ARTICLE IN PRESS

Journal of Behavioral and Experimental Economics xxx (xxxx) xxx–xxx

Contents lists available at ScienceDirect

Journal of Behavioral and Experimental Economics

journal homepage: www.elsevier.com/locate/jbee

Giving to charity to signal smarts: evidence from a lab experiment☆

Felipe Montano-Campos[a], Ricardo Perez-Truglia[*,b]

[a] Universidad de San Andres, Argentina
[b] University of California, Los Angeles, US

ARTICLE INFO

Keywords:
Charitable giving
Signaling
Income
Intelligence

JEL classification:
C51
D11
D12

ABSTRACT

The literature on charitable giving suggests that individuals may use their charitable donations to signal their altruism or their income. We argue that, rather than signaling income *per se*, individuals may want to signal other unobservable characteristics that correlate to income, such as their intelligence. We designed a laboratory experiment to test this hypothesis. We assigned endowments to individuals who could spend all or part of those endowments on a charitable donation. We cross-randomized the visibility of donations and the individuals' perceptions about the effect of intelligence on the allocation of endowments. We found that the effect of donation visibility on donation amounts depends sharply on whether the individuals perceive that endowments are determined by intelligence. This evidence suggests that, consistent with our hypothesis, subjects may engage in charitable giving to signal their smarts.

1. Introduction

Studies show that individuals are more generous in public than in private (Karlan and McConnell, 2014a). The typical interpretation is that individuals use pro-social behavior as a signal of altruism (Bénabou and Tirole, 2006) or income (Glazer and Konrad, 1996; Bracha and Vesterlund, 2017). We propose that individuals also may want to signal other unobservable characteristics that correlate with income. For instance, they may want to show off the intelligence and effort that made them rich. In this paper, we present novel evidence from a laboratory experiment designed to disentangle this mechanism.

In our laboratory experiment, we assigned individuals to private endowments of different sizes. Subjects could spend some of their endowments on a charitable donation, and keep the rest of the endowment in cash. We cross-randomized two conditions: *private/public* and *random/meritocratic*. The first randomization affected the visibility of their charitable donation. In the *private* condition, subjects were informed about the donation amounts made by everyone else participating in the experiment but not their names. In the *public* condition, subjects received the same feedback about donation amounts, plus the full names of the donors. According to the conspicuous consumption model, individuals will donate more in the *public* condition than in the *private* condition, because the former is a costly signal of their endowments (Cole et al., 1995; Bagwell and Bernheim, 1996).

The second randomization was intended to create exogenous yet non-deceptive variation in participants' beliefs about the correlation between intelligence and endowments. For example, if an individual believes that endowments are uncorrelated with other individual characteristics, then the public-private gap in donations would measure the demand for a signal of endowment *per se*. However, if an individual thinks that endowments are perfectly correlated to intelligence, then the public-private gap in donations would measure the demand for a signal of endowment, as before, along with the demand for a signal of intelligence. Thus, we can test the hypothesis that individuals care about signaling intelligence by studying how the public-private gap in donations changes with perceptions about the correlation between endowments and intelligence.

We devised a non-deceptive method to create exogenous variation in these beliefs. We administered a 30-minutes intelligence test to all subjects a week before the main round of the experiment. For individuals assigned to the *random* condition, endowment amounts were assigned purely by chance, so there was no correlation between their intelligence test scores and endowments. For individuals assigned to the *meritocratic* condition, endowment amounts were assigned based on their intelligence test scores, so there was a perfect correlation between their scores and endowments. As long as we compare pairs of individuals who share the same intelligence test scores and same endowment amounts, then the only difference between participants in the

☆ We thank David Laibson for very useful input at the beginning of the project. This project was reviewed in advance and approved by the IRB at Harvard University. Funding for the experiment was provided by the Russell Sage Foundation. We thank Fiorella Benedetti for outstanding research assistance.
* Corresponding author.
E-mail addresses: jmontanocampos@udesa.edu.ar (F. Montano-Campos), ricardo.truglia@anderson.ucla.edu (R. Perez-Truglia).

https://doi.org/10.1016/j.socec.2018.08.004
Received 23 October 2017; Received in revised form 16 June 2018; Accepted 19 August 2018

Please cite this article as: Montano Campos, F., Journal of Behavioral and Experimental Economics, https://doi.org/10.1016/j.socec.2018.08.004

random and *meritocratic* conditions should be their perceptions about the correlation between endowments and intelligence test scores (i.e., zero correlation in the *random* condition and perfect correlation in the *meritocratic* condition).

Our experiment reveals that when the individuals think that endowments are uncorrelated to intelligence, the public-private gap is negative and statistically insignificant. This finding suggests that, if anything, individuals get negative utility from revealing to others that they were assigned to a high endowment by chance. Indeed, this evidence is consistent with Bracha and Vesterlund (2017), who find a negative effect of donation visibility on donation amounts when endowments are private. More important, the public-private gap in donation amounts becomes positive when individuals believe that endowments were assigned based on intelligence. This increase in the public-private gap is statistically significant, economically substantial, and robust across specifications. This finding suggests that individuals want to use their public donations to signal their intelligence test scores.

This paper belongs to a literature studying social signaling in the laboratory and in the field.[1] In particular, this study focuses on the role of social signaling for charitable giving. Studies show that individuals are more generous in public than in private (Andreoni and Petrie, 2004; Rege and Telle, 2004; Dana et al., 2006; Karlan and McConnell, 2014a), which is consistent with the hypothesis that individuals use pro-social behavior as a signal of altruism or fairness (Bénabou and Tirole, 2006; Andreoni and Bernheim, 2009; Ariely et al., 2009). Other studies argue that individuals use charitable contributions to signal their income (Glazer and Konrad, 1996; Bracha and Vesterlund, 2017). We contribute to this literature by showing that charitable donations may be used to signal attributes that correlate with income, such as talent or intelligence.

Our study is also related to a literature on conspicuous consumption beyond charitable giving. This literature suggests that individuals engage in conspicuous consumption to signal their income.[2] We contribute to this literature by providing evidence that individuals may be interested in signaling attributes that are correlated to income rather than signaling income *per se*. To the best of our knowledge, there is only one paper testing this mechanism: Clingingsmith and Sheremeta (2015). In the context of a lab experiment, they provide subjects with the opportunity to purchase chocolate truffles, and they randomly vary the visibility of chocolate consumption and whether the income available for chocolate consumption is linked to social status. Consistent with our findings in the context of charitable giving, Clingingsmith and Sheremeta (2015) find that making consumption choices visible leads to a large increase in demand when income is linked to status, but not otherwise. Our paper is also related to McManus and Rao (2015), who designed a novel sorting experiment to facilitate ability signaling in the lab. However, contrary to our findings and those of Clingingsmith and Sheremeta (2015), McManus and Rao (2015) find that social observation discouraged high-ability sorting.

The rest of the paper proceeds as follows. Section 2 introduces the conceptual design, experimental design, and hypothesis and regression specification. Section 3 reports the implementation details, descriptive statistics and randomization balance, main results and robustness checks. The last section concludes.

[1] According to the social signaling theory, individuals may engage in behavior, such as donating to a charity or buying an expensive car, to signal certain characteristics that would be otherwise unobservable to their peers.

[2] Evidence indicates that, consistent with income signaling, richer individuals consume more highly observable goods (Heffetz, 2011; Charles et al., 2009). Moreover, recent experimental evidence supports this interpretation (Roth, 2014; Bursztyn et al., 2017).

2. Research design

2.1. Conceptual design

Consider an individual who must decide how much to give away from a fixed endowment. First, we would like to randomize whether the donation is observed by others or not. To do this without using deception is simple, we randomize participants into two groups: in one group the donations are *public*, and in the other group they are *private*. Second, holding fixed the individuals and their endowments, we want to randomize whether they think that endowments and intelligence are correlated. The ideal way to do this is to randomize all endowments and then randomize whether we tell each individual that the endowments are correlated to intelligence. However, we cannot do that, because that would be deceptive (i.e., we would be lying to the individuals whom we tell that the endowments are *meritocratic*). Instead, we randomize individuals into one of two groups, in which the endowments are determined either randomly or by intelligence test scores. This achieves the goal that individuals in the first group think that the endowments are *random*, and individuals in the second group think that the endowments are correlated to intelligence (*meritocratic*).

Comparing these two groups introduces another key difference, a sorting effect, due to the correlation between intelligence and endowments. This sorting effect can influence the distribution of donations. For example, evidence suggests that intelligence is highly correlated to altruism (Millet and Dewitte, 2007).[3] Consequently, assigning higher endowments to individuals with higher intelligence test scores may affect the average donation. To isolate the desired effect (i.e., perceptions about meritocracy) from the sorting effect we compare groups of individuals who have the same intelligence test score and were assigned (randomly or meritocratically) to the same endowment. Because these individuals share the same combination of intelligence test scores and endowments, comparing across the *meritocratic* and *random* groups is not associated with any sorting effect. The remaining variation, thus, corresponds to the desired mechanisms of perceptions about meritocracy.

2.2. Experimental design

Appendix A shows the English translation of the instructions shown to the subjects, for each and every treatment group. The experiment is divided in two rounds. The first round takes place in a classroom during which subjects were given 30 minutes to complete an analytical test similar to the quantitative portion of the Graduate Record Examination. We explain to subjects that scores from this type of exam are routinely used to screen candidates applying to competitive jobs and graduate school. We also encouraged individuals to perform the test in the best possible way because their chances of earning money in the experiment increase with their test performance. This was not deceptive: at this round, students had not been randomized into treatment groups yet, and thus there was a 50% chance that they would be randomized into the meritocratic group (where higher scores translate into higher payments).

One week later, the students complete the second round of the experiment online. Each participant receives a link to an online survey, including all the instructions for this second round. In this survey, individuals are told about their $35 fixed payment (just for participating in the experiment). Second, subjects find out about the treatment group that was randomly assigned to them. Third, they find out about their endowment ($10, $20, $30, or $40). Last, they are asked to decide how much of their endowment they wanted to donate to a charitable cause:

[3] Millet and Dewitte (2007) conducted a repeated public good game and show that more intelligent individuals behave more altruistic in contributing to the public good.

the donations are directed to the university' s student council, which will then use the funds to buy school supplies for disadvantaged schools in the area.[4] Subjects have the option to donate from $0 up to their endowment amount, in $10 increments. Once the second round is finalized, subjects can collect their final payments (the fixed payment of $35 plus the amount of their endowment that they did not donate) in a sealed envelope from an office located at the university campus.

During this second round of the experiment, subjects were randomized into one of four treatment groups, each with an equal number of subjects: *random-private, random-public, meritocratic-private*, and *meritocratic-public*. In the *random* treatment group, a random 25% of the subjects were assigned either $40, $30, $20, or $10 each. In the *meritocratic* group, the endowments were assigned according to the subject' s performance on the analytical test: subjects who scored in the top 25% were assigned $40 each; participants with the next 25% best scores were assigned $30 each; participants with the next 25% best scores were assigned $20 each; and participants in the bottom 25% of test scores were assigned $10 each.

Before letting the subjects choose their donation amounts, we informed them that they would be receiving an email listing the distribution of donations in their groups. Students were shown a screen with a sample of the email that would be sent out (see Appendix A.2). In the *private* condition, subjects were informed that they would receive emails with a list of anonymized donations by all participants in their group. For example, the list would indicate that Anonymous Participant 1 donated $10, Anonymous Participant 2 donated $20, Anonymous Participant 3 donated $30, and so on for all subjects in the group. In the *public* condition, subjects were informed that they would receive emails listing all donor names and donation amounts. For example, the list would show that Jane Doe donated $10, John Doe donated $20, Justin Doe donated $30, and so on for all subjects in the group.

Subjects received information on the entire distribution of contributions regardless of whether they were assigned to the *public* or *private* conditions. The only difference between these two conditions was whether the information was anonymized. This allowed us to disentangle the effect of publicity from the effect of the information *per se*. This allow us to rule out other potential effects from visibility, such as the altruistic desire to set an example for others to follow (Karlan and McConnell, 2014a).

Last, after making the donation decision, we asked subjects a few background questions, such as gender, age, college major, whether they understood the rules of the experiment, and whether they wanted to be contacted for future experiments – a copy of the full questionnaire is provided in Appendix B.

2.3. Hypothesis and regression specification

Consider the following linear regression framework:

$$Y_i = \alpha \cdot Public_i + \theta \cdot Meritocratic_i + \beta \cdot Public_i {*} Meritocratic_i + \gamma \cdot X_i + \delta_i + \epsilon_i$$

Y_i is the donation of individual i and ϵ_i denotes the usual error term. $Public_i$ is a dummy variable that takes the value 1 when the donation is *public* and 0 if *private*. $Meritocratic_i$ is a dummy variable that takes the value 1 if the endowments were assigned based on intelligence test scores and 0 if they were randomly assigned. X_i is a vector with a typical set of control variables: family income, preferences for redistribution and three dummies for college major.

δ_i is a set of intelligence-by-endowment fixed effects. This set of dummies allows us to compare between pairs of individuals who are similar in intelligence test scores and endowments, and thus eliminate the sorting effect of the *meritocratic* vs. *random* conditions. In the baseline specification, this set of dummies indicate each unique combination between intelligence test scores (in 3-point increments) and endowments ($10, $20, $30 and $40). In the next section, we also present results under alternative specifications.

The interpretation of our parameters of interest are as follows. Parameter α measures the effect of publicity in the random-allocation world. The parameter α would capture social signaling when allocations are random. An $\alpha > 0$ could also arise for a multiplicity of reasons that have been discussed in the charitable giving literature. For example, $\alpha > 0$ could represent a demand for signaling income *per se*, as in the conspicuous consumption models (Glazer and Konrad, 1996; Bracha and Vesterlund, 2017). Additionally, $\alpha > 0$ could represent a demand for a signal of generosity (Bénabou and Tirole, 2006; Millet and Dewitte, 2007; Ariely et al., 2009; Andreoni and Bernheim, 2009), or an effort to avoid being seen as stingy (Samek and Sheremeta, 2014; 2017). Also, $\alpha > 0$ could be due to reasons beyond social signaling, such as setting an example for others to follow (Karlan and McConnell, 2014b).

The parameter θ measures whether *private* donations are affected by the belief that endowments were assigned according to intelligence test scores. In particular, $\theta < 0$ would indicate that individuals feel less guilty about not donating when endowments are *meritocratic*, because they feel that they "deserve" their allocation.

Last, β provides a test of the main hypothesis. This parameter measures whether the effect of donation publicity on donation amount is higher when the individuals believe that endowments are correlated to intelligence test scores. Our hypothesis is that, if individuals demand a signal of their intelligence test scores, then $\beta > 0$.

However, one key assumption is that the other benefits of publicity that have been identified in the charitable giving literature, such as the demand to be seen as generous, avoid being seen as stingy or lead by example, do not depend on whether the allocations are random or meritocratic. In that case, those mechanisms should not be picked up by the parameter $\beta > 0$. This seems to us like a reasonable assumption – for example, if I make a visible gift of $100, that should speak highly about my altruism regardless of whether I earned the $100 by building a rocket or by winning the lottery. However, there are potential violations to this assumption – for instance, if individuals think that altruism is correlated to intelligence, then giving may become a stronger signal of altruism when the endowments are meritocratic.

3. Results

3.1. Implementation details

We recruited a pool of students from the Universidad de San Andres, a small private university from Buenos Aires (Argentina), to participate in this and other (unrelated) experiments. The opportunity to sign up for the experiments was publicized through flyers that were posted around the university campus (see the flyer in the Appendix C). The flyer was not specific to this particular experiment, and just provided generic information about the typical length of the experiments and the typical payment amounts. At that time, there were about 255 undergraduate students per cohort, adding up to a total of about 1020 students. During the first two years, all students share the same core classes regardless of the major they choose. These characteristics make it quite likely that any two subjects know each other, and a non-negligible probability that they are close friends. This provides a setting in which it is possible that individuals care about their social image (Winniford et al., 1997).

In the fall of 2011, roughly 200 students signed up by email to be invited to participate in an experiment. We invited all interested students to come to one of the classrooms at a specific date and time to take part in this experiment. A total of 112 students showed up and completed the first round of the experiment. Conditional on participating in the first round of the experiment, all of the subjects completed the second round a week later. There were at least three reasons for the

[4] The school administrators informed us that the students perceived this organization and cause to be legitimate.

Table 1
Summary statistics.

	Mean	St. Dev.	Min	Max
Donation	12.05	9.50	0	40
Share Donated	0.55	0.38	0	1
Intelligence Test Score	9.43	2.99	3	17
Age	20.53	2.02	18	27
Female	0.46	0.50	0	1
Income	2.46	0.90	1	5
Father's Education	5.75	1.21	1	7
Preferences for Redistribution	3.99	0.93	1	5
Understanding	2.85	0.41	1	3
Future Experiment	0.99	0.09	0	1
Economics	0.41	0.49	0	1
Business/Accounting	0.26	0.44	0	1
Law/Politics	0.15	0.36	0	1
Other Majors	0.18	0.38	0	1

Notes: $N = 112$. Donation refers to the amount donated by each subject in the second round of the experiment. Share Donated refers to the proportion of endowment donated (donation/endowment). Intelligence Test Scores is the score obtained by individuals in the first round of the experiment in the analytical test. Age provides information about years old of students. Female is a dummy variable that takes value 1 for girl students. Income is a categorical variable that goes from 1 (much lower than the average income of classmates) to 5 (much higher than the average income of classmates). Father's Education is a categorical variable that goes from 1 (less than High School) to 7 (postgraduate degree). Preferences for Redistribution is a categorical variable that goes from 1 (I strongly disagree with expanding social programs) to 5 (I strongly agree with expanding social programs). Understanding is a categorical variable that goes from 1 (limited understanding of the experiment rules) to 3 (completely understanding of the experiment rules). Future experiment is a dummy variable that takes value 1 whether students wanted to be contacted for future experiments. Economics takes value 1 whether student's major is Economics. Business/Accounting takes value 1 whether student's major is either Business Administration or Accountancy. Law/Politics takes value 1 whether student's major is either Law, Political Science or Educational Science. Finally, Other Majors takes value 1 whether student's major Communications or International Affairs.

lack of attrition. First, the second round of the experiment was easier, as it took place online and lasted for only 10 minutes. Second, we sent email reminders every morning to participants who had not completed the second round until they completed it (most students completed the survey on the same day, and everyone had completed it within a week). Third, even though they had already gone through the (longer and more tedious) first round, the participants could not be paid until they completed the second round of the experiment.

3.2. Descriptive statistics and randomization balance

Table 1 shows summary statistics of the data. Regarding their demographics, the average subject was 20.5 years old, and 46% of them were female. Subjects reported to understand the rules of the experiment quite well: on a scale from 1 (limited understanding) to 3 (completely understanding), the average understanding score was 2.85. Additionally, 99% of the subjects reported that they desired to be invited to future experiments. Table 1 also presents summary statistics for the main outcomes for the analysis: the average donation in the experiment was $12.05 Argentinian pesos ($2.75 U.S. dollars at the time of the experiment), and the average share of endowment donated was 55%. Fig. 1a provides a histogram of the amount donated – by construction, this outcome can take one of five values: $0, $10, $20, $30 or $40. Fig. 1b shows the distribution of donations expressed as shares of endowment – by construction, this outcome can take one of seven values: 0%, 25%, 33%, 50%, 67%, 75% or 100%.

In the second round of the experiment, the participants were randomly assigned to one of four groups, with 28 subjects each. Even though the subjects were randomly assigned to these 4 groups, it is important to verify that the randomization was indeed successful. Table 2 presents sample statistics for each treatment group and the p-value from the test of the null hypothesis of equal means across the four treatment groups. We find that the sample is balanced in terms of gender, age, score and other observable characteristics. The only difference that is (borderline) statistically significant occurs in preferences for redistribution (p-value = 0.096) -a categorical variable that goes from 1 (I strongly disagree with expanding social programs) to 5 (I strongly agree with expanding social programs). However, this difference does not necessarily imply that the randomization was unsuccessful: we expect one out of every ten tests to be statistically significant at the 10% level by chance. Nevertheless, to fall on the safe side, we include preferences for redistribution in the set of control variables for the regression analysis.

3.3. Main results

Table 3 shows the regression results. Column (1) presents the results for the baseline specification, which uses a Poisson regression model so that the coefficients can be interpreted as semi-elasticities. In column (1) of Table 3, the coefficient on *Meritocratic* (-0.384) is negative and statistically significant at the 1% level. This coefficient suggests that, relative to perceiving endowments to be assigned at *random*, perceiving that endowments are assigned by intelligence reduced the desired donation amount by 31.9% ($e^{-0.384} - 1$). This effect is not only statistically significant but also economically substantial. Our favorite interpretation is that, under the belief of meritocracy, individuals feel that they deserve their allocations and thus feel less guilty about not sharing them. This finding coincides with existing experimental evidence that indicates that income source plays a central role in the decision to donate or share in experiments. For instance, dictators are less likely to give income earned compared to windfall income (Hoffman et al., 1994; Ruffle, 1998; Cherry et al., 2002; Oxoby and Spraggon, 2008; Carlsson et al., 2013), and individuals are less likely to donate when they believe that their endowments are determined by their own efforts (Tonin and Vlassopoulos, 2017).

Column (1) of Table 3 also shows that the estimated coefficient on *Public* is -0.197. This coefficient implies that, when individuals think that endowments are randomly assigned, they donate 17.9% ($e^{-0.197} - 1$) less if their donations will be made *public*. However, we must take this finding with a grain of salt, because the coefficient is not statistically significant at the 10% level. The direction of the effect is inconsistent with the desire to signal attributes such as altruism, income – and it is also inconsistent with the desire to lead by example. One possible explanation is that individuals get negative utility from revealing that they were lucky and received a large endowment, due to guilt, shame or social preferences. Indeed, this finding is consistent with the results from Bracha and Vesterlund (2017). They conducted an experiment where, like in our setting, endowment size was private information and each donation was randomly assigned to be public or private. Bracha and Vesterlund (2017) reports that publicizing donations reduced donation amounts by 14.65%, which is in the same order of magnitude as the corresponding effect in our data.

Column (1) of Table 3 also reports the coefficient on *Public*Meritocratic*, which provides the test of the main hypothesis. The estimated coefficient (0.459) is positive, large in magnitude, and statistically significant (p-value = 0.03). This coefficient implies that the publicity premium increases from -17.9% ($e^{-0.197} - 1$) when individuals believe that endowments are *random* to +29.9% ($e^{-0.197+0.459} - 1$) when individuals believe that endowments are assigned by intelligence test scores. This evidence suggest that, consistent with our main hypothesis, individuals want to use conspicuous consumption to signal their intelligence.

Recall that one potential confounding factor is that subjects may not be interested in signaling intelligence *per se*, but some other factor that is correlated to intelligence. For instance, if individuals think that

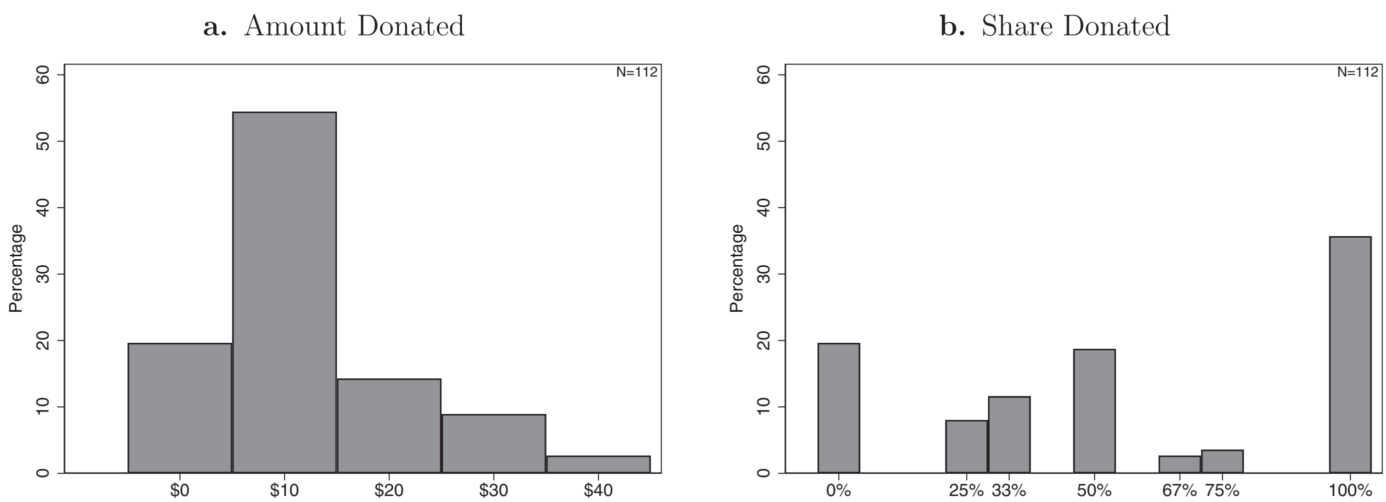

Fig. 1. Distribution of donations. *Notes: N* = 112. The figure in Panel (a) shows the distribution of the amount donated by all subjects in the experiment and the figure in Panel (b) shows the distribution of the share donated by all subjects in the experiments. In the second round of the experiment individuals were told about their endowment ($10, $20, $30 or 40$), and were asked to decide how much of their endowment they wanted to donate, from $0 to their total endowments, in $10 increments. *Amount Donated* refers to the amount donated by each individual and *Share Donated* refers to the percentage of endowment donated by each individual.

Table 2
Randomization balance test.

	All	Random-private	Random-public	Meritocratic-private	Meritocratic-public	F-test P-value
Intelligence Test Score	9.429	10.107	9.036	8.786	9.786	0.317
	(0.282)	(0.594)	(0.513)	(0.557)	(0.583)	
Age	20.527	19.929	20.571	20.893	20.714	0.317
	(0.191)	(0.385)	(0.383)	(0.393)	(0.360)	
Female	0.455	0.429	0.429	0.500	0.464	0.944
	(0.047)	(0.095)	(0.095)	(0.096)	(0.096)	
Income	2.459	2.429	2.481	2.500	2.429	0.988
	(0.086)	(0.195)	(0.154)	(0.174)	(0.166)	
Father's Education	5.748	5.750	5.778	5.786	5.679	0.982
	(0.115)	(0.294)	(0.209)	(0.214)	(0.200)	
Preferences for Redistribution	3.991	4.214	3.593	4.000	4.143	0.096
	(0.088)	(0.130)	(0.215)	(0.171)	(0.168)	
Understanding	2.847	2.929	2.778	2.893	2.786	0.346
	(0.039)	(0.050)	(0.082)	(0.060)	(0.107)	
Economics	0.411	0.464	0.321	0.321	0.536	0.271
	(0.047)	(0.096)	(0.090)	(0.090)	(0.096)	
Business/Accounting	0.259	0.250	0.321	0.214	0.250	0.845
	(0.042)	(0.083)	(0.090)	(0.079)	(0.083)	
Law/Politics	0.152	0.179	0.071	0.250	0.107	0.264
	(0.034)	(0.074)	(0.050)	(0.083)	(0.060)	
Other Majors	0.179	0.107	0.286	0.214	0.107	0.255
	(0.036)	(0.060)	(0.087)	(0.079)	(0.060)	
Observations	112	28	28	28	28	

Notes: All Individual characteristics obtained from baseline survey. Standard errors in parenthesis. First column presents statistics for all individuals in the experiment. The following four columns correspond to the four treatments groups in the experiment. *Random*refers that the endowments were assigned randomly. *Meritocratic*refers that the endowments were assigned according to individuals' performance in the analytical test. In the *Private*condition individuals received a list of anonymized donations made by all participants in their same group. In the *Public*condition individuals received a list of not anonymized donations made by all participants in their same group (i.e., full names of donors was shown). Final column presents p-value for a F-test of the null hypothesis of equal means across the four treatment groups. For more data definitions, see notes to Table 1.

intelligence test scores are associated with higher earnings potential, then individuals may use the signal of intelligence as an indirect signal of future income. Similarly, if individuals think that intelligence is positively associated with altruism, then individuals may want to use a signal of intelligence as an indirect signal of altruism. However, the negative coefficient on *Public* indicates that, under random allocations, individuals were not interested in signaling income or altruism directly. Thus, if they did not take the opportunity to signal those attributes *directly*, it seems unlikely that they will try to signal them *indirectly*.

3.4. Robustness checks

Columns (2) to (7) of Table 3 present some robustness checks based on alternative specifications. In the baseline specification from column (1), we include individual characteristics as control variables in addition to the intelligence-by-endowment fixed effects. In column (2), we exclude those additional control variables. The main coefficient of interest, on *Public*Meritocratic*, increases slightly from 0.459 (column (1)) to 0.518 (column (2)), and still remains statistically significant at the 5% level.

Columns (3) and (4) present alternative definitions of intelligence-

Table 3
Main experimental results.

	Amount donated						Share donated					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Public	−0.197	−0.224	−0.194	−0.205	−2.913	−2.821	−0.179	−0.192	−0.196	−0.175	−0.133	−0.111
	(0.213)	(0.167)	(0.202)	(0.205)	(1.791)	(2.736)	(0.177)	(0.192)	(0.209)	(0.216)	(0.0896)	(0.120)
Meritocratic	−0.384***	−0.349**	−0.323**	−0.219*	−3.652*	−4.693**	−0.356***	−0.318*	−0.268*	−0.139	−0.160**	−0.203**
	(0.144)	(0.165)	(0.139)	(0.118)	(1.877)	(2.126)	(0.136)	(0.167)	(0.145)	(0.112)	(0.0740)	(0.0904)
Public*Meritocratic	0.459**	0.518**	0.425*	0.381*	5.376**	5.702**	0.448**	0.488**	0.407*	0.316	0.230*	0.253*
	(0.213)	(0.233)	(0.256)	(0.219)	(2.647)	(2.597)	(0.200)	(0.242)	(0.246)	(0.242)	(0.120)	(0.143)
Mean of Dep. Variable	$12.05	$12.05	$12.05	$12.05	$12.05	$12.05	55%	55%	55%	55%	55%	55%
Additional Controls	Yes	No	Yes	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Intelligence-by-Endowment FE	I	I	II	III	I	I	I	I	II	III	I	I
Model	Poisson	Poisson	Poisson	Poisson	Interval Regression	OLS	Poisson	Poisson	Poisson	Poisson	Interval Regression	OLS

Notes: $N = 112$. Heteroscedasticity-robust standard errors in parenthesis. * significant at the 10% level, ** at the 5% level, *** at the 1% level. *Amount Donated* refers to the exact amount donated by individuals. *Share Donated*refers to the proportion of endowment donated (donation/endowment). *Public* is a dummy variable that takes the value 1 when the donation of the subject is observable to others, and 0 if is not observable. *Meritocratic* is a dummy variable that takes the value 1 if the endowments were assigned based on intelligence test scores and 0 if they were randomly assigned. *Additional Controls* denote a set of additional controls: one variable for income, one variable for preferences for redistribution and three dummies for College major. *Intelligence-by-Endowment FEs* are dummies for the following groups: I. All combinations between the intelligence test score (in 3-point increments) and four endowment amounts; II. All combinations between the five quintiles of the intelligence test score and four endowment amounts; III. All combinations between the ten deciles of the intelligence test score and four endowment amounts. For more data definitions, see notes to Table 1.

by-endowment fixed effects. In column (3), we use the quintiles of the intelligence test score, and in column (4) we use deciles of the test scores. The coefficient on *Public*Meritocratic* decreases slightly from 0.459 in (column (1)) to 0.425 in (column (3)) and to 0.381 in (column (4)) and it still remains statistically significant, although now at the 10% level.

Columns (5) and (6) use the amount donated as the dependent variable but employ different regression models from the Poisson model in column (1). Column (5) presents results using an Interval Regression model, which accounts for individuals who want to choose different amounts but are forced to choose one of four possible amounts. The coefficient on *Public*Meritocratic* remains statistically significant at the 5% level in column (5). The magnitudes of the coefficients are not directly comparable between columns (1) and (5) because of the different regression models. Thus, the coefficient on *Public*Meritocratic* from column (5) corresponds to roughly 44.6% (=(5.376)/(12.05)) of the mean donation amount. This effect is in the same order of magnitude as the corresponding 58.2% ($e^{0.459} - 1$) effect implied by column (1).

In column (6), we employ an ordinary least squares model with the amount donated as the dependent variable. Unlike the Interval Regression model, this regression does not account for the censored nature of the dependent variable. In practice, this makes little difference: the coefficients are similar between columns (5) and (6), both in terms of magnitude and statistical significance.

The last six columns replicate the first six columns, but using share donated instead of amount donated as dependent variable. The results are broadly robust across these two alternative dependent variables. For instance, compare the baseline regressions from column (1) and column (7). The key coefficients, on *Public*Meritocratic*, are similar in magnitude and statistical significance across the two regressions: 0.459 (p-value = 0.032) using amount donated as dependent variable, and 0.448 (p-value = 0.025) using the share donated as dependent variable. In the rest of pairwise comparisons between columns (1)–(6) and columns (7)–(12), the key coefficients are similar in magnitude and statistical significance. The only exception is given by the pair of columns (4) and (10), which uses the third (most demanding) version of the intelligence-by-endowment fixed effects. The coefficients on *Public*Meritocratic* are similar to each other (0.381 in column (4) versus 0.316 in column (10)), but while it is statistically significant when using amount donated as dependent variable (p-value = 0.082), from column (4), it is not statistically significant when using share donated as dependent variable (p-value = 0.191), from column (10).

Finally, we discuss the net effects from the *meritocratic* treatments in the presence of sorting effects. Consider donations in the *public* condition, between *meritocratic* and *random* conditions. On the one hand, we expect a positive effect on donations through the signaling-smarts channel. On the other hand, there can be negative channels, such as the attribution channel, wherein individuals feel it is fair to keep their endowments and thus donate less, and the sorting effect, wherein we allocate more money to smarter people who, on average, may be more selfish and/or less interested in their social image.

Table 4
Average donation, by treatment groups.

	Random-public	Meritocratic-public	Random-private	Meritocratic-private
Average Amount Donated ($)	13.21	11.43	13.57	10.00
	(1.93)	(1.83)	(1.87)	(1.54)
Average Share Donated (%)	56.85	57.14	60.12	47.62
	(6.85)	(7.88)	(7.25)	(7.25)
Observations	28	28	28	28

Notes: Average Amount (Share) Donated is the mean amount (proportion of endowment) donated within each treatment group (standard error in parenthesis). Observations denote the number of subjects in each treatment group. *Random*refers that the endowments were assigned randomly. *Meritocratic*refers that the endowments were assigned according to individuals' performance in the analytical test. In the *Private*condition individuals received a list of anonymized donations made by all participants in their same group. In the *Public*condition individuals received a list of not anonymized donations made by all participants in their same group (i.e., full names of donors was shown).

Table 4 presents the average amount donated in each treatment group. In the *public* condition, the net effect of moving from *random* to *meritocratic* is a decrease in the average contribution from $13.21 to $11.43.[5] That is, the negative channels dominate. This finding suggests

[5] The net effect is also negative in the *private* condition – however, that is not surprising given that the positive channel (signaling smarts) is absent.

that, despite the added room for signaling smarts, high meritocracy can hinder generosity.

4. Conclusions

We propose that individuals may engage in public generosity because they want to signal other unobservable characteristics that correlate with income, such as their intelligence. We conducted a laboratory experiment designed to test this hypothesis. In this experiment, we manipulated the beliefs about the publicity of consumption and the correlation between endowments and intelligence. We provide evidence that, consistent with our hypothesis, individuals use visible consumption to signal their intelligence.

Our findings have some implications for the understanding of social signaling. The current theory and evidence takes a partial equilibrium approach in which types are fixed (e.g., some individuals are richer, or more altruistic) and then individuals try to publicize their types through some form of costly signaling. Even though attributes such as intelligence can be treated as exogenous, individuals may not be able to signal those attributes directly. Instead, they may only be able to signal by products of those attributes that are determined endogenously, such as their income or education. This can have significant implications for equilibrium outcomes. Intuitively, whether individuals want to work hard or not depends on whether they expect that the fruit of their effort will be a strong or a weak signal of their intelligence. Investigating these theoretical implications is an avenue for future research.

Our findings also have some implications for fundraising practices. Nonprofit organizations, such as charities and universities, seem to be well aware that they can use donation visibility to boost contributions (Glazer and Konrad, 1996). Our evidence suggests that these publicity incentives may be even more effective if they can signal other related attributes such as intelligence. For example, rather than just publicizing the amounts given by individual donors, charities may also want to publicize the intellectual or business achievements of its contributors. Universities usually post plaques outside of classrooms listing the names of the biggest donors and the amounts donated by them. Right next to each name and amount, these plaques could list the main achievement of each contributor, such as whether they came up with an invention, held an important position at a corporation or the government. The evaluation of the effectiveness of this type of fundraising strategies is an avenue for future research.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.socec.2018.08.004.

References

Andreoni, J., Bernheim, B.D., 2009. Social image and the 50–50 norm: a theoretical and experimental analysis of audience effects. Econometrica 77 (5), 1607–1636.

Andreoni, J., Petrie, R., 2004. Public goods experiments without confidentiality: a glimpse into fund-raising. J. Public Econ. 88 (7), 1605–1623.

Ariely, D., Bracha, A., Meier, S., 2009. Doing good or doing well? image motivation and monetary incentives in behaving prosocially. Am. Econ. Rev. 99 (1), 544–555.

Bagwell, L.S., Bernheim, B.D., 1996. Veblen effects in a theory of conspicuous consumption. Am. Econ. Rev. 86 (3), 349–373.

Bénabou, R., Tirole, J., 2006. Incentives and prosocial behavior. Am. Econ. Rev. 96 (5), 1652–1678.

Bracha, A., Vesterlund, L., 2017. Mixed signals: charity reporting when donations signal generosity and income. Games Econ. Behav. 104, 24–42.

Bursztyn, L., Ferman, B., Fiorin, S., Kanz, M., Rao, G., 2017. Status Goods: Experimental Evidence from Platinum Credit Cards. NBER. Working Paper No. 23414

Carlsson, F., He, H., Martinsson, P., 2013. Easy come, easy go. Exp. Econ. 16 (2), 190–207.

Charles, K.K., Hurst, E., Roussanov, N., 2009. Conspicuous consumption and race. Q. J. Econ. 124 (2), 425–467.

Cherry, T.L., Frykblom, P., Shogren, J.F., 2002. Hardnose the dictator. Am. Econ. Rev. 92 (4), 1218–1221.

Clingingsmith, D., Sheremeta, R.M., 2015. Status and the demand for visible goods: experimental evidence on conspicuous consumption. Working Paper.

Cole, H.L., Mailath, G.J., Postlewaite, A., 1995. Incorporating concern for relative wealth into economic models. Fed. Reserve Bank Minneapolis. Q.Rev. 19 (3), 12. Federal Reserve Bank of Minneapolis

Dana, J., Cain, D.M., Dawes, R.M., 2006. What you don't know won't hurt me: costly (but quiet) exit in dictator games. Organizational Behav. Hum. Decis.Processes 100 (2), 193–201.

Glazer, A., Konrad, K.A., 1996. A signaling explanation for charity. Am. Econ. Rev. 86 (4), 1019–1028.

Heffetz, O., 2011. A test of conspicuous consumption: visibility and income elasticities. Rev. Econ. Stat. 93 (4), 1101–1117.

Hoffman, E., McCabe, K., Shachat, K., Smith, V., 1994. Preferences, property rights, and anonymity in bargaining games. Games Econ. Behav. 7 (3), 346–380.

Karlan, D., McConnell, M., 2014. Hey look at me: the effect of giving circles on giving. Journal of Economic Behavior and Organization, Vol. 106, pp. 402–412.

Karlan, D., McConnell, M.A., 2014. Hey look at me: the effect of giving circles on giving. J. Econ. Behav. Organiz. 106, 402–412.

McManus, T., Rao, J., 2015. Signaling smarts? Revealed preferences for self and social perceptions of intelligence. J. Econ. Behav. Organiz. 110, 106–118..

Millet, K., Dewitte, S., 2007. Altruistic behavior as a costly signal of general intelligence. J. Res. Personality 41 (2), 316–326.

Oxoby, R.J., Spraggon, J., 2008. Mine and yours: property rights in dictator games. J. Econ. Behav. Organiz. 65 (3), 703–713.

Rege, M., Telle, K., 2004. The impact of social approval and framing on cooperation in public good situations. J. Public Econ. 88 (7), 1625–1644.

Roth, C., 2014. Conspicuous Consumption and Peer Effects Among the Poor: Evidence from a Field Experiment. CSAE. Working Paper WPS/2014-29.

Ruffle, B.J., 1998. More is better, but fair is fair: tipping in dictator and ultimatum games. Games Econ. Behav. 23 (2), 247–265.

Samek, A., Sheremeta, R.M., 2014. Recognizing contributors: an experiment on public goods. Exp. Econ. 17 (4), 673–690.

Samek, A., Sheremeta, R.M., 2017. Selective recognition: how to recognize donors to increase charitable giving. Econ. Inq. 55 (3), 1489–1496.

Tonin, M., Vlassopoulos, M., 2017. Sharing one's fortune? An experimental study on earned income and giving. J. Behav. Exp. Econ. 66, 112–118.

Winniford, J.C., Carpenter, D.S., Grider, C., 1997. Motivations of college student volunteers: a review. Naspa J. 34 (2), 134–146.