

# MORAL UNIVERSALISM: MEASUREMENT AND ECONOMIC RELEVANCE<sup>\*</sup>

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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. 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 economic and social behaviors, we document that 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 consistently 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., 2016) and when it derives from real social identities (Fershtman and Gneezy, 2001; Goette et al., 2006; Bernhard et al., 2006; Sutter and Kocher, 2004; Bauer et al., 2018; Berge et al., 2018; Lane, 2016).

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 and managerial 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 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 in large, representative samples. 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 and business-related 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 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 predictive of a pre-registered set of economic and social behaviors and outcomes, ranging from donations, to investment behavior, to 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 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.<sup>1</sup>

Ideally, we would like to capture universalism for the universe of potential in-groups,

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<sup>1</sup>Bystander setups are not an innovation of our paper but have been employed previously in a wide number of experimental settings. However, previous work has not been designed to investigate the research questions that we are interested in here, i.e., the existence of a universalism trait that is roughly constant across multiple, different types of in-groups; heterogeneity of this trait in the population; and linkages to economic behaviors or sociodemographic characteristics.

yet this is infeasible in practice. To avoid baking our own biases into the choice of in-groups, 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.

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. We therefore combine them into a summary statistic of universalism in altruism.

We estimate an individual’s degree of universalism in trust using similar techniques. 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. Again, this yields summary measures of domestic, foreign, and global universalism, which are highly correlated. We collapse these into a summary statistic of universalism in the trust domain.

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 the survey question of interest 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  $\rho \approx 0.50 - 0.65$ , which is as high as a financially incentivized test-retest correlation benchmark. We also validate the univer-

salism in trust questions by showing that responses are highly correlated with beliefs in a structured experimental cheating task ( $\rho \approx 0.75$ ).

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-in-group 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. While the identity of in-groups matters relatively little, individual-level 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. Here, some exogenous demographics can plausibly be understood as determinants of universalism. 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 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 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. Thus, the type of universalism that we pick up with our survey games is related to economically meaningful differences in real donation decisions, highlighting the external validity of our measurement tool. This result further highlights that universalism is fundamentally different from the *level* of altruism and trust: universalists and non-universalists donate roughly the same amounts, on average – they just give to very different organizations.

Second, we elicit measures of home bias in equity investments. It is well-known in the finance literature that people on average 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 actual and hypothetical equity investments, we find that universalists exhibit less home bias.

Third, we obtain a measure of “home bias” in 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 environment 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

socially-rewarding 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.

The remainder of the paper is organized as follows. Section 2 describes our measurement tool and corresponding validation steps. Section 3 summarizes the survey logistics and pre-registration. Sections 4 and 5 study consistency and individual-level heterogeneity in universalism as well as sociodemographic correlates. Section 6 shows results relating universalism to economic behaviors and outcomes, and Section 7 concludes.

## 2 Conceptualizing and Measuring Universalism

### 2.1 Universalism

A key hypothesis behind this paper following Tabellini (2008) 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*. 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. By definition, universalists are relatively more altruistic towards those that are socially far away but relatively less altruistic towards those that are socially close. This clarifies that universalists are not “more” or “less” moral. Universalism in trust can be conceptualized in an analogous fashion.<sup>2</sup>

Figure 1 rests on the idea that the slope of an individual’s altruism (or trust) with respect to social distance is a reasonably general trait that does not strongly vary across different types of in-groups. 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.

A key issue is the types of in-groups we consider. Throughout, we define and work with the following types of social identities:

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<sup>2</sup>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.

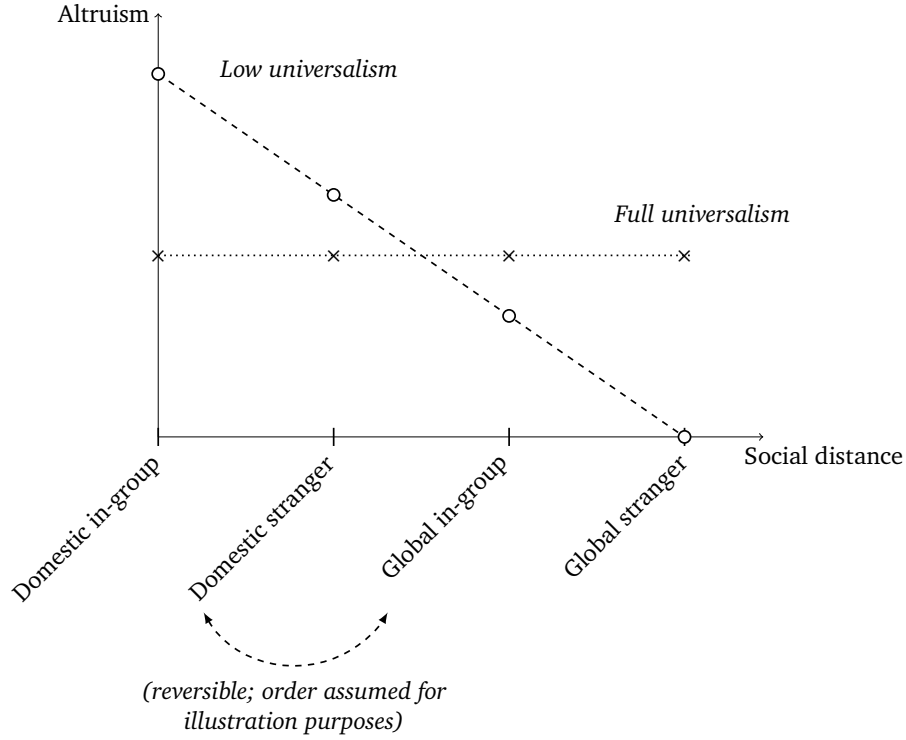


Figure 1: Illustration of heterogeneity in universalism with respect to altruism.

1. Domestic in-group members (e.g., one of your neighbors, or someone in the U.S. who shares your religious beliefs)
2. Domestic strangers
3. Global in-group members (e.g., someone anywhere in the world who shares your religious beliefs)
4. Global strangers

As 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  $3. < 4.$  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, (iv) measure universalism in a way that is compatible with the framework summarized in Fig-



ure 1, and (v) provide an instrument that can be easily implemented at scale and low cost in surveys and experiments.<sup>3</sup>

**Universalism in altruism.** To measure universalism in altruism, we use a “bystander” money allocation game.<sup>4</sup> 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.

Our objective is to measure moral universalism with respect to a large set of potential in-groups, including both nationals and foreigners. Thus, respondents completed a total of 16 hypothetical money allocation tasks that fall into three categories, based on the discussion in Section 2.1: domestic universalism, foreign universalism, 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, 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).<sup>5</sup> 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. 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 randomly-selected person who lives anywhere in the world. Foreign universalism then corresponds

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<sup>3</sup>Recent 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).

<sup>4</sup>Bystander 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).

<sup>5</sup>We excluded “close family” and “close friends” because pilot data revealed very little variation in respondents’ parochialism with respect to these groups.

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. 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 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; Dohmen et al., 2011). 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).

Of course, the fact that some survey questions tend to be highly predictive of experimental choices does not imply that this is true of all survey questions. 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. This approach has the attractive features that (i) the survey questions are precisely defined; (ii) survey responses lend themselves to a quantitative interpretation; and (iii) responses to hypothetical questions are typically substantially more strongly correlated with incentivized

experimental decisions than more qualitative questions (Falk et al., 2015).

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 and resulting measurements within-subject and across hypothetical and incentivized rounds. To obtain a benchmark value, a subset of subjects conducted the incentivized tasks twice, delivering an incentivized test-retest correlation (see Falk et al. (2015)). A possible concern with such a design is that spillover effects, for instance due to concerns for consistency (e.g. Cialdini (2006); Falk and Zimmermann (2013)), might arise. We hence followed Falk et al. (2015) and implemented a one-week time lag between the two survey rounds.

In 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 identify group membership and thus incentivize the measure, subjects were asked to provide answers to the following set of questions: (a) their favorite hobby<sup>6</sup>; (b) their age<sup>7</sup>; (c) ethnicity/ race; (d) informal political affiliation as either a Democrat, Republican, or Independent; and (e) religious denomination. 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.<sup>8</sup> We sampled AMT workers with the appropriate characteristics to serve as recipients in the financially-incentivized money allocation games.

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.<sup>9</sup> The benchmark test-retest ORIV correlation in another sam-

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<sup>6</sup>The options here were: movies, TV series, video games, sports, outside recreational activities (canoeing, hiking), art, and music.

<sup>7</sup>Subjects were asked to choose the decade they were born in; the options were “Before 1930”, “1931 – 1940”, “1941 – 1950”, and so on so forth until “1991 – 2001”.

<sup>8</sup>If selected, one of six decisions (the dictator game eliciting revealed altruism, plus five incentivized universalism tasks) would be randomly selected and implemented. That is, out of a separate set of MTurk Workers who completed elicitations of group memberships, some would be randomly selected to receive payment according to the financially-incentivized decisions of participants of our validation exercise. Subjects of the validation exercise were explained this process before making their decisions.

<sup>9</sup>In the first round,  $N = 145$  subjects completed the hypothetical versions of our allocation tasks, while another  $N = 255$  subjects completed the financially-incentivized versions. 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. The  $N = 145$  subjects who first completed a hypothetical version of our money allocation tasks then

ple of  $N = 79$  subjects is  $\rho = 0.45$ , and hence very close to our incentivized-hypothetical correlation.<sup>10</sup>

When we consider only the subset of subjects who completed both stages of the survey in at least the median response time, the correlation between incentivized and hypothetical decisions increases to ORIV  $\rho = 0.66$ , which is again very close to the benchmark test-retest correlation in the financially incentivized above-median response times sample (ORIV  $\rho = 0.70$ ).<sup>11</sup> 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.

**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.

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. 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

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completed the incentivized versions in the second round. Of the  $N = 255$  subjects who first completed the incentivized versions,  $N = 158$  would complete the hypothetical counterparts in the second round, and the remaining  $N = 97$  would repeat the incentivized condition. Of 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.

<sup>10</sup>Standard correlation coefficients are as follows: for the average incentivized and average unincentivized universalism measures,  $\rho = 0.38$ ; for the test-retest benchmark,  $\rho = 0.33$ . These correlations are in the same range as those in the validation exercises by Falk et al. (2015).

<sup>11</sup>Standard correlation coefficients are  $\rho = 0.49$  and  $\rho = 0.63$ , respectively.

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  $\rho = 0.75$ ).<sup>12</sup>

**Robustness to Larger Set of Social Groups.** Our measurement tool relies on the selection of a set of specific social groups. To tie our hands in the selection process, we chose this set of groups based on a crowd-sourcing exercise as described above. A potential concern, however, might still be that we would have obtained a very different universalism measure had we chosen different potential in-groups. 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. Appendix A.2.1 describes this validation exercise in more detail.

## 2.4 Digression: A Short Measurement Module

We develop short versions of our measurement modules that are based on fewer questions (in-groups). These short modules may prove useful for researchers interested in eliciting moral universalism under tight time constraints. As we discuss in Appendix B, in our survey to be presented below, almost all 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

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<sup>12</sup>Standard correlation coefficient is  $\rho = 0.60$ .

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 ( $\rho = 0.94$ ).<sup>13</sup> However, we emphasize that *any* combination of one domestic, one foreign, and one global universalism question will represent an almost perfect proxy for overall universalism.

#### **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*

## **3 Survey Design and Logistics**

### **3.1 Logistics**

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 nationally representative sample. The physical process was that *Dynata* kept sampling respondents until our pre-specified quotas were satisfied. Since we view throwing away data as scientifically questionable,

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<sup>13</sup>We present the short module for universalism in trust in Appendix B.

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.

### 3.2 Pre-Registration

The 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.

## 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.

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 systematically 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. While a substantial portion of variation—36%—is explained by the *interaction* between respondent and social group fixed effects (suggesting subjects respond meaningfully around their own average level of universalism to the identity in question), relatively little of the variation can be predicted by certain social groups eliciting *systematically* more or less universalist responses.

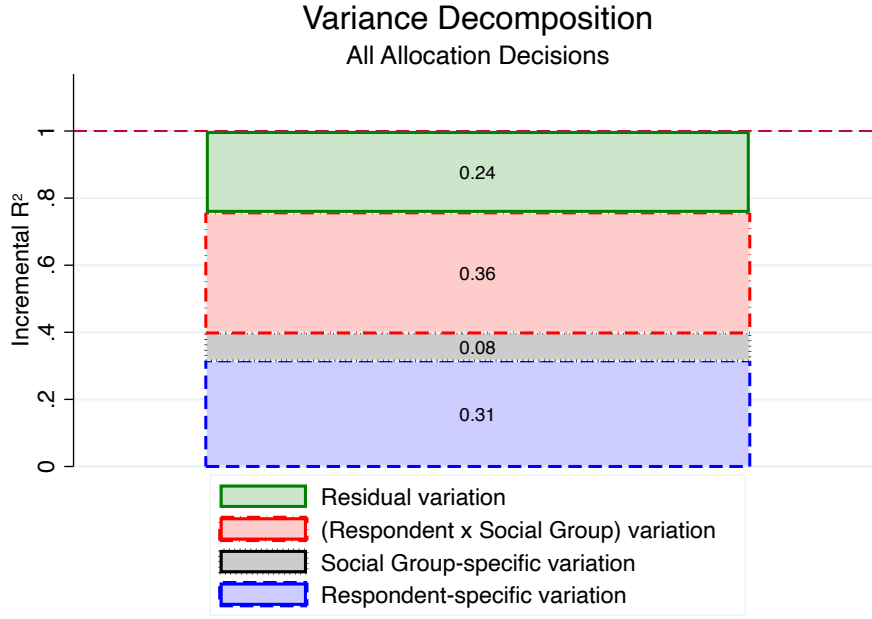


Figure 2: This figure presents the incremental  $R^2$  for a series of stacked regressions of all 32 allocation decisions made by all 6,591 respondents in our survey on various fixed effects. That is, we stack all allocation decisions across both choice domains into a single column, regressing this column 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.

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, the 36% of variation in the data 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 trust (above and beyond how universalist they are in general). Thus, while there is large and meaningful variation in average universalism across respondents, this appears to reflect deliberate and internally consistent group-specific 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 clearly 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 the 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.<sup>14</sup> 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 large heterogeneity in universalism across respondents.

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<sup>14</sup>Figure 12 in Appendix E reproduces this distribution for universalism in altruism and trust separately.

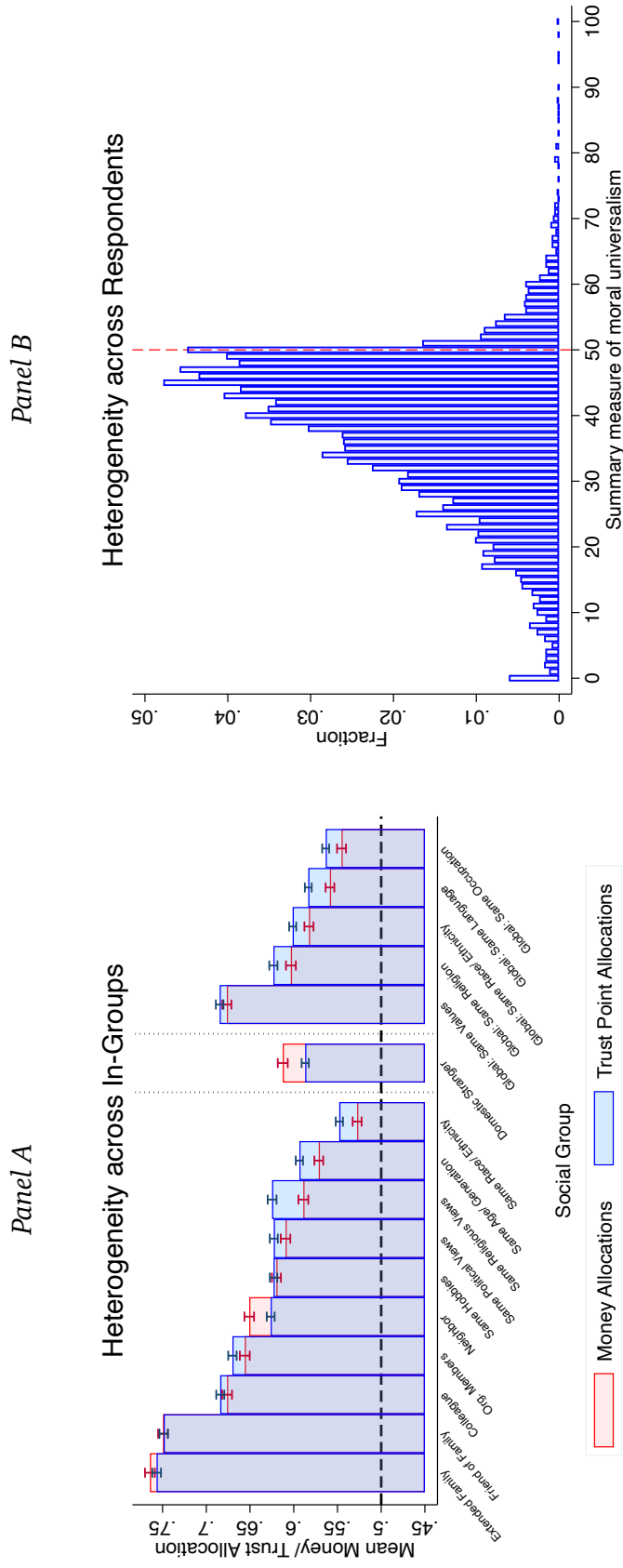


Figure 3: This figure presents heterogeneity in decision-making along two components of the variation in data from our universalism tasks. In Panel A, we present *in-group-specific variation*: average allocations of money and trust points given to every social group across all subjects, along with 95% confidence intervals. The first ten bars show average allocations to domestic in-groups in tradeoffs vis-à-vis a domestic stranger. The 11th bar shows the average allocation to a domestic stranger relative to a global stranger. Bars 12–16 show average allocations to a global in-group member vis-à-vis a global stranger. Each bar is composed of  $N = 6,591$  individuals. In Panel B, we present *respondent-specific variation*: the distribution of the composite measure of moral universalism across our sample of the U.S. population. The amounts reflect allocations to the individual in each decision who is not the immediate in-group member, so that the measure is decreasing in in-group favoritism.

## 5 Sociodemographic Correlates of Universalism

Having identified individual-level heterogeneity as a main source of variation in our data, we now turn to examining sociodemographic correlates of this heterogeneity. Here, some exogenous demographic characteristics like age and gender can plausibly be interpreted as determinants of heterogeneity in universalism. While existing literature has provided extensive evidence that people are not fully universalist, on average, 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 (from 0 to 10), frequency of church attendance (on a scale from 0 to 5), and an indicator for atheism, agnosticism, or no religion.

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 one's in-groups relative to a random stranger. Perhaps surprisingly, we find that richer people (in terms of income and / or wealth) are substantially *less* universalist, on average.<sup>15</sup> While descriptive in nature, these results plausibly shed some light on the origins of variation in universalism in the population, in particular regarding variables such as age, gender, and cognitive skills.

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<sup>15</sup>Appendix E.2 presents tabular results separately for universalism in altruism and in trust, respectively. Appendix E.3 further presents correlations between universalism and other types of social attitudes such as equity-vs.-efficiency preferences.

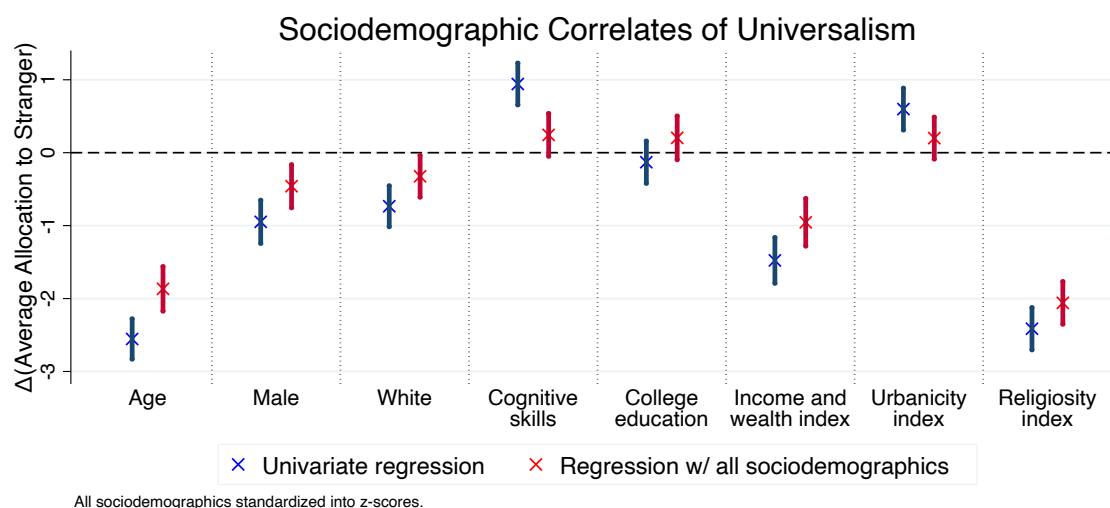


Figure 4: This figure presents OLS estimates from regressions of the composite measure of moral universalism on each of the given sociodemographic correlates, along with 95% confidence intervals. All sociodemographics are standardized into z-scores. Since moral universalism increases with allocation 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.

## 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 behaviors and outcomes: (i) the structure of people’s donations; (ii) home bias in equity investments; (iii) education choices; and (iv) the structure of people’s social networks. This list of topics is unified by a common theme: they relate to how an individual makes trade-offs involving options that are more or less “local.”<sup>16</sup>

**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 vari-

<sup>16</sup>Appendix C.2 contains details on the elicitation procedure for all variables discussed in this section.

ables 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) are potentially “bad controls” in the sense that these variables 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%.

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 suggests 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 asset classes, trade, and consumption goods. 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 do not trust that managers of foreign companies conduct their regular business and believe that they instead 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 universalism 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. This is motivated by the observation that, by definition, 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 weaker social networks that they can count on when in need, and feel socially isolated and 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' self-assessment of whether they live a fulfilled social life or feel rather lonely, on a scale from zero to ten.

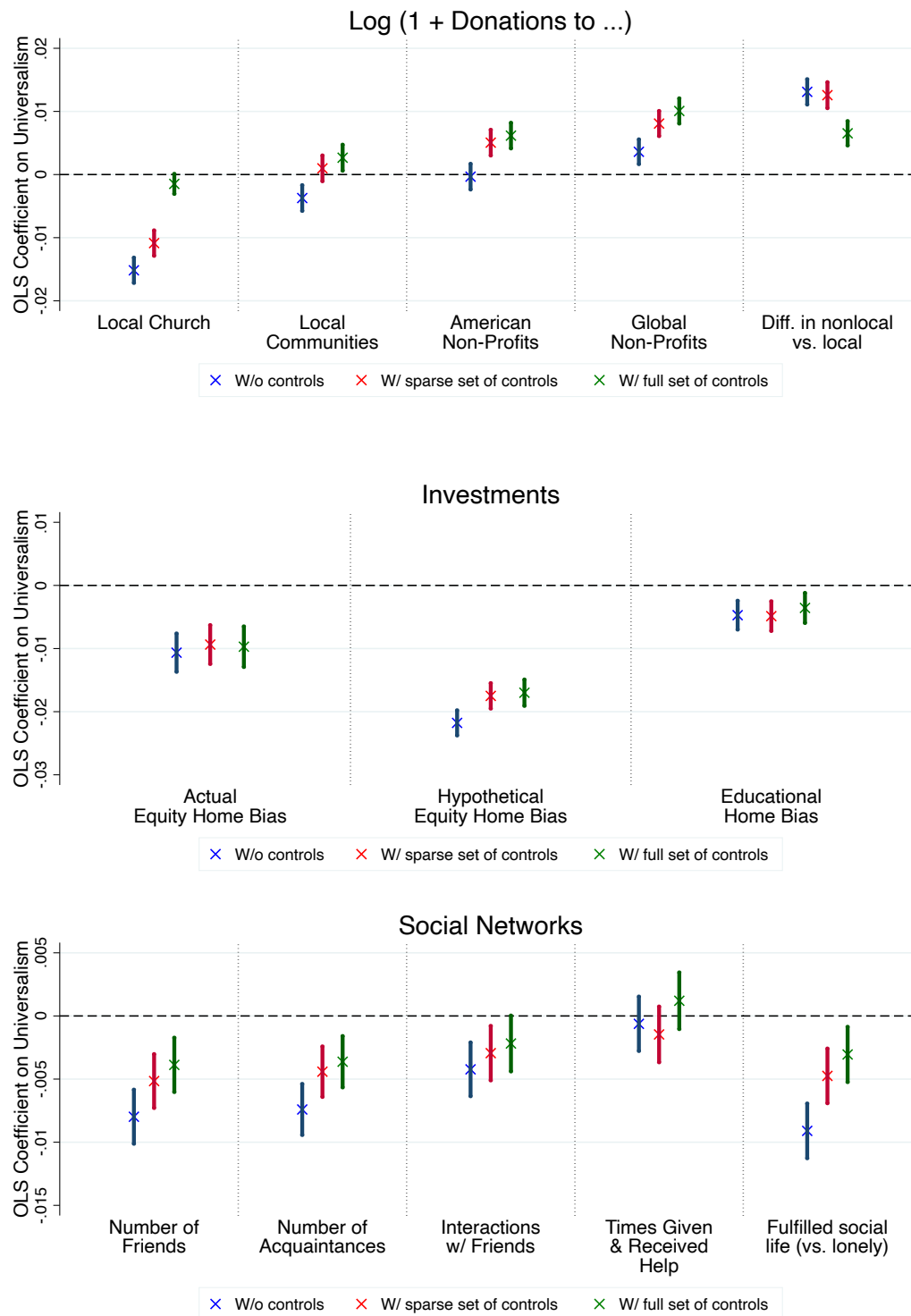


Figure 5: This figure presents OLS coefficients for the regression of a given outcome variable on the composite measure of moral universalism, along with 95% confidence intervals. All dependent variables are standardized into z-scores. Other than the first panel, the following outcome variables are in 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.

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.

**Robustness Checks.** The analyses presented above use the composite measure of universalism. Appendix F.2 reports robustness checks for universalism in altruism and universalism in trust separately; the results are always similar.<sup>17</sup>

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.4, we show that we get almost identical results in this sub-sample. In the Appendix, we also discuss the issue of order effects more generally and present a set of additional robustness checks.

## 7 Conclusion

Economists and psychologists have long been interested in different dimensions of in-group favoritism. In both social psychology and behavioral economics, the primary tool to study parochialism has been through laboratory experiments. These experiments have proved valuable in providing a nuanced understanding of the existence of and contributing factors to parochialism. At the same time, empirical work that links measures of in-group favoritism to economic behavior, or attempts to understand heterogeneity in the population, is rare.

This paper sought to build a bridge between the 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. By deploying this tool in a large-scale survey of the U.S. population, we have

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<sup>17</sup>Appendix F.3 additionally presents results making use of the instrumentation strategies of Gillen et al. (2019) to correct for measurement error.



derived three main results. First, universalism is a relatively domain-general parameter at the level of the individual that does not strongly depend on the specific in-group in question, which has direct implications for the measurement of universalism in applied work. Second, universalism exhibits considerable variation in the population, and this variation is systematically linked to observables such as age and religiosity. Finally, heterogeneity in universalism is systematically predictive of important dimensions of economic and social behavior.

In addition to our main elicitation tool for universalism, we have also provided a shorter measurement module that is easy and fast to implement, yet maintains almost all of the explanatory power of the full universalism measure. Our measures of universalism also include components on domestic universalism, foreign universalism, and global universalism, each of which could in principle be leveraged by researchers who are interested in these specific contexts. We hence believe that this paper opens up the possibility for more applied work on the role of universalism in economic decision making. In Enke et al. (2019), we take a first step in this direction by deploying our universalism measurement tool in a new seven-country survey to study the link between universalism and the structure of political ideology.

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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.

How would I split the money?



\$0	Randomly-Selected Person A who lives in the United States	A friend of a family member (e.g., your sibling's closest friend)	\$0
<hr/>			
\$0	Randomly-Selected Person B who lives in the United States	A member of your extended family (e.g., your cousin)	\$0
<hr/>			
\$0	Randomly-Selected Person C who lives in the United States	A former or current colleague at work or school	\$0
<hr/>			
\$0	Randomly-Selected Person D who lives in the United States	Someone who shares your religious beliefs (e.g., a fellow Christian)	\$0
<hr/>			
\$0	Randomly-Selected Person E who lives in the United States	A member of one of your past or current organizations (local church, leisure club or association, etc.)	\$0
<hr/>			

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.

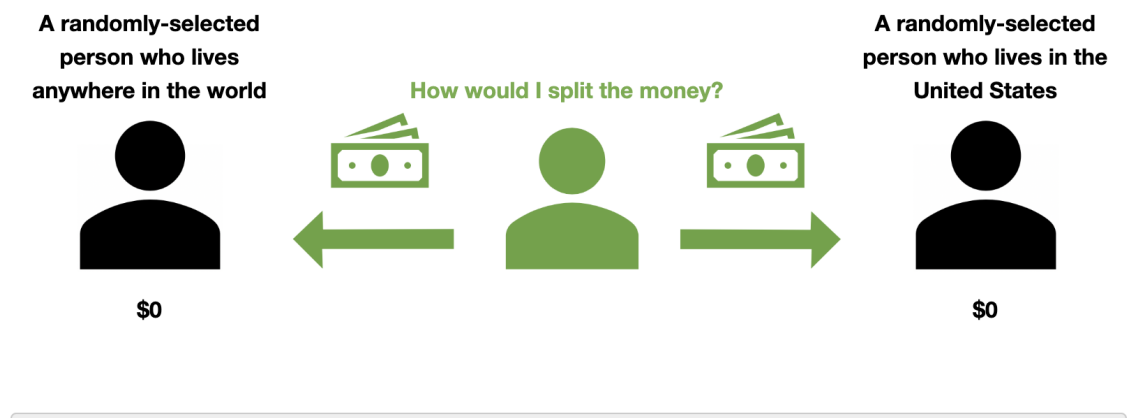



Figure 7: Example decision screen for foreign universalism in altruism. Across subjects, it was randomized whether the domestic social group appeared on the left or on the right. The layout for the task eliciting foreign universalism in *trust* is identical to this layout, with the exception of necessary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

Whom do I trust more?



I trust the individual on the left much more	I trust the two individuals to the same extent	I trust the individual on the right much more
<div style="display: flex; justify-content: space-between; align-items: flex-start;"> <span>0</span> <div style="text-align: center;"> <b>Randomly-Selected Person M who lives in the United States</b> </div> </div>		<div style="display: flex; justify-content: space-between; align-items: flex-start;"> <div style="text-align: center;"> <b>A friend of a family member (e.g., your sibling's closest friend)</b> </div> <span>0</span> </div>
<hr style="border: 1px solid #ccc;"/>		
<div style="display: flex; justify-content: space-between; align-items: flex-start;"> <span>0</span> <div style="text-align: center;"> <b>Randomly-Selected Person N who lives in the United States</b> </div> </div>		<div style="display: flex; justify-content: space-between; align-items: flex-start;"> <div style="text-align: center;"> <b>A member of your extended family (e.g., your cousin)</b> </div> <span>0</span> </div>
<hr style="border: 1px solid #ccc;"/>		
<div style="display: flex; justify-content: space-between; align-items: flex-start;"> <span>0</span> <div style="text-align: center;"> <b>Randomly-Selected Person O who lives in the United States</b> </div> </div>		<div style="display: flex; justify-content: space-between; align-items: flex-start;"> <div style="text-align: center;"> <b>A former or current colleague at work or school</b> </div> <span>0</span> </div>
<hr style="border: 1px solid #ccc;"/>		
<div style="display: flex; justify-content: space-between; align-items: flex-start;"> <span>0</span> <div style="text-align: center;"> <b>Randomly-Selected Person P who lives in the United States</b> </div> </div>		<div style="display: flex; justify-content: space-between; align-items: flex-start;"> <div style="text-align: center;"> <b>Someone who shares your religious beliefs (e.g., a fellow Christian)</b> </div> <span>0</span> </div>
<hr style="border: 1px solid #ccc;"/>		
<div style="display: flex; justify-content: space-between; align-items: flex-start;"> <span>0</span> <div style="text-align: center;"> <b>Randomly-Selected Person Q who lives in the United States</b> </div> </div>		<div style="display: flex; justify-content: space-between; align-items: flex-start;"> <div style="text-align: center;"> <b>A member of one of your past or current organizations (local church, leisure club or association, etc.)</b> </div> <span>0</span> </div>
<hr style="border: 1px solid #ccc;"/>		

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.

**Validation exercise.**  $N = 300$  U.S. MTurk Workers completed money allocation tasks with a larger selection of 25 domestic social groups and 15 global social groups.<sup>18</sup>

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 cor-

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<sup>18</sup>For lists of these groups, see Appendix A.3.



relation 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  $\rho = 0.08$  and the maximum 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.3 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 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 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.

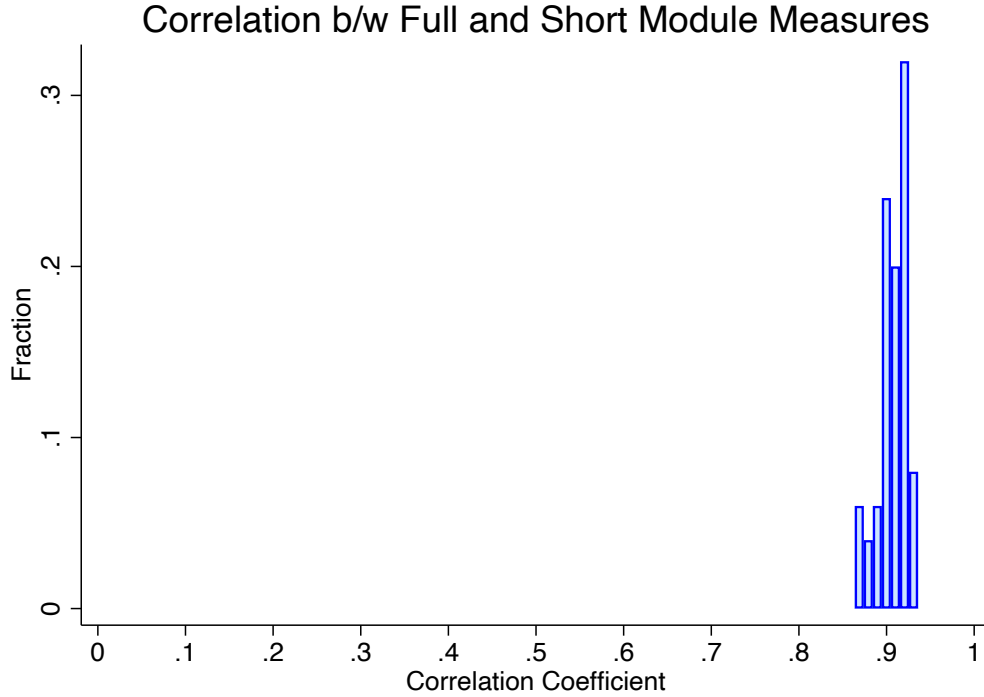


Figure 9: Distribution of correlation coefficients for all possible variants of a modular version of our measurement tool for universalism in altruism. Coefficients present the correlation between the given short measure and our full survey measure. The minimum of these correlation coefficients is  $\rho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

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 universalism in trust is  $\rho = 0.93$ ) is:

**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 randomly-selected 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

### C.1 Sample Characteristics of *Dynata* Survey

Category	Population (%)	Study Sample (%)	
		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)			

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 their country of residence, in which the most unequal split of money maximizes total payoffs. The measure captures how much a subject deviates 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) self-described 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 approximate 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

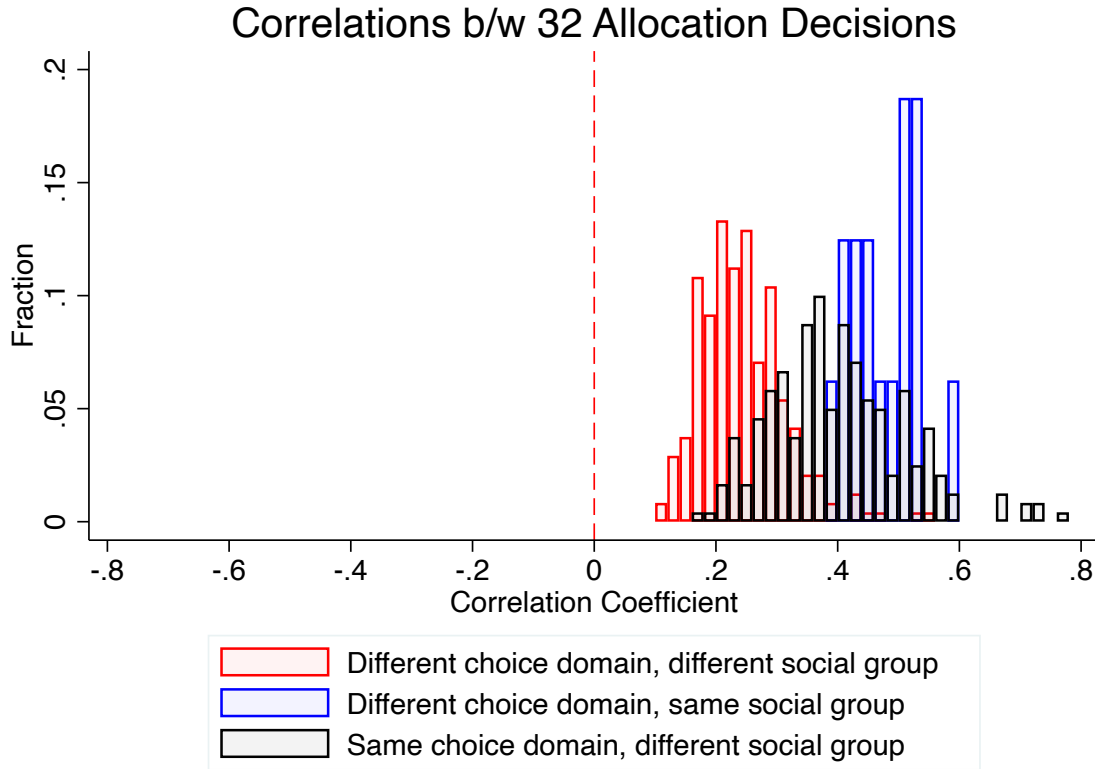


Figure 10: This figure presents the distribution of all pairwise correlation coefficients for the set of all money and trust point allocations in our survey. In red, we plot the correlation coefficients for decisions involving different choice domains (altruism vs. trust) *and* a different social group.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

Table 2: Correlations between Universalism Measures

(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

\*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

## D.2 Histograms of Allocation Decisions

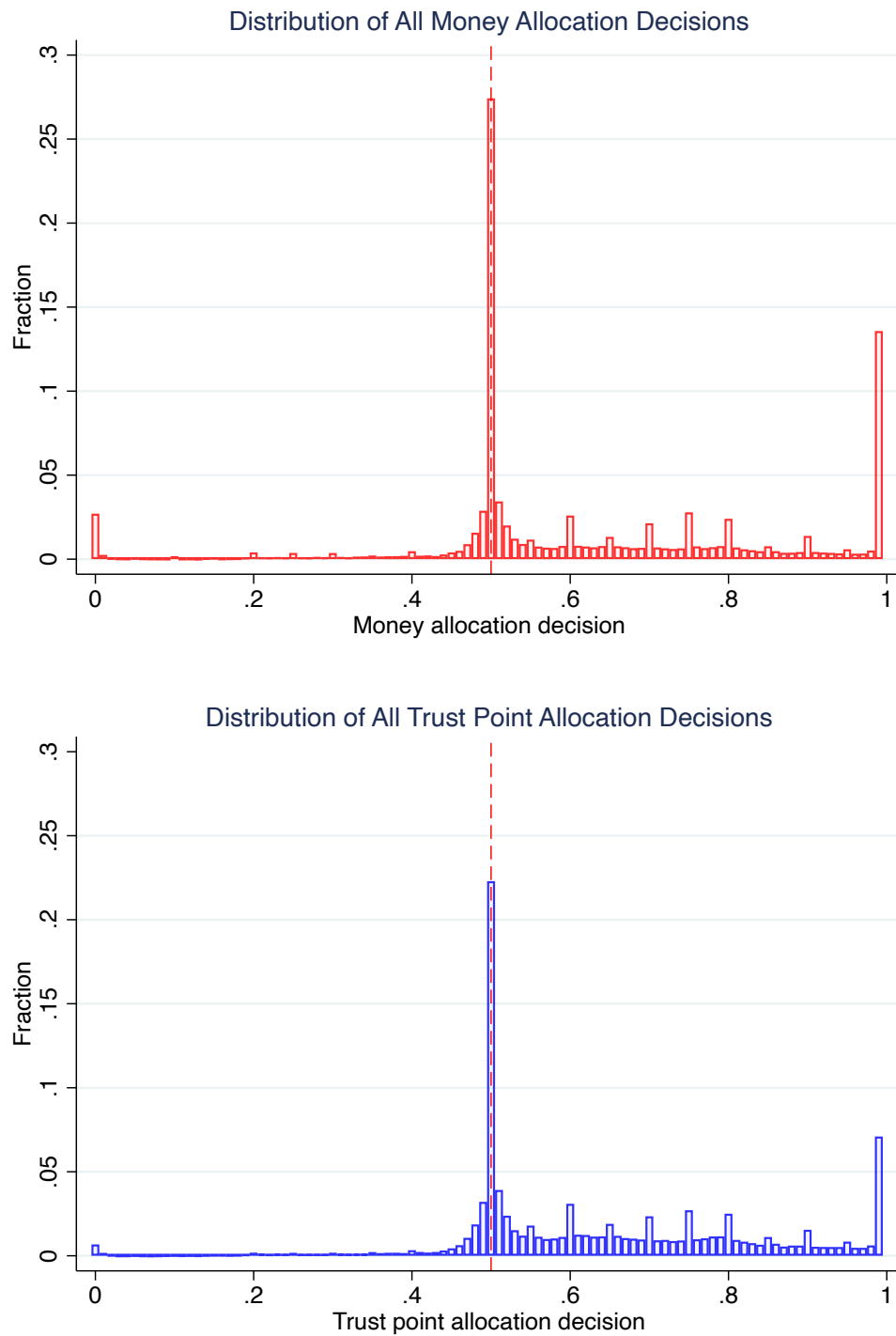


Figure 11: Distributions of all money and trust point allocations to in-group members in our full sample.

## E Additional Analyses on Heterogeneity and Correlates

### E.1 Heterogeneity

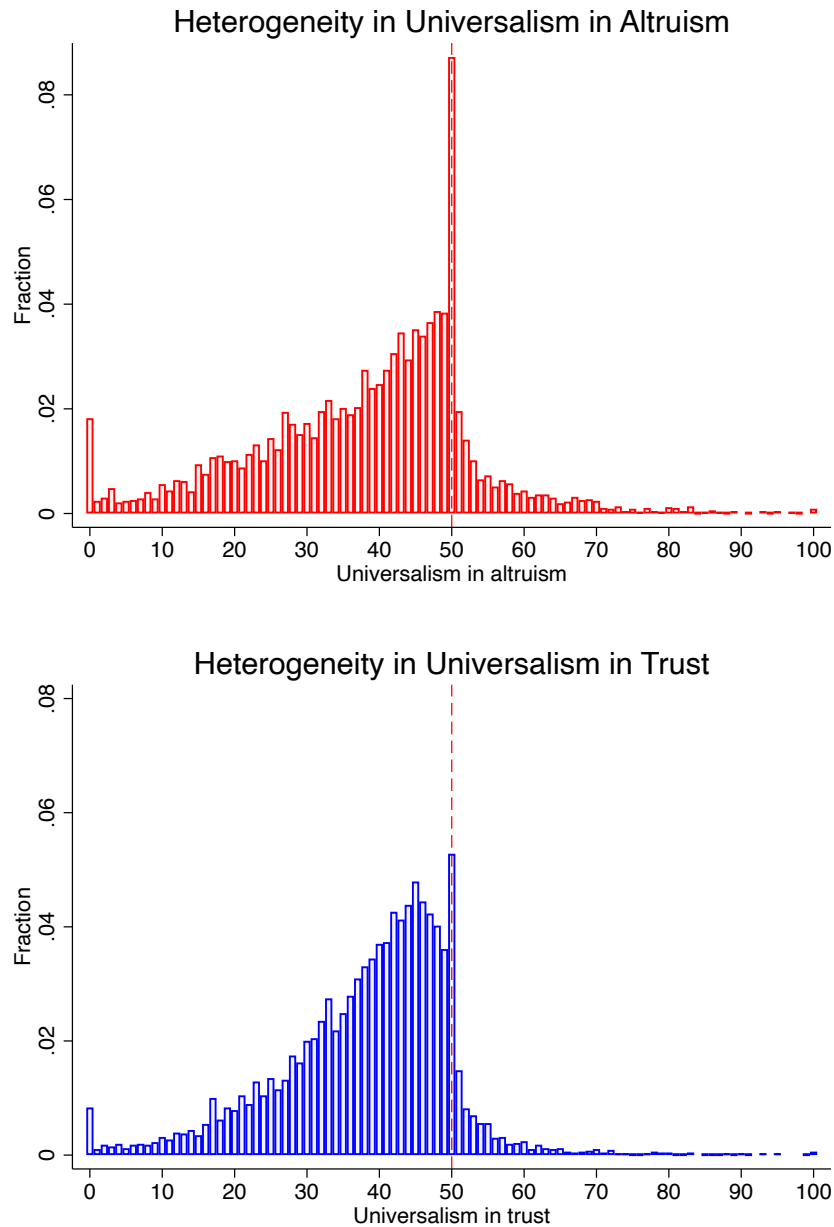


Figure 12: Distributions of universalism in altruism and in trust across our sample of the U.S. population. The measures average each subject's money allocation and trust point decisions, respectively, across a variety of social groups. These average amounts reflect allocations to random strangers, so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

## **E.2 Sociodemographics**

Table 3: Sociodemographic correlates of universalism

	<i>Dependent variable: Composite measure of moral universalism</i>											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(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	6591	6591	6591	6591	6591	6591	6591	6591	6491	6591	6491	6491
R <sup>2</sup>	0.05	0.01	0.05	0.00	0.01	0.00	0.02	0.02	0.00	0.04	0.08	0.08

Notes. OLS estimates, robust standard errors in parentheses. Each observation is one subject. The dependent variable is the composite measure of moral universalism, corresponding to the average of all allocation decisions across social groups and across both altruism and trust. In columns 7, 9, and 10, we have a smaller number of observations as population densities could not be obtained for all subjects. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .



Table 4: Sociodemographic correlates of universalism in altruism

	<i>Dependent variable: Moral universalism w.r.t. altruism, all groups</i>											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(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	6591	6591	6591	6591	6591	6591	6591	6591	6491	6591	6491	6491
R <sup>2</sup>	0.05	0.01	0.05	0.00	0.01	0.00	0.01	0.01	0.00	0.03	0.07	0.07

Notes. OLS estimates, robust standard errors in parentheses. Each observation is one subject. The dependent variable is the measure of moral universalism with respect to altruism and all groups, corresponding to the average of all allocation decisions across social groups and across altruism. In columns 7, 9, and 10, we have a smaller number of observations as population densities could not be obtained for all subjects. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 5: Sociodemographic correlates of universalism in trust

	<i>Dependent variable: Moral universalism w.r.t. trust, all groups</i>											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(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	6591	6591	6591	6591	6591	6591	6591	6591	6491	6591	6491	6491
R <sup>2</sup>	0.02	0.00	0.03	0.00	0.00	0.00	0.01	0.01	0.00	0.04	0.06	0.06

Notes. OLS estimates, robust standard errors in parentheses. Each observation is one subject. The dependent variable is the measure of moral universalism with respect to trust and all groups, corresponding to the average of all allocation decisions across social groups and across trust. In columns 7, 9, and 10, we have a smaller number of observations as population densities could not be obtained for all subjects. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

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

Table 6: Correlations between Universalism and Social Attitudes

Social preference	Universalism w.r.t...	Social group(s) considered	$\rho$
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

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

## **F Additional Analyses on Economic Behaviors and Outcomes**

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

Table 7: Universalism and behaviors / outcomes

	Local Church			Local Communities			Dependent variable: <i>Log (1+Donations to ...)</i>					Global Non-Profits			Diff. in nonlocal vs. local		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)		
	American Non-Profits			American Non-Profits			American Non-Profits			American Non-Profits			American Non-Profits				
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)		
Age	0.0093*** (0.00)	0.0048*** (0.00)	0.0048*** (0.00)	0.0062*** (0.00)	0.0062*** (0.00)	0.0054*** (0.00)	0.0097*** (0.00)	0.0097*** (0.00)	0.0092*** (0.00)	0.0092*** (0.00)	0.0068*** (0.00)	0.0062*** (0.00)	-0.0023*** (0.00)	-0.0023*** (0.00)	0.00085 (0.00)		
Male	0.043* (0.02)	0.11*** (0.02)	0.11*** (0.02)	0.033 (0.03)	0.033 (0.03)	0.036 (0.02)	0.035 (0.02)	0.035 (0.02)	0.028 (0.02)	0.028 (0.02)	0.091*** (0.03)	0.091*** (0.02)	0.011 (0.03)	0.011 (0.03)	-0.046* (0.02)		
Cognitive skills [0-5]	-0.016 (0.01)	0.020*** (0.01)	0.020*** (0.01)	-0.0039 (0.01)	-0.0039 (0.01)	-0.0054 (0.01)	0.021** (0.01)	0.021** (0.01)	0.014 (0.01)	0.014 (0.01)	0.0043 (0.01)	0.0032 (0.01)	0.013 (0.01)	0.013 (0.01)	-0.016* (0.01)		
Income and Wealth Index (z-score)	0.16*** (0.01)	0.11*** (0.01)	0.11*** (0.01)	0.25*** (0.01)	0.25*** (0.01)	0.22*** (0.01)	0.26*** (0.01)	0.26*** (0.01)	0.21*** (0.01)	0.21*** (0.01)	0.23*** (0.01)	0.19*** (0.01)	0.0088 (0.01)	0.0088 (0.01)	0.017 (0.01)		
African-American	0.28*** (0.04)	0.023 (0.04)	0.023 (0.04)	-0.088** (0.04)	-0.088** (0.04)	-0.12*** (0.04)	0.039 (0.04)	0.039 (0.04)	-0.030 (0.04)	-0.030 (0.04)	0.041 (0.04)	-0.044 (0.04)	-0.15*** (0.04)	-0.15*** (0.04)	-0.031 (0.04)		
Native American	0.049 (0.13)	0.079 (0.11)	0.079 (0.11)	0.16 (0.13)	0.16 (0.13)	0.18 (0.13)	0.20 (0.14)	0.20 (0.14)	0.23* (0.13)	0.23* (0.13)	0.088 (0.13)	0.12 (0.13)	0.028 (0.13)	0.028 (0.13)	0.052 (0.12)		
Asian	0.043 (0.05)	0.048 (0.04)	0.048 (0.04)	-0.082 (0.06)	-0.082 (0.06)	-0.11** (0.06)	0.032 (0.06)	0.032 (0.06)	-0.066 (0.06)	-0.066 (0.06)	0.17*** (0.06)	0.091 (0.06)	0.083 (0.06)	0.083 (0.06)	0.011 (0.06)		
Hispanic	0.061 (0.05)	-0.059 (0.04)	-0.059 (0.04)	-0.13*** (0.05)	-0.13*** (0.05)	-0.14*** (0.05)	-0.028 (0.05)	-0.028 (0.05)	-0.076 (0.05)	-0.076 (0.05)	-0.019 (0.05)	-0.068 (0.05)	-0.034 (0.05)	-0.034 (0.05)	0.0058 (0.05)		
Other non-white	0.085 (0.09)	0.065 (0.07)	0.065 (0.07)	0.11 (0.10)	0.11 (0.10)	0.096 (0.10)	0.18* (0.10)	0.18* (0.10)	0.15 (0.10)	0.15 (0.10)	0.082 (0.10)	0.062 (0.10)	0.011 (0.09)	0.011 (0.09)	0.012 (0.08)		
College-educated		0.16*** (0.02)	0.16*** (0.02)			0.21*** (0.02)			0.27*** (0.02)	0.27*** (0.02)					0.014 (0.02)		
Urbanicity Index (z-score)		-0.0026 (0.01)	-0.0026 (0.01)			-0.026** (0.01)			0.066*** (0.01)	0.066*** (0.01)					0.083*** (0.01)		
Religiosity Index (z-score)		0.65*** (0.01)	0.65*** (0.01)			0.11*** (0.01)			0.094*** (0.01)	0.094*** (0.01)					-0.41*** (0.01)		
Observations	6591	6591	6491	6591	6591	6491	6591	6591	6491	6591	6591	6491	6591	6591	6491		
R <sup>2</sup>	0.03	0.09	0.48	0.00	0.09	0.11	0.00	0.11	0.14	0.00	0.08	0.11	0.02	0.03	0.19		

Notes. OLS estimates, robust standard errors in parentheses. Each observation is one subject. The independent variable is our composite measure of universalism, which averages all decisions across all social groups and across both altruism and trust. All dependent variables are in logs, and then standardized into z-scores. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 8: Universalism and behaviors / outcomes

	No. of friends			No. of acquaintances			Dependent variable: Interaction w/ friends					Times rec'd and given help			Fulfilled social life		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)
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)	-0.0030*** (0.00)	-0.0030*** (0.00)
Age	0.0028*** (0.00)	0.0028*** (0.00)	0.0020** (0.00)	0.0041*** (0.00)	0.0041*** (0.00)	0.0034*** (0.00)	0.00020 (0.00)	-0.00062 (0.00)	-0.00062 (0.00)	-0.00062 (0.00)	-0.0043*** (0.00)	-0.0055*** (0.00)	0.0074*** (0.00)	0.0074*** (0.00)	0.0064*** (0.00)	0.0064*** (0.00)	0.0064*** (0.00)
Male	0.028 (0.03)	0.028 (0.03)	0.031 (0.03)	0.0069 (0.03)	0.0069 (0.03)	0.0065 (0.03)	0.11*** (0.03)	0.12*** (0.03)	0.12*** (0.03)	0.12*** (0.03)	-0.033 (0.03)	-0.0078 (0.03)	-0.028 (0.03)	-0.028 (0.03)	-0.018 (0.02)	-0.018 (0.02)	-0.018 (0.02)
Cognitive skills [0-5]	0.026*** (0.01)	0.026*** (0.01)	0.025** (0.01)	0.091*** (0.01)	0.091*** (0.01)	0.084*** (0.01)	-0.021** (0.01)	-0.021** (0.01)	-0.016 (0.01)	-0.016 (0.01)	-0.0042 (0.01)	0.0076 (0.01)	-0.028*** (0.01)	-0.028*** (0.01)	-0.024** (0.01)	-0.024** (0.01)	-0.024** (0.01)
Income and Wealth Index (z-score)	0.17*** (0.02)	0.17*** (0.02)	0.13*** (0.02)	0.19*** (0.02)	0.19*** (0.02)	0.14*** (0.02)	0.062*** (0.01)	0.062*** (0.01)	0.047*** (0.02)	0.047*** (0.02)	0.051*** (0.01)	0.041*** (0.01)	0.21*** (0.02)	0.21*** (0.02)	0.18*** (0.02)	0.18*** (0.02)	0.18*** (0.02)
African-American	-0.35*** (0.05)	-0.35*** (0.05)	-0.40*** (0.05)	-0.42*** (0.04)	-0.42*** (0.04)	-0.45*** (0.04)	-0.13*** (0.05)	-0.13*** (0.05)	-0.16*** (0.05)	-0.16*** (0.05)	0.040 (0.05)	-0.0052 (0.05)	0.10** (0.05)	0.10** (0.05)	0.044 (0.05)	0.044 (0.05)	0.044 (0.05)
Native American	0.031 (0.17)	0.031 (0.17)	0.048 (0.16)	-0.064 (0.17)	-0.064 (0.17)	-0.100 (0.17)	-0.025 (0.19)	-0.025 (0.19)	-0.0099 (0.19)	-0.0099 (0.19)	0.025 (0.16)	0.051 (0.16)	-0.13 (0.17)	-0.13 (0.17)	-0.13 (0.16)	-0.13 (0.16)	-0.13 (0.16)
Asian	0.043 (0.06)	0.043 (0.06)	-0.019 (0.06)	-0.19*** (0.06)	-0.19*** (0.06)	-0.25*** (0.06)	-0.17*** (0.06)	-0.17*** (0.06)	-0.19*** (0.06)	-0.19*** (0.06)	-0.28*** (0.05)	-0.23*** (0.05)	0.0070 (0.05)	0.0070 (0.05)	-0.035 (0.05)	-0.035 (0.05)	-0.035 (0.05)
Hispanic	-0.097* (0.05)	-0.097* (0.05)	-0.12** (0.05)	-0.18*** (0.05)	-0.18*** (0.05)	-0.20*** (0.05)	-0.018 (0.05)	-0.018 (0.05)	-0.034 (0.05)	-0.034 (0.05)	-0.013 (0.05)	-0.011 (0.06)	0.12** (0.05)	0.12** (0.05)	0.092* (0.05)	0.092* (0.05)	0.092* (0.05)
Other non-white	-0.018 (0.11)	-0.018 (0.11)	-0.042 (0.11)	0.031 (0.11)	0.031 (0.11)	0.0097 (0.11)	0.034 (0.10)	0.034 (0.10)	0.0061 (0.10)	0.0061 (0.10)	0.23** (0.11)	0.25** (0.11)	-0.10 (0.09)	-0.10 (0.09)	-0.14* (0.08)	-0.14* (0.08)	-0.14* (0.08)
College-educated	0.25*** (0.03)	0.25*** (0.03)	0.25*** (0.03)	0.28*** (0.03)	0.28*** (0.03)	0.28*** (0.03)	0.074*** (0.03)	0.074*** (0.03)	0.074*** (0.03)	0.074*** (0.03)	0.048* (0.03)	0.048* (0.03)	0.16*** (0.03)	0.16*** (0.03)	0.16*** (0.03)	0.16*** (0.03)	0.16*** (0.03)
Urbanicity Index (z-score)	0.024* (0.01)	0.024* (0.01)	0.024* (0.01)	-0.00026 (0.01)	-0.00026 (0.01)	-0.00026 (0.01)	0.0085 (0.01)	0.0085 (0.01)	0.0085 (0.01)	0.0085 (0.01)	-0.057*** (0.01)	-0.057*** (0.01)	0.026** (0.01)	0.026** (0.01)	0.026** (0.01)	0.026** (0.01)	0.026** (0.01)
Religiosity Index (z-score)	0.12*** (0.01)	0.12*** (0.01)	0.12*** (0.01)	0.086*** (0.01)	0.086*** (0.01)	0.086*** (0.01)	0.073*** (0.01)	0.073*** (0.01)	0.073*** (0.01)	0.073*** (0.01)	0.17*** (0.01)	0.17*** (0.01)	0.15*** (0.01)	0.15*** (0.01)	0.15*** (0.01)	0.15*** (0.01)	0.15*** (0.01)
Observations	6591	6591	6491	6591	6591	6491	6591	6591	6491	6591	6591	6491	6591	6591	6491	6491	6491
R <sup>2</sup>	0.01	0.06	0.09	0.01	0.09	0.11	0.00	0.01	0.02	0.00	0.01	0.04	0.01	0.08	0.11	0.11	0.11

Notes. OLS estimates, robust standard errors in parentheses. Each observation is one subject. The independent variable is our composite measure of universalism, which averages all decisions across all social groups and across both altruism and trust. Number of friends, number of acquaintances, interactions with friends, and times received and given help are all in logs. All dependent variables are standardized into z-scores. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 9: Universalism and behaviors / outcomes

	Actual home bias			Dependent variable: Hypothetical home bias			Educational Home Bias		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Composite universalism	-0.011*** (0.00)	-0.0094*** (0.00)	-0.0097*** (0.00)	-0.022*** (0.00)	-0.017*** (0.00)	-0.017*** (0.00)	-0.0047*** (0.00)	-0.0049*** (0.00)	-0.0036*** (0.00)
Age		0.0056*** (0.00)	0.0056*** (0.00)		0.013*** (0.00)	0.013*** (0.00)		0.0011 (0.00)	0.00057 (0.00)
Male		-0.0030 (0.04)	0.0017 (0.04)		-0.035 (0.02)	-0.028 (0.02)		0.019 (0.03)	0.044* (0.03)
Cognitive skills [0-5]		0.015 (0.02)	0.015 (0.02)		-0.020** (0.01)	-0.015 (0.01)		-0.0088 (0.01)	0.0058 (0.01)
Income and Wealth Index (z-score)		-0.040 (0.04)	-0.012 (0.04)		-0.0028 (0.01)	0.0049 (0.01)		-0.089*** (0.01)	-0.051*** (0.01)
African-American		-0.11 (0.11)	-0.096 (0.11)		-0.30*** (0.05)	-0.29*** (0.05)		-0.17*** (0.05)	-0.15*** (0.05)
Native American		-0.044 (0.26)	0.094 (0.24)		-0.068 (0.15)	-0.084 (0.15)		0.036 (0.18)	-0.0091 (0.17)
Asian		0.038 (0.08)	0.074 (0.08)		-0.18*** (0.06)	-0.15** (0.06)		-0.17*** (0.06)	-0.055 (0.06)
Hispanic		0.057 (0.10)	0.057 (0.10)		-0.013 (0.05)	-0.0015 (0.05)		-0.13** (0.06)	-0.095 (0.06)
Other non-white		0.14 (0.13)	0.14 (0.12)		-0.058 (0.10)	-0.055 (0.10)		-0.12 (0.10)	-0.050 (0.10)
College-educated			-0.15*** (0.05)			-0.045* (0.03)			-0.25*** (0.03)
Urbanicity Index (z-score)			-0.018 (0.02)			-0.033*** (0.01)			-0.081*** (0.01)
Religiosity Index (z-score)			-0.033 (0.02)			0.034*** (0.01)			0.045*** (0.01)
Observations	2908	2908	2871	6591	6591	6491	6591	6591	6491
R <sup>2</sup>	0.02	0.03	0.03	0.07	0.13	0.13	0.00	0.01	0.04

Notes. OLS estimates, robust standard errors in parentheses. Each observation is one subject. The independent variable is our composite measure of universalism, which averages all decisions across all social groups and across both altruism and trust. All dependent variables are standardized into z-scores. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

## F.2 Universalism in Altruism and Universalism in Trust

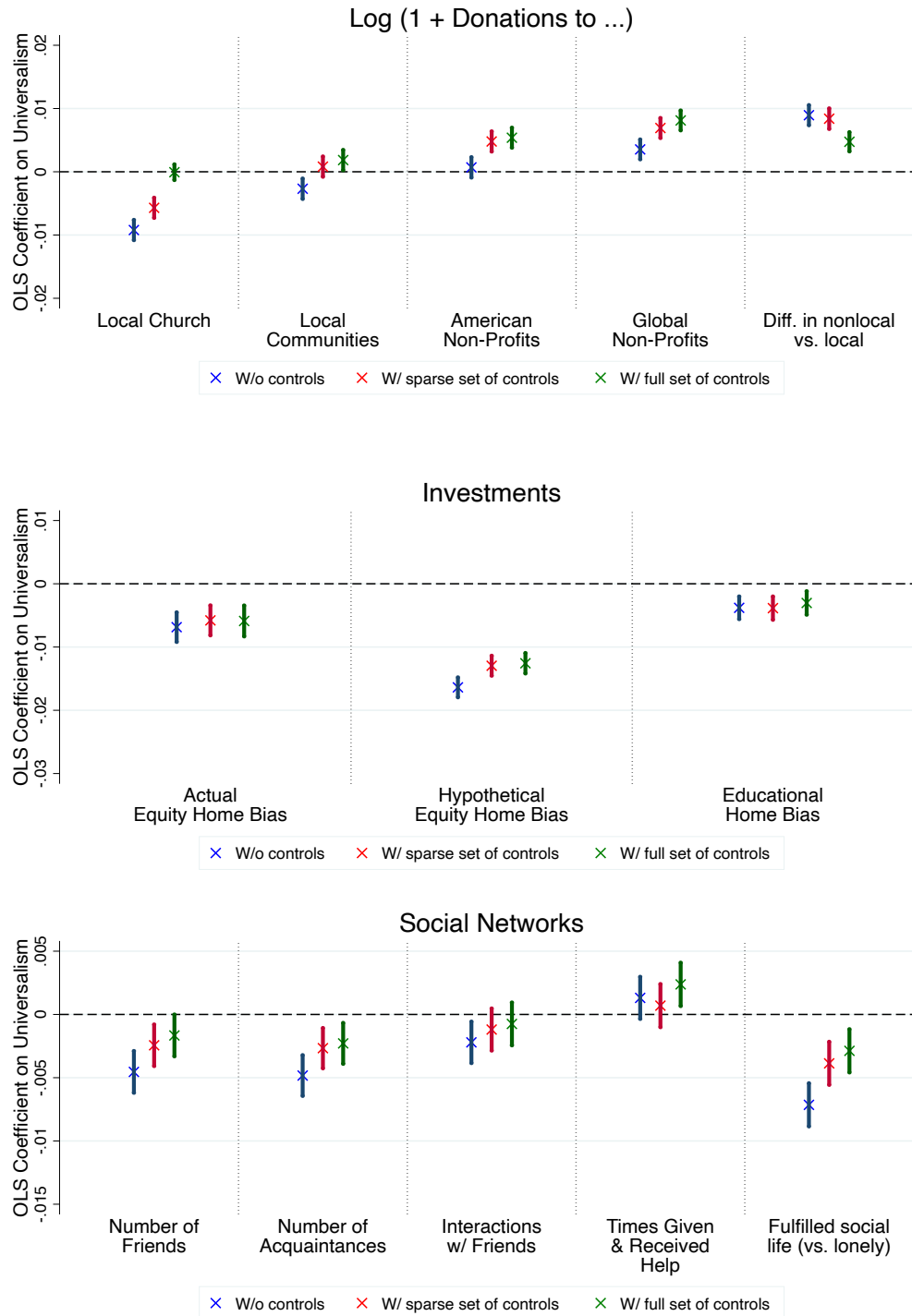


Figure 13: This figure presents OLS coefficients for the regression of the given outcome variable on our measure of universalism w.r.t. altruism, along with 95% confidence intervals. Other than the first panel, the following outcome variables are in logs: (1) number of friends, (2) number of acquaintances, (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.



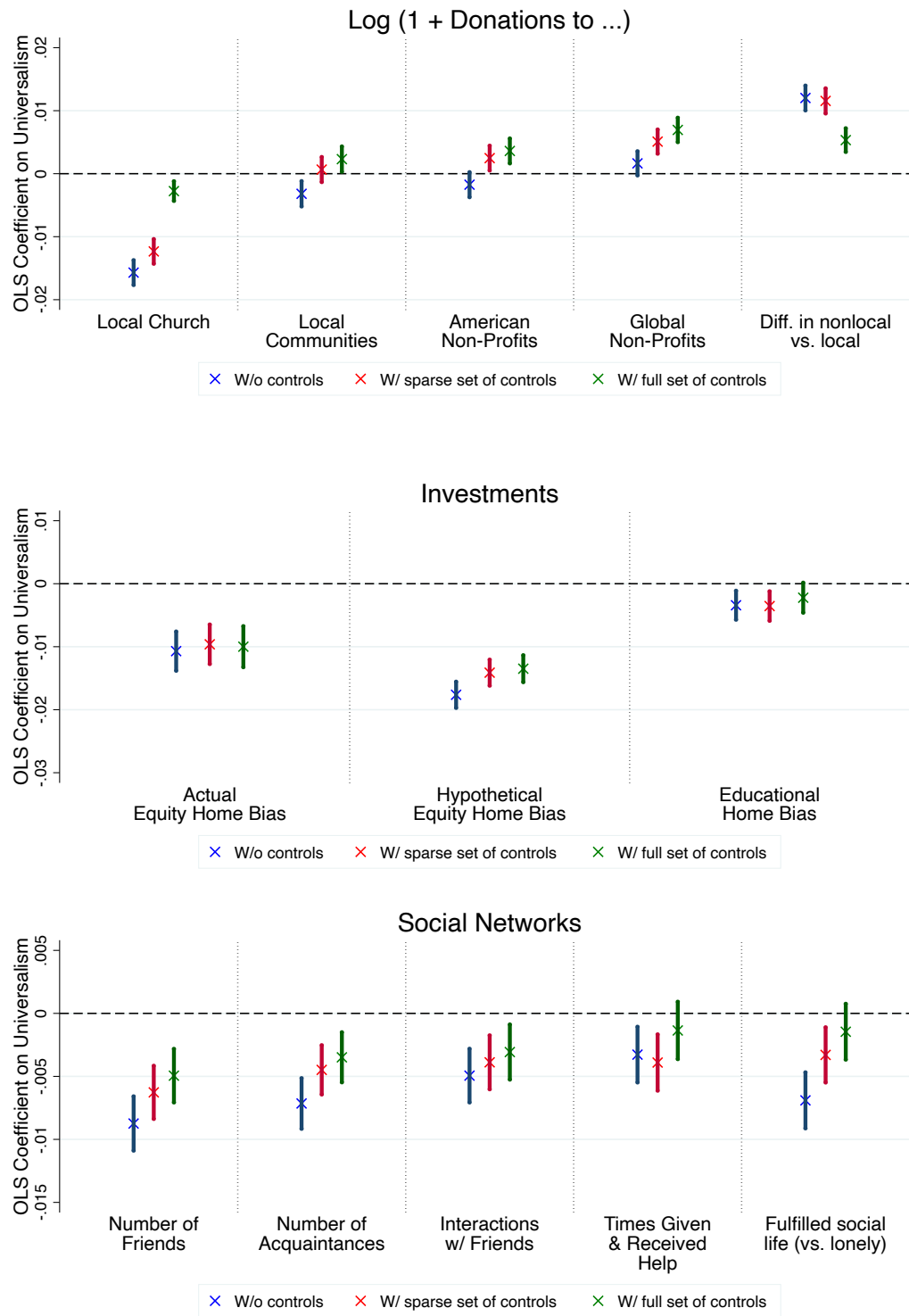


Figure 14: This figure presents OLS coefficients for the regression of the given outcome variable on our measure of universalism w.r.t. trust, along with 95% confidence intervals. Other than the first panel, the following outcome variables are in logs: (1) number of friends, (2) number of acquaintances, (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.3 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 15, 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.

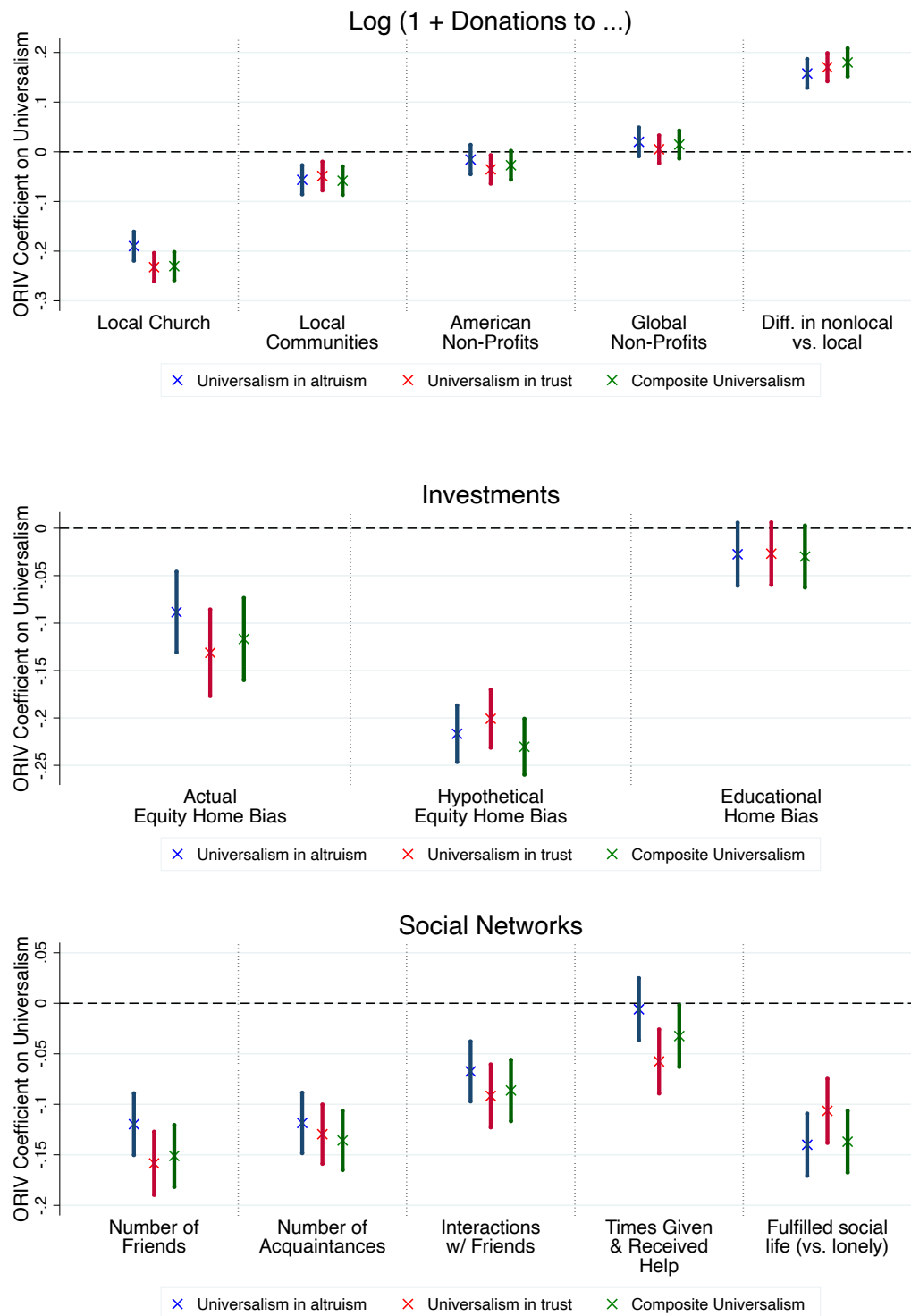


Figure 15: This figure presents coefficients for the stacked, ORIV regression of the given outcome variables on duplicate elicitations of our composite measure of moral universalism, along with 95% confidence intervals. As recommended by Gillen et al. (2019), both the universalism measures and outcome 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.4 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.

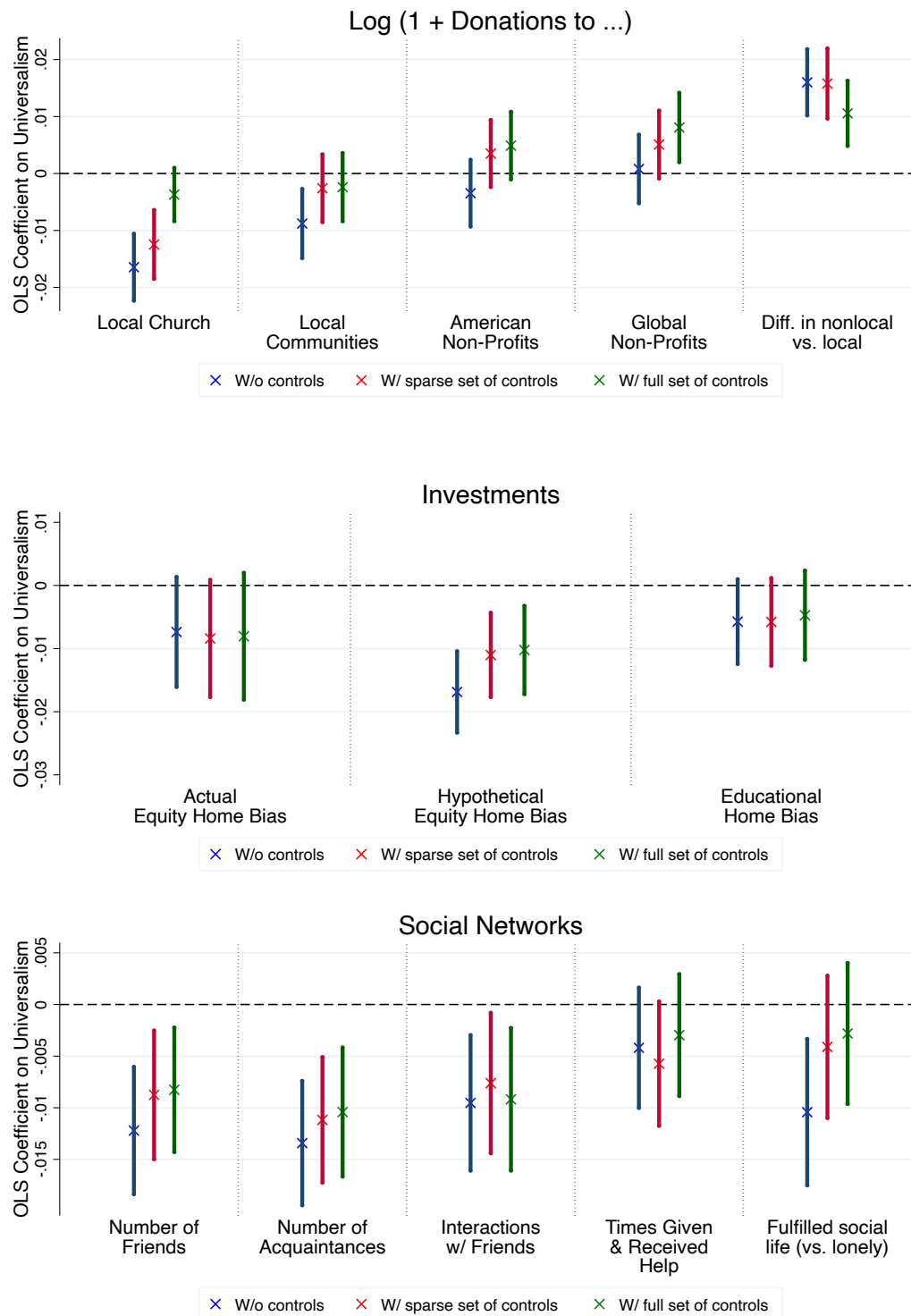


Figure 16: This figure presents OLS coefficients for the regression of a given outcome variable on the composite measure of moral universalism, along with 95% confidence intervals. The sample consists only of those respondents who completed elicitations of outcome variables before measurement of moral 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.

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

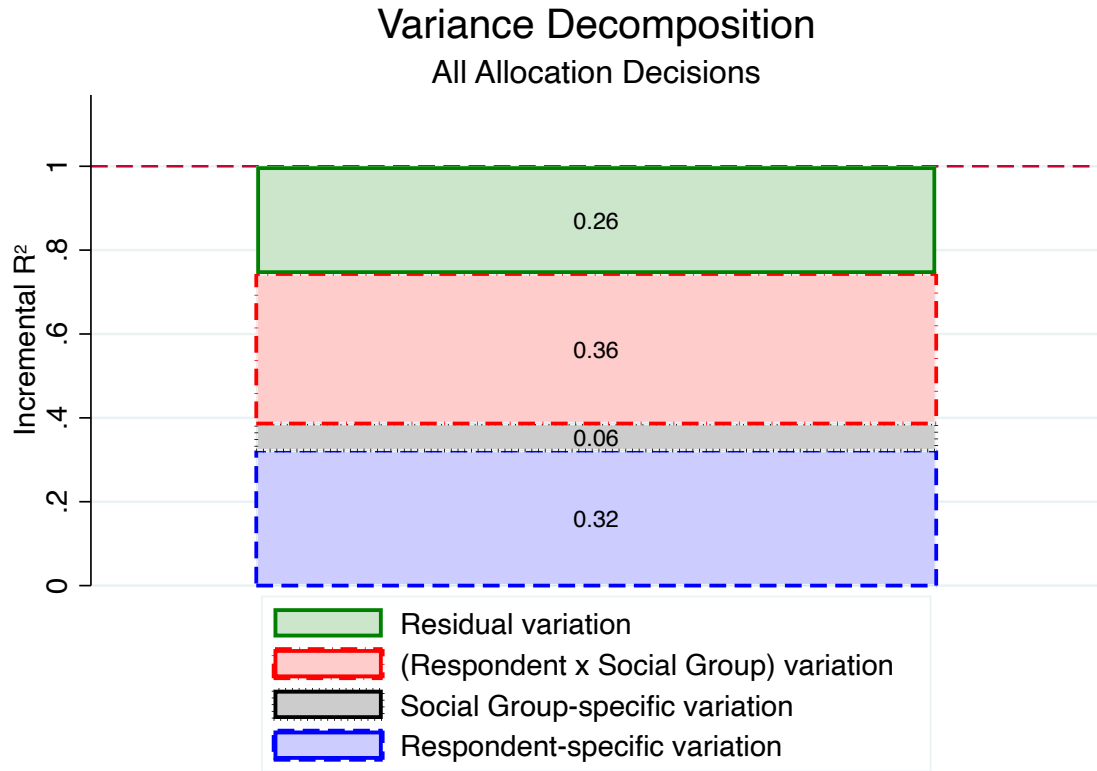


Figure 17: This figure presents the incremental  $R^2$  for a series of stacked regressions of all 32 allocation decisions made by all respondents in the representative sample of our survey on various fixed effects. That is, we stack all allocation decisions across both choice domains into a single column, 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.

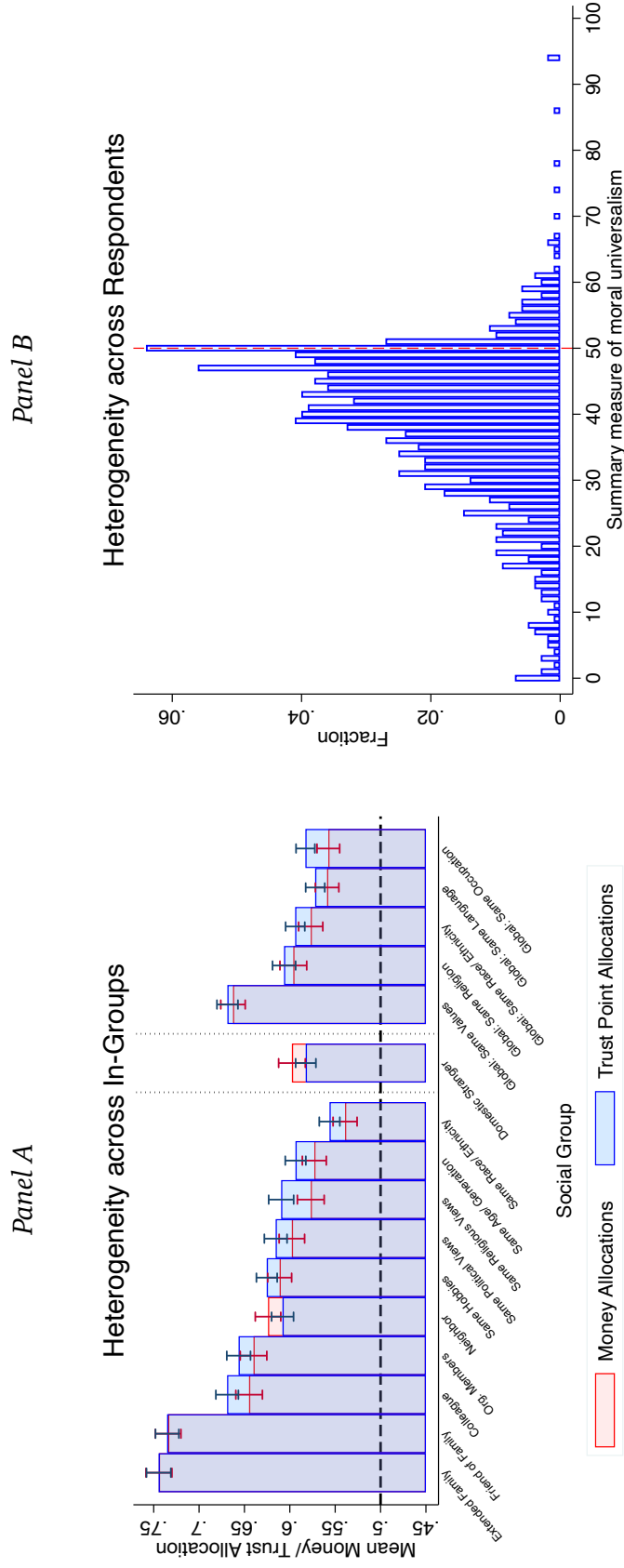


Figure 18: This figure presents heterogeneity in decision-making along two key components of the variation in data from our universalism tasks. In Panel A, we present *in-group-specific variation*: average allocations of money and trust points given to every social group across all subjects, along with 95% confidence intervals. The first ten bars show average allocations to domestic in-groups in tradeoffs vis-à-vis a domestic stranger. Bars 12–16 show average allocations to a global in-group member vis-à-vis a global stranger. The 11th bar shows the average allocation to a domestic stranger relative to a global stranger. Bars 12–16 show average allocations to a global in-group member vis-à-vis a domestic stranger. Each bar is composed of  $N = 999$  individuals. In Panel B, we present *respondent-specific variation*: the distribution of composite measure of moral universalism across our representative sample of the U.S. population. The measure is a summary statistic averaging each subject's allocation decisions across both altruism and trust and across all social groups. These average amounts reflect allocations to random strangers, so that the measure is decreasing in subjects' in-group favoritism.

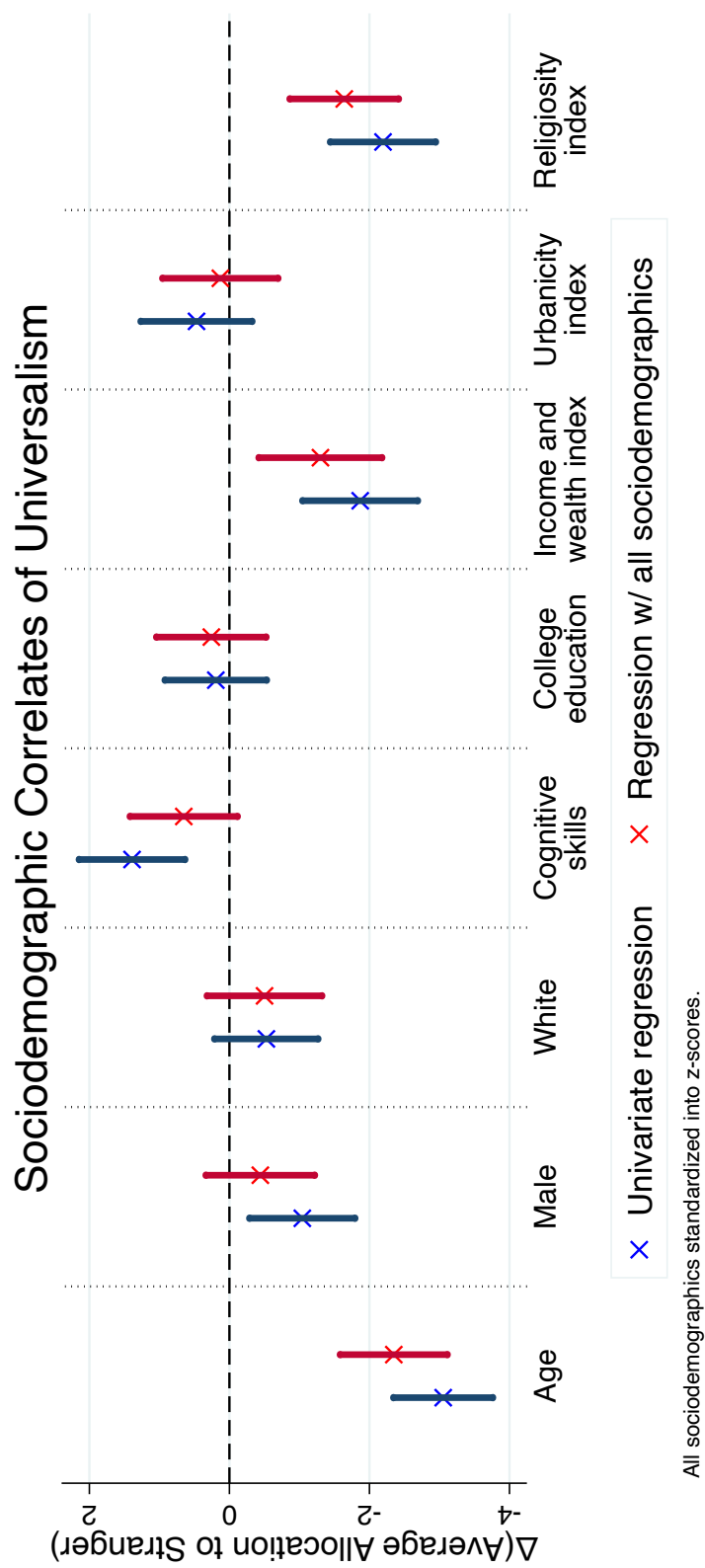


Figure 19: This figure presents OLS estimates from regressions of the composite measure of moral universalism on each of the given sociodemographic correlates, along with 95% confidence intervals. Since moral universalism increases with allocation amounts to a stranger vis-à-vis an in-group member, negative 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 17.5 years. For the male indicator, one standard deviation is 0.49, and for the white indicator, one standard deviation is equal to 0.48.



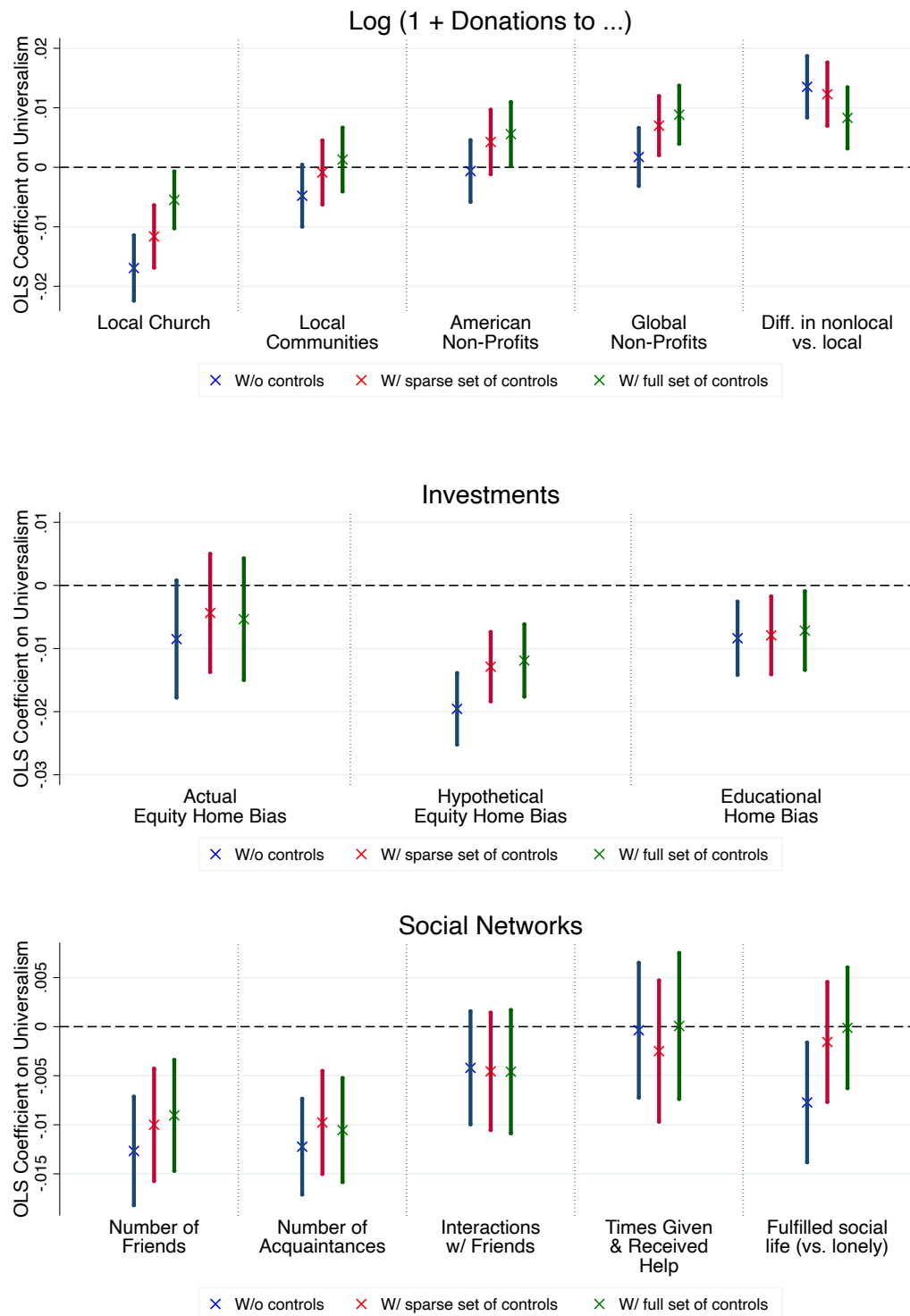


Figure 20: This figure presents OLS coefficients for the regression of the given outcome variable on our composite measure of moral universalism, along with 95% confidence intervals. All dependent variables are standardized into z-scores. Other than the first panel, the following outcome variables are 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.