Inequality and Giving

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Abstract

Standard theories predict that greater inequality will increase charitable giving, though tax return data suggest the opposite may be true. We develop a model which, incorporating insights from behavioral economics and social psychology, allows the distribution of resources to affect giving decisions. We test the theory in an experiment on donations to a real-world charity. By randomizing the income distribution, we identify the causal effect of inequality on giving behavior. We find greater inequality causes giving to fall.

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In much of the world, the inequality of income and wealth have risen rapidly.[1] Economic inequality could inspire political disaffection, reduce social cohesion, and lessen equality of opportunity. We show that rising inequality causes a reduction of a crucial prosocial behavior: charitable giving.

Standard models predict charitable giving will increase when inequality rises, because concentration of resources can mitigate free-riding. In contrast to the prevailing public goods theory of giving, this paper models the giving decision in a way where the concentration of resources itself can affect the utility of giving. The effect of this behavioral parameter is ambiguous: Inequality could increase giving by provoking empathy of those with disproportionate resources, and therefore increase the giving of the rich beyond a pure income effect, or greater inequality could depress giving by weakening social cohesion.

We estimate causal effects in an experiment that presents subjects with an opportunity to donate to a partner charity under exogenously varied inequality conditions. By isolating the effect of inequality, we differentiate our research from related studies of social pressures and giving behavior (DellaVigna et al. 2012; Andreoni et al. 2017a; List 2011). Other empirical work analyzing the relationship between inequality and giving is correlational in nature, and the results are ambiguous (Payne and Smith 2015; Duquette 2018).

Our findings are clear: increasing inequality significantly decreases total donations. A 1% increase in inequality decreases donations by about 0.2%. Further investigation demonstrates that this inequality effect is robust to functional form assumptions and the inclusion of additional controls for relative endowment, and that it is observed on both

[1] In the thoroughly studied case of the United States, Piketty et al. (2018) document an increase in the share of pretax national income flowing to the top 1% of households rising from about 10% to about 20% over 1975–2014. Saez and Zucman (2016) compute that the wealth share of the top 0.1% has risen from 7% to 22% over 1974–2012. Other papers have used different methodologies to compute less rapid increases in inequality over the recent decades (Kopczuk and Saez 2004; Auten and Splinter 2016). Increasing inequality has been documented in other developed countries as well, particularly Anglophone countries (Canada, UK, Ireland, India), though in other countries this trend is weaker or not apparent. See Piketty (2014); Veall (2012); Alvaredo et al. (2013); World Inequality Database (2019).

extensive and intensive margins of the giving decision.

The robustness of this effect under laboratory conditions is striking. External validity is a legitimate concern, and the specific point estimates from a decision over a few tens of dollars in a lab setting likely do not generalize to a more representative sample. Motivated by previous literature finding that behavior may differ by income source (*e.g.* Fong 2007), we investigate if it matters whether subjects earn their endowments, relative to pure unearned "house" money. It does not. In both cases, inequality lowers charitable giving. That our findings are large, robust, and coherent with the direction of effects in *e.g.* Duquette (2018), suggests that the effect of inequality on real-life prosocial behavior may be quite substantial.

Direct effects of economic inequality on prosocial behavior imply that inequality may be self-reinforcing. Charities play an important role in mitigating inequality, relieving poverty through provision of social services, and equalizing opportunity through education. If inequality reduces donations directly, such groups will likely cut back their services, further entrenching inequality.

This paper proceeds as follows: Section 1 reviews the theoretical and empirical literatures on charitable giving and behavioral responses to inequality. We present the main points of our theoretical model in section 2, while reserving derivations and proofs for the appendix. We then explain the design of our experiment in section 3. The experimental data are summarized and described (§4) before we present our results (§5). Section 6 concludes.

1 Inequality and Altruism, in Theory and in Data

The canonical models of charitable giving are variations on a public goods provision problem with voluntary contributions. While many of these include behavioral effects accounting for the "warm glow" satisfaction of giving, the distribution of resources tends to enter such models in a fully rational manner, determining only who gives how much, and who free-rides on that giving. This means that these models only consider inequality to the extent that the free-riding problem is transformed—and they predict unequivocally that free-riding is worse when endowments are more equal. However, the literatures on inequality and charitable giving and on the behavioral effects of inequality on prosocial behavior imply these models may be incomplete.

1.1 Public goods models and inequality

Charitable giving is traditionally treated as a voluntary contribution to an economic public good. The classic public goods model describes a finite number of discrete agents who give to a nonrival good out of self-interest (Samuelson 1954), which leads to a free-riding problem wherein agents give until their private marginal benefit, which is weakly less than social marginal benefit, equals private marginal cost. Bergstrom, Blume and Varian (1986) demonstrate that a transfer from non-contributors to contributors will increase provision when households can free-ride. In the limiting case, when resources are sufficiently concentrated with a single household, the free-riding problem is solved: the rich household is the only contributor. Conversely, underprovision of public goods is more severe when the resource distribution is very equal.

These models have grown more sophisticated and realistic by adding terms for "warm glow" or "joy-of-giving" that capture utility from one's own contributions to a good, as well as "pure altruism," or utility in the total level of the public good irrespective of its source (Andreoni 1990). However, adding joy-of-giving terms to the model mitigates rather than eliminates the free-rider problem and the implied positive relationship between inequality and giving. Thus, they too predict that inequality will increase giving, albeit less sharply than in the pure altruism case.

A separate literature considers whether distaste for inequality can increase pro-social behavior. Behavioral models of "inequality aversion" model agents whose utility functions include others' consumption and exhibit a distaste for inequality (Fehr and Schmidt 1999; Bolton and Ockenfels 2000). Derin-Güre and Uler (2009, 2010) embed an inequality aversion term in an Andreoni (1990) warm glow model, and demonstrate in cross-sectional data that persons who are more concerned about inequality give more to charity. By assumption, however, inequality-aversion preferences cannot predict that inequality leads to lower giving.

Theories that permit any giving-reducing role for inequality are difficult to find; we know only of Mayo and Tinsley (2009), who propose a model wherein high-income households give less under rising inequality because income-shaped perceptions of relative returns to luck and skill affect perceptions of merit and reduce generosity. (For this reason our experimental framework will test for differential responses when endowments are strictly luck-based and when they are in part earned via completion of a task.)

1.2 Inequality and giving in the data

In the experimental settings, inequality causes changes in behavior in experimental settings not predicted by the prevailing models. Lab experiments with voluntary within-experiment redistribution find that greater inequality of endowments generally *decreases* contributions (Chan et al. 1996; Buckley and Croson 2006; Côté et al. 2015). This effect is causal; when endowments are randomized, the players randomly awarded more funds give lower shares of income to the within-experiment public goods (Anderson et al. 2008; Chan et al. 2008).[2]

Empirical evidence on the relationship between inequality and charitable contributions is scant outside the lab. Contradicting the prevailing theoretical paradigm, Duquette (2018) documents a strong negative correlation between income inequality and giving of top earners

[2] Adding some subtlety, Uler (2011) introduces a tax to the public goods game and finds that while greater pre-tax inequality decreases contributions, greater after-tax inequality increases total contributions.

in US and Canadian historical data. Payne and Smith (2015) on the other hand find that changes in neighborhood-level inequality is positively associated with total charitable giving in Canada, although with important nonlinearities and interactions that admit a different association among high earners.

These findings highlight the need for a new theoretical framework to think about inequality and prosocial behavior.

1.3 Empathetic responses and a new model of giving

We conjecture that inequality may affect giving decisions in ways not captured in existing models through psychological mechanisms. Larger differences in economic circumstances might provoke sharper empathetic responses from those with more resources, increasing giving. On the other hand, the greater dissimilarity between rich and poor as inequality increases could actually *reduce* empathy through the channel of increased social distance. The overall effect of these behavioral influences of inequality on giving is therefore ambiguous.

Social interactions have long been known to influence giving. Simply being asked to give increases the likelihood and amount of charitable giving (List 2011; Meer and Rosen 2011; Edwards and List 2014). Revelations of others' giving increases contributions (Vesterlund 2003; Shang and Croson 2009; DellaVigna et al. 2012; Smith et al. 2015), as does other seemingly irrelevant social influences, such as solicitor attractiveness (Soetevent 2011). Psychologists have found extensive empirical evidence that social influences on altruistic choices are driven by empathetic identification with a group, rather than more abstract concerns like self-image, fear of social shunning for inaction, or afterlife optimization (Batson 2011).

To the extent that inequality increases perceptions of need within one's group, this mechanism should increase giving. For this reason, wherever possible fundraising appeals focus on a representative beneficiary of the organizations' programs. Campaigns that help prospective donors to empathize with recipients are more successful (Einolf et al. 2013; Fong and Oberholzer-Gee 2011; Fong and Luttmer 2011; Andreoni and Rao 2011).

However, this channel could decrease giving to the extent that inequality narrows the scope of who is and is not in the prospective donor's "group" thanks to increased social distance. Social distance is the idea that people feel less affinity with, or empathy for, others who are unlike them, and may behave less prosocially toward them. Reductions in prosocial behavior caused by increases in social distance have been observed in the lab (Charness et al. 2007; Charness and Gneezy 2008) and in observational data (Alesina and La Ferrara 2000; Luttmer 2001; Hungerman 2008; Dahlberg et al. 2012; Andreoni et al. 2016) While this literature has focused on ethnic and racial diversity, unequal incomes also appear to reduce prosocial behaviors in the same manner (Alesina and La Ferrara 2000), and the social distance effects of ethnic diversity and income inequality appear to be mutually reinforcing (Beach and Jones 2017).

Like ethnic diversity, income and wealth inequality could increase the "social distance" among neighbors and across places, whose understanding of and empathy for each other lessens as they pass through life with increasingly different resource constraints and consumption lifestyles. Increases in economic diversity between donor and recipient create social distance and weaken empathy: subjects in fMRIs exhibit processing in brain regions that govern ingroup/outgroup decisions when deciding whether to give, and the nature of this response is affected by subjects' perceived socioeconomic status (Moll et al. 2006; Ma et al. 2011). Solicitors with low social and demographic distance from the potential donor are more effective (Meer 2011).

Drawing on these insights, we proceed by describing a new model of charitable giving with a behavioral influence of the income distribution on marginal utility that is not constrained in advance. We discuss different ways that behavioral influences might manifest in our model before testing among competing cases in our experiment.

2 A distribution-dependent theory of philanthropy

Imagine an economy with an infinite number households denoted $i \in [0, 1]$ distributed uniformly over the unit line. This economy has two goods, consumption (x_i) and warm glow from charitable giving (g_i). The production of total public goods from individual contributions is not affected by any household's choice, and so we omit total public good provision from the utility function.[3] Households' warm glow response to giving may be shaped by an unequal distribution of endowments and their own place within it, measured by statistic ϕ_i. Households optimize an additively separable utility function $u(x_i)+v(g_i;\phi_i)$ that is identical for all i, with the properties that u is increasing and concave in x_i and v in g_i.[4]

Households choose their consumption and giving subject to a budget constraint with endowment ω_i and price of charitable giving p. We can think of p as the (pre-tax) cost of giving a dollar to charity relative to (after-tax) consumption, or in the context of a fundraising experiment as the price implied by a contribution match.

$$\omega_i \geq x_i + pg_i \tag{1}$$

We further assume that charitable giving cannot take negative values, $g_i \geq 0$.

[3] That is, though total giving or "pure altruism" is possibly still valuable to the household, total giving $G \equiv \int_0^1 g_i$ won't show up in the first order condition because no individual household has mass, and therefore $dG/dg_i = 0$. Decreasing crowd-out in scale is demonstrated empirically using observational data in Ribar and Wilhelm (2002) and in a lab experiment with an actual charity in Ottoni-Wilhelm, Vesterlund and Xie (2017).

[4] That is, utility derivatives have the standard properties

$$u_{ix} > 0,\ v_{ig} > 0,\ u_{ixx} \leq 0 \text{ and } v_{igg} \leq 0$$

The last piece of this model is the distribution of incomes. Without loss of generality, let us assume that households are ordered by income so that $i \geq j \Leftrightarrow \omega_i \geq \omega_j \ \forall i, j$.

2.1 Optimal decisions

Taking constrained first-order conditions of the household's problem,

$$\max_{x_i, g_i} u(x_i) + v(g_i; \phi_i) \text{ s.t. } \omega_i \geq x_i + pg_i \text{ and } g_i \geq 0 \tag{2}$$

yields a first order condition for interior optima

$$pu'(x_i) = v'(g_i; \phi_i), \tag{3}$$

while for non-donors, we know that at the margin they would prefer additional consumption beyond their entire endowment to any amount of giving. In the following discussion we will focus the following discussion on the comparative statics of the interior case.[5]

2.2 Distribution-dependent utility and giving

So far we have been non-specific about the form or importance of extra parameter ϕ_i. Many possible pathways for influence of income inequality on giving seem plausible by introspection and are supported by an existing social science literature. We will focus on two possible behavioral pathways described by the behavioral influence of the variance of the endowment distribution on the marginal utility of giving. Our empirical design will be able to test for the effect of both pathways simultaneously relative to a third, null case where the distribution of resources does not affect giving at all. That is,

Case 0: Irrelevance of Inequality. For all possible distributions $\{\omega_i\}$ and all i,

[5] In the experiment described in later sections of this paper, over 99 percent of participants made a strictly positive charitable contribution in at least one round of the game.

$v(g_i, \phi_i) = v(g_i)$. Households to not receive different utility in giving or make different decisions due to changes in others' endowments.

The two other possibilities are that inequality increases giving (all else equal) or that it decreases it; we refer to these possible behavioral effects as "inequality aversion" and "social distance," after well-documented behavioral phenomena described in section 1.3.

Case 1: Inequality aversion. Households are aware of the degree of inequality, measured as the variance of endowments $\left(\phi_i = \int_0^1 (\omega_j - \bar{\omega})^2 dj, \text{where } \bar{\omega} \text{ is the mean}\right)$, and all else equal receive more satisfaction from giving when inequality is *greater* $\left(\frac{\partial v_i'}{\partial \phi_i} > 0\right)$. This case is consistent with a sizable literature that finds experimental participants are willing to incur costs to themselves to reduce within-experiment inequity.

Case 2: Social distance. Households are aware of the degree of inequality $\left(\phi_i = \int_0^1 (\omega_j - \bar{\omega})^2 dj\right)$, and all else equal receive more satisfaction from giving when inequality is *lesser* $\left(\frac{\partial v_i'}{\partial \phi_i} < 0\right)$. This is consistent with a literature that finds that people are less prosocial when they live in neighborhoods with people who are more economically or racially diverse.

These two cases do not exhaust all possible behavioral responses of giving to the income distribution; for example, it may be that donors respond to the skewness of the distribution, or that the sign of $\frac{\partial v_i'}{\partial \phi_i}$ changes in complex ways with income, or that in a real-world environment donors respond to imperfect "rules of thumb" that inaccurately capture the income distribution. Additionally, our experimental design will not attempt to distinguish between underlying mechanisms that create observationally equivalent behavioral effects; rather, we are only interested in assessing whether inequality causes net changes in giving beyond standard price and income effects.

Rather, we view this model as a fresh starting point for incorporation of the income or wealth distribution into joy-of-giving utility functions. (In the appendix to this paper, we will extend the regression model used to analyze our experimental data to test for the importance of some other moments of the endowment distribution for giving; inclusion of these other regressors explains little additional variation in giving behavior and does not meaningfully change the magnitudes or statistical significance reported for our treatment variables.)

2.3 Comparative statics

We now briefly explain the effects of changes in inequality and other parameters of interest on giving under this utility function. Details of the proofs and demonstrations of each result are provided in appendix A1. The primary difference between this model and traditional price theory is an effect of others' endowments, distinct from traditional conceptions of price and income effects on consumption behavior.

Proposition 1. *An increase in the price of giving p reduces g.*

This follows naturally from the normality of both goods in a two-goods model.

Proposition 2. *An increase in the endowment of a particular household increases that household's giving.*

A shift in a single household's endowment does not affect the variance of endowments, and so all candidate utility functions respond to an income shock through the traditional channel of an outward-shifted budget constraint, purchasing more of both normal goods.

Proposition 3. *An uniform increase in endowments of* all *households increases their giving.*

Again, a uniform increase in income leaves the variance unchanged, so only wealth/income effects matter.

Proposition 4. *If the marginal utility of giving is increasing in the variance of endowments, and inequality increases (decreases) without a change in the mean (a mean-preserving spread), the change in incomes will increase (decrease) donations for those above (below) the mean income.*

A mean-preserving spread by definition increases variance without changing the mean or the sum of all endowments. If households have a behavioral response to inequality, then it will be of the same sign for all households (with the positive or negative sign of that effect being the distinction between cases 1 and 2). However, a mean-preserving change in the income distribution by definition makes some households richer; this creates an income effect as in proposition 2 that may offset the behavioral effect of the redistribution. Depending on the signs of the change and of the marginal utility of inequality, one half of the distribution will have an ambiguous effect on their giving level.

Proposition 5. *If the marginal utility of giving is increasing (decreasing) in the variance of endowments, then an increase in inequality increases (decreases) giving net of endowment effects.*

By analogy, the change in giving behavior from an increase in inequality has an income effect (on actual endowments) and a shadow "price" effect (because the marginal utility of giving changes with a shock to ϕ); analogous to a compensated price shock, the effect of a change in ϕ *net of the income effect* follows from the relationship between inequality and joy-of-giving. If a change in the income distribution lowers the marginal utility of giving, those whose absolute endowments rise may still give more to the extent that their greater incomes more than offset the substitution effect, but they will spend a greater proportion of their endowments on consumption at the margin. For households who are worse off after a redistribution, both the income and the substitution effects are in the direction of a giving decrease.

2.4 Summary of Theoretical Predictions

In summary, the theory presented above presents an alternative framework for thinking about how inequality might affect giving decisions, based on well-documented findings from social psychology and behavioral economics. In the null case, households trade off their own consumption and the warm glow from giving without reference to the income distribution, similar to the large-population public goods model of Ribar and Wilhelm (2002). If households experience a social distancing effect as inequality increases, then this can be modeled as diminishing marginal warm glow with respect to inequality, and those households will tend to give less as endowments grow more dispersed, all else equal. On the other hand, if households primarily experience heightened inequality aversion when inequality increases, then we model this effect as an increase in marginal warm glow, and households will give more at the margin as inequality increases.

In our experimental setting, we test for all of these possibilities with versions of shocks to participants' endowments and to the variance in endowments.

3 Experimental Design

Given both the stylized facts about giving and the Propositions from Section 2.3, the primary objective of the laboratory experiment was to cleanly identify how people's willingness to donate depends on a within-experiment endowment distribution. An important feature of this setup is its focus on a real, external charity: unlike related literature that looks at how the within-experiment distribution affects within-experiment redistribution, we collaborated with a real charity as the recipient of donations. This has the benefit of isolating the behavioral effect of inequality on giving from concerns about group identity and other unobservables.

The second objective was to test how price sensitive donations are, captured by varying the rate at which we match donations. This was of concern to our collaborating charity, and of increased policy relevance given the increase in the standard deduction in the US tax code.[6]

A final objective was to test if the above two objectives differed between luck-based and reward-based distributions, as suggested by Luccasen and Grossman (2017). We tested this by implementing an additional effort task at the start of half the sessions. The effort task based on Gill and Prowse (2012) rewarded participants with increased tokens based on the number of sliders they could precisely position under time pressure.

An application to conduct an experiment involving human subjects was approved by the University of Tennessee's Institutional Review Board (application reference IRB-17-03776-XP) in May 2017. Subsequently the experiment was conducted at the University of Tennessee's Experimental Economics Lab, and was implemented using z-Tree (Fischbacher 2007).

One-hundred and twenty participants took part in the experiment, held over six sessions in September 2017. Participants were drawn from the UT Experimental Economics mailing-list, the vast majority of which were full-time undergraduates at the university. The mean number of participants per session was 20, and the median was 22.

Participants were told the purpose of the study was to "examine how inequality and subsidies affect charitable-giving", but not the specific research question. They were wel-

[6]Public Law 115–97, known colloquially known as the Tax Cut and Jobs Act (a short title removed from the final bill), increased the standard deduction of the individual federal income tax significantly while eliminating or substantially reducing many itemized deductions. As a result, the share of itemizers is expected to fall from 26 percent of all tax units to 11 percent (Tax Policy Center 2018). Filers who do not itemize do not receive a federal tax deduction for charitable contributions, substantially increasing the "price" of making a gift. While a large literature has traditionally found that the average household response elasticity is about -1, or "treasury neutral," (Andreoni 2006) more recent work has found that charities' donation receipts are significantly more price-elastic (Duquette 2016), possibly because the major donors exhibit greater tax-sensitivity than the typical household (Fack and Landais 2010).

comed to the lab and thanked for the willingness to participate. They were informed the experiment was in conjunction with United Way of Greater Knoxville, and provided a short summary of their objectives ("many programs, ranging from delivering hot meals to elderly citizens, to providing job training to people with intellectual disabilities.") United Way were invited to collaborate in the experiment because they are large and relatively uncontroversial organization in the community, providing assistance to more than 100,000 people (20%) in Knox County every year. Despite this, knowledge of the organization was quite low: in a post-experiment survey, 58% of participants reported being 'Not Familiar at all' or 'Not particularly familiar' with the charity. Copies of the annual report and promotional materials were available to participants (see appendix A2), but take-up was very low.

Participants were provided with an informed consent sheet, and given instructions. Real money was allocated to participants via tokens worth either 3 or 5 cents each, and donations solicited. Participants were told in advance that donations would be incentivized with a varying match rate. They were also informed that they would be told how many tokens they have been allocated, and some information about how many tokens other people were allocated. The consent sheet and full instructions are presented in the appendix (§A3 and §A4), but the key provisions were:

> "In this experiment, the computer will allocate a number of tokens to each of you. In each round you will be told how many tokens you received, and some information on how many tokens other people in the experiment received. Tokens are worth money, and will be converted into cash at the end of the experiment. Each token is worth 5 (3) cents, and so you can think of 100 tokens being worth 5 (3) dollars.
>
> You will be asked how many of these tokens you are willing to donate to United Way.
>
> To incentivize donations, you will see on-screen that we will match any donations with a varying amount. For example, you might see 'For every token you donate, we will match this with two more.' This means United Way would receive three tokens in total for every one you donate. The amounts donated will remain confidential.
>
> We will analyze how participants' donations depend on this information. We will run several rounds of this experiment. From these, the computer will select two to count for real, and payments will be based on those two rounds. At the end of the last session, you will be asked to fill out a questionnaire and paid for the tokens from the

> two selected rounds, in addition to the $5 show-up fee. Everybody will be paid in private after showing the record sheet. You are under no obligation to tell others how much you earned."

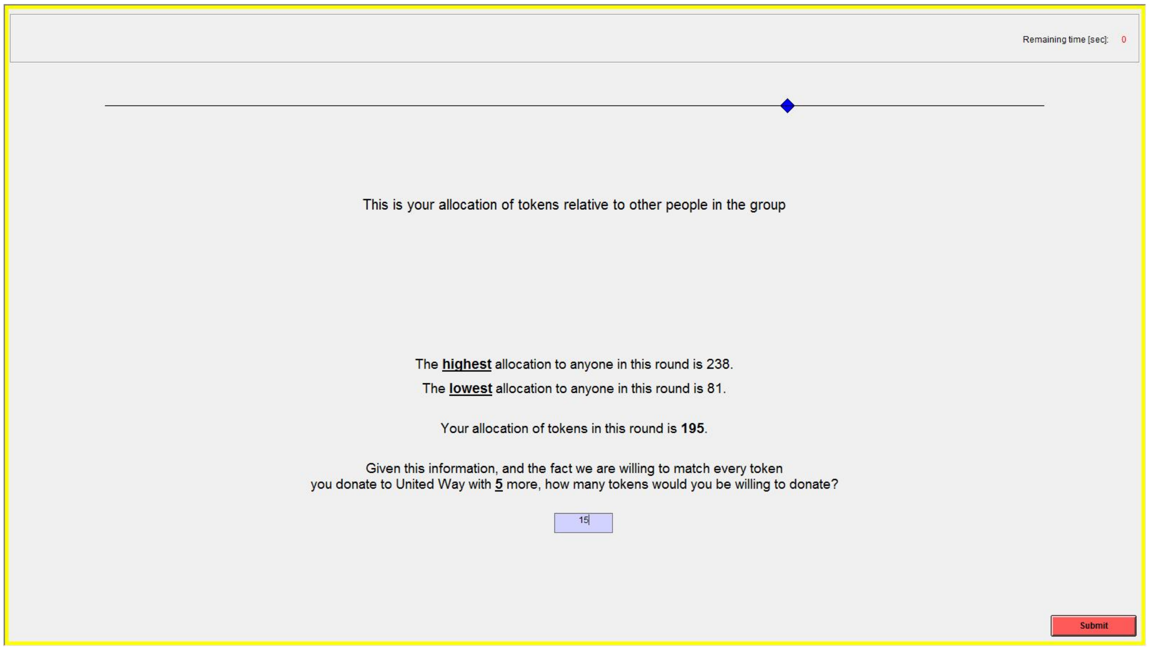
All sessions began with clearly-specified practice rounds. In sessions where participants conducted the slider task to allocate tokens, an additional paragraph of instructions were read aloud, and a practice round of the task were included.

The first treatment is the allocation of tokens. This is the experimental analogue of Proposition 2. By way of a simple income effect, it is expected there is a positive relationship between tokens and contributions. The second treatment, endowment inequality, is more subtle. This links back to Proposition 4, and the ϕ parameter of the utility function in Section 3. Participants are told the highest and lowest token allocation each round. This revelation is truthful, and reflects the fact that the number of tokens were drawn from distributions of length varying across rounds. This serves to make dispersion of token endowments, as well as the the subject's own allocation of tokens, salient. The distribution of tokens is clear through the figure towards the top of the screen, demonstrated in Figure 1. The subject's own endowment relative to the min and max is plotted to help visualize the distance to the minimum and maximum points.

As demonstrated in Figure 1, subjects are told their allocation, the maximum allocation, and the minimum allocation in each round. The figure towards the top illustrates their relative position in graphical form, highlighting their position relative to others.

The third treatment is a donation match. This affects the price of giving, as per Proposition 1. Immediately above the input box for the number of tokens to be donated, participants are informed of the match rate. Participants had been told they would face a varying match rate. In this particular example, the match rate is 5. The full list of match rates is outlined in Table 1. The specific instructions read "Your allocation of tokens in this round is **195.** Given this information, and the fact we are willing to match every token you

Figure 1: Primary experimental screen



donate to United Way with **5** more, how many tokens would you be willing to donate?"

In the example, the user indicates they will donate 15 tokens. After submitting the number of tokens, a confirmation screen repeats the donation level, and makes explicit that the matches increase the donations. This is shown in Figure A1 in appendix A5.

Table 1 below provides details of the distributions for the experiment. Consider the fifth and sixth round of the unearned income treatment sessions. Participants were separated into two groups randomly by z-Tree. For example, in rounds 5 and 6, Group 1 participants drew an endowment worth between 200 ($10) and 500 ($25) tokens, and were prompted to donate based on match rates of $2x$ or $8x$. Group 2's endowments came from the same distribution, but the match rates were reversed ($8x$ or $2x$). Thus within the two rounds, endowments are drawn and remain constant, but match rates vary, and the order of the

Table 1: Allocation distributions and match rates for unearned income treatments

Round Number	Endowment distribution	Match rates
1 & 2	U[50, 300]	1, 5
3 & 4	U[0, 1000]	2, 0
5 & 6	U[200, 500]	2, 8
7 & 8	U[200, 800]	1, 6
9 & 10	U[100, 400]	10, 5
11 & 12	U[0, 200]	0, 6
13 & 14	U[0, 300]	2, 3
15 & 16	U[100, 200]	0, 3
17 & 18	U[100, 500]	1, 2
19 & 20	U[200, 300]	4, 2
21 & 22	U[50, 500]	0, 1
23 & 24	U[0, 200]	4, 1

match rates vary. This has the effect of identifying the effect of the match rate from within person-round variation, i.e. only the match rate changes. Furthermore we switched the ordering of the rounds, and included some additional variation (different upper- and lower-limits) across sessions, keeping the mean endowment approximately constant. (The z-Tree code for all rounds and sessions, as well as our experimental data, will be publicly shared upon publication.)

As discussed above, the final objective of the experiment was to test if responses systematically varied between random-based endowments and effort-based endowments, cf. Erkal et al. (2011). This was motivated by concerns in the literature that experimental participants treat endowments that are randomly distributed differently to 'earned income' in ways that interact with the extent of inequality (Gee et al. 2017).[7] This treatment was implemented in half of the sessions, where the experimental rounds outlined above were prefaced with a slider effort task (Gill and Prowse 2012). A screen shot of this task is

[7]This concern is particularly important for studies of charitable giving, where subjects may substitute between effort (as in volunteering) and cash giving (Feldman 2010).

shown in Figure A2 in appendix A5.

The slider task has participants position sliders at precise points on the screen, rewarding those who successfully position a greater number in a given amount of time. This is a relatively simple manual task, and so the reward is based primarily on positioning the sliders quickly rather than returns to skill or other factors. This has been used previously as a pure effort task (Gill and Prowse 2012; Gee et al. 2017). In effort-task sessions, participants' tokens were still drawn randomly from an interval, but those who completed above-median scores in a particular session had that number doubled, while people with below-median scores had no adjustment made.[8]

The rest of the experiment proceeded as discussed above. We will show later that the results fail to reject a null of this treatment having no effect, cohering with other work in the area (Cherry et al. 2005). As the interaction effects of effort scores with the other treatments are all statistically insignificant, we will simply conclude that there is no evidence that the results systematically differ between random-allocations and effort-allocations.

4 Data Description

Summary statistics of the experiment are presented below. The experiment lasted 24 rounds, generating 2,880 observations on contributions. The value of the tokens were either $0.03 or $0.05, depending on the session. The average endowment was 260 tokens per round, and 49 (19%) of those are contributed on average.[9] To account for zero-contributions, we

[8] In the effort-task sessions, random draws were from a distribution with the same minimum but half the width of the uniform distributions reported in Table 1, with the above-median participants then having their random draws doubled. This adjustment changed the sampling distribution from which endowments were drawn from, but preserves the theoretical min and max, which are the important statistics for the visual presentation of endowment dispersion (Figure 1).

[9] Note that donations were encouraged with generous match rates.

use the inverse hyperbolic sine transformation to generate log-like values.[10] Two rounds were randomly selected to count for payment, meaning the average earnings (including $5 the show-up fee) was about $22. Of course, payouts and donations varied. The highest donation was 850 tokens. At an exchange rate of $0.05, that participant donated $42.50 of money that would otherwise be theirs.

To vary the price of charitable giving, we matched contributions with external funds. Karlan and List (2007) have previously found that match rates of 2:1 and 3:1 had little additional impact above a 1:1 match. While our matching treatments are primarily of that magnitude (the mean rate is 2.7), we extend the analysis by varying the rates from as low as zero to as high as 10:1. These high match rates simulate the effect of low tax prices of giving during the middle-20th Century, when marginal tax rates over 90% on top earners, and when giving as a share of income was very high and income inequality low (Duquette 2018).

Reflecting the student population, the average age was 21 and largely identified as from middle-class households. In a post-experiment questionnaire, participants reported that they understood the experiment and were adequately compensated for their time.

[10]The inverse hyperbolic sine function, $\operatorname{arcsinh} x = \ln\left(x + \sqrt{x^2+1}\right)$, converges quickly to $\ln x + \ln 2$ for positive values of x. Since $\ln 2$ is absorbed into a constant term in a regression specification, a regression using inverse hyperbolic sine transformation will give very similar results to a log-transform, and we thus informally refer to "log" values for this function throughout this manuscript. The primary advantage of the inverse hyperbolic sine over the natural logarithm is that it is defined, continuous and differentiable at $x = 0$.

In the appendix, we present results of untransformed (level-level) and Poisson regressions with very similar results to our main specification, demonstrating that this transformation's handling of zero values does not drive our results.

Table 2: Summary statistics of Philanthropy Experiment

	Mean	Std. Dev	N	Min	Max
Session ID	3.27	1.73	2,880	1	6
Person ID	60.50	34.65	2,880	1	120
Experimental Period	12.50	6.92	2,880	1	24
Endowment (tokens)	260.12	183.17	2,880	2	998
Match rate	2.72	2.42	2,880	0	10
Contribution (tokens)	48.62	82.36	2,880	0	850
Contribution, % of tokens	19.07	24.29	2,880	0	100
Log of contributions	3.40	1.77	2,880	0	7
Extent of inequality	343.67	214.18	2,880	73	987
Distance to Highest Endowment	187.71	178.94	2,880	0	987
Distance to Lowest Endowment	155.96	160.88	2,880	0	987
Age	20.72	1.99	2,880	18	32
Male	0.57	0.50	2,880	0	1
Married	0.02	0.13	2,880	0	1
First experiment	0.38	0.48	2,880	0	1
Social Class (1-5 scale)	2.93	1.01	2,880	1	5
Well compensated (1-5 scale)	4.23	0.92	2,880	1	5
Understand experiment (1-5 scale)	4.35	0.90	2,880	1	5
Exchange rate (USD/tokens)	0.04	0.01	2,880	0.03	0.05
Economics courses taken	1.77	2.05	2,880	0	12

Table 3: Distribution of Donations

		Percentile					
Variable	Min	*20th*	*40th*	*50th*	*60th*	*80th*	Max
Contribution (tokens)	0	4	11	20	25	70	850
Contribution (% of endowment)	0	2.04	6.9	9.85	13.04	31.25	100

5 Results

In this section, we present our experimental results. We begin with graphical explorations of simple relationships between giving by experiment subjects and treatments of interest in the raw data. We then proceed to simultaneous testing of our treatments in a multivariate regression, and then an exploration of heterogeneous effects by placement within the endowment distribution. We conclude with a hurdle model examination of intensive- and extensive-margin decision-making and a test for the importance of a work task in giving behavior.

5.1 Visual Associations Between Giving and Treatments

Before proceeding to our main analysis, we explore the relationships between giving and selected treatments: the matching incentive and the dispersion of the income distributions.[11]

Figure 2 plots the distribution of giving behavior as a dot-and-whisker plot for each individual match rate offered in the experiment. It is evident that increasing matches increases donations and, with one exception, monotonically. That one exception is a match rate of 5 which, interestingly, encourages more donations than a rate of 6 or 8. We assume this is because of the salience of the number five in the base 10 number system, but cannot be sure. A tenfold match rate only encourages 14% more $(0.96-0.82 = 0.14)$ donations than a fivefold match rate, indicating that while matches do succeed in encouraging donations, they are an expensive method of doing so.

Figure 3 presents a scatterplot of average contribution in tokens as a function of the width of the within-round token distribution. Inequality is measured by the differences between the min and max of the distribution within rounds. For ease of visibility, we use

[11] A plot of giving against one's own endowment shows a very strong, and unsurprising, positive association and is omitted for brevity.

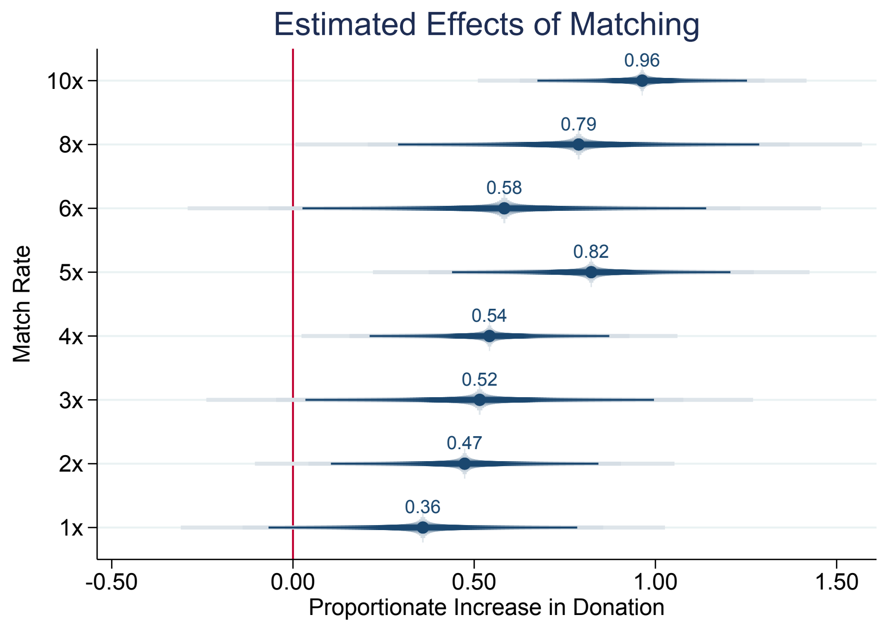


Figure 2: Estimated treatment effects of randomly-assigned match rate on donations. Omitted category is no match. The point estimates are specified for each match rate, in the middle of the estimate's 95% confidence interval.

twenty bins so that rounds of similar inequality are averaged together into a single marker. To account for higher average endowments in wider distributions, giving is regressed on individual endowment and the residuals averaged to construct this plot.[12] A line fits the relationship through the binned-scatterplot. There is an obvious negative relationship between endowment inequality and participants' average generosity. This negative relationship, which conditions on one's own endowment, is striking and is perhaps the main result of the entire paper. The variables here are reported in levels; a similar analysis conducive to an elasticity interpretation finds that a 1% increase in the width of the distribution

[12]For example, as Table 1 reports, rounds 11 and 12 drew from a $U[0, 200]$ distribution, while rounds 3 and 4 drew from a $U[0, 1000]$ distribution. Residualizing giving with respect to endowment allows us to plot the correlation between bin width and giving without allowing the fivefold increase in expected endowment across these two rounds to confound the relationship.

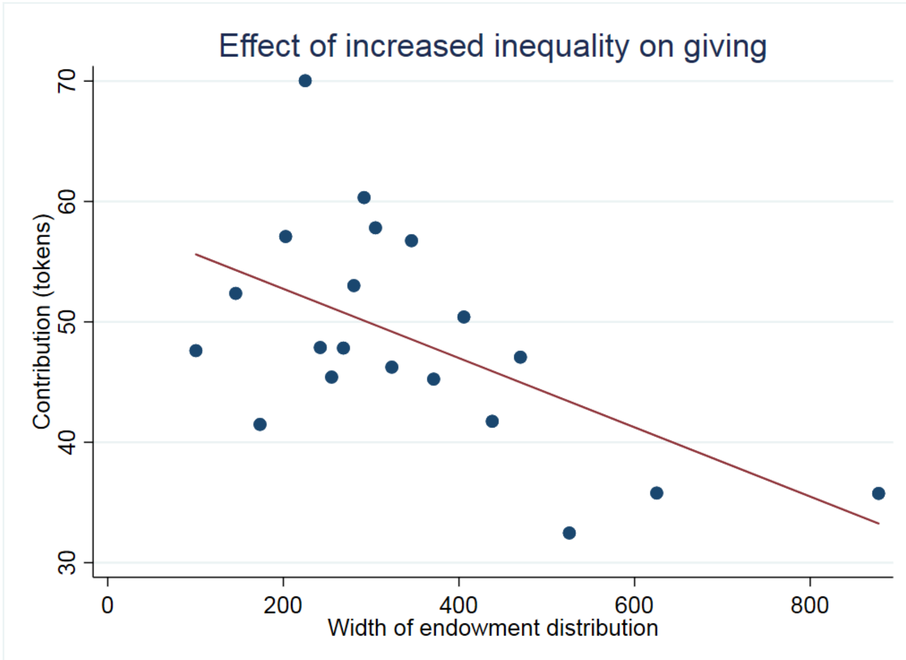


Figure 3: Average tokens donated relative to dispersion of the distribution, holding endowment constant. Giving is declining in inequality. As with all bin-scatter figures, each dot represents the average of many datapoints.

decreases donations by 0.25%.

Next, we test for these and other treatments' effects on contributions simultaneously, in a multivariate setting.

5.2 Multivariate Hypothesis Testing

While patterns visible in the raw data suggest that match rates and inequality have effects on giving, our main research question is whether these associations are observed in a multivariate regression of giving on these treatments and sets of control variables simultaneously. Table 4 presents these estimates. We include five specifications of the primary question: how do the treatments affect (the log of) contributions to charity? Column 1 reports the regression of contributions on the participant's token endowment and match rate and the within-round dispersion of the distribution of tokens.[13] Columns 2, 3 and 5 add standard control variables on the participant's sociodemographics and economics course experience;[14] Columns 3 and 4 add fixed-effects for the experimental session and for the subject, respectively (testing for any common within-round or within-subject confounding unobservables), while Column 5 is a random effects specification.

We see that the results are consistent across specifications. The use of random versus fixed effects does not change the estimates meaningfully, as expected given the experimental nature of the setup. As the outcome variable is the log of contributions, we can interpret the coefficients in percentage terms.[15]

Consider the random effects model shown in Column 5. The first result is that contributions are increasing in income. This is unsurprising, although the magnitude is interesting. The mean number of tokens was approximately 250; our estimates indicate that exogenously increasing this by 100 sees donations increase by 36%.[16] In terms of levels, this is about sixteen tokens. Do note the relatively high contribution rate is encouraged by matching grants.

The second result is that the matches ("For every token you donate, we will match it with x more") succeed in eliciting greater giving, although it is expensive to do so. In our fixed effect model of Column 4 we find a coefficient of 0.092, meaning increasing the match rate from e.g. 2 to 3 causes donations to increase by 9.2%.[17] We plot the coefficient

[13] A table in the appendix adds the visual midpoint of the distribution to set of treatments; the findings with respect to price, own endowment, and inequality are unchanged, and visual midpoint is not strongly associated with giving behavior.

[14] The full list of control variables are age, sex, marital status, self-reported social class, a dummy variable for previous experience with experiments, Likert scales for satisfaction with compensation and level of understanding of the experiment, and the number of economics courses taken. Results are shown in the appendix.

[15] Alternative functional forms are presented in appendix A6; our results are not dependent upon the functional form of the regression.

[16] Of course as log specifications are only precise for marginal changes, it is more accurate to say a one-unit increase in tokens leads to donations increasing by 0.36%.

[17] The literature on charitable giving and tax policy typically models this variable not as a match but

Table 4: Effects of various treatments on log of contributions donated

	(1)	(2)	(3)	(4)	(5)
Endowment ('00s)	0.40***	0.43***	0.42***	0.36***	0.36***
	(0.042)	(0.037)	(0.036)	(0.026)	(0.013)
Match rate	0.081***	0.088***	0.086***	0.092***	0.092***
	(0.015)	(0.016)	(0.016)	(0.017)	(0.0076)
Extent of inequality ('00s)	-0.072**	-0.096***	-0.095***	-0.060***	-0.061***
	(0.023)	(0.015)	(0.013)	(0.0074)	(0.011)
Control variables		✓	✓		✓
Experimental session FE			✓		
Participant FE				✓	
Random Effects					✓
Observations	2,880	2,880	2,880	2,880	2,880
Adjusted R^2	0.153	0.284	0.297	0.287	0.287

Standard errors in parentheses. SEs in Models 1–4 clustered at the session level.
$^{***}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

estimates for each of the offered match rates in Figure 2.

Of particular interest, however, are the results on the width of the distribution of tokens itself. The variable denoted "Extent of inequality" is defined as the difference in hundreds of tokens between the min and max of the empirical distribution, and is presented to the subjects as a simple proxy for the dispersion of the endowment distribution.[18] The interpretation of these coefficients is thus the causal effect of increasing the dispersion of the distribution, holding both one's own endowment and the match rate fixed.

as a "tax price" of one minus the marginal tax rate (for filers eligible to itemize). The transformation of a match rate m to 1 into an implicit price p is $p = \frac{1}{1+m}$. For our mean match rate of 2.7 and a representative semi-elasticity of match rate of 0.85, this translates to a tax elasticity of contributions of -0.32, lower than has typically been found in tax return data, but consistent with findings of a low response to match rates from fundraising field experiments (*e.g.* Karlan and List 2007, Karlan, List and Shafir 2011, Hungerman and Ottoni-Wilhelm 2017, Huck and Rasul 2011, Huck et al. 2015).

[18] Table A9 in the appendix shows the results when the treatment is the standard deviation of within-round endowments.

In all cases, the coefficient is negative, significant, and in line with our theoretical predictions. Expanding inequality lowers giving. The coefficient on increasing inequality from Column 2 implies that expanding the extremes of the distribution by 50 tokens on both sides causes giving to fall by about 9.6%. In the latter specifications the effect size, at around 6%, is still quite large.

We consider this quite a remarkable result: increasing inequality lowers giving. While some previous lab experiments have found inequality decreases contributions of participants in a public goods game, we believe this is the first paper to show this extends to a real-world charity setting. As this result cannot be attributed to within-group redistributive motives, this is clear evidence of a real behavioral effect of inequality influencing choices.

Returning to our theoretical framing, the results are consistent with a social distancing mechanism in the utility function (case 2): when the dispersion of incomes is increased, then in the raw data and conditional on other controls, giving decreases. This finding also rejects the null (case 0) of a preference function that is indifferent to the distribution of endowments, as well as case 1's suggestion of a positive relationship between variance and giving driven by inequality aversion.

5.3 Heterogeneous Inequality Effects By Within-Round Position

We extend our analysis of the inequality-giving relationship by exploring the importance of within-round relative economic position for the donation decision. A growing experimental literature has suggested that income position is an important behavioral factor in economic behavior, and that the psychological mechanisms underpinning charitable giving decisions are complex (DellaVigna et al. 2012; Andreoni et al. 2017a,b). Erkal et al. (2011) shows a non-monotonic relationship between earnings and charitable giving (specifically, people who rank first in an effort-experiment are less likely to donate than people who come second).

Within-round placement appears to matter for giving decisions. Figure 4 plots the share of endowments given for individual observations (each participant in each round). The horizontal axis reports the observation's within-round endowment in relative terms.[19] The vertical axis reports the share of the participant's endowment given to the United Way in that round. A loess curve, marked in red, fits the distribution of giving/endowment as a function of relative endowment nonparametrically. The vertical axis is cut off at 42% for better visibility. We see from the figure that the mean contribution to charity (as a percentage of total endowment) is relatively flat around 17%, but there is evidence that both the bottom and top quintiles give more, perhaps 22%.

As the endowments are randomized, we can further investigate the nonlinear income-giving relationship by regressing donations on endowment decile. Specifically, and analogously to Figure 4, we regress giving/endowment on a vector of indicator variables splitting endowment position into tenths.[20] This approach is robust to some of the limitations of nonparametric methods, which are often unstable approaching the periphery of the distribution.

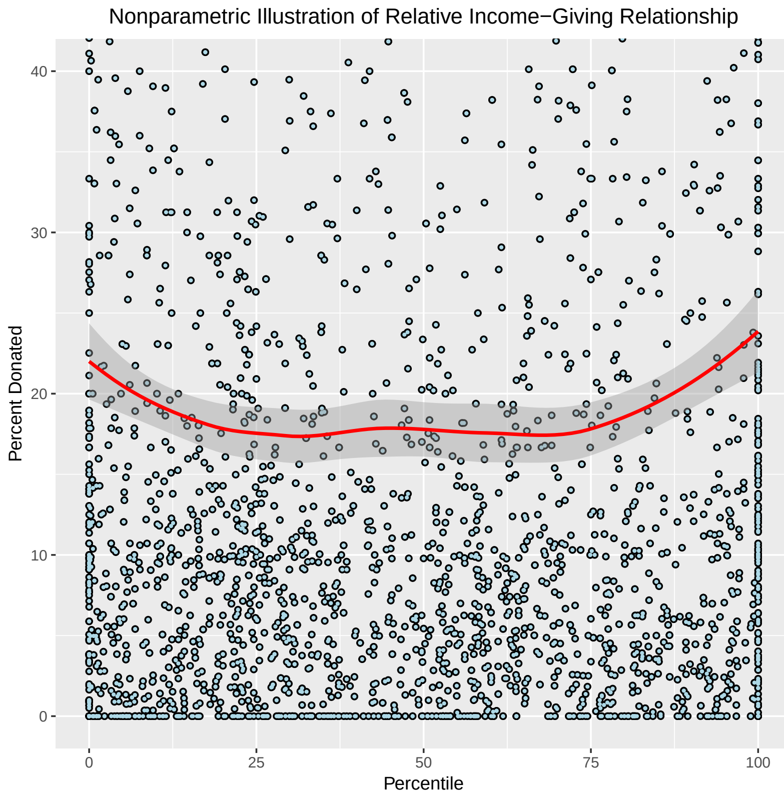
The results of this regression are depicted graphically in Figure 5, plotting the point estimates and associated 95% confidence intervals for donations as a function of relative placement. Point estimates correspond to the percentage-point difference in giving relative to endowments in the comparison group, those between 40 and 50 percent of the way between the lowest and highest endowments. As in our nonparametric plot, we see that within-distribution location appears to matter for giving, following a non-linear "U-shaped" pattern of higher giving as a share of endowment at the extremes than in the middle.

As an intriguing aside, the clearly visible U-shape in Figures 4 and 5 mirrors the non-

[19] That is, observations are ordered on the horizontal axis by $\text{Relative Endowment} = \frac{\text{Endowment} - \text{Min Endowment}}{\text{Max Endowment} - \text{Min Endowment}}$.

[20] That is, we divide subjects into bins for those with endowments less than 10% of the distance from the lowest to the highest endowment, 10-20%, 20-30%, ..., 90-100%.

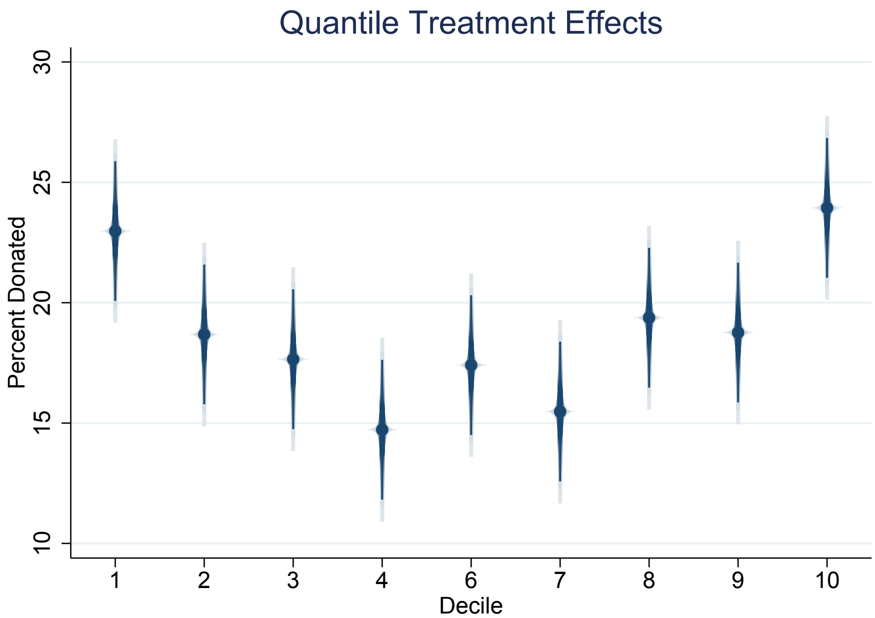
Figure 4: Scatterplot of contributions



linear relationship between giving and income that has been regularly observed in naturally occurring data, with very low- and high-income households donating greater income shares than middle-income households. The existing literature explains the U-shape as a data artefact (Clotfelter and Steuerle 1981; Clotfelter 1985; Auten et al. 2000; List 2011); our results suggest it may instead reflect a genuine nonlinearity in the role of socioeconomic position on giving behavior.[21] We believe this is the first experimental verification of this

[21]Since charitable giving is generally thought of as a luxury good, it is not surprising that high-income households give greater income shares than middle-income households. The decreasing left-hand-side of the U, however, is a bit of a puzzle. A common explanation is that low-income households only appear to be more generous than middle-income households, and that the upturn of the U at the low end is driven

Figure 5: Regression of donation on randomly-assigned decile of endowment distribution. Omitted category is 5th decile. The U-shaped relationship is noticeable.



observation; it merits further study.

These nonlinear relationships between income and giving are visible across inequality conditions and in plots by absolute endowment. Figure 6 plots a localized nonparametric regression of tokens given on endowment, divided into subsamples of responses from rounds with above- or below-median inequality. For visibility, individual responses are not shown. As noted in table 1, only high-inequality rounds have endowments over 500 tokens (for budgetary reasons), and so the common support of the curves is on the left side of the

either by a small number of high-wealth, low-income households (Savoie and Havens 1998) or by failure to account for data censoring correctly (Schervish and Havens 2001), though James and Sharpe (2007) argue that this is incorrect. Other explanations for the U-shape include income variation in religiosity (Auten et al. 2000) or social norms around giving and reciprocity (Wiepking 2007; Piff et al. 2010). None of these mechanical or social explanations explain our finding of a U-shape in giving from the random assignment of endowment position within a laboratory.

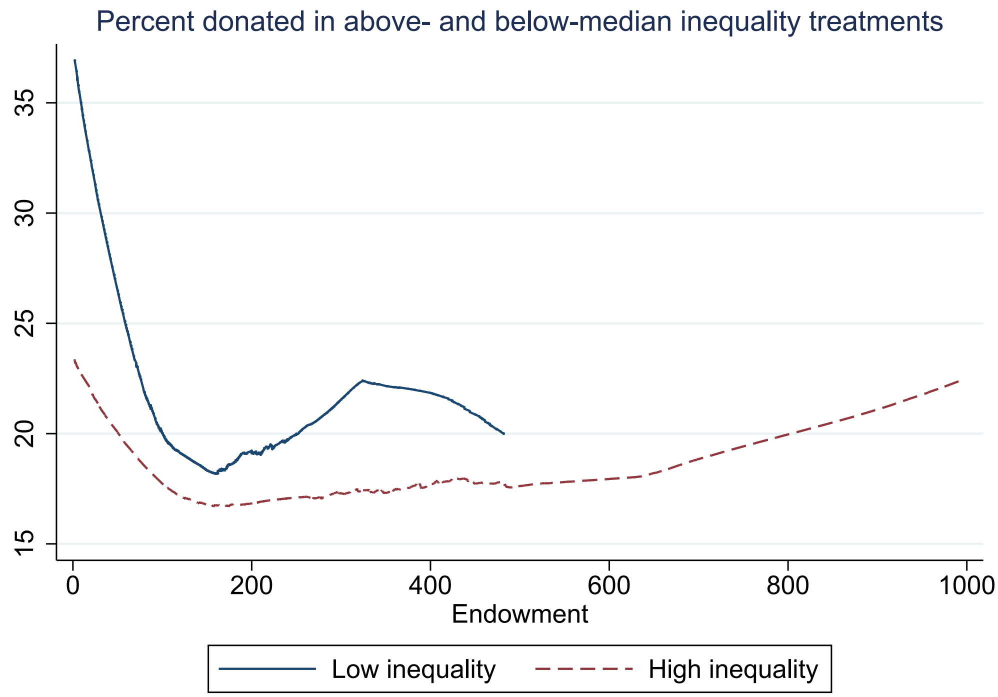


Figure 6: LOESS local nonparametric regression of tokens donated on endowment. Solid and dashed lines respectively mark observations from above- and below-median inequality treatments.

graph.

While the usual caveats about nonparametric smoothing apply, there are two clear lessons from this plot: (1) on average, subjects give more in low-inequality than high-inequality rounds, out of similar endowments, by a difference of about 2–5 percentage points; (2) The U-shaped relationship between endowment and the giving/endowment ratio holds for both subgroups, suggesting that the relationship shown in figures 4 and 5 is not spurious.

We conclude our exploration of nonlinear effects of inequality and relative endowment by extending our regression model. Specifically, we extend the regressions of Table 4 to include regressors for relative endowment, heterogeneous inequality effects by endowment, or both. These regressions are presented in Table 5. "Endowment (Absolute)" is endowment in tokens, as defined previously, while "Endowment (Positional)" is a variable between 0 and 1 expressing the participant's within-round endowment as a share of the distance from the within-round min to the within-round max endowments. Column 1 replicates the specification of Table 4, Column 3 (with session fixed effects and control variables), adding a new term for positional endowment. Column 2 adds an interaction of inequality with absolute endowment to the base regression. Column 3 adds both relative endowment and interaction terms. Columns 4 through 6 repeat the specifications of 1–3, with participant fixed effects to control for participant and session unobservables and demographics.

We find that within-distribution heterogeneity is an important pathway for much of the inequality effect. In Columns 1 and 4, adding an effect for positional endowment reduces the coefficient on inequality from 6–7% to less than 3%, i.e. by more than half. We also see that the point estimates on interactions are large and negative, and capture most of the negative effect previously captured in the inequality level. Although the inclusion of these interaction effects for absolute and positional inequality push the base coefficient towards zero, the sum total effect of inequality remains negative (and significant) in these specifications. Consistent with our conjecture that social distance reduces the impulse to give, the interaction coefficients mean that inequality reduces giving by substantially attenuating the "income effect" of larger endowments. The estimates in Column 2, for example, suggest that a one standard deviation increase in inequality reduces the semi-elasticity of absolute endowment by 0.9. Participants' giving increases more slowly in endowment as inequality rises.

Table 5: Effect of Visual Endowment Position on Giving

	(1)	(2)	(3)	(4)	(5)	(6)
Endowment (Absolute)	0.298***	0.638***	0.488***	0.296***	0.551***	0.475***
	(0.038)	(0.034)	(0.026)	(0.037)	(0.036)	(0.043)
Match Rate	0.092***	0.077***	0.082***	0.095***	0.084***	0.085***
	(0.016)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)
Extent of Inequality	-0.029*	0.017	0.059*	-0.027*	0.040*	0.062**
	(0.014)	(0.028)	(0.028)	(0.013)	(0.018)	(0.020)
Endowment (Positional)	0.806***		1.115***	0.464***		0.841***
	(0.170)		(0.151)	(0.071)		(0.084)
Inequality × Absolute		-0.040***	-0.017*		-0.036***	-0.013**
		(0.005)	(0.007)		(0.004)	(0.005)
Inequality × Positional			-0.201**			-0.230***
			(0.054)			(0.032)
Control Variables	✓	✓	✓			
Session Fixed Effects	✓	✓	✓			
Person Fixed Effects				✓	✓	✓
R^2	0.310	0.311	0.319	0.720	0.725	0.728
N	2,880	2,880	2,880	2,880	2,880	2,880

5.4 Extensive and Intensive Margin Decisions

Given that over a fifth of observations consisted of zero contributions, we researched differences between the intensive and extensive margin decisions of participants. We employ the exponential hurdle model proposed by Cragg (1971). This approach combines a selection model for clearing the hurdle of zero contributions and the intensive margin of how much thereafter to donate. The set of independent variables in this model are identical to those in Table 4, Column 2. Results are shown in Table 6.

The first two columns display results from a probit of zero or non-zero contributions. The results are qualitatively consistent with the earlier analysis, namely positive effects of own income/endowment and match rates, and a negative effect of inequality. Holding the other factors constant, higher inequality increases the likelihood of not donating anything. Columns 3 and 4 display results from an exponential model on how many tokens to donate,

Table 6: Hurdle model results

	Binary Hurdle		*Overall Effect*		*Comparison*
	Coef. Score	Marg. Effect	Coef. Score	ATE	OLS
Endowment ('00s)	0.14***	0.20***	0.40***	16.23***	22.3***
	(0.23)	(0.004)	(0.040)	(2.69)	(3.88)
Match rate	0.18***	0.02***	0.008	1.37***	1.71**
	(0.032)	(0.005)	(0.0073)	(0.46)	(0.66)
Extent of inequality ('00s)	-0.032**	-0.004***	-0.089***	-3.65***	-3.43**
	(0.012)	(0.002)	(0.016)	(0.84)	(1.09)
Control variables	✓	✓	✓	✓	✓
Non-zero Observations	2,560	2,560	2,560	2,560	2,560
Total Observations	2,880	2,880	2,880	2,880	2,880

Results of Cragg exponential hurdle model on extensive margin (zero/positive) contributions, and total effect (controlling for the hurdle) tokens are donated. Marginal effects are evaluated at means of independent variables.

conditional on the hurdle approach in the first two columns. The results are again similar to the earlier analysis with positive effects of own endowment and match rates, and a negative effect of inequality. We see the overall effect of the match rate on donations is relatively low: conditional on the hurdle, increasing the match rate by one unit increases donations by about 1.37 tokens. This in consistent with the relatively flat gradient seen in Figure 2, namely that match rates do inspire additional contributions but are an expensive way to do it.[22] The right-hand side column shows the full-sample OLS results as a baseline comparison. We see the sign, magnitudes, and statistical significance of OLS and the hurdle model paint very similar pictures. Controlling for the mass at zero reduces the effect of own endowment (22.3 to 16.2) and match rate (1.71 to 1.37), but slightly increases the effect of inequality (-3.43 to -3.65).

[22]Further extensive margin analysis, condensing the continuous giving decision into a binary outcome, is included in Table A10 in the appendix.

5.5 External Validity and a Work Task

Given the artificial nature of lab experiments, it is natural to be skeptical of the external validity of the findings. One concern is how likely the results are to be replicated outside of the controlled environment. We have two responses to this.

Our first response was to incorporate both earned and randomly allocated income sessions into the experiment. In half of the sessions, participants' endowments were completely random. In the other half, as discussed in Section 3, participants first engaged in an effort task that rewarded high-achievers. The task, proposed by Gill and Prowse (2012), is explicitly designed to require effort and induce feelings of reward.

Table 7 shows the effects of our matching and distribution-width treatments interacted with the effort task. Unlike the primary treatments, none of the interaction effects are statistically significant at the conventional 95% level. We cannot conclude that the effort task made any noticeable difference to participant behavior. We included the effort task expecting it to provide some insight, and were surprised by the null result. That said, it is somewhat reassuring that the results are robust.

Our second response to external validity concerns is that the experiment was primarily motivated by an existing feature of real-world data, the negative association between inequality and giving rates documented in Duquette (2018). Those associations do not prove a causal relationship between the two, and it has been an open question whether this relationship was an interesting correlation or something deeper.

It is in this context that a controlled environment for testing causal mechanisms is particularly useful, and mitigates against oft-quoted arguments against lab experiments. To be sure, there are reasons our experimental setup may not generalize to field environments; our subject pool was not representative of the general population, and the decision to give to a charity preselected by the researchers may not capture the decision to give to a charity of the subject's choosing. However, we argue that the most prominent threat to external validity of our central finding — the low stakes and transitory nature of the treatment — should attenuate our central finding, not strengthen it.

Table 7: No noticeable effects of effort task

	(1)	(2)	(3)	(4)	(5)
Endowment ('00s)	0.37***	0.40***	0.39***	0.34***	0.34***
	(0.080)	(0.080)	(0.082)	(0.035)	(0.018)
Match rate	0.078***	0.093***	0.092***	0.096***	0.097***
	(0.015)	(0.014)	(0.019)	(0.021)	(0.0094)
Extent of inequality ('00s)	-0.092**	-0.082*	-0.080**	-0.054***	-0.054***
	(0.032)	(0.034)	(0.028)	(0.0048)	(0.014)
Endowment × Effort task	0.058	0.052	0.063	0.047	0.047*
	(0.082)	(0.091)	(0.094)	(0.043)	(0.026)
Match rate × Effort task	0.016	-0.013	-0.015	-0.012	-0.013
	(0.025)	(0.012)	(0.029)	(0.031)	(0.016)
Extent of inequality × Effort task	0.034	-0.027	-0.027	-0.010	-0.012
	(0.046)	(0.048)	(0.038)	(0.014)	(0.022)
Control variables		✓	✓		✓
Effect type	None	None	Session	Individual	Random
N	2,880	2,880	2,880	2,880	2,880

6 Discussion

Rising inequality has been studied extensively by economists, largely to describe and document its evolution over time (Piketty et al. 2018; Saez and Zucman 2016; Auten and Splinter 2016). No doubt because it is challenging to find exogenous changes in inequality in naturally occurring data, less work has been done on inequality's causal effects.

This paper has demonstrated that economic inequality has a negative, causal effect on charitable giving. Our experiment used exogenous variation in the dispersion of the endowments participants received to identify the effect of inequality on gifts to a real charitable organization. We observe lower overall giving as the distribution of endowments becomes more unequal. We further identify income and matching effects consistent with charitable giving as a normal good, with small match-effect magnitudes in line with the field experiment literature (*e.g.* Karlan, List and Shafir 2011, Huck, Rasul and Shephard 2015). Results are consistent across specifications, and independent of whether endowments are earned or unearned.

Though these findings were produced in a laboratory setting, the case for their external validity is strong on at least three grounds. First, variation in the distributions of a few dollars' worth of tokens are almost certainly less important and less salient than the actual distribution of income and wealth. Second, by offering subjects the opportunity to give to a United Way, we remove one of the barriers to external validity present for more traditional public goods games — subjects were giving real money to a real charity, at real cost to themselves. In this sense, our experiment was a hybrid between lab and field methodologies, as the laboratory acted solely as a tool to randomize inequality for an otherwise real decision process. Third, in their research comparing giving elasticities across settings, Eckel and Grossman (2008) found magnitudes from a field experiment "are very similar to (and insignificantly different from) [those from] lab experiments."

Moreover, the power of our relatively large sample (thousands of observations over hundreds of subjects) suggest that these findings are unlikely to be spurious. We demonstrate in Appendix A7 that our results should shift the prior beliefs of the vast majority of readers. Using the framework of Maniadis et al. (2014, 2017) and applying reasonable parameter values, an unbiased reader's post-study belief that inequality negatively affects giving should be at least 15 percentage points higher than her prior after our study. Further analysis presented in appendix A8 demonstrates that our findings are not under-rejected after adjusting p-values for the testing of multiple hypotheses.

Our empirical results are inconsistent with the prevailing theory of voluntary public good provision, but are consistent with the theoretical framework developed in this paper. The model allows for behavioral changes in joy-of-giving motivation in response to shifts in the income distribution. We believe this work is the first step toward a new empirical and theoretical literature on charitable giving that focuses on inequality and other behavioral influences in addition to the classic questions of crowd-out and tax price of giving.

These findings are important both for understanding the economics of charitable giving and for public policy regarding inequality and social cohesion. If charitable giving is an expression of civic feeling lessened by disparate economic situations of potential donors, then voluntary contributions will not work against rising inequality; rather, inequality will undermine charitable contributions. It is possible that civil societies have multiple equilibria, one of high social cohesion and high giving supporting each other, others of low giving and low social cohesion. Policies that support one of these while discouraging the other will have ambiguous results. While this paper has focused on charitable giving, we believe it likely that the negative, causal effect of inequality extends to other forms of prosocial behavior, and our method could be applied to the study of social influences on, for example, voting behavior (as in Messer et al. 2010) or conservation (Ito et al. 2018). If so, inequality imposes a direct, first-order social cost on the public that is currently not widely discussed or well-understood. We hope that future research will speak to these broader questions.

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Appendices

"Inequality and Giving"

For Online Publication

A1 A Model of Distribution-Dependent Joy-Of-Giving

This appendix derives the results of the model in detail.

A1.1 General Properties of the Utility Function

While our paper focuses on, and finds empirical support for, joy-of-giving that depends on the dispersion of endowments, we deliberately write the ϕ_i term generally to permit extension to other behavioral dimensions in future research. In this section we consider the general properties of this utility function, both to enrich the reader's understanding and as a service to researchers interested in extending this work, before deriving properties specific to the function where ϕ_i measures endowment variance.

Let households i be uniformly distributed on the unit line with measure 1, $i \in [0, 1]$. Each household has endowment ω_i to allocate between personal consumption x_i and voluntary contributions to charity, g_i. Without loss of generality, let households be ordered by their endowments, so that $i \geq j \Leftrightarrow \omega_i \geq \omega_j \forall i, j$.

These households maximize a separable utility function,

$$U_i = u(x_i) + v(g_i; \phi_i), \tag{4}$$

which has the properties that both consumption and giving have positive, marginally decreasing payoffs, and that zero consumption is intensely displeasurable,

$$\begin{aligned} u'(x_i) &\geq 0 \quad\quad v'(g_i) \geq 0 \\ u''(x_i) &\leq 0 \quad\quad v''(g_i) \leq 0 \\ \lim_{x_i \to 0} u(x_i) &= -\infty. \end{aligned}$$

This decision is subject to the constraints that $g_i \geq 0$ and the budget

$$\omega_i \geq x_i + pg_i, \tag{5}$$

where p is the price of giving (determined by a matching incentive or a tax deduction).

One more argument affects the households' decision: ϕ is a sufficient statistic describing the distribution of endowments and the households' place within it, which we will here write

$$\phi_i = \phi\left(\omega_i, \{\omega_j\}_{j=0}^{1}\right).$$

A final general assumption: giving *nothing* gives the same joy-of-giving regardless of the income distribution: $v(0, \phi_i) = v(0, \phi_i^\star),\ \forall \phi_i, \phi_i^\star$.

A1.2 First Order Condition

If the nonnegativity constraint on giving binds, then the household obviously chooses $x_i = \omega_i, g_i = 0$, and it must be the case that $u'(\omega_i) \geq v'(0; \phi_i)$.

At an interior solution the household will equalize marginal utility from the two goods, so that

$$v_i' = pu_i' \tag{6}$$

A1.3 Comparative Statics

An infinitesimal change in parameters will not change the behavior of non-donors, but will have a corresponding effect on those who do give. Next, we derive the responses of donors to changes in p and ω_i.

Totally differentiate budget constraint (5),

$$dx_i = d\omega_i - pdg_i - g_i dp \tag{7}$$

and also the first order condition (3),

$$v_i'' dg_i + \frac{\partial v_i'}{\partial \phi_i} d\phi_i = u_i' dp + pu_i'' dx_i \tag{8}$$

Replace the x_i differential in (8) using equation (7) and group terms with the same differential to get an implicit function of g_i in terms of parameters and the preference function.

$$dg_i \left(v_i'' + p^2 u_i'' \right) = dp \left(u_i' - pu_i'' g_i \right) + d\omega_i \left(pu_i'' \right) - \frac{\partial v_i'}{\partial \phi_i} d\phi_i \tag{9}$$

Proposition 1: Price effect

Divide (9) by parameter differentials to get comparative statics in p with respect to price,

$$\frac{dg_i}{dp} = \frac{u_i' - pu_i'' g_i}{v_i'' + p^2 u_i''} \leq 0. \tag{10}$$

Because of the assumption of concave utility, price increases decrease giving. The distributional term drops out because $d\phi_i / dp = 0$.

Own-income shocks

A shock to one's own wealth is slightly more complicated,

$$\frac{dg_i}{d\omega_i} = \frac{pu_i''}{v_i'' + p^2 u_i''} + \frac{\partial v_i'}{\partial \phi_i} \frac{1}{v_i'' + p^2 u_i''} \frac{d\phi_i}{d\omega_i} \tag{11}$$

The first term is a traditional wealth effect, and is weakly positive. The second term captures changes driven by changes in the household's joy-of-giving caused by changes in *relative* income.

A small change to all endowments

Let some underlying parameter of the endowment distribution change infinitesimally. Then the accumulation of all of the changes in endowments can be written

$$\frac{dg_i}{d\omega_i} = \frac{pu_i''}{v_i'' + p^2 u_i''} + \frac{1}{v_i'' + p^2 u_i''} \int_{j=0}^{1} \frac{\partial v_i'}{\partial \phi_i} \frac{d\phi_i}{d\omega_j} dj \tag{12}$$

The first term is a traditional wealth effect, and is weakly positive. The second term captures changes driven by changes in the household's joy-of-giving caused by changes in the income distribution.

A1.4 Variance-dependent Utility

Let households i be uniformly distributed on the unit line with measure 1, $i \in [0,1]$. Each household has endowment ω_i to allocate between personal consumption x_i and voluntary contributions to charity, g_i. Without loss of generality, let households be ordered by their endowments, so that $i \geq j \Leftrightarrow \omega_i \geq \omega_j \forall i, j$.

Define ϕ_i as the variance of endowments,

$$\phi_i \equiv \int_0^1 (\omega_j - \bar{\omega})^2 dj, \tag{13}$$

where $\bar{\omega}$ denotes the mean, $\bar{\omega} \equiv \int_0^1 \omega_j dj$. Having specified this functional form for the behavioral influence of the income distribution on giving, we can now derive specific predictions.

Proposition 2: Own-endowment shock

Because the household is only concerned with the overall variance of endowments, changes to any single endowment (including their own) does not induce any behavioral effect:

$$\frac{d\phi_i}{d\omega_j} = \int_0^1 \left(\frac{d\omega_k}{d\omega_j} - \frac{d\bar{\omega}}{d\omega_j} \right)^2 dk = \int_j^j (1-0)^2 dk = 0$$

Setting the value of this term equal to zero in equation (11) returns

$$\frac{dg_i}{d\omega_i} = \frac{pu_i''}{v_i'' + p^2 u_i''}, \tag{14}$$

which is simply the household's response to an outward shift in the budget constraint without any behavioral role for the income distribution.

Also note that, by the envelope theorem, the total difference in giving with respect to changes in wealth is equal to the partial difference at the optimum, so that

$$\frac{dg_i}{d\omega_i} = \frac{pu_i''}{v_i'' + p^2 u_i''} = \frac{\partial g_i}{\partial \omega_i}. \tag{15}$$

Proposition 3: Uniform endowment change

Let us define a uniform increase in the distribution of income, so that for all i, $\omega_i' = \omega_i + a$, where a is a small constant. It follows that $\bar{\omega}' = \bar{\omega} + a$, and we denote by ϕ_i' household i's new summary statistic in the shifted income distribution. Plugging into the definition of ϕ_i,

$$\begin{aligned}
\phi_i' &= \int_0^1 \left(\omega_j' - \int \omega_k' dk \right)^2 dj \\
&= \int_0^1 \left(\omega_j + a - \left(\int \omega_k + a dk \right) \right)^2 dj \\
&= \int_0^1 \left(\omega_j + a - \bar{\omega} - a \right)^2 dj \\
&= \phi_i.
\end{aligned}$$

Under this utility function, a level shift in endowments does not change ϕ_i for any agent. Therefore, as in equation (14), any changes in giving are solely through the channel of a traditional endowment effect.

Proposition 4: Mean-preserving increase in variance

Now consider a mean-preserving spread that is proportional to the original distribution's distance between i and the mean. Let $\omega_i' = \omega_i + b(\omega_i - \bar{\omega})$ for all i, where b is a small, positive constant. This does not change the mean of the distribution: $\bar{\omega}' = \int_0^1 \omega_i + b(\omega_i - \bar{\omega}) di = \bar{\omega} + b\bar{\omega} - b\bar{\omega} = \bar{\omega}$. However, individual households do experience changes in their absolute wealth and in ϕ_i. Below-mean households are poorer; above-mean households are richer.

From the definition of ϕ_i it follows that

$$\begin{aligned}\phi_i' &= \int_0^1 (\omega_j' - \bar{\omega}')^2 dj \\ &= \int_0^1 (\omega_j + b(\omega_j - \bar{\omega}) - \bar{\omega})^2 dj \\ &= \int_0^1 \left((1+b)(\omega_j - \bar{\omega})\right)^2 dj \\ &= (1+b)^2 \phi_i \geq \phi_i,\end{aligned}$$

so that the change to ϕ_i is the same for all i (indeed, ϕ_i is invariant across i). The effect of this shock on individual donor decisions can be seen by setting $dp = 0$ in equation (9),

$$dg_i \underbrace{\left(v_i'' + p^2 u_i''\right)}_{\ominus} = \underbrace{pu_i''}_{\ominus} d\omega_i - \frac{\partial v_i'}{\partial \phi_i} \underbrace{d\phi_i}_{\oplus}, \tag{16}$$

where the sign of the second derivatives and $d\phi_i$ are known, and so the knowability and direction of the sign of dg_i depends on $d\omega_i$ (which is a function of i's position relative to the mean) and the sign of $\frac{\partial v_i'}{\partial \phi_i}$ (which is determined by the shape of the preference function). Combining the two possible signs of these items for four possible cases, the sign of dg_i is reported in the following tabulation.

Change in marginal utility	Change in endowment	Change in giving
$\frac{\partial v_i'}{\partial \phi_i} \geq 0$	$\omega_i \geq \bar{\omega} \Leftrightarrow d\omega_i \geq 0$	$dg \geq 0$
	$\omega_i \leq \bar{\omega} \Leftrightarrow d\omega_i \leq 0$	ambiguous
$\frac{\partial v_i'}{\partial \phi_i} \leq 0$	$\omega_i \geq \bar{\omega} \Leftrightarrow d\omega_i \geq 0$	ambiguous
	$\omega_i \leq \bar{\omega} \Leftrightarrow d\omega_i \leq 0$	$dg \leq 0$

For either case of the sign of $\frac{\partial v_i'}{\partial \phi_i}$, the effect will be unambiguous on one side of the mean endowment and ambiguous on the other, because of opposite signs on the wealth and behavioral effects.

Proposition 5: Inequality and Conditional Giving

We now demonstrate that a change in ϕ_i causes a change in giving that, after controlling for changes to each agent's endowment, is of uniform sign. From equations (9) and (16), we know that a change in ϕ_i implies marginal changes

$$dg_i = d\omega_i \frac{pu_i''}{v_i'' + p^2 u_i''} - \frac{1}{v_i'' + p^2 u_i''} \frac{\partial v_i'}{\partial \phi_i} d\phi_i. \tag{17}$$

Divide by $d\omega_i$ and express ratios of differentials in terms of meaningful changes resulting from an increase in inequality.

$$\frac{dg_i}{d\omega_i} = \frac{pu_i''}{v_i'' + p^2 u_i''} - \frac{1}{v_i'' + p^2 u_i''} \frac{\partial v_i'}{\partial \phi_i} \frac{1}{\frac{d\omega_i}{d\phi_i}}. \tag{18}$$

Use equation (15) to subtract the partial effect of a change in wealth from both sides.

$$\frac{dg_i}{d\omega_i} - \frac{\partial g_i}{\partial \omega_i} = \frac{pu_i''}{v_i'' + p^2 u_i''} - \frac{1}{v_i'' + p^2 u_i''} \frac{\partial v_i'}{\partial \phi_i} - \frac{pu_i''}{v_i'' + p^2 u_i''} \tag{19}$$

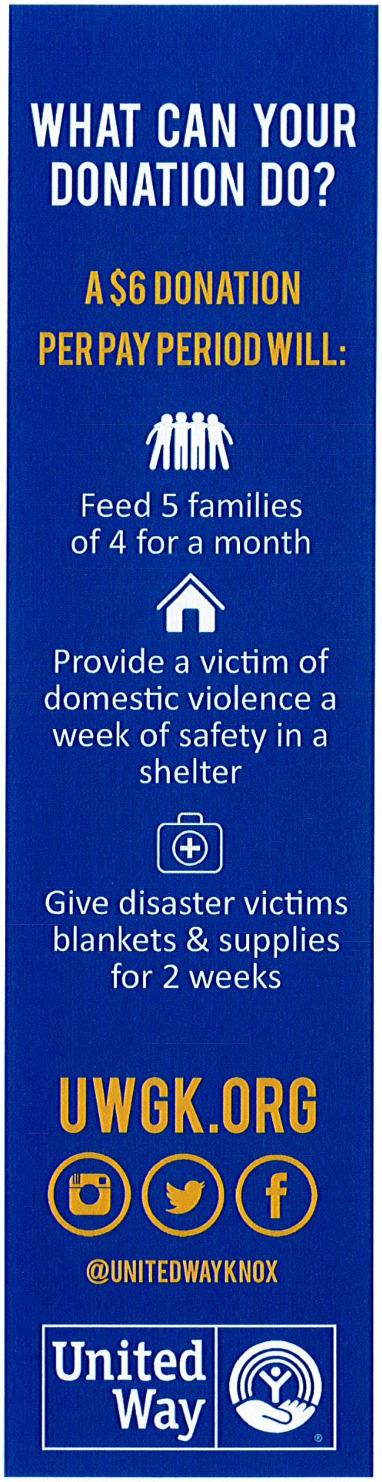
$$= \frac{-1}{v_i'' + p^2 u_i''} \frac{\partial v_i'}{\partial \phi_i} \frac{1}{\frac{d\omega_i}{d\phi_i}}. \tag{20}$$

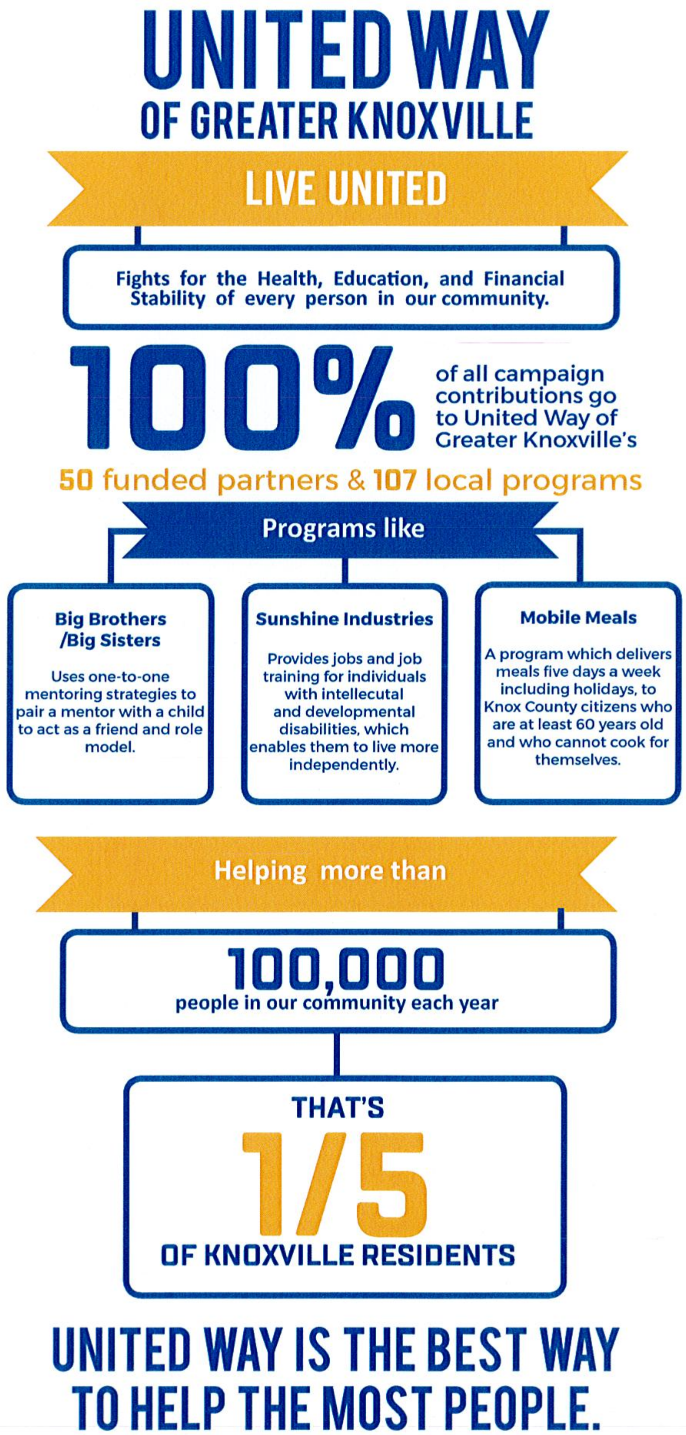
This gives us an expression for the change in giving with respect to a change in the income distribution, net of the endowment effect.

$$\left(\frac{dg_i}{d\omega_i} - \frac{\partial g_i}{\partial \omega_i} \right) \frac{d\omega_i}{d\phi_i} = \frac{-1}{v_i'' + p^2 u_i''} \frac{\partial v_i'}{\partial \phi_i}. \tag{21}$$

Because price is strictly positive and second derivatives of the utility function negative, the sign of this expression is known and is the same as the sign of $\frac{\partial v_i'}{\partial \phi_i}$. The conditional effect of inequality on giving is of the same sign for all i and follows from this expression. When greater inequality increases the marginal utility from giving ($\frac{\partial v_i'}{\partial \phi_i} \geq 0$), then an increase in inequality ($d\phi_i \geq 0$) leads to greater giving, conditional on endowment. When marginal utility is declining in inequality, the expression has the opposite sign, and conditional giving is lower.

A2 Additional Informational Materials





1301 Hannah Avenue
Knoxville, TN 37921
865-523-9131
865-522-7312 fax
www.uwgk.org

United Way

United Way of Greater Knoxville

Dear UT Experimental Economics Participant,

The purpose of this notice is to confirm that United Way of Greater Knoxville is an official partner in the study you are about to participate in. We are working with Prof Hargaden on this experiment. We have provided feedback on the wording of the questions to be asked. We assure you that this study has our support, and that any donations participants make are indeed real donations. If you would like to learn more about our work, we have provided Prof Hargaden with informational materials which you are welcome to view.

In addition to securing some additional funds for activities, this laboratory experiment will provide scientific evidence on people's perceptions about charitable giving. We thank you for participating in this research project.

Yours sincerely,

Ben Landers

Ben Landers
President & CEO of United Way of Greater Knoxville

LIVE UNITED™

A3 Informed Consent Document

Informed-Informational Document about Research

You are being asked to participate in the research project described below. Your participation in this study is entirely voluntary and you may refuse to participate, or you may decide to stop your participation at any time. Should you refuse to participate in the study or should you withdraw your consent and stop participation in the study, your decision will involve no penalty or loss of benefits to which you may be otherwise entitled. You are being asked to read the information below carefully, and ask questions about anything you don't understand before deciding whether or not to participate.

Title: Philanthropy Experiment 2017

Principal Investigator(s): Enda Patrick Hargaden, Ph.D.

PURPOSE OF THE STUDY

The purpose of this research is to examine how inequality and subsidies affect charitable-giving.

PROCEDURES

The research procedures are as follows: You will receive all the instructions describing your task and your payoffs from this task on your computer screen. All information is correct and true. Our research protocols specifically forbid us from providing incorrect or misleading information. You have the right to raise your hand and ask questions about the experiment protocols at any time. Your decisions will remain confidential and your choices will not be identified.

EXPECTED DURATION

The total anticipated time commitment will be approximately 60 minutes.

RISKS OF PARTICIPATION

There are no anticipated risks associated with participation in this project.

BENEFITS TO THE SUBJECT

There is no direct benefit received from your participation in this study, but your participation will help the investigator(s) better understand how economic institutions directly influence the decision making of its members.

CONFIDENTIALITY OF RECORDS

Every effort will be made to maintain the confidentiality of your study records. The data collected from the study will be used for educational and publication purposes, however, you will not be identified by name. For federal audit purposes, the participant's documentation for this research project will be maintained and safeguarded by Enda Hargaden for a minimum of three years after completion of the study. After that time, the participant's documentation may be destroyed.

FINANCIAL COMPENSATION

Your earnings are determined by the decisions you make in this experiment and are clearly explained in the instructions. At the end of the session, you will be paid privately and in cash.

INVESTIGATOR'S RIGHT TO WITHDRAW PARTICIPANT

The investigator has the right to withdraw you from this study at any time.

CONTACT INFORMATION FOR QUESTIONS OR PROBLEMS

The investigator has offered to answer all your questions. If you have additional questions during the course of this study about the research or any related problem, you may contact the Principal Investigator, Enda Hargaden, Ph.D., at phone number 865-974-8802 or by email at enda@utk.edu. If you have any questions about your rights as a research participant, you may also contact the Office of Compliance at phone number 865-974-7697 or by email at irb@utk.edu.

You have agreed to waive your signature on this form. Your voluntary participation is indicated by clicking the appropriate button on your screen to begin the experiment, and you may cease your participation at any time. In order to ensure anonymity of all respondents, please do not speak to those around you and direct any questions to the moderator by raising your hand. Such participation does not release the investigator(s), institution(s), sponsor(s) or granting agency(ies) from their professional and ethical responsibility to you.

A4 Instructions

I welcome everyone to the UT Experimental Economics Laboratory. My name is _________ and joining me is __________. We are researchers from the Department of Economics. We understand that many of you have busy schedules and thus really appreciate your willingness to participate.

In this study you will be asked to make a series of market-like decisions. Know that there are no right and wrong decisions. However, your earnings in this experiment are based on the decisions you make. What these means is that not everyone will make the same amount, and that some decisions will lead to higher earnings than others.

The money you will be paid with comes from a research grant, and this money can only be used to pay experiment participants. You will be paid, in cash, after the experiment is completed.

In some cases, the decision-making setting may be unfamiliar to you. This is normal. In writing the instructions for this experiment, we have done our very best to clearly describe to you all relevant information from which to base your decisions. It is important for the integrity of this research that you understand the instructions and we encourage you to ask questions as we go through the instructions.

There are two important protocols in experimental economics that we would like you to be aware of. First, we are forbidden from using deception. What this means is that the instructions contain only true information. There are no hidden tasks and the experiment works exactly as stated in the instructions.

Second, your decisions are confidential. What this means is that you have been randomly assigned an ID number. All decisions you make will be associated with this ID number and not your name. Therefore, when we analyze the data and present results your name in no way will be affiliated with this study.

We have provided everyone with a pencil, calculator, and paper. Use these items, if you wish, as you make your decisions.

Has everyone had a chance to read the "Informed Consent Sheet"? Is everyone comfortable with the risks involved with participation in this experiment?

We will now proceed by going through the instructions together. Please listen carefully as I read the instructions aloud. Do not hesitate to stop me at any time to ask a question.

The purpose of this experiment is to research people's willingness to donate to charity. Our collaborating partner for this experiment is United Way of Greater Knoxville, one of the largest charities in our community. United Way funds many programs, ranging from delivering hot meals to elderly citizens, to providing job training to people with intellectual disabilities. I have here a confirmation letter from United Way and some copies of their Annual Report if anyone would like to know more about the organization.

In this experiment, the computer will allocate a number of tokens to each of you. In each round you will be told how many tokens you received, and some information on how many tokens other people in the experiment received. Tokens are worth money, and will be converted into cash at the end of the experiment. Each token is worth 5 cents, and so you can think of 100 tokens being worth 5 dollars.

You will be asked how many of these tokens you are willing to donate to United Way.

To incentivize donations, you will see on-screen that we will match any donations with a varying amount. For example, you might see "For every token you donate, we will match this with two more." This means United Way would receive three tokens in total for every one you donate. The amounts donated will remain confidential.

We will analyze how participants' donations depend on this information. We will run several rounds of this experiment. From these, the computer will select two to count for real, and payments will be based on those two rounds. At the end of the last session, you will be asked to fill out a questionnaire and paid for the tokens from the two selected rounds, in addition to the $5 show-up fee. Everybody will be paid in private after showing the record sheet. You are under no obligation to tell others how much you earned.

You are free to leave at any time and you will still receive the show-up fee.

[To determine how many tokens you receive, at the start of the game you will have to do a task. This is called the slider task. You will see many "sliders" on screen. These are scales that go from zero to one hundred. By using the mouse and/or the keyboard, you need to slide the pointer across and position it at exactly 50. The more of these you successfully position at 50, the higher your score, and the more tokens you will receive when we get to the experiment. We will do a practice round of this game, where you will have one minute to position as many slides as you can at exactly 50. This is just for practice, and will not count for real.]

We will first run two practice rounds to familiarize you with the experiment. These are for practice, and will not count for real.

A5 Experimental Screens

This section presents images of additional screens shown to subjects in the course of the experiment. Figure A1 presents the confirmation screen confirming the subject's decision after submission. Figure A2 presents the slider screen used as an effort task in some treatments.

Figure A1: Confirmation Screen

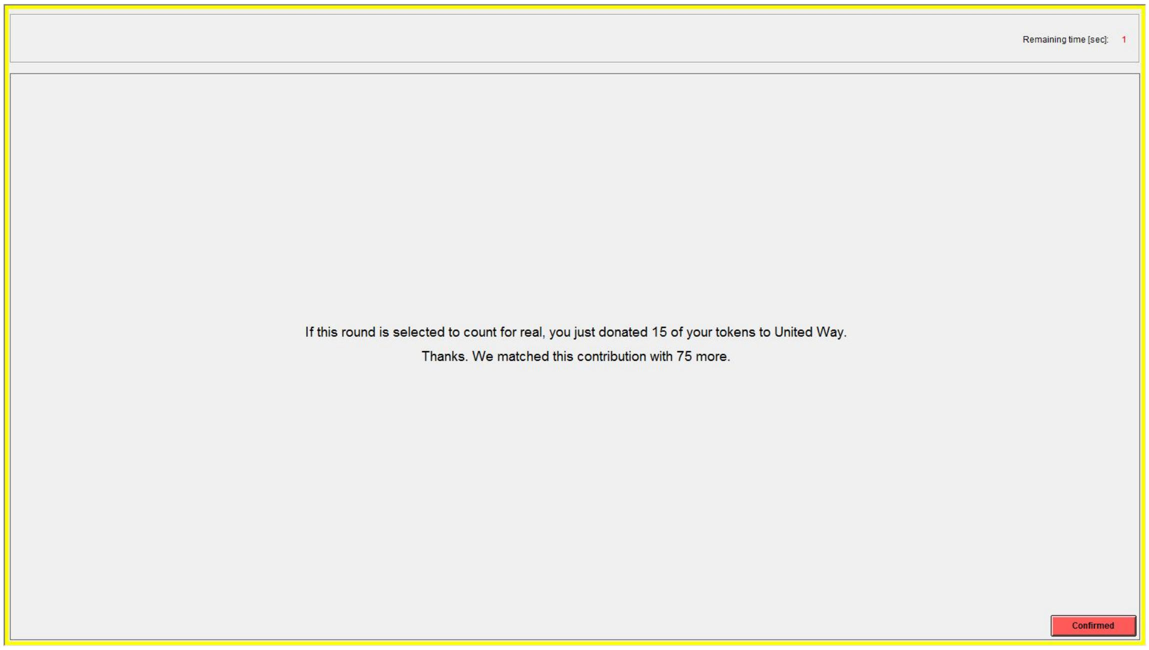


Figure A2: Slider Screen

A6 Supplemental Regression Tables

This section presents supplemental regression tables.

A6.1 Coefficients on suppressed control variables

Table A1 presents selected specifications from multivariate regression table 4 with the full set of regressors shown. (The no-controls and fixed-effects specifications, which had no suppressed regressors, are omitted.) Subjects are more generous if they are first-time participants, and less generous if they have taken more economics courses.

A6.2 Alternative functional forms

The succeeding tables present regressions with functional form alternatives to table 4, which estimates semi-elasticities. While we believe semi-elasticities are most appropriate for capturing the relationship between highly skewed giving and very even endowment and inequality variables, these extra specifications should allay any reader concerns that specification choices drive the reported associations.

Table A2 reestimates the main specifications in a log-log format, so that the reported coefficients can be interpreted as elasticities. Table A3 estimates the specification in levels for all variables. Lastly, table A4 allows for nonlinear income effects in giving by turning the continuous variable for endowment into a vector of indicator variables corresponding to endowments within bins of width 50.[23]

All of these tables are qualitatively consistent with the results in table 4. The only notable change is that in the log-log table, one specification (column 1) has a statistically insignificant confidence interval about the inequality variable. To be clear, the point estimate is essentially identical to columns 4 and 5, and the large and negative estimated magnitude is consistent with other estimations reported in this paper, but the omission of controls or fixed effects increases the imprecision of the standard errors. The level-level regressions are not meaningfully different from table 4. All other specifications in these tables report a large, negative, and statistically significant coefficient on inequality, notwithstanding the differences in functional forms.

A6.3 Additional behavioral regressors

Table A5 repeats the level-level framework with the addition of a new variable "visual midpoint" that captures the visual center point between reported mins and maxes of the distribution. Table A6 repeats this specification, but uses the actual mean endowment observed in the round, capturing the collective endowment of all players in a round, which varies at random depending on draws realized from the round's distribution. Inclusion of

[23]So, a variable equal to 1 if endowment $\in [0, 49]$ and zero otherwise, another for endowments between 50 and 99, and so on.

these controls for the actual or perceived middle of the endowment distribution does not change the results, and confirms for the concerned reader that it is the dispersion of the distribution and not its midpoint that drives our findings.

Table A7 considers whether giving behavior is correlated with order of play in a way that confounds the results, perhaps because the participants learn about their own preferences or grow tired over the course of the game. (Remember that our experiment does not inform participants about others' choices, so any learning would be self-oriented.) We flexibly allow for changes in preferences or self-knowledge over the course of the experiment by including either round fixed-effects (column 2) or terms for round and round squared as regressors (columns 3–5). In general, these additional terms are not strongly correlated with giving decisions, and their inclusion does not meaningfully change our estimated coefficients on variables of interest.

Table A8 adds a control for "relative inequality," measured as the ratio of the distribution's width to its visual midpoint.[24] The inclusion of this additional controls does not meaningfully affect our primary inequality measure's magnitude or significance. The coefficient on relative inequality is, like that on absolute inequality, negative, though it is not statistically different from zero in all specifications.

Table A9 replaces the min-max distance measure of inequality with the standard deviation of within-round endowments. This potentially captures responses proportional to variance of endowments, as in the functional form example given in section 2.2.[25] Consistent with the other checks in this subsection, the alternate measure does attenuate statistical significance in column 1 somewhat, but the negative association between inequality and giving remains large and negative in all specifications, and statistically different from zero in other specifications.

A6.4 Extreme values checks

In our main results, we handle extreme values (large gifts and zero contributions) with the inverse hyperbolic sine transformation, which (like a log transform) makes a skewed distribution more symmetric, while still keeping zero values in the data set. In the main text, we consider the importance of extensive- versus intensive-margin decision-making in a hurdle model. Here, we present some additional checks confirming that the way we handle extreme values is not leading to erroneous estimates of the causal effect of inequality on giving.

[24]For example, in our usual inequality measure, a round with endowments distributed over [100,300] is more unequal than one distributed over [50,150], because the former has width 200 and the latter width 100. But if subjects focus on the *relative* width of the inequality measure, these rounds have the same inequality, $\frac{300-100}{200} = \frac{150-50}{100} = 1$.

[25]Note, however, that subjects do not actually observe the full distribution of endowments, and that the standard deviation is affected by the subjects own endowment where the "extent of inequality" measure is not; for these reasons we prefer the min-max distance for our main specification.

Table A10 estimates a linear probability model of the decision to give. The dependent variable is 0 if the subject does not contribute tokens in a particular round and 1 if the subject makes a strictly positive contribution. Independent varaibles are as defined previously. Consistent with our discussion of the hurdle model, inequality has a negative effect on the extensive-margin decision to give with economically significant coefficients, although statistical significance varies across sets of control variables.

We also present Poisson regressions (so that the logarithm of contributions is taken with respect to expected rather than observed values). These specifications are in table A11. The signs and statistical significance of the variables of interest are unchanged; if anything statistical significance is more robust than in the log-linear baseline.

Lastly, A12 checks that the primary multivariate estimations of the paper are not driven by a small number of extreme values, by dropping observations for (1) study participants with the average contributions at or above the 99th percentile, or at or below the 1st percentile, and (2) rounds with inequality weakly above or below the 99th/1st percentiles. The specifications are otherwise identical to those in table 4. The magnitudes and statistical significance of the coefficients is little-changed, with the exception that the coefficient on inequality in column 1 goes from statistical significance at the 5% level to significance at the 10% level; point estimates are not substantially different. This confirms that a few unusual observations do not drive the results of our experiment.

Table A1: Effects of control variables on percent of endowment donated

	(1)	(2)	(3)
Endowment ('00s)	0.43***	0.42***	0.36***
	(0.037)	(0.036)	(0.026)
Match rate	0.088***	0.086***	0.092***
	(0.016)	(0.016)	(0.017)
Extent of inequality ('00s)	-0.096***	-0.095***	-0.061***
	(0.015)	(0.013)	(0.0075)
Age	-0.029	-0.036	-0.032
	(0.032)	(0.031)	(0.034)
Male	-0.39	-0.38	-0.39
	(0.23)	(0.25)	(0.24)
Married	0.73	0.67	0.70
	(1.15)	(1.15)	(1.15)
Social Class (1-5 scale)	-0.026	-0.031	-0.019
	(0.12)	(0.12)	(0.12)
First experiment	1.08***	1.10***	1.07***
	(0.18)	(0.25)	(0.18)
Well compensated (1-5 scale)	-0.038	-0.019	-0.034
	(0.13)	(0.11)	(0.13)
Understand experiment (1-5 scale)	0.0062	0.019	0.0071
	(0.089)	(0.078)	(0.092)
Economics courses taken	-0.086**	-0.082*	-0.085***
	(0.032)	(0.032)	(0.033)
Effect type	None	Session	Random
N	2,880	2,880	2,880

Table A2: Effects of inequality treatment in terms of elasticity interpretation

	(1)	(2)	(3)	(4)	(5)
Endowment ('00s)	0.38***	0.41***	0.41***	0.34***	0.34***
	(0.042)	(0.037)	(0.037)	(0.025)	(0.013)
Match rate	0.088***	0.095***	0.093***	0.097***	0.097***
	(0.016)	(0.016)	(0.016)	(0.017)	(0.0075)
Extent of inequality (log)	-0.14	-0.25***	-0.25***	-0.14***	-0.14***
	(0.073)	(0.042)	(0.047)	(0.020)	(0.036)
Control variables		✓	✓		✓
Experimental session FE			✓		
Participant FE				✓	
Random Effects					✓
Observations	2,880	2,880	2,880	2,880	2,880

Table A3: Effects of various treatments on level of contributions donated

	(1)	(2)	(3)	(4)	(5)
Endowment ('00s)	21.2***	22.3***	22.1***	19.1***	19.2***
	(3.90)	(3.88)	(3.80)	(3.28)	(0.66)
Match rate	1.43*	1.71**	1.68**	1.91**	1.90***
	(0.67)	(0.66)	(0.61)	(0.68)	(0.38)
Extent of inequality ('00s)	-2.49*	-3.43**	-3.22***	-1.57***	-1.65***
	(1.21)	(1.09)	(0.77)	(0.29)	(0.54)
Control variables		✓	✓		✓
Experimental session FE			✓		
Participant FE				✓	
Random Effects					✓
Observations	2,880	2,880	2,880	2,880	2,880

Table A4: Results with flexible specification for endowment

	(1)	(2)	(3)	(4)	(5)
Match rate	0.078***	0.082***	0.080***	0.088***	0.088***
	(0.015)	(0.015)	(0.016)	(0.0073)	(0.0073)
Extent of inequality ('00s)	-0.054*	-0.079**	-0.076**	-0.033***	-0.034***
	(0.026)	(0.020)	(0.021)	(0.011)	(0.011)
Endowment dummies	✓	✓	✓	✓	✓
Control variables		✓	✓		✓
Experimental session FE			✓		
Participant FE				✓	
Random Effects					✓
Observations	2,880	2,880	2,880	2,880	2,880

Results show effect of inequality on giving declines, and remains negative and significant when continuous variable endowment is broken into 50-token bins.

Table A5: Effects of various treatments on level of contributions donated

	(1)	(2)	(3)	(4)	(5)
Endowment ('00s)	21.1***	23.3***	22.9***	18.9***	19.1***
	(4.20)	(4.23)	(4.00)	(3.24)	(0.80)
Match rate	1.41	1.87**	1.81**	1.89**	1.89***
	(0.75)	(0.72)	(0.67)	(0.69)	(0.39)
Extent of inequality ('00s)	-2.70**	-1.80**	-1.95***	-1.77**	-1.77**
	(0.85)	(0.61)	(0.45)	(0.47)	(0.78)
Visual midpoint ('00s)	0.48	-3.84	-3.03	0.50	0.29
	(2.52)	(1.96)	(1.94)	(1.21)	(1.44)
Control variables		✓	✓		✓
Experimental session FE			✓		
Participant FE				✓	
Random Effects					✓
Observations	2,880	2,880	2,880	2,880	2,880

Table A6: Results are robust to including mean endowment as a control

	(1)	(2)	(3)	(4)	(5)
Endowment ('00s)	0.39***	0.43***	0.42***	0.32***	0.33***
	(0.051)	(0.046)	(0.046)	(0.024)	(0.016)
Match rate	0.079***	0.088***	0.086***	0.087***	0.087***
	(0.016)	(0.017)	(0.017)	(0.017)	(0.0077)
Extent of inequality ('00s)	-0.085**	-0.097***	-0.096***	-0.092***	-0.092***
	(0.026)	(0.021)	(0.016)	(0.015)	(0.014)
Mean Endowment ('00s)	0.040	0.0031	0.0026	0.10**	0.099***
	(0.057)	(0.053)	(0.051)	(0.027)	(0.027)
Control variables		✓	✓		✓
Experimental session FE			✓		
Participant FE				✓	
Random Effects					✓
Observations	2,880	2,880	2,880	2,880	2,880

Table A7: Inclusion of experiment round controls has little effect

	(1)	(2)	(3)	(4)	(5)
Endowment ('00s)	0.43***	0.43***	0.43***	0.36***	0.36***
	(0.037)	(0.039)	(0.037)	(0.025)	(0.013)
Match rate	0.088***	0.086***	0.092***	0.096***	0.096***
	(0.016)	(0.017)	(0.015)	(0.016)	(0.0080)
Extent of inequality ('00s)	-0.096***	-0.11***	-0.098***	-0.060***	-0.061***
	(0.015)	(0.027)	(0.013)	(0.0066)	(0.011)
Experimental round			-0.020	-0.017	-0.017
			(0.018)	(0.018)	(0.011)
Experimental round^2			0.00087	0.00076	0.00077*
			(0.00065)	(0.00060)	(0.00043)
Experimental round FE		✓			
Control variables	✓	✓	✓		✓
Participant FE				✓	
Random Effects					✓
Observations	2,880	2,880	2,880	2,880	2,880

Table A8: Controlling for relative inequality has little effect

	(1)	(2)	(3)	(4)	(5)
Endowment ('00s)	0.39***	0.42***	0.42***	0.32***	0.33***
	(0.050)	(0.047)	(0.047)	(0.027)	(0.016)
Match rate	0.081***	0.087***	0.085***	0.089***	0.089***
	(0.016)	(0.016)	(0.017)	(0.017)	(0.0076)
Extent of inequality ('00s)	-0.066*	-0.090**	-0.089**	-0.025**	-0.027**
	(0.030)	(0.026)	(0.027)	(0.0086)	(0.013)
Relative inequality	-0.00032	-0.00033	-0.00038	-0.0018***	-0.0018***
	(0.00092)	(0.00100)	(0.00093)	(0.00045)	(0.00044)
Control variables		✓	✓		✓
Experimental session FE			✓		
Participant FE				✓	
Random Effects					✓
Observations	2,880	2,880	2,880	2,880	2,880

Table A9: Effects when inequality is measured as standard deviation of endowments

	(1)	(2)	(3)	(4)	(5)
Endowment ('00s)	0.38***	0.42***	0.42***	0.36***	0.36***
	(0.044)	(0.036)	(0.035)	(0.025)	(0.013)
Match rate	0.085***	0.089***	0.087***	0.093***	0.092***
	(0.015)	(0.015)	(0.016)	(0.016)	(0.0076)
Within-round S.D.	-0.14	-0.27***	-0.28***	-0.18***	-0.18***
	(0.078)	(0.043)	(0.029)	(0.014)	(0.034)
Control variables		✓	✓		✓
Experimental session FE			✓		
Participant FE				✓	
Random Effects					✓
Observations	2,880	2,880	2,880	2,880	2,880

Table A10: Effects on binary giving decision

	(1)	(2)	(3)	(4)	(5)
Endowment ('00s)	0.018***	0.021***	0.021***	0.020**	0.020***
	(0.0037)	(0.0042)	(0.0038)	(0.0055)	(0.0032)
Match rate	0.022***	0.023***	0.022***	0.023***	0.023***
	(0.0043)	(0.0046)	(0.0047)	(0.0047)	(0.0019)
Extent of inequality ('00s)	-0.0037	-0.0065*	-0.0066**	-0.0054	-0.0055**
	(0.0031)	(0.0026)	(0.0025)	(0.0037)	(0.0026)
Control variables		✓	✓		✓
Experimental session FE			✓		
Participant FE				✓	
Random Effects					✓
Observations	2,880	2,880	2,880	2,880	2,880

Table A11: Poisson model of tokens contributed

	(1)	(2)	(3)	(4)	(5)
Endowment ('00s)	0.34***	0.34***	0.34***	0.29***	0.29***
	(0.029)	(0.030)	(0.027)	(0.0098)	(0.0019)
Match rate	0.036***	0.048***	0.045***	0.045***	0.045***
	(0.0099)	(0.011)	(0.0094)	(0.011)	(0.0011)
Extent of inequality ('00s)	-0.075***	-0.083***	-0.080***	-0.043***	-0.043***
	(0.028)	(0.028)	(0.023)	(0.0095)	(0.0019)
Control variables		✓	✓		✓
Experimental session FE			✓		
Participant FE				✓	
Random Effects					✓
Observations	2880	2880	2880	2856	2880

Table A12: Baseline regressions excl. extreme values for contributions or inequality

	(1)	(2)	(3)	(4)	(5)
Endowment ('00s)	0.408***	0.438***	0.437***	0.377***	0.379***
	(0.023)	(0.022)	(0.021)	(0.021)	(0.014)
Match rate	0.082**	0.090**	0.087**	0.093***	0.093***
	(0.018)	(0.018)	(0.019)	(0.016)	(0.008)
Extent of inequality ('00s)	-0.065*	-0.100***	-0.095***	-0.064***	-0.066***
	(0.017)	(0.011)	(0.008)	(0.016)	(0.012)
Control variables		✓	✓		✓
Experimental session FE			✓		
Participant FE				✓	
Random Effects					✓
Observations	2650	2650	2650	2650	2650

A7 Posterior Beliefs About These Findings

How much should the reader be convinced that our findings describe a general principle of the effect of inequality on prosocial behavior? In this appendix, we consider our findings' Post-Study Probability of being true, rather than a false positive. We do so by implementing the PSP calculation of Maniadis et al. (2014), who provide the following formula for PSP as a function of statistical power $(1-\beta)$, the threshold p-value for rejection of the null (α), the number of researchers working on similar questions (k), and the reader's prior belief that the study's finding is true before reading (π).[26]

$$\mathrm{PSP}^{Comp} = \frac{(1-\beta^k)\pi}{(1-\beta^k)\pi + [1-(1-\alpha)^k](1-\pi)} \tag{22}$$

In this paper, we have used the common 5% threshold for rejection of the null, so $\alpha = 0.05$. A power calculation implemented using Stata 15's `power rsquared` command finds that our statistical power is high, such that $1-\beta = 0.9851$. How convinced the reader ought to be by our findings depend on prior beliefs about the plausibility of our hypothesis, and about the number of research teams working on similar questions.

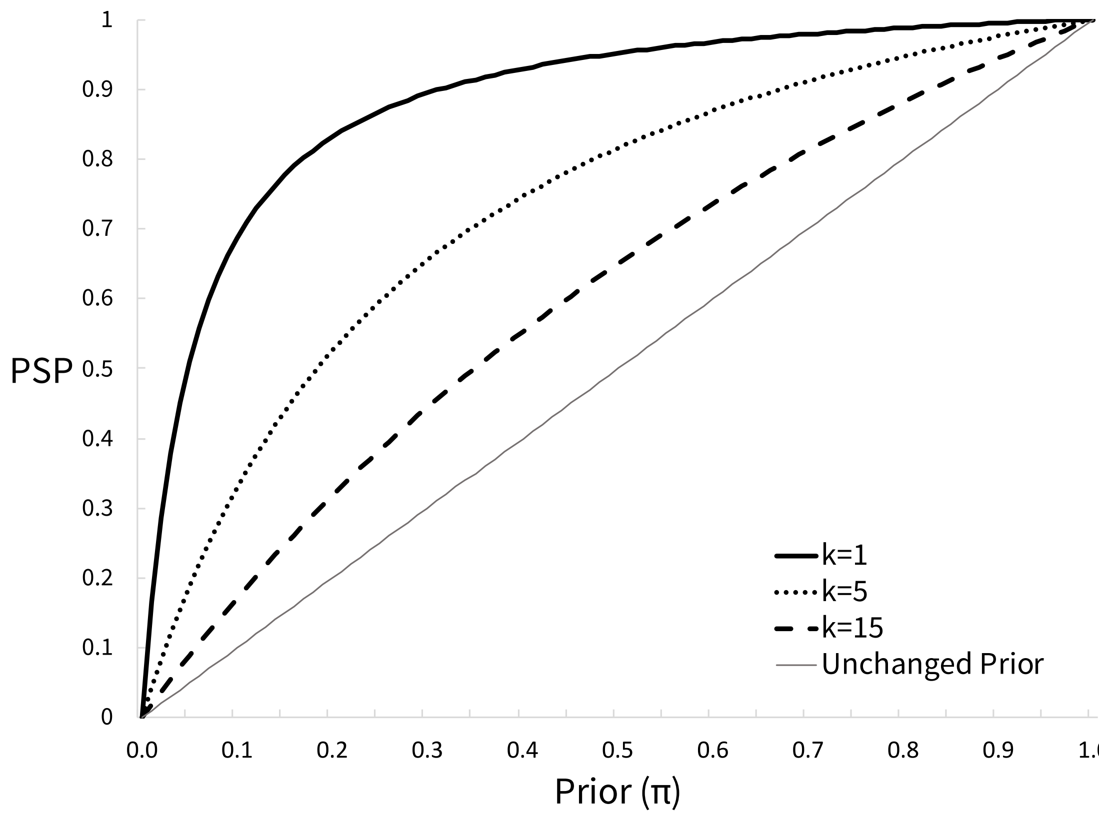
Figure A3 plots the PSP of this study as a function of π. The vertical axis marks the probability that this study reports a true finding (PSP), while the horizontal axis marks values of π. Following Maniadis et al., we plot three series for values of k equal to 1, 5, and 15. A 45-degree line marks posterior beliefs that are the same as prior beliefs.

How convincing one finds this study depends on prior beliefs that inequality affects giving, and about the number of researchers working on related topics. Starting from a neutral prior of $\pi = 0.5$, are 0.95 ($k = 1$), 0.82 ($k = 5$), and 0.65 ($k = 15$); that is, even if the reader believes our result may arise from a moderate number of researchers asking "similar" research questions, a shift in one's probabilistic belief that inequality decreases giving in excess of ten percentage points is reasonable.

Of course, as Maniadis et al.; Maniadis et al. argue, our findings will be most convincing only if and when they are replicated independently. To that end, our data and our z-Tree and Stata code are available to other researchers upon request, and will be posted online upon publication.

[26]Maniadis et al. (2014) extend their equation to include an additional term u that captures the bias of researchers toward "interesting" findings. The nature of our research question is that positive, negative, or zero effects of inequality on giving would a priori have been *equally* interesting and so we omit u from our analysis.

Figure A3: Post-Study Probability by Prior Beliefs and Competition



Notes: Lines plot the Maniadis et al. (2014) post-study probability that our findings are true, conditional on prior beliefs about truth π and number of researchers k.

A8 Multiple Hypothesis Testing

In this section, we check whether our test statistics over-reject the null of no finding due to multiple hypothesis testing. Table A13 restates our primary set of regression results (from Table 4), this time making explicit both the p values presented in that table, as well as a set of p-values adjusted for multiple hypotheses using the method of Bonferroni (1935). As List et al. (2016) note, the Bonferroni method is conservative, tending to *under*-reject when covariances within data structures support hypothesis rejection in the presence of multiple hypotheses.

Despite this conservatism, the Bonferroni-adjusted p-values presented in Table A13 remain quite small; only one coefficient of interest in one specification (our inequality variable in the no-control regression) changes significance level relative to commonly cited rejection thresholds: with Bonferroni $p \approx 0.76$, it is significantly different from zero at the 10% but not 5% level after MHT adjustment.

As our statistical significance looks broadly the same even after the conservative Bonferroni adjustment, we conclude that our findings are not overstated due to MHT.

Table A13: Estimation with MHT-adjusted p-values

Variable	β	Standard Error	Unadjusted p-value	Bonferroni p-value
Baseline model				
Endowment ('00s)	0.3968	0.0421	0.0002	0.0007
Match rate	0.0814	0.0153	0.0032	0.0095
Extent of inequality ('00s)	-0.0715	0.0227	0.0254	0.0762
With Controls				
Endowment ('00s)	0.4262	0.0369	0.0001	0.0003
Match rate	0.0878	0.0159	0.0026	0.0079
Extent of inequality ('00s)	-0.0961	0.0148	0.0013	0.0039
With Controls and Session Fixed Effects				
Endowment ('00s)	0.4241	0.0362	0.0001	0.0002
Match rate	0.0859	0.0162	0.0032	0.0096
Extent of inequality ('00s)	-0.0954	0.0132	0.0008	0.0024
With Individual Fixed Effects				
Endowment ('00s)	0.3583	0.0262	0.0000	0.0001
Match rate	0.0918	0.0167	0.0027	0.0081
Extent of inequality ('00s)	-0.0598	0.0074	0.0005	0.0015
With Controls and Random Effects				
Endowment ('00s)	0.3605	0.0132	0.0000	0.0000
Match rate	0.0917	0.0076	0.0000	0.0000
Extent of inequality ('00s)	-0.0609	0.0107	0.0000	0.0000