative to a college that is more distant but ranked slightly higher. The middle panel of Figure 5 summarizes the results. We find that higher universalism is consistently correlated with lower home bias in educational investments. In terms of quantitative magnitudes, an increase of ten dollars or trust points (about one standard deviation) in universalism is associated with a decrease on a scale from 0 (definitely recommend the local college) to 10 (definitely recommend the distant college) of 0.11.

Social Networks. In a final step, we study the relationship between universalism and the structure of people's social networks. Our definition of universalism implies that, for a given level of altruism, universalists expend less altruism towards socially close people. It is thus conceivable that universalists forge fewer close relationships but instead spread their time and affection more broadly. While this may in itself have benefits, it could also imply that universalists have fewer friends and feel lonely. Indeed, moral philosophers in the communitarian tradition have long argued that moral universalism produces atomized and socially isolated individuals (Sandel, 2005; Etzioni, 1994).

We present the first empirical examination of this conjecture. Our survey contained six additional questions, regarding: (i) how many friends a respondent has, which we defined as "individuals with whom you feel mutual bonds of affinity and with whom you would feel comfortable sharing personal information"; (ii) how many acquaintances a respondents has, which we defined as "individuals you know and with whom you would feel comfortable spending some time, but only for more superficial or professional purposes"; (iii) how often a respondent interacts with friends in a typical week; (iv) how often a respondent both (a) gave help to and (b) received help from members of the local community within the month prior to completing the survey; and (v) respondents' selfassessment of whether they live a fulfilled social life or feel rather lonely, on a scale from zero to ten.

The results are reported in the bottom panel of Figure 5. Again, all outcome variables are standardized into z-scores. We find that universalists indeed have fewer close social connections: they report having fewer friends, fewer acquaintances, meeting with friends less often, and being more lonely. We do not find meaningful correlations with the number of times the respondent received or gave help to others. Overall, these results are consistent with the idea that while universalists treat socially distant people relatively well, they are relatively less inclined to invest into close relationships, which might have implications for their loneliness. Of course, since these analyses are correlational in nature, it is also conceivable that the structure of a respondent's friendship network affects their measured moral universalism.

6.2 Robustness Checks

Different universalism measures. The analyses presented above use the composite measure of universalism. Appendix F.3 reports robustness checks for universalism in altruism and universalism in trust separately; the results are always similar.¹⁴

Appendix F.7 replicates all analyses using a binary measure of universalism based on a median split of respondents. Moreover, this Appendix shows that we get similar results focusing on specific parts of the distribution of universalism.

Multiple hypothesis testing. Because we test correlations between universalism and many different outcome variables, multiple hypothesis testing is a potential concern (though our analyses were pre-registered). In Appendix F.6, we adjust all p-values using the False Discovery Rate procedure (Anderson, 2008), with very similar results.

Misreporting. In terms of survey procedures, a potential concern about linking measures of individual preferences or beliefs to self-reported outcomes and behaviors is misreporting. For example, participants may "figure out" the purpose of the survey after having completed the universalism elicitation questions, and then provide information on field behaviors "in line" with their decisions in the universalism module. To address this, 10% of our sample completed the questionnaire that elicits information about economic and social behaviors *before* they complete any universalism questions. In Appendix F.5, we show that we get almost identical results in this sub-sample.

7 Discussion

We conclude the paper by discussing and interpreting our universalism measure in light of the results, and by offering thoughts on how applied researchers can leverage specific subsets of our survey items to optimally trade off predictive power and time constraints.

7.1 Interpretation of Universalism Measure

The benefits of having a relatively simple and scalable survey-based universalism measure naturally come at the cost that we cannot tease out the precise mechanisms behind observed universalism. Our preferred interpretation is that the money allocation games capture universalism in altruism, and the trust point games universalism in trust. However, we acknowledge that social or cultural norms could also contribute to observed

¹⁴Appendix F.4 additionally presents results making use of the instrumentation strategies of Gillen et al. (2019) to correct for measurement error.

behavior in these games. We now discuss the relationship between our measure and a number of established concepts from the literature that could conceivably affect our measure.

Inequality aversion. Behavior in our money allocation tasks could be affected by inequality aversion. To gauge the relevance of this, our survey includes a controlled hypothetical choice measure of equity vs. efficiency concerns (following Fisman et al., 2017). In this decision, respondents were asked to split hypothetical money between two random strangers from the U.S., where the most unequal outcome was the most efficient one. In our data, nearly 70% of respondents indicate a strong aversion to inequality by splitting the money equally *despite* the efficiency costs that this equal split entails. This is in stark contrast to our universalism games, where only 27% of decisions correspond to splitting the money equally, even though there are no efficiency costs associated with an equal split. This suggests that respondents are indeed strongly inequality averse when allocating money between two anonymous strangers, but that universalism is more important in tradeoffs between in-group members and strangers. Indeed, our summary measure of universalism and the degree of aversion to inequity derived from the equityvs.-efficiency measure only exhibit a correlation of $\rho = 0.07$.¹⁵ Moreover, heterogeneity in inequality aversion cannot explain variation in universalism in trust beliefs, or why universalism in trust and universalism in altruism are highly correlated.

Relative income of recipients. Recall that our survey explicitly instructed respondents to imagine that the recipients in the money allocation game are equally rich. Still, a potential concern is that respondents allocate money based on perceived relative income of the social groups. This would predict that richer people – who tend to have richer neighbors, friends, family, and colleagues – appear *more* universalist in our survey because they seek to transfer money to the relatively poorer, socially-distant recipient. In our survey data, however, income and wealth are both significantly *negatively* correlated with universalism. This is indicative that considerations based on relative income do not meaningfully drive heterogeneity in universalism in altruism. A fortiori, a preferences-based concern for the poor cannot explain heterogeneity in universalism in trust beliefs.

Relation to taste-based and statistical discrimination. Universalism in altruism and trust confer an immediate relationship to the familiar concepts of taste-based and belief-based (statistical) discrimination. Notice, however, that the most conventional form of statistical discrimination (imperfect information but rational beliefs) has difficulty in rationalizing the stylized fact that the vast majority of respondents believe that their own

¹⁵See Appendix E.3 for correlations between measures of universalism and other social preferences.

in-groups are more trustworthy than the average resident – this cannot be true, on average. This might point to inaccurate statistical discrimination (Bohren et al., 2019), or beliefs that are distorted for motivated reasons such as a desire to rationalize attitudes towards certain groups. More generally, the traditional distinction between taste-based and belief-based discrimination does not explain why universalism in altruism and universalism in trust are highly correlated. To us, this correlation suggests that taste- and belief-based discrimination are intertwined, such as when altruism towards a specific group is type-based and reciprocal in nature (people may allocate money towards those groups that they believe to consist of "good types," as in Levine, 1998).

Respondent effort. Finally, high levels of universalism could also reflect low effort if respondents heuristically click in the middle of the slider. To address this, our survey included a large number of quality checks, according to which we drop inattentive or exceedingly fast respondents. Perhaps as a result, we find that survey response times (a common proxy for effort) and universalism are uncorrelated ($\rho = -0.006$). Moreover, recall from Section 4.1 that the variance decomposition (in particular the large R^2 of respondent-times-social-group fixed effects) arguably provided clear evidence that heterogeneity in universalism reflect deliberate and internally consistent decisions rather than heuristic decision-making by respondents.¹⁶

7.2 Moving Forward: A Short Measurement Module

This paper sought to build a bridge between the existing influential experimental paradigms to study parochialism and at-scale analyses of applied economic settings. For this purpose, we have developed a simple and experimentally-validated tool to measure universalism. We believe that this paper opens up the possibility for more applied work on the role of universalism in economic decision making. In this respect, other researchers who face tight budget or time constraints may elect to deploy only a subset of our full universalism module. First, our universalism module includes separate components on domestic universalism, foreign universalism, and global universalism, each of which could in principle be leveraged in isolation by researchers who are interested in these specific contexts.

Second, we here develop short versions of our measurement modules that are based on fewer questions (in-groups). As we discuss in Appendix B, in our survey, almost all

¹⁶The respondent-times-social-group fixed effects also allow an exploration of whether highuniversalism participants are less consistent than low-universalism respondents, as would be the case if observed universalism results from heuristically clicking in the middle of the slider used to make allocation decisions. When we implement the variance decomposition separately for above- and below-median universalism, we find that respondent-times-social-group fixed effects explain almost exactly the same fraction of the variation in both groups.

three-question versions of our measurement module exhibit correlations around $\rho \approx 0.90$ with our main measure that is based on 16 questions. In fact, even the *separate allocation decisions* exhibit correlations of $\rho \approx 0.50-0.70$ with the main universalism measures. This suggests that very short measurement modules will capture a large fraction of the information that is contained in our full module. We note here that three-question module that explains the largest fraction of the variation in universalism in altruism in our data ($\rho = 0.94$). We present the short module for universalism in trust in Appendix B. Notice that these short measurement modules will only capture the majority of the relevant variation if the researcher is interested in estimating variation in universalism in the population, the choice of groups will certainly matter.

Short module for universalism in altruism.

- 1. Domestic: Split \$100 between a member of one of your past or current organizations (local church, leisure club or association, etc.) and randomly-selected U.S. person
- 2. Foreign: Split \$100 between randomly-selected U.S. person and randomly-selected person from anywhere in world
- 3. Global: Split \$100 between someone who speaks your same language and lives anywhere in the world and randomly-selected person from anywhere in world

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ONLINE APPENDIX

A Details on Measurement Tool and Validation

A.1 Screenshots

In each row below, how would you split \$100 between a randomly-selected person who lives in the United States and the individual displayed on the right (who is part of a particular social group)?

The closer you drag the slider to one individual, the more money you allocate to that individual. Please assume all individuals below have the same income, **all live in the United States**, and would not find out that it was you who sent them the money.



Figure 6: Example decision screen for domestic universalism in altruism. Subjects would see two of these screens consecutively, where five of the ten groups would be presented on each screen. Across all subjects, the order of the ten social groups was randomized, and whether all social groups appeared on the left or all appeared on the right was also randomized for any given choice domain. The layout for tasks eliciting global universalism in altruism is identical to that of domestic groups.

How would you split \$100 between a randomly-selected person who lives anywhere in the world and a randomly-selected person who lives in the United States?

The closer you drag the slider to one individual, the more money you allocate to that individual. Please assume both individuals below have the same income, and would not find out that it was you who sent them the money.



Figure 7: Example decision screen for foreign universalism in altruism. Across subjects, it was randomized whether the domestic social group appeared on the left or on the right. The layout for the task eliciting foreign universalism in *trust* is identical to this layout, with the exception of necessary changes to the instructions and to graphics, as consistent with the layout for trust tasks presented in Figure 8.

In each row below, how would you split 100 "trust points" between a randomly-selected person who lives in the United States, and the individual displayed on the right (who is part of a particular social group)?

The closer you drag the slider to one individual, the more you trust that individual, relative to the other individual. Please assume **all of the individuals below live in the United States**.



Figure 8: Example decision screen for domestic universalism in trust. Subjects would see two of these screens consecutively, where five of the ten groups would be presented on each screen. Across all subjects, the order of the ten social groups was randomized, and whether all social groups appeared on the left or all appeared on the right was also randomized for any given choice domain. The layout for tasks eliciting global universalism in trust is identical to that of domestic groups.

A.2 Details on Ex-Ante Experimental Validation

A.2.1 Crowd-sourcing and Robustness to the choice of groups

Our measurement tool relies on a specific subset of social groups that is small relative to the universe of all candidate groups. To alleviate concerns about the sensitivity of both our tool and results to the particular set of social groups chosen, we followed a twofold approach: (i) we implemented a crowd-sourcing exercise to tie our hands in the selections process of the groups; (ii) we conducted a validation exercise with a substantially larger set of groups.

Crowd-sourcing exercise. We partitioned a list of 27 domestic social groups into two broad categories: those groups people typically interact with closely (i.e., specific individuals you know, such as close family members), and those groups for which it is impossible to interact with all members, but who people have consciousness of kind or of a common set of characteristics (e.g., someone of your same race or ethnicity). From each of these two sets of groups, N = 200 subjects on Amazon MTurk were asked to select the five social groups people typically most identify with. We repeated this same exercise with an additional N = 200 subjects on Amazon MTurk, but with a set of 15 *global* social groups (where distinction in terms of interaction was naturally not made).

From these two separate crowd-sourcing exercises, we selected five domestic social groups people typically identify with the most out of the *interaction* list, and five domestic social groups from the *non-interaction* list. We excluded "close family" and "close friends", as they lacked variation across respondents. We finally selected the five most-selected global social groups. Combined, these fifteen groups make up the domestic and global versions of our universalism measures.

Robustness to larger set of social groups. Our measurement tool uses a specific set of social groups. Though we tied our hands in the selection process of these groups as described above, a potential concern might still be that we would have obtained a very different universalism measure had we chosen different in-groups to include in our measure. To address this concern, with a sample of N = 300 AMT workers, we implemented our money allocation games with a superset of 40 social groups. That is, for each out of 25 domestic and 15 global groups, respondents were asked to split hypothetical \$100 between a member of that group and a randomly-selected person. We then compute the correlation between our main universalism measure described above and universalism as constructed from a random subset of the superset of social groups. We consistently find very high correlations ($\rho \approx 0.9$) between our main measure and universalism as constructed from these random subsets of the broader set of social

groups. This suggests that the selection of the specific set of social groups does not play a big role in assessing heterogeneity in universalism across individuals. For lists of these groups, see Appendix A.2.5.

We construct measures of universalism that rely on these larger sets. The raw correlation coefficient between the domestic ten-group measure in our main sample and the expanded, domestic 25-group measure is $\rho = 0.96$. Excluding the original ten social groups from the latter measure (i.e., correlating our main measurement with the measure involving only the 15 new social groups), the correlation coefficient is $\rho = 0.89$.

For the global versions, the correlation between the 5-group measure in our main sample and the expanded measure with 15 groups is $\rho = 0.93$. Excluding the five social groups included in the main survey from the larger universalism measure, the correlation coefficient between the five-group measure and the corresponding ten-group measure is $\rho = 0.84$.

More granularly, we take the pairwise correlation between money allocations to all 25 domestic social groups. For example, we take the correlation between all allocations given to "Someone who lives in your local neighborhood" and all allocations given to "One of your close friends", and do this for all possible pairings of social groups (excluding all pairings of a social group with itself). Both the mean and median correlation coefficients for allocations to domestic social groups are $\rho = 0.29$, while the minimum is $\rho = 0.45$. That is, higher allocations to one social group are always at least weakly correlated with higher allocations to any other social group.

For the foreign versions of the universalism measures, these statistics are $\rho = 0.42$ for both the mean and median, $\rho = 0.25$ for the minimum, and $\rho = 0.61$ for the maximum.

Lastly, we randomly generate universalism measures where we randomly draw groups from the superset of 25 domestic social groups and 15 global social groups to obtain measures based on ten domestic social groups and five global social groups. We randomly create a total of 2,500 such measures. The mean of all correlations between these 2,500 measures and the main measure formed from the 10 domestic social groups and 5 social groups in our main survey is $\rho = 0.93$, while the minimum is $\rho = 0.85$.

A.2.2 Validation Study for Universalism in Altruism

All of the decisions in our survey are hypothetical in nature. We follow Falk et al. (2015) in conducting an experimental validation exercise that incentivizes these hypothetical conditions for a separate set of respondents on Amazon MTurk. Over a one-week time lag, we elicit from N = 303 respondents both an *unincentivized* and *incentivized* version of our universalism in altruism tasks for five specific social groups for which incentivizing over MTurk is feasible. These N = 303 respondents completed these two versions of

our measurements in random order (i.e., N = 145 first completed the unincentivized version, while N = 158 completed the incentivized version first).

All subjects were informed at the beginning of this first round of the survey that the survey would consist of two parts, and that they would receive an invitation to complete the second part of the survey one week after completing the first round. As indicated by Falk et al. (2015), the benchmark with which to compare the correlation between decisions made in the incentivized and unincentivized environments is not 1. Instead, it is the correlation between decisions made over the same one-week time lag, always under the *incentivized* condition that, ex-ante, would arguably capture respondents' actual preferences. Comparing to a test-retest correlation allows us to account for the noise we'd expect to see even in respondents who complete incentivized versions of our measures both times. As such, we also had N = 97 respondents complete the incentivized conditions in both rounds.¹⁷

We compare the resulting correlation coefficients from the average incentivizedunincentivized and test-retest benchmark in Section 2.3 of the main text.

A.2.3 Validation Study for Universalism in Trust

We validate our universalism in trust tool with a measure of beliefs about others' behavior in a structured experimental cheating game. Here, subjects were asked to predict the behavior of another subject (with a given group membership) in a task that was built to resemble the widely used die-in-a-cup cheating task developed by Fischbacher and Föllmi-Heusi (2013). We elected to elicit beliefs as opposed to actions in, e.g., a trust game because trust game behavior is known to be affected by preference-based considerations such as altruism and inequity aversion. While this task clearly does not add financial incentivization, because the cheating game questions are more preciselydefined they add value by providing conceptual clarity. As such, this exercise is meant to show that our trust point allocation tasks correlate with more widely-implemented measures or trust that are potentially more intuitive (if more costly to describe and implement) for respondents.

In Amazon MTurk sessions, subjects were asked to imagine another participant of the survey would be responsible for allocating \$200 between themselves and the subject. The hypothetical participant's decision, however, would be dictated by a random draw from a set of 201 cards, each one numbered with a different integer from 0–200.

¹⁷Of the 158 subjects randomly-assigned to complete the incentivized condition in the first stage and the hypothetical condition in the second stage, 146 (92.4%) completed both stages. Of the 145 who completed the hypothetical condition first and then the incentivized condition second, 127 (87.6%) completed both stages. Finally, of the 97 subjects assigned to complete incentivized conditions in both stages, 79 (81.4%) completed both rounds.

Importantly, instructions clarified that the drawn card would only be observable to the other participant, and not to the experimenter or to the subject. The actual allocation of money would be determined by the hypothetical participants' *report*, thus mimicking cheating setups as in Fischbacher and Föllmi-Heusi (2013). Instructions emphasized that the other participant could cheat by reporting a different number than was actually drawn and thus by keeping more of the \$200 amount than the game entitled them to.

Subjects were asked to imagine that the card drawn by the hypothetical participant contained the number "100", such that both the other participant and the subject should receive \$100 if the hypothetical participant were completely honest. The task for the subject in the cheating game was to predict which of two people would cheat more to the subject's disadvantage (and by how much), if both a randomly-selected person from their country and a member of their in-group would independently play the cheating game with the subject. That is, the subject was asked to imagine that two different players with different social identities both drew a "100" and were then asked to predict which of the two players would cheat more on them by reporting a higher number.

N = 300 subjects completed both our trust questions and the structured cheating game. We find that our composite measure of universalism in trust exhibits a substantial positive correlation with a composite measure of beliefs in the cheating game (ORIV $\rho = 0.75$).¹⁸

A.2.4 Power Analyses for Validation Exercises

Universalism w.r.t	Condition	ORIV ρ	Ν	Power	Minimum Detectable Correlation
Altruism	Incentivized-Unincentivized	0.50	273	80%	0.362
Altruism	Incentivized-Unincentivized	0.50	273	90%	0.338
Altruism	Test-Retest	0.45	79	80%	0.162
Altruism	Test-Retest	0.45	79	90%	0.112
Trust	-	0.75	300	80%	0.670
Trust	-	0.75	300	90%	0.656

Table 1: Power Analyses: Minimum Detectable Correlations for Validation Exercises

A.2.5 List of Social Groups

Domestic social groups included in main survey. (i) A member of your extended family (e.g., your cousin); (ii) A member of one of your past or current organizations (local church, leisure club or association, etc.); (iii) Someone who lives in your local neighborhood; (iv) A friend of a family member (e.g., your sibling's closest friend); (v) A former or current colleague at work or school; (vi) Someone who shares your interests or hobbies (e.g., a fellow fan of the same sports team, or a fellow runner); (vii) Someone

¹⁸Standard correlation coefficient is $\rho = 0.60$.

who shares your religious beliefs (e.g., a fellow Christian); (viii) Someone of your same age/ generation; (ix) Someone who shares your political views (e.g., a fellow left-winger, or a fellow right-winger, etc.); (x) Someone of your same race/ ethnicity (e.g., a fellow Hispanic person).

Global social groups included in main survey. (i) Someone who speaks your same language and lives anywhere in the world; (ii) Someone who shares your religious beliefs (e.g., a fellow Christian) and lives anywhere in the world; (iii) Someone of your same race/ ethnicity (e.g., a fellow Hispanic person) who lives anywhere in the world; (iv) Someone who shares your values and lives anywhere in the world; (v) Someone who shares your occupation/ profession and lives anywhere in the world.

Additional domestic social groups included in validation exercise (Appendix A.2.1).

(i) A member of your close family (e.g., your sibling); (ii) One of your close friends; (iii) A friend of one of your close friends; (iv) One of your acquaintances (who is also not a friend); (v) A member of your distant family (e.g., a cousin of your parent); (vi) One of your distant friends; (vii) Someone who shares your values; (viii) Someone of your same occupation/ profession; (ix) Someone who speaks your same language; (x) Someone of your same social class (e.g., a fellow working class member); (xi) Someone of your same gender; (xii) Someone who resides in your same city; (xiii) Someone who resides in your same state; (xiv) Someone of your same sexual orientation; (xv) Someone of your same educational attainment.

Additional global social groups included in validation exercise (Appendix A.2.1). (i) Someone who shares your interests or hobbies (e.g., a fellow fan of the same sports team, or a fellow runner) and lives anywhere in the world; (ii) Someone of your same gender who lives anywhere in the world; (iii) Someone of your same age/ generation who lives anywhere in the world; (iv) Someone who shares your political views (e.g., a fellow left-winger, or a fellow right-winger, etc.) and lives anywhere in the world; (v) Someone of your same social class (e.g., a fellow working class person, or middle class individual) who lives anywhere in the world; (vi) Someone of your same educational attainment who lives anywhere in the world; (vii) Someone of your same sexual orientation who lives anywhere in the world; (viii) Someone of your same sexual orientation who lives anywhere in the world; (viii) Someone of your same sexual orientation who lives anywhere in the world; (viii) Someone who lives in the same continent as you, and not in your same country; (ix) Someone who lives in a country that is an international ally to your country; (x) Someone who lives in a country that is a member state of the same international organization as your own country's (e.g., NATO, European Union, etc.).

B Short Measurement Module

In this section, we present a three-group version of our measurement tool that can be implemented in surveys where time is a constraint. In Section 4, we showed a significant portion of variation in our allocation tasks is driven by respondent rather than social group fixed effects. We now additionally show that allocations across our universalism tasks are highly consistent across decision domains and in-groups and that, in turn, a smaller set of social groups is sufficient to capture a significant portion of the information elicited by our full measurement tool.

To highlight this degree of consistency, from the superset of all social groups in our survey we take all combinations of three groups consisting of one domestic social group, one global social group, and the foreign vs. domestic decision. For each of these 50 combinations of groups and for both altruism and trust, we calculate the corresponding universalism measure as described in Section 2.2.

We compute each of these alternative measurements and correlate them with our full measures. Figure 9 plots these correlation coefficients for the case of universalism in altruism. Even the "worst" of the alternative, short versions of our measurement tool performs well relative to the full measure, with a correlation coefficient of $\rho = 0.86$. The results are identical for the case of universalism in trust.

We emphasize again that *any* combination of one domestic, one foreign, and one global universalism question will represent a meaningful proxy for overall universalism. Thus, researchers may wish to select in-groups based on their interests and the topic of their study. Still, in our data, the largest of these correlation coefficients ($\rho = 0.94$) corresponds to the following combination of groups that make up the short module for universalism in altruism:

Short module for universalism in altruism.

- 1. Domestic: Split \$100 between a member of one of your past or current organizations (local church, leisure club or association, etc.) and randomly-selected U.S. person
- 2. Foreign: Split \$100 between randomly-selected U.S. person and randomly-selected person from anywhere in world
- 3. Global: Split \$100 between someone who speaks your same language and lives anywhere in the world and randomly-selected person from anywhere in world

For universalism in trust, the recommended short module (for which the correlation coefficient with the full measure of universalim in trust is $\rho = 0.93$) is:


Figure 9: Distribution of correlation coefficients for all possible variants of a modular version of our measurement tool for universalism in altruism. Coefficients present the correlation between the given short measure and our full survey measure. The minimum of these correlation coefficients is $\rho = 0.86$, while the largest is $\rho = 0.94$.

Short module for universalism in trust.

- 1. Domestic: Split 100 trust points between a member of one of your past or current organizations (local church, leisure club or association, etc.) and randomly-selected U.S. person
- 2. Foreign: Split 100 trust points between randomly-selected U.S. person and randomlyselected person from anywhere in world
- 3. Global: Split 100 trust points between someone of your same race/ ethnicity who lives anywhere in the world and randomly-selected person from anywhere in world

C Additional Details on Survey Design and Logistics

		Stud	y Sample (%)
Category	Population (%)	Full	Representative
Gender			
Male	49	40.5	48.5
Female	51	59.5	51.5
Age			
18–29	21	12.1	21.8
30–39	16	19.5	16.8
40–49	16	19.1	16.8
50–59	17	23.3	16.4
60–69	14	5.7	14.8
≥70	16	20.3	13.3
Income			
Below 15,000	11	5.2	16.7
15,000–24,999	9	6.0	9.2
25,000–34,999	9	8.3	8.9
35,000–49,999	12	12.0	10.7
50,000–74,999	17	21.9	17.1
75,000–99,999	13	15.1	13.2
100,000–149,999	15	16.6	15.3
150,000–199,999	7	8.0	6.3
200,000 or more	7	7.1	2.5
Ancestry			
White	63	79.1	62.7
African-American	17	8.1	17.2
Hispanic	12	5.7	12.0
Asian	5	4.8	5.0
Other	3	2.4	3.1
Education			
No high school	11	0.9	6.1
High school	29	13.6	29.4
Some college	29	28.9	28.0
Bachelor's degree or higher	31	56.6	36.4
Employment Status (for those at most 65)			

C.1 Sample Characteristics of *Dynata* Survey

Employed full-time	67	70.7	67.0
Not employed full-time	33	29.3	33.0

Note: Income ranges are in annual amounts of USD.

C.2 Description of Main Survey Variables

Domestic universalism in altruism. Universalism with respect to altruism (preferences), measured through bystander dictator games over the local currency analogue of hypothetical \$100, between a domestic member of one's in-groups relative to a domestic stranger. The measure averages the ten corresponding money allocation decisions.

Foreign universalism in altruism. Universalism with respect to altruism (preferences), measured through a bystander dictator game over the local currency analogue of hypothetical \$100 between a domestic stranger and a global stranger.

Global universalism in altruism. Universalism with respect to altruism (preferences), measured through bystander dictator games over the local currency analogue of hypothetical \$100, between a global member of one's in-groups relative to a global stranger. The measure averages the five corresponding money allocation decisions.

Summary measure of universalism in altruism. Unweighted average of domestic universalism in altruism, foreign universalism in altruism, and global universalism in altruism.

Domestic universalism in trust. Trust analogue of domestic universalism in altruism, where the bystander dictator game is instead over 100 trust points.

Foreign universalism in trust. Trust analogue of foreign universalism in altruism, where the bystander dictator game is instead over 100 trust points.

Global universalism in trust. Trust analogue of global universalism in altruism, where the bystander dictator game is instead over 100 trust points.

Summary measure of universalism in trust. Trust analogue of the summary measure of universalism in altruism. That is, unweighted average of domestic universalism in trust, foreign universalism in trust, and global universalism in trust.

Composite measure of universalism. Unweighted average of (i) summary measure of universalism in altruism and (ii) summary measure of universalism in trust.

Dictator game behavior (revealed altruism). Altruism as elicited through a standard dictator game over \$100 between the self and a domestic stranger.

Generalized trust. Generalized trust in others as elicited through an allocation of trust points on a scale from 0 to 100. Respondents were prompted to consider their trust in a domestic stranger, where 0 meant that they believe they "cannot trust a randomly-selected person very much", and 100 meant they believe "a randomly-selected person can in general be trusted a great deal."

Equity-efficiency preferences. Elicitation of preferences for efficiency over equity (Fisman et al., 2017), as given by a bystander dictator game between two randomly-selected people from the respondent's country of residence, in which the most unequal split of money maximizes total payoffs. Specifically, a decision in the middle of the slider would allocate to each recipient \$50. For every extra dollar allocated to one individual over another, the other individual would only lose \$0.50. In turn, the most extreme allocation decisions would grant \$100 to one recipient and \$25 to the other. The measure captures how much a subject prefers efficiency over equity by deviating from an equal, 50:50 split of the money.

Communal moral values. Respondent's preference for communal moral values, as elicited with one loyalty and one fairness item of Haidt's Moral Foundations Questionnaire. Specifically, the difference between subject's rating on a scale from 0 (not at all relevant) to 5 (extremely relevant) regarding the relevance of "Whether or not someone showed a lack of loyalty" and "Whether or not some people were treated differently than others" in determining whether something is right or wrong.

Religiosity Index. Composite measure from a principal component analysis of: (i) selfdescribed religiosity on a scale from 0 (not at all religious) to 10 (very religious); (ii) church attendance on a scale from 0 to 5; and (iii) an indicator for atheism, agnosticism, or no religion. Standardized into z-scores.

Income and Wealth Index. Composite measure from a principal component analysis of: (i) log income (from free-form text entry), and (ii) log net worth (from free-form text entry). Standardized into z-scores.

Urbanicity Index. Composite measure from a principal component analysis of: (i) the population density in respondent's zip code, and (ii) respondent's neighborhood size on a scale from 0 to 9. Standardized into z-scores.

Educational attainment. Respondent's educational attainment. The four educational categories were: (i) no high school, (ii) high school, (iii) some college or vocational training, (iv) bachelor's degree or higher.

College-educated indicator. Indicator for a college education, from the educational attainment variable.

Cognitive skills. Respondent's score on a Raven's Progressive Matrices IQ test.

Donation amounts. Total log dollar amounts given over the past twelve months by each respondent to the following four causes, respectively: (i) Local church or other local religious organizations; (ii) Local communities and groups (e.g., local firefighters, schools, libraries, and city-sponsored functions), excluding local churches; (iii) Non-profit organizations that work towards a better life for people in America in general (e.g., Feeding America); and (iv) Non-profit organizations that work towards a better life for people around the world (e.g., United Way Worldwide).

Difference in nonlocal versus local donation amounts. Log dollar amount of nonlocal donations (those donations to non-profits focused on the United States as a whole or the entire world) minus the log dollar amount of local donations (those donations to local churches and local communities).

Actual home bias. Subjects were asked to estimate the actual total dollar amount of dollars invested in both domestic (based within-the-US) and foreign (based outside-the-US) stocks in their own equity portfolio(s). The proportion of their total stock investments made up by domestic stocks made up each subject's degree of actual home bias.

Hypothetical home bias. Respondent's allocation (out of \$100) to stocks of a domestic manufacturer relative to a foreign manufacturer. In forming these hypothetical portfolios, subjects were told to assume none of their investments would be taxed.

Educational home bias. Subject's response to the following prompt: "Suppose you have a child that asks you for advice. Your child is undecided between attending (a) a local college or (b) a college that is geographically distant but ranked slightly higher.

On a scale from 0 to 10, how strongly would you advise your child to attend either college?", where 0 represented "Would definitely advise to attend the local college", and 10 represented "Would definitely advise to attend the distant college".

Number of friends. Respondent's estimate of the number of people they consider to be friends, defined in the survey as those "individuals with whom you feel mutual bonds of affinity and with whom you would feel comfortable sharing personal information".

Number of acquaintances. Respondent's estimate of the number of people they consider to be acquaintances, defined in the survey as those "individuals you know and with whom you would feel comfortable spending some time, but only for more superficial or professional purposes."

Interactions with friends. Respondent's report on how often they are able to spend time with one of their friends during a typical week, elicited by asking respondents: "Thinking about a typical week, approximately how often per week do you get to spend time with one of your friends?"

Times given and received help. Combination of the aproximate number of times over the month prior to completing the survey that a respondent "relied on someone you know from your local community for help (e.g., assisting with some household chore, watching after a child, etc.)" and in turn "helped someone you know from your local community (e.g., assisting them with some household chore, watching after a child, etc.)".

Quality of social life. Respondent's self-description of the quality of their social life on a scale from 0 ("I feel rather lonely") to 10 ("I have a fulfilling social life").

D Additional Analyses on the Structure of Universalism

D.1 Consistency in Universalism and the Existence of Types



Figure 10: This figure presents the distribution of all pairwise correlation coefficients for the set of all money and trust point allocations in our survey. In red, we plot the correlation coefficients for decisions involving different choice domains (altruism vs. trust) *and* a different social group. In blue, the correlation coefficients for decisions involving the same social group but a different choice domain. Finally, in black we plot the correlation coefficients involving the same choice domain, but different social groups.

	(1)								
	Altruism: domestic	Altruism: global	Altruism: foreign	Altruism: all	Trust: domestic	Trust: global	Trust: foreign	Trust: all	Summary
Altruism: domestic	1								
Altruism: global	0.664***	1							
Altruism: foreign	0.320^{***}	0.441^{***}	1						
Altruism: all	0.767***	0.838^{***}	0.798^{***}	1					
Trust: domestic	0.543^{***}	0.464***	0.235^{***}	0.487^{***}	1				
Trust: global	0.459***	0.559^{***}	0.268^{***}	0.511^{***}	0.732^{***}	1			
Trust: foreign	0.227^{***}	0.306^{***}	0.404^{***}	0.405^{***}	0.387^{***}	0.459^{***}	1		
Trust: all	0.479^{***}	0.526^{***}	0.378^{***}	0.563^{***}	0.825^{***}	0.863^{***}	0.788***	1	
Summary	0.722^{***}	0.790***	0.691^{***}	0.910^{***}	0.720^{***}	0.754^{***}	0.650^{***}	0.855***	1
* $p < 0.05$, ** $p < 0.01$,	$^{***} p < 0.001$								

Universalism Measures
between
Correlations
Table 3:

D.2 Histograms of Allocation Decisions



Figure 11: Distributions of all money and trust point allocations to in-group members in our full sample.

E Additional Analyses on Heterogeneity and Correlates

E.1 Heterogeneity



Figure 12: Distributions of universalism in altruism and in trust across our sample of the U.S. population. The measures average each subject's money allocation and trust point decisions, respectively, across a variety of social groups. These average amounts reflect allocations to random strangers, so that the measure is decreasing in subjects' in-group favoritism. Data underlying figure is from the full study sample.

E.2 Sociodemographics

			D	spendent ν	variable: (Jomposite	measure (of moral ui	niversalisn	и		
	(1)	(2)	(3)	(4)	(5)	(9)	(2)	(8)	(6)	(10)	(11)	(12)
Age	-0.15^{***} (0.01)		-0.15^{***} (0.01)								-0.11^{***} (0.01)	-0.11^{***} (0.01)
Male		-1.93^{***} (0.31)	-0.92*** (0.31)								-0.92^{***} (0.31)	-0.94*** (0.31)
White				-1.80^{***} (0.35)							-0.80** (0.36)	-0.80** (0.36)
Cognitive skills [0-5]					0.77*** (0.12)						0.22^{*} (0.12)	0.20 (0.12)
College-educated						-0.26 (0.30)		0.69^{**} (0.32)				0.41 (0.31)
Income and Wealth Index (z-score)							-1.48^{***} (0.16)	-1.59*** (0.17)			-0.90^{***} (0.16)	-0.96*** (0.17)
Urbanicity Index (z-score)									0.60*** (0.15)		0.23 (0.15)	0.20 (0.15)
Religiosity Index (z-score)										-2.42^{***} (0.15)	-2.06^{**} (0.15)	-2.07*** (0.15)
Observations R ²	6591 0.05	6591 0.01	6591 0.05	6591 0.00	6591 0.01	6591 0.00	6591 0.02	6591 0.02	6491 0.00	6591 0.04	6491 0.08	6491 0.08
<i>Notes.</i> OLS estimates, robust standar universalism, corresponding to the av have a smaller number of observations	rd errors in /erage of al Is as popula	l parenthe I allocatic ation dens	ses. Each in decision ities could	observatic s across s not be ob	on is one ocial grou tained fo	subject. ⁷ ups and a r all subje	The depercross both ects. $* p <$	ident vari $_{1}$ altruism 0.10, ** p	able is the and trust < 0.05, *	e composit In colum ** $p < 0.0$	e measure ns 7, 9, aı l.	of moral id 10, we

Table 4: Sociodemographic correlates of universalism

			Dep	endent va	riable: M	oral unive	rsalism w.	r.t. altruisı	n, all gro	sdn		
	(1)	(2)	(3)	(4)	(2)	(9)	(2)	(8)	(6)	(10)	(11)	(12)
Age	-0.19*** (0.01)		-0.19^{***} (0.01)								-0.15*** (0.01)	-0.15*** (0.01)
Male		-2.53*** (0.39)	-1.27*** (0.39)								-1.24^{***} (0.39)	-1.26^{***} (0.39)
White				-2.04*** (0.44)							-0.75* (0.45)	-0.75* (0.45)
Cognitive skills [0-5]					0.97*** (0.15)						0.30^{*} (0.15)	0.28^{*} (0.16)
College-educated						-0.16 (0.37)		0.91^{**} (0.39)				0.58 (0.39)
Income and Wealth Index (z-score)							-1.63*** (0.19)	-1.77*** (0.21)			-0.92^{***} (0.19)	-1.00^{***} (0.20)
Urbanicity Index (z-score)									0.65*** (0.19)		0.25 (0.18)	0.20 (0.19)
Religiosity Index (z-score)										-2.42^{***} (0.19)	-1.95*** (0.19)	-1.96^{***} (0.19)
Observations R^2	6591 0.05	6591 0.01	6591 0.05	6591 0.00	6591 0.01	6591 0.00	6591 0.01	6591 0.01	6491 0.00	6591 0.03	6491 0.07	6491 0.07
<i>Notes.</i> OLS estimates, robust standard with respect to altruism and all groups 10, we have a smaller number of obse	d errors in s, correspo rvations as	parenthes nding to tl populatic	ses. Each o he average on densitie	bservation of all allo s could nc	n is one s cation de ot be obta	ubject. Th cisions ac ained for a	ie depend ross socia ill subjecti	ent variab l groups a s. $* p < 0$.	le is the 1 nd across 10, ** p <	measure of altruism. 1 c 0.05, *** _l	moral un n columns o < 0.01.	versalism 7, 9, and

Table 5: Sociodemographic correlates of universalism in altruism

			Ι	Jependent	variable:	Moral un	iversalism	w.r.t. trus	t, all grou	sd		
	(1)	(2)	(3)	(4)	(5)	(9)	(2)	(8)	(6)	(10)	(11)	(12)
Age	-0.11^{***} (0.01)		-0.11^{***} (0.01)								-0.074*** (0.01)	-0.074*** (0.01)
Male		-1.33^{***} (0.31)	-0.57* (0.32)								-0.60* (0.31)	-0.61^{*} (0.31)
White				-1.56^{***} (0.37)							-0.85** (0.38)	-0.85** (0.38)
Cognitive skills [0-5]					0.57*** (0.12)						0.13 (0.13)	0.12 (0.13)
College-educated						-0.37 (0.30)		0.48 (0.32)				0.24 (0.32)
Income and Wealth Index (z-score)							-1.33^{***} (0.16)	-1.40*** (0.17)			-0.89*** (0.17)	-0.92^{***} (0.17)
Urbanicity Index (z-score)									0.54^{***} (0.15)		0.22 (0.15)	0.20 (0.15)
Religiosity Index (z-score)										-2.42^{***} (0.15)	-2.18^{***} (0.15)	-2.18^{***} (0.15)
Observations R ²	6591 0.02	6591 0.00	6591 0.03	6591 0.00	6591 0.00	6591 0.00	6591 0.01	6591 0.01	6491 0.00	6591 0.04	6491 0.06	6491 0.06
<i>Notes.</i> OLS estimates, robust standard respect to trust and all groups, corres a smaller number of observations as p	l errors in p ponding to opulation o	arenthese the avera densities c	es. Each ob ge of all al could not b	servation location d e obtaine	is one sub lecisions a d for all s	jject. The across soc ubjects. *	depender ial group: p < 0.10,	It variable s and acro $^{**} p < 0.0$: is the me ss trust. I 35, *** <i>p</i> <	asure of m n columns ¢ 0.01.	oral univers 7, 9, and 1	alism with 0, we have

Table 6: Sociodemographic correlates of universalism in trust

E.3 Correlation b/w Universalism and Other Social Attitudes

Social preference	Universalism w.r.t	Social group(s) considered	ρ
Dictator game behavior	Altruism	All	0.305
Dictator game behavior	Altruism	Domestic	0.349
Dictator game behavior	Altruism	Foreign	0.179
Dictator game behavior	Altruism	Global	0.237
Dictator game behavior	Composite Summary	All	0.287
Dictator game behavior	Trust	All	0.190
Dictator game behavior	Trust	Domestic	0.216
Dictator game behavior	Trust	Foreign	0.0953
Dictator game behavior	Trust	Global	0.175
Generalized trust	Altruism	All	0.0871
Generalized trust	Altruism	Domestic	0.0874
Generalized trust	Altruism	Foreign	0.0510
Generalized trust	Altruism	Global	0.0794
Generalized trust	Composite Summary	All	0.0996
Generalized trust	Trust	All	0.0895
Generalized trust	Trust	Domestic	0.0878
Generalized trust	Trust	Foreign	0.0543
Generalized trust	Trust	Global	0.0840
Preference for efficiency over inequity	Altruism	All	-0.0557
Preference for efficiency over inequity	Altruism	Domestic	-0.0512
Preference for efficiency over inequity	Altruism	Foreign	-0.0408
Preference for efficiency over inequity	Altruism	Global	-0.0440
Preference for efficiency over inequity	Composite Summary	All	-0.0677
Preference for efficiency over inequity	Trust	All	-0.0652
Preference for efficiency over inequity	Trust	Domestic	-0.0298
Preference for efficiency over inequity	Trust	Foreign	-0.0567
Preference for efficiency over inequity	Trust	Global	-0.0727
Rel. importance of communal moral values	Altruism	All	-0.157
Rel. importance of communal moral values	Altruism	Domestic	-0.0809
Rel. importance of communal moral values	Altruism	Foreign	-0.134
Rel. importance of communal moral values	Altruism	Global	-0.155
Rel. importance of communal moral values	Composite Summary	All	-0.171
Rel. importance of communal moral values	Trust	All	-0.145
Rel. importance of communal moral values	Trust	Domestic	-0.0898
Rel. importance of communal moral values	Trust	Foreign	-0.147
Rel. importance of communal moral values	Trust	Global	-0.112

Table 7: Correlations between Universalism and Social Attitudes

Notes. See Appendix C.2 for details on the construction of the social attitudes variables.

E.4 Adjustment for Multiple Hypothesis Testing

This section presents sharpened two-stage q-values to control for the false discovery rate, following Benjamini et al. (2006) and Anderson (2008).

Sociodemographic	Regression Specification	Original p-value	q-value
Age	Univariate	0.0000	0.001
Male	Univariate	0.0000	0.001
White	Univariate	0.0000	0.001
Cognitive skills	Univariate	0.0000	0.001
College education	Univariate	0.3819	0.051
Income and wealth index	Univariate	0.0000	0.001
Urbanicity index	Univariate	0.0000	0.001
Religiosity index	Univariate	0.0000	0.001

Table 8: Two-Stage q-values: Sociodemographics, Univariate Regressions

Table 9: Two-Stage q-values: Sociodemographics, Multivariate Regressions

Sociodemographic	Regression Specification	Original p-value	q-value
Age	Multivariate	0.0000	0.001
Male	Multivariate	0.0025	0.004
White	Multivariate	0.0274	0.023
Cognitive skills	Multivariate	0.1049	0.056
College education	Multivariate	0.1852	0.075
Income and wealth index	Multivariate	0.0000	0.001
Urbanicity index	Multivariate	0.1751	0.075
Religiosity index	Multivariate	0.0000	0.001

E.5 Additional Analyses of Sociodemographic Correlates



Figure 13: This figure presents OLS estimates from probit regressions of a binary indicator of above or below median moral universalism on each of the given sociodemographic correlates, along with 95% confidence intervals. All sociodemographics are standardized into z-scores.



Figure 14: This figure presents OLS estimates from regressions of the composite measure of moral universalism on each of the given sociodemographic correlates, for splits of our sample based on quartiles of moral universalism, along with 95% confidence intervals. All sociodemographics are standardized into z-scores. Since moral universalism increases with allocation amounts to a stranger vis-à-vis an ingroup member, negative coefficients in this figure represent a *decrease* in moral universalism given a one-standard-deviation increase in the given sociodemographic correlate.

F Additional Analyses on Economic Behaviors and Outcomes

F.1 Tabular presentation of results: Summary measure of universalism

		Local Church	ц.	Loci	al Communi	ties	Amei	Dependeni rican Non-P	t variable: Lc 'rofits	og (1+Donati Glol	ions to) bal Non-Prof	îts	Diff. in	nonlocal vs	i. local	Total I	Jonation Am	ounts
	(1)	(2)	(3)	(4)	(5)	(9)	(2)	(8)	(6)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Composite universalism	-0.015*** (0.00)	-0.011*** (0.00)	-0.0015* (0.00)	-0.0037*** (0.00)	0.00098 (0.00)	0.0027** (0.00)	-0.00033 (0.00)	0.0051*** (0.00)	0.0062*** (0.00)	0.0036*** (0.00)	0.0081*** (0.00)	0.010*** (0.00)	0.013*** (0.00)	0.013*** (0.00)	0.0065*** (0.00)	-0.010*** (0.00)	-0.0038*** (0.00)	0.0017* (0.00)
Age		0.0093*** (0.00)	0.0048*** (0.00)		0.0062*** (0.00)	0.0054*** (0.00)		0.0097*** (0.00)	0.0092*** (0.00)		0.0068*** (0.00)	0.0062*** (0.00)	-	-0.0023*** (0.00)	0.00085 (0.00)		0.012*** (0.00)	0.0094*** (0.00)
Male		0.043* (0.02)	0.11*** (0.02)		0.033 (0.03)	0.036 (0.02)		0.035 (0.02)	0.028 (0.02)		0.091*** (0.03)	0.091*** (0.02)		0.011 (0.03)	-0.046* (0.02)		0.039 (0.02)	0.071*** (0.02)
Cognitive skills [0-5]		-0.016 (0.01)	0.020*** (0.01)		-0.0039 (0.01)	-0.0054 (0.01)		0.021^{**} (0.01)	0.014 (0.01)		0.0043 (0.01)	0.0032 (0.01)		0.013 (0.01)	-0.016* (0.01)		0.024** (0.01)	0.038*** (0.01)
Income and Wealth Index (z-score)		0.16*** (0.01)	0.11^{***} (0.01)		0.25*** (0.01)	0.22^{***} (0.01)		0.26*** (0.01)	0.21^{***} (0.01)		0.23*** (0.01)	0.19*** (0.01)		0.0088 (0.01)	0.017 (0.01)		0.30*** (0.01)	0.24^{***} (0.01)
African-American		0.28*** (0.04)	0.023 (0.04)		-0.088** (0.04)	-0.12*** (0.04)		0.039 (0.04)	-0.030 (0.04)		0.041 (0.04)	-0.044 (0.04)		-0.15*** (0.04)	-0.031 (0.04)		0.13*** (0.04)	-0.046 (0.04)
Native American		0.049 (0.13)	0.079 (0.11)		0.16 (0.13)	0.18 (0.13)		0.20 (0.14)	0.23* (0.13)		0.088 (0.13)	0.12 (0.13)		0.028 (0.13)	0.052 (0.12)		0.10 (0.14)	0.12 (0.13)
Asian		0.043 (0.05)	0.048 (0.04)		-0.082 (0.06)	-0.11^{**} (0.06)		0.032 (0.06)	-0.066 (0.06)		0.17*** (0.06)	0.091 (0.06)		0.083 (0.06)	0.011 (0.06)		0.073 (0.05)	0.011 (0.05)
Hispanic		0.061 (0.05)	-0.059 (0.04)		-0.13*** (0.05)	-0.14*** (0.05)		-0.028 (0.05)	-0.076 (0.05)		-0.019 (0.05)	-0.068 (0.05)		-0.034 (0.05)	0.0058 (0.05)		-0.0018 (0.05)	-0.085* (0.04)
Other non-white		0.085 (0.09)	0.065 (0.07)		0.11 (0.10)	0.096 (0.10)		0.18^{*} (0.10)	0.15 (0.10)		0.082 (0.10)	0.062 (0.10)		0.011 (0.09)	0.012 (0.08)		0.13 (0.09)	0.10 (0.08)
College-educated			0.16*** (0.02)			0.21^{***} (0.02)			0.27*** (0.02)			0.21*** (0.02)			0.014 (0.02)			0.29*** (0.02)
Urbanicity Index (z-score)			-0.0026 (0.01)			-0.026** (0.01)			0.066*** (0.01)			0.061^{***} (0.01)			0.083*** (0.01)			0.025 ^{**} (0.01)
Religiosity Index (z-score)			0.65*** (0.01)			0.11^{***} (0.01)			0.094*** (0.01)			0.14^{***} (0.01)			-0.41*** (0.01)			0.40^{***} (0.01)
Observations R^2	6591 0.03	6591 0.09	6491 0.48	6591 0.00	6591 0.09	6491 0.11	6591 0.00	6591 0.11	6491 0.14	6591 0.00	6591 0.08	6491 0.11	6591 0.02	6591 0.03	6491 0.19	6591 0.01	6591 0.16	6491 0.32
Notes. OLS estimates, robust standar	d errors in d then star	parentheses.	. Each obser	vation is one $n < 0.10$. **	subject. Th $n < 0.05$	ie independ€ ** n < 0.01.	ent variable	is our comp	osite measu	tre of univers	alism, which	n averages a	ll decisions	across all se	ocial groups	and across	both altruisr	n and trust.

Table 10: Universalism and behaviors / outcomes

		No. of friends	5	No.	of acquaintaı	nces	Depe Intera	<i>ndent variab</i> action w∕ frie	le: ends	Times r	ec'd and giv	en help	Ful	filled social l	ſſe
	(1)	(2)	(3)	(4)	(5)	(9)	(2)	(8)	(6)	(10)	(11)	(12)	(13)	(14)	(15)
Composite universalism	-0.0080*** (0.00)	-0.0052*** (0.00)	-0.0039*** (0.00)	-0.0074*** (0.00)	-0.0044*** (0.00)	-0.0036*** (0.00)	-0.0042*** (0.00)	-0.0029*** (0.00)	-0.0022* (0.00)	-0.00062 (0.00)	-0.0015 (0.00)	0.0012 (0.00)	-0.0091*** (0.00)	-0.0047*** (0.00)	-0.0030*** (0.00)
Age		0.0028 (0.00)	0.0020** (0.00)		0.0041*** (0.00)	0.0034*** (0.00)		-0.00020 (0.00)	-0.00062 (0.00)		-0.0043*** (0.00)	-0.0055*** (0.00)		0.0074*** (0.00)	0.0064*** (0.00)
Male		0.028 (0.03)	0.031 (0.03)		0.0069 (0.03)	0.0065 (0.03)		0.11^{***} (0.03)	0.12^{***} (0.03)		-0.033 (0.03)	-0.0078 (0.03)		-0.028 (0.03)	-0.018 (0.02)
Cognitive skills [0-5]		0.026*** (0.01)	0.025** (0.01)		0.091^{***} (0.01)	0.084^{***} (0.01)		-0.021^{**} (0.01)	-0.016 (0.01)		-0.0042 (0.01)	0.0076 (0.01)		-0.028*** (0.01)	-0.024** (0.01)
Income and Wealth Index (z-score)		0.17*** (0.02)	0.13*** (0.02)		0.19*** (0.02)	0.14^{***} (0.02)		0.062*** (0.01)	0.047*** (0.02)		0.051*** (0.01)	0.041^{***} (0.01)		0.21*** (0.02)	0.18*** (0.02)
African-American		-0.35*** (0.05)	-0.40*** (0.05)		-0.42*** (0.04)	-0.45*** (0.04)		-0.13*** (0.05)	-0.16*** (0.05)		0.040 (0.05)	-0.0052 (0.05)		0.10^{**} (0.05)	0.044 (0.05)
Native American		0.031 (0.17)	0.048 (0.16)		-0.064 (0.17)	-0.100 (0.17)		-0.025 (0.19)	-0.0099 (0.19)		0.025 (0.16)	0.051 (0.16)		-0.13 (0.17)	-0.13 (0.16)
Asian		0.043 (0.06)	-0.019 (0.06)		-0.19*** (0.06)	-0.25*** (0.06)		-0.17*** (0.06)	-0.19*** (0.06)		-0.28*** (0.05)	-0.23*** (0.05)		0.0070 (0.05)	-0.035 (0.05)
Hispanic		-0.097* (0.05)	-0.12** (0.05)		-0.18*** (0.05)	-0.20*** (0.05)		-0.018 (0.05)	-0.034 (0.05)		-0.013 (0.05)	-0.011 (0.06)		0.12^{**} (0.05)	0.092* (0.05)
Other non-white		-0.018 (0.11)	-0.042 (0.11)		0.031 (0.11)	0.0097 (0.11)		0.034 (0.10)	0.0061 (0.10)		0.23^{**} (0.11)	0.25^{**} (0.11)		-0.10 (0.09)	-0.14* (0.08)
College-educated			0.25*** (0.03)			0.28*** (0.03)			0.074*** (0.03)			0.048* (0.03)			0.16*** (0.03)
Urbanicity Index (z-score)			0.024^{*} (0.01)			-0.00026 (0.01)			0.0085 (0.01)			-0.057*** (0.01)			0.026** (0.01)
Religiosity Index (z-score)			0.12^{***} (0.01)			0.086*** (0.01)			0.073*** (0.01)			0.17*** (0.01)			0.15*** (0.01)
Observations R^2	6591 0.01	6591 0.06	6491 0.09	6591 0.01	6591 0.09	6491 0.11	6591 0.00	6591 0.01	6491 0.02	6591 0.00	6591 0.01	6491 0.04	6591 0.01	6591 0.08	6491 0.11
Notes. OLS estimates, robust standar both altruism and trust. Number of fr *** $p < 0.01$.	d errors in p: iends, numb	arentheses. E	ach observati tances, intera	on is one sulactions with	bject. The ind friends, and	lependent va times receive	riable is our d and given	composite m help are all i	easure of un n logs. All d	iversalism, ¹ ependent va	which avera ariables are	ges all decisio standardized	ons across all into z-score	l social group s. $* p < 0.10$,	s and across $*^* p < 0.05$,

Table 11: Universalism and behaviors / outcomes

	Α	ctual home b	ias	Dep Hypo	<i>endent varia</i> othetical hon	<i>ble:</i> ne bias	Educa	tional Home	e Bias
	(1)	(2)	(3)	(4)	(5)	(9)	(2)	(8)	(6)
Composite universalism	-0.010*** (0.00)	-0.0038*** (0.00)	0.0017* (0.00)	-0.011*** (0.00)	-0.0094*** (0.00)	-0.0097*** (00.0)	-0.022*** (0.00)	-0.017*** (0.00)	-0.017*** (0.00)
Age		0.012^{***} (0.00)	0.0094*** (0.00)		0.0056*** (0.00)	0.0056*** (0.00)		0.013*** (0.00)	0.013^{***} (0.00)
Male		0.039 (0.02)	0.071^{***} (0.02)		-0.0030 (0.04)	0.0017 (0.04)		-0.035 (0.02)	-0.028 (0.02)
Cognitive skills [0-5]		0.024^{**} (0.01)	0.038*** (0.01)		0.015 (0.02)	0.015 (0.02)		-0.020^{**} (0.01)	-0.015 (0.01)
Income and Wealth Index (z-score)		0.30^{***} (0.01)	0.24^{***} (0.01)		-0.040 (0.04)	-0.012 (0.04)		-0.0028 (0.01)	0.0049 (0.01)
African-American		0.13^{***} (0.04)	-0.046 (0.04)		-0.11 (0.11)	-0.096 (0.11)		-0.30^{***} (0.05)	-0.29*** (0.05)
Native American		0.10 (0.14)	0.12 (0.13)		-0.044 (0.26)	0.094 (0.24)		-0.068 (0.15)	-0.084 (0.15)
Asian		0.073 (0.05)	0.011 (0.05)		0.038 (0.08)	0.074 (0.08)		-0.18^{***} (0.06)	-0.15** (0.06)
Hispanic		-0.0018 (0.05)	-0.085* (0.04)		0.057 (0.10)	0.057 (0.10)		-0.013 (0.05)	-0.0015 (0.05)
Other non-white		0.13 (0.09)	0.10 (0.08)		0.14 (0.13)	0.14 (0.12)		-0.058 (0.10)	-0.055 (0.10)
College-educated			0.29*** (0.02)			-0.15^{***} (0.05)			-0.045* (0.03)
Urbanicity Index (z-score)			0.025^{**} (0.01)			-0.018 (0.02)			-0.033^{***} (0.01)
Religiosity Index (z-score)			0.40^{***} (0.01)			-0.033 (0.02)			0.034^{***} (0.01)
Observations R^2	6591 0.01	6591 0.16	6491 0.32	2908 0.02	2908 0.03	2871 0.03	6591 0.07	6591 0.13	6491 0.13
Notes. OLS estimates, robust standard of universalism, which averages all define z-scores.* $p < 0.10, ** p < 0.05, *$	errors in p cisions acro $* p < 0.01$	arentheses.] ss all social {	Each observa groups and a	ation is one Icross both a	subject. The altruism and	independent trust. All de _f	variable is o bendent vari	our composi ables are sta	te measure andardized

Table 12: Universalism and behaviors / outcomes

			Log (1 + Dona	ttions to)			Dependent	variable: Investm	ents		S	ocial Networks		
			American	Global	Diff. in nonlocal	Total Donation	Actual	Hypothetical	Educational	No. of	No. of	Interaction	Times rec'd	Fulfilled
	Local Church (1)	Local Communities (2)	Non-Profits (3)	Non-Profits (4)	vs. local (5)	Amounts (6)	home bias (7)	home bias (8)	Educational home bias (9)	friends (10)	acquaintances (11)	w/ friends (12)	and given help (13)	social life (14)
Composite universalism	-0.0015* (0.00)	0.0026** (0.00)	0.0061**** (0.00)	0.0100*** (0.00)	0.0065*** (0.00)	0.0016* (0.00)	-0.0097**** (0.00)	-0.017*** (0.00)	-0.0036*** (0.00)	-0.0039*** (0.00)	-0.0037*** (0.00)	-0.0022* (0.00)	0.0011 (0.00)	-0.0030*** (0.00)
Age	0.0047*** (0.00)	0.0049*** (0.00)	0.0087*** (0.00)	0.0058*** (0.00)	0.00071 (0.00)	0.0090**** (00.0)	0.0057*** (0.00)	0.013*** (0.00)	0.00051 (0.00)	0.0016** (0.00)	0.0030*** (0.00)	-0.00060 (0.00)	-0.0058*** (0.00)	0.0065*** (0.00)
Male	0.11*** (0.02)	0.040 (0.02)	0.033 (0.02)	0.096*** (0.02)	-0.045* (0.02)	0.075*** (0.02)	0.0010 (0.04)	-0.026 (0.02)	0.045* (0.03)	0.035 (0.03)	0.011 (0.03)	0.12^{***} (0.03)	-0.0049 (0.03)	-0.018 (0.02)
African-American	0.020 (0.04)	-0.13*** (0.04)	-0.043 (0.04)	-0.054 (0.04)	-0.034 (0.04)	-0.055 (0.04)	-0.094 (0.11)	-0.29*** (0.05)	-0.15**** (0.05)	-0.41*** (0.05)	-0.46*** (0.04)	-0.16*** (0.05)	-0.012 (0.05)	0.045 (0.05)
Native American	0.081 (0.11)	0.18 (0.13)	0.24^{*} (0.13)	0.13 (0.13)	0.054 (0.12)	0.13 (0.13)	0.092 (0.24)	-0.080 (0.15)	-0.0081 (0.17)	0.054 (0.16)	-0.093 (0.17)	-0.010 (0.19)	0.056 (0.16)	-0.13 (0.16)
Asian	0.048 (0.04)	-0.11** (0.06)	-0.066 (0.06)	0.091 (0.06)	0.011 (0.06)	0.011 (0.05)	0.074 (0.08)	-0.15** (0.06)	-0.055 (0.06)	-0.019 (0.06)	-0.25*** (0.06)	-0.19*** (0.06)	-0.23**** (0.05)	-0.035 (0.05)
Hispanic	-0.062 (0.04)	-0.14*** (0.05)	-0.085* (0.05)	-0.075 (0.05)	0.0036 (0.05)	-0.091** (0.04)	0.058 (0.10)	-0.0054 (0.05)	-0.096 (0.06)	-0.13^{**} (0.05)	-0.20*** (0.05)	-0.034 (0.05)	-0.016 (0.06)	0.092^{*} (0.05)
Other non-white	0.064 (0.07)	0.091 (0.10)	0.15 (0.10)	0.057 (0.10)	0.011 (0.08)	0.096 (0.08)	0.14 (0.12)	-0.057 (0.10)	-0.050 (0.10)	-0.047 (0.11)	0.0048 (0.11)	0.0063 (0.10)	0.25** (0.12)	-0.14 (0.08)
Cognitive skills [0-5]	0.020** (0.01)	-0.0077 (0.01)	0.011 (0.01)	0.00096 (0.01)	-0.017* (0.01)	0.036*** (0.01)	0.015 (0.02)	-0.016 (0.01)	0.0054 (0.01)	0.023^{**} (0.01)	0.082**** (0.01)	-0.016 (0.01)	0.0061 (0.01)	-0.024** (0.01)
College-educated	0.16*** (0.02)	0.22**** (0.02)	0.28*** (0.02)	0.21^{***} (0.02)	0.015 (0.02)	0.29*** (0.02)	-0.15*** (0.05)	-0.043^{*} (0.03)	-0.25**** (0.03)	0.25*** (0.03)	0.28^{***} (0.03)	0.074*** (0.03)	0.050* (0.03)	0.16^{***} (0.03)
Income and Wealth Index (z-score)	0.11*** (0.01)	0.22**** (0.01)	0.22^{***} (0.01)	0.19*** (0.01)	0.017 (0.01)	0.24*** (0.01)	-0.012 (0.04)	0.0052 (0.01)	-0.051*** (0.01)	0.13^{***} (0.02)	0.14*** (0.02)	0.047*** (0.02)	0.041*** (0.01)	0.18^{***} (0.02)
Urbanicity Index (z-score)	-0.0025 (0.01)	-0.026** (0.01)	0.066^{***} (0.01)	0.061^{***} (0.01)	0.083*** (0.01)	0.025^{**} (0.01)	-0.017 (0.02)	-0.033^{***} (0.01)	-0.081^{***} (0.01)	0.024^{*} (0.01)	0.000027 (0.01)	0.0085 (0.01)	-0.057*** (0.01)	0.026^{**} (0.01)
Religiosity Index (z-score)	0.65*** (0.01)	0.10*** (0.01)	0.092^{***} (0.01)	0.14^{***} (0.01)	-0.41*** (0.01)	0.40*** (0.01)	-0.033 (0.02)	0.033^{***} (0.01)	0.045*** (0.01)	0.12^{***} (0.01)	0.085**** (0.01)	0.074^{***} (0.01)	0.17*** (0.01)	0.15*** (0.01)
Log (1 + Total Survey Response Time)	0.016 (0.01)	0.050*** (0.02)	0.063*** (0.02)	0.050*** (0.02)	0.016 (0.02)	0.046*** (0.02)	-0.011 (0.03)	0.028 (0.02)	0.0077 (0.02)	0.045** (0.02)	0.051^{***} (0.02)	-0.0022 (0.02)	0.034* (0.02)	-0.0035 (0.02)
Observations R^2	6491 0.48	6491 0.11	6491 0.14	6491 0.11	6491 0.19	6491 0.32	2871 0.03	6491 0.13	6491 0.04	6491 0.09	6491 0.11	6491 0.02	6491 0.04	6491 0.11
<i>Notes</i> . OLS estimates, robust standard er variables are standardized into z-scores. [*]	rors in parenthe $p < 0.10$, ** $p <$	ses. Each observation 0.05, *** $p < 0.01$.	n is one subject	t. The indeper	ndent variable is c	our composite me	asure of unive	ersalism, which	ι averages all decisions a	cross all socia	l groups and acı	oss both altru	ism and trust. Al	l dependent

Table 13: Universalism and behaviors / outcomes



F.2 Histograms of Outcome Variables











F.3 Universalism in Altruism and Universalism in Trust

Figure 15: This figure presents OLS coefficients for the regression of the given outcome variable on our measure of universalism w.r.t. altruism, along with 95% confidence intervals. Other than the first panel, the following outcome variables are in logs: (1) number of friends, (2) number of acquaintances, (3) interactions with friends, and (4) times given and received help. The sparse set of controls consists of age, gender, race, cognitive skill, and our composite economic index (of log net worth and log income). To these, the full set of controls adds an indicator for college education, urbanicity, and religiosity.



Figure 16: This figure presents OLS coefficients for the regression of the given outcome variable on our measure of universalism w.r.t. trust, along with 95% confidence intervals. Other than the first panel, the following outcome variables are in logs: (1) number of friends, (2) number of acquaintances, (3) interactions with friends, and (4) times given and received help. The sparse set of controls consists of age, gender, race, cognitive skill, and our composite economic index (of log net worth and log income). To these, the full set of controls adds an indicator for college education, urbanicity, and religiosity.

F.4 ORIV Analysis

Gillen et al. (2019) propose instrumentation strategies to address measurement error in experimental settings. To ensure that the relationships we measure between moral universalism and outcome variables are neither attenuated by nor simply artefacts of measurement error, we employ their obviously-related instrumental variables (ORIV) estimator. To do so, we treat different subsets of our universalism tasks as duplicate elicitations (in their notation, X^a and X^b) of universalism (X^*).

As pre-registered, the order of social groups presented in our survey is randomized within the domestic and global categories. As such, the first measure of universalism (X^a) is constructed just like the main measure described in Section 2, except that it only uses the five domestic groups that (randomly) appear first and the three global groups that (randomly) appear first in the survey for each subject. We do not include the foreign decision as there was only *one* of these elicitations.

Analogously, the second measure of universalism (X^b) is constructed just like the main measure described in Section 2, except that it only uses the five domestic groups that (randomly) appear last and the two global groups that (randomly) appear last in the survey.

Replicating our analyses of outcome variables in Section 6 with the ORIV estimator and the two duplicate elicitations described above delivers the results in Figure 17, where we plot the ORIV coefficients from instrumenting for universalism in altruism, universalism in trust, and composite universalism. Since each subject appears twice when implementing ORIV, standard errors are clustered at the subject level. We find that in almost all cases the results with the ORIV estimator are similar to those with OLS. Following Gillen et al. (2019), a correlation between the two pre-registered instruments X^a and X^b of $\rho = 0.80$ suggests low measurement error.



Figure 17: This figure presents coefficients for the stacked, ORIV regression of the given outcome variables on duplicate elicitations of our composite measure of moral universalism, along with 95% confidence intervals. As recommended by Gillen et al. (2019), both the universalism measures and outcome variables are standardized into z-scores so they have the same scale. Other than the first panel, the following outcome variables were originally in logs, before standardization: (1) number of friends, (2) number of acquaintances, (3) interactions with friends, and (4) times given and received help. Data underlying figure is from the full study sample.

F.5 Order Effects

We measure both universalism and economic outcomes in the same survey. In order to address experimenter demand effects, we randomized the order in which respondents completed: (i) the tasks to measure universalism, and (ii) elicitations of outcome variables. We show that the correlations between the resulting measurements do not depend on the order of questions.

First, we document that the distribution of responses to the outcome questions do not differ across the two randomizations. Specifically, of 13 outcome variables and across the two orderings, the means of only two of these variables are statistically different from each other at the 5% level, with the mean p-value for each of the 13 t-tests being 0.41. Standardizing all outcome variables, the coefficient on an order dummy on the mean of each of these variables ranges from 0.003 to 0.153 standard deviations of the within-variable mean, documenting very small effect sizes.

Hypothetical home bias is measured as 0.153 standard deviations lower on average when a respondent's universalism is measured first. Home bias as measured by a respondent's *actual* equity portfolio is unaffected by having completed our measurement of universalism first. Those who completed tasks to measure universalism first indicated an average 0.080 standard deviation larger difference between nonlocal and local donations. In both cases, while differences between the means are statistically significant due to sample sizes, effect sizes are small.

We also run the same regressions as those conducted in Section 6 on our full sample, but also include a dummy capturing whether the respondent first completed the measurement tasks for moral universalism, and an interaction between the order dummy and moral universalism. If experimenter demand played a role in the correlation between universalism and our outcome variables, the coefficient on the interaction variable should be significant and of the same sign as the raw effect of universalism.

Of 13 regressions of outcome variables on universalism and the above order variables, only in three (log donations to global causes, actual home bias, and hypothetical home bias) is the coefficient on the interaction variable of the same sign as the coefficient on universalism. Of these, none are significant at the 5% level, with p-values ranging from 0.115 to 0.326. The coefficient on the interaction variable is significant at the 5% level for only one of our outcome variables (p=0.037), where for those who completed measurement of universalism first, the coefficient on universalism for the reported log number of acquaintances is slightly less negative: -0.0067 versus -0.0136, both in standard deviations of the average number of acquaintances.

Finally, we replicate Figure 5, where the sample is constrained to those who completed the elicitations of these outcome variables before measurement of universalism.



Figure 18: This figure presents OLS coefficients for the regression of a given outcome variable on the composite measure of moral universalism, along with 95% confidence intervals. The sample consists only of those respondents who completed elicitations of outcome variables before measurement of moral universalism. All dependent variables are standardized into z-scores. Other than the first panel, the following outcome variables are in logs: (1) number of friends, (2) number of acquaintances, (3) interactions with friends, and (4) times given and received help. The sparse set of controls consists of age, gender, race, cognitive skill, and our composite economic index (of log net worth and log income). To these, the full set of controls adds an indicator for college education, urbanicity, and religiosity.

Outcome Variable	No controls	Sparse controls	Full Controls
Log (1 + Donations to Local Church)	73.22 (0.00)	60.57 (0.00)	460.97 (0.00)
Log (1 + Donations to Local Communities)	5.30 (0.00)	53.33 (0.00)	56.28 (0.00)
Log (1 + Donations to American Non-Profits)	0.45 (0.72)	57.78 (0.00)	71.32 (0.00)
Log (1 + Donations to Global Non-Profits)	4.68 (0.00)	43.89 (0.00)	56.88 (0.00)
Diff. in logs between nonlocal and local donations	55.74 (0.00)	16.57 (0.00)	101.47 (0.00)
Log (1 + Total Donation Amounts)	32.98 (0.00)	98.58 (0.00)	254.98 (0.00)
Actual Home Bias	16.27 (0.00)	5.77 (0.00)	5.90 (0.00)
Hypothetical Home Bias	155.83 (0.00)	92.47 (0.00)	75.46 (0.00)
Advice: Distant College	5.49 (0.00)	6.87 (0.00)	16.97 (0.00)
Log (1 + No. of Friends)	18.93 (0.00)	29.65 (0.00)	38.42 (0.00)
Log (1 + No. of Acquaintances)	19.28 (0.00)	52.13 (0.00)	54.31 (0.00)
Log (1 + Interaction w/ Friends)	6.17 (0.00)	7.65 (0.00)	8.72 (0.00)
Log (1 + Times Given / Rec'd Help Within Local Community)	0.74 (0.53)	6.36 (0.00)	18.14 (0.00)
Fulfilled Social Life	22.96 (0.00)	40.20 (0.00)	46.66 (0.00)

Table 14: Outcome variable regressions: F-statistics and corresponding p-values for F-tests

F.6 Adjustment for Multiple Hypothesis Testing

This section presents sharpened two-stage q-values to control for the false discovery rate, following Benjamini et al. (2006) and Anderson (2008).

Table 15: Two-Stage q-values: Outcome Variable Regressions, No Controls

Outcome Variable	Control variable specification	Original p-value	q-value
Log (1 + Donations to Local Church)	No controls	0.0000	0.001
Log (1 + Donations to Local Communities)	No controls	0.0004	0.001
Log (1 + Donations to American Non-Profits)	No controls	0.7510	0.121
Log (1 + Donations to Global Non-Profits)	No controls	0.0003	0.001
Diff. in logs between nonlocal and local donations	No controls	0.0000	0.001
Log (1 + Total Donation Amounts)	No controls	0.0000	0.001
Actual Home Bias	No controls	0.0000	0.001
Hypothetical Home Bias	No controls	0.0000	0.001
Advice: Distant College	No controls	0.0001	0.001
Log (1 + No. of Friends)	No controls	0.0000	0.001
Log $(1 + No. of Acquaintances)$	No controls	0.0000	0.001
Log (1 + Interaction w/ Friends)	No controls	0.0001	0.001
Log (1 + Times Given / Rec'd Help Within Local Community)	No controls	0.5734	0.097
Fulfilled Social Life	No controls	0.0000	0.001

Outcome Variable	Control variable specification	Original p-value	q-value
Log (1 + Donations to Local Church)	Sparse controls	0.0000	0.001
Log (1 + Donations to Local Communities)	Sparse controls	0.3462	0.053
Log (1 + Donations to American Non-Profits)	Sparse controls	0.0000	0.001
Log (1 + Donations to Global Non-Profits)	Sparse controls	0.0000	0.001
Diff. in logs between nonlocal and local donations	Sparse controls	0.0000	0.001
Log (1 + Total Donation Amounts)	Sparse controls	0.0002	0.001
Actual Home Bias	Sparse controls	0.0000	0.001
Hypothetical Home Bias	Sparse controls	0.0000	0.001
Advice: Distant College	Sparse controls	0.0001	0.001
Log (1 + No. of Friends)	Sparse controls	0.0000	0.001
Log (1 + No. of Acquaintances)	Sparse controls	0.0000	0.001
Log (1 + Interaction w/ Friends)	Sparse controls	0.0076	0.002
Log (1 + Times Given / Rec'd Help Within Local Community)	Sparse controls	0.1949	0.031
Fulfilled Social Life	Sparse controls	0.0000	0.001

Table 16: Two-Stage q-values: Outcome Variable Regressions, Sparse Controls

Table 17: Two-Stage q-values: Outcome Variable Regressions, Full Controls

Outra was Washington			1
Outcome variable	Control variable specification	Original p-value	q-value
Log (1 + Donations to Local Church)	Full controls	0.0694	0.022
Log (1 + Donations to Local Communities)	Full controls	0.0112	0.007
Log (1 + Donations to American Non-Profits)	Full controls	0.0000	0.001
Log (1 + Donations to Global Non-Profits)	Full controls	0.0000	0.001
Diff. in logs between nonlocal and local donations	Full controls	0.0000	0.001
Log (1 + Total Donation Amounts)	Full controls	0.0658	0.022
Actual Home Bias	Full controls	0.0000	0.001
Hypothetical Home Bias	Full controls	0.0000	0.001
Advice: Distant College	Full controls	0.0034	0.004
Log (1 + No. of Friends)	Full controls	0.0004	0.001
Log $(1 + No. of Acquaintances)$	Full controls	0.0005	0.001
Log (1 + Interaction w/ Friends)	Full controls	0.0538	0.020
Log (1 + Times Given / Rec'd Help Within Local Community)	Full controls	0.2936	0.074
Fulfilled Social Life	Full controls	0.0064	0.006





Figure 19: This figure presents OLS coefficients for the regression of a given outcome variable on an indicator of above median moral universalism, along with 95% confidence intervals. All dependent variables are standardized into z-scores. Other than the first panel, the following outcome variables are in logs: (1) number of friends, (2) number of acquaintances, (3) interactions with friends, and (4) times given and received help. The sparse set of controls consists of age, gender, race, cognitive skill, and our composite economic index (of log net worth and log income). To these, the full set of controls adds an indicator for college education, urbanicity, and religiosity.


Figure 20: This figure presents OLS coefficients for the regression of a given outcome variable on the composite measure of moral universalism for a median split of our sample based on the economic index, along with 95% confidence intervals. All dependent variables are standardized into z-scores. Other than the first panel, the following outcome variables are in logs: (1) number of friends, (2) number of acquaintances, (3) interactions with friends, and (4) times given and received help. All regressions include no controls.



Figure 21: This figure presents OLS coefficients for the regression of a given outcome variable on the composite measure of moral universalism, for splits of our sample based on quartiles of moral universalism, along with 95% confidence intervals. All dependent variables are standardized into z-scores. Other than the first panel, the following outcome variables are in logs: (1) number of friends, (2) number of acquaintances, (3) interactions with friends, and (4) times given and received help. All regressions include no controls.

G Replication of analyses based on pre-registered, representative *Dynata* sample



Figure 22: This figure presents the incremental R^2 for a series of stacked regressions of all 32 allocation decisions made by all respondents in the representative sample of our survey on various fixed effects. That is, we stack all allocation decisions across both choice domains into a single column, regressing this column sequentially on respondent, social group, and respondent-times-social-group fixed effects. It shows that 32% of variation in the data can be explained by heterogeneity in respondents' average level of universalism, while 6% of the variance in the data can be attributed to heterogeneity in average universalism across social groups. Finally, 36% of the variation in the data can be explained by respondent-times-in-group fixed effects, i.e., by how individual respondents choose allocations specific to the given in-group, beyond what would be predicted by their average universalism level, the choice domain, and the specific social group.











Figure 25: This figure presents OLS coefficients for the regression of the given outcome variable on our composite measure of moral universalism, along with 95% confidence intervals. All dependent variables are standardized into z-scores. Other than the first panel, the following outcome variables are in logs: (1) number of friends, (2) number of acquaintances, (3) interactions with friends, and (4) times given and received help. The sparse set of controls consists of age, gender, race, cognitive skill, and our composite economic index (of log net worth and log income). To these, the full set of controls adds an indicator for college education, urbanicity, and religiosity.