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Trust in Government and Support for Redistribution

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Abstract

In many countries safety nets consist predominantly of universal subsidies on food and fuel. A key question for policy makers willing to shift to targeted safety nets is under what conditions middle-class citizens would be supportive of redistributive programs. Results from a behavioral experiment based on a nationally representative sample in Jordan reveal that increasing transparency in benefit delivery makes middle-class citizens (particularly among the youth and low-trust individuals) more willing to forgo their own welfare to benefit the poor. Moreover, increasing transparency enhances the relative support for cash-based safety nets, which have greater impact on poverty compared with in-kind transfers, but may be perceived as more prone to elite capture.

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1 Introduction

Trust is an essential ingredient of altruistic behavior. A growing body of literature emphasizes its importance in shaping individual decisions to support the provision of public goods. In a prominent example, a social experiment found that charitable provision was significantly lower if the solicitors of contributions belonged to a minority group that inspired a lower level of trust among potential donors (List and Price 2009). This behavior can be explained by a principal-agent problem: the principal (i.e. potential donor) can choose, under no obligation, whether to give money to benefit a group on whose behalf the agent (i.e. donation solicitor) is purportedly acting. The agent's optimal strategy is to provide the principal with credible signals that the transferred resources will be used as the principal intends. Mechanisms that enhance the credibility of principal-agent transactions can be particularly important in securing support for public goods, including those that benefit only a subset of the population—such as redistributive policies.[1]

Obtaining citizens' support for redistributive policies may be especially important in countries where resources are scarce and governments historically enjoy little public trust in their capacity to deliver goods and services fairly and efficiently.[2] In fact, even in nondemocratic regimes, citizens have ways to retaliate against unpopular use of public funds or to try to control rent-seeking (Acemoglu, Hassan, and Tahoun 2014). But while there are high expectations about the positive effects of increased transparency on citizens' trust in — and therefore their support for — redistributive policies, there is a dearth of rigorous evidence on this topic.

In this paper, we offer experimental evidence on the effect of trust-enhancing measures on public support for redistributive policies. We conducted a behavioral experiment on a nationally representative sample of the middle class in Jordan—the Jordan Gives experiment—to identify the effect of enhanced transparency on the support for, and the preferred design of, social safety nets.[3] The experiment involved a nationally-representative sample of 420 participants recruited from 21 middle-class primary sampling units (PSUs) (ie localities) across Jordan; within each PSU, participants were randomly assigned to treatment and control groups of 10 subjects. Each participant received a fuel voucher roughly comparable to the daily minimum wage.[4] Participants in the control

[1]For example, the literature has established that trust in the goverment is an important determinant of compliance with taxation obligations of citizens and firms (Barone and Mocetti 2011; Friedman, Johnson, and Kaufmann 2000; Johnson et al. 2000; Silverman, Slemrod, and Uler 2014).

[2]A government's failure to provide such credibility could affect the provision of public goods in different forms: in more mature democracies through voting outcomes, and in other cases, through tax evasion, exercise of corruption, or public protest.

[3]Social safety nets (SSNs), also known as social assistance or welfare schemes, are defined as noncontributory transfers targeted to the poor or vulnerable. They include income or in-kind support and can be made conditional on certain behaviors of recipients' households (e.g. conditional cash transfers, CCTs) or provided without any conditions (e.g. unconditional cash transfers, UCTs) (Grosh et al. 2008, World Bank 2009).

[4]The experiment was followed by a Becker, DeGroot, and Marschak (1964) auction to establish valuation

group had to decide whether to give to people in need, without expecting anything in return. Participants in the treatment group were given the opportunity to verify whether their transfers actually reached poor individuals.

Our experimental design simulates at the micro level the choice faced by middle-class citizens on whether to support a shift of public resources toward targeted interventions that benefit the poor, under different designs of those interventions and different degrees of certainty about the delivery of the benefits to the intended recipients. The experiment thus allows us to provide rigorous evidence on: (1) the propensity for redistribution and relative support for different redistributive methods; and (2) the underlying impact of transparency and trust on redistributive preferences. In doing so, this paper bridges the gap between evidence obtained from traditional opinion surveys and the behavioral literature.

We have two main sets of results. First, although the effect of transparency on altruism is not statistically significant for the whole sample, the transparency-enhancing treatment caused significant increases in the support for redistribution among two groups of participants: "low-trust" individuals and "youth." The first group consists of individuals who needed a credible signal of trustworthiness in order to exhibit altruism—people who were *ex ante* suspicious about the implementation of social safety nets. In the control group, such low-trust individuals were significantly less likely to give to the poor than individuals who trusted that social safety nets reach the intended beneficiaries. In the treatment group, on the other hand, the giving rate of low-trust individuals matched that of high-trust participants, suggesting that the transparency-enhancing treatment mitigated the effect of their initial mistrust on altruistic behavior. The second group—youth, defined with different age thresholds—represents those least likely to give to the poor in the absence of the material guarantees of delivery. With increased transparency, this group experienced the highest increase in the rate of giving.

Second, we provide evidence that the transparency-enhancing treatment particularly increased redistribution to the poor through unconditional cash transfers as opposed to in-kind or conditional cash transfers. In the control group, unconditional in-kind transfers were equally preferred to unconditional cash transfers. The treatment group, however, had higher rates of giving through unconditional cash transfers, which became the most popular benefit delivery option. In sum, enhancing transparency of delivery increased the support for the delivery option—cash—that is generally considered most efficient in reducing poverty relative to in-kind benefits. In fact, recent opinion surveys in four Arab countries, including Jordan, showed that the majority of the poor prefer cash-based transfers to in-kind benefits (Silva, Levin, and Morgandi 2013).[5] However, in the absence of

of the fuel voucher. This type of auction is a mechanisms commonly used in the literature to induce individuals to reveal their willingness to pay for a given good (Noussair, Robin , and Ruffieux 2004). It showed that 95 percent of the participants considered the fuel vouchers to be equivalent to cash.

[5]The other countries surveyed were the Arab Republic of Egypt, Lebanon, and Tunisia.

a credible signal of trustworthiness from the state, cash transfers are also likely to be perceived as carrying the highest risk of capture.

Our paper contributes to several strands of research. Much of the existing literature on preferences for redistribution is based on opinion survey data (for example, Alesina and Angeletos 2005; Alesina, Di Tella, and MacCulloch 2004; Alesina and La Ferrara 2002). We add to this literature by providing rigorous evidence from a field experiment that elicits preferences on redistribution in a setting where participants face real trade-offs, while maintaining the national representativeness of the results. In doing so, we also complement and extend the existing behavioral economics literature that investigates altruistic behavior using samples of higher-education students in laboratory settings (e.g., Charness and Rabin 2002; Fehr and Schmidt 2003). While also using real trade-offs to evaluate subjects' preferences, inference from laboratory studies explores human behavior through selected samples that may not be representative of the population of interest. Field behavioral experiments testing altruistic behavior have been less common, have never been based on nationally representative samples, and have not tested the effects of enhanced transparency (Parra 2011; Johansson-Stenman, Mahmud, and Martinsson 2009).

Finally, the causal estimates we offer based on a nationally representative sample contribute to inform pressing policy issues in developing countries, where many governments are deliberating whether and how to shift resources away from costly universal subsidies, which benefit the middle and upper classes the most, and toward more efficient forms of social safety nets (Silva, Levin, and Morgandi 2013, World Bank 2015).[6] Besides the redistributive implications, one of the main challenges of a policy shift away from subsidies is the high deficit of trust that governments face as they attempt to replace an easy-to-monitor price subsidy for everyone with a targeted social safety net that can deliver better impacts for the poor and vulnerable at a lower cost. Indeed, important episodes of civil unrest in Jordan in the past decade were linked to attempted reforms of utility or consumption subsidies (Atamanov, Jellema, and Serajuddin 2015).[7]

[6]The Jordan Gives experiment was accompanied by a survey module added to the the Gallup World Poll on citizens' willingness to reform fuel subsidies, preferred design of reformed safety nets, and willingness to support the reform if preferred safety nets were implemented. The survey was conducted on nationally representative samples in Egypt, Jordan, Lebanon and Tunisia. Most respondents in all four countries preferred savings from fuel or diesel subsidy reform to be distributed to the poor and spent on improving social services. Half of the respondents in Jordan preferred the savings to be distributed only to poor families. More than 20 percent of Jordanians who initially opposed the idea of any subsidy reform would support fuel subsidy reform if the savings were to be distributed to the poor, alone or combined with education and healthcare spending.

[7]A similar situation occured during energy subsidy reform episodes in many other countries (see IMF 2013). As in those cases, mitigating measures were considered as part of the reform in an attempt to generate public support for the reform and offset adverse effects on the poor. In November 2012, a few months after the Jordan Gives experiment, the petroleum subsidy was removed and an unconditional cash transfer was created to compensate the poor and vulnerable. To ensure transparent administration

The remainder of the paper proceeds as follows. Section 2 describes the experiment. Section 3 presents the empirical model. Section 4 reports the main results, highlighting how much participants decided to give, which delivery option encouraged more giving, how the transparency-enhancing treatment affected preferences for giving, and how trust and age affected the impact of the treatment and the relative preferences across the different program designs. Section 5 discusses alternative explanations for the increase in giving because of a transparency shock and checks the robustness of the estimates of the interaction between treatment and trust. Section 6 concludes.

2 Research design and data

2.1 Sample design and selection

The Jordan Gives experiment was carried out with 420 participants in 21 PSUs in Jordan, on a nationally representative sample of the Jordanian middle class. Participants were identified through a three-stage process: (1) 21 PSUs were drawn from a sampling frame of middle-class enumeration areas in Jordan based on the 2004 census; (2) within each PSU, households were selected using a random walk method; and (3) adults were recruited (one per household) to participate in the experiment using a Kish (1949) table. Based on extensive piloting, a protocol was devised to ensure that two groups of 10 randomly assigned individuals each could be constituted in each PSU (10 for treatment, 10 for control) at the same time and place (see Annex 1 for more details).

At the recruitment stage, the invitation letter explained that all participants who appear at the specified place and time (usually a local public school the day after recruitment) would receive a fuel voucher of JD 5 (about US$7.50) as a show-up fee and that there would be a chance to keep JD 10 more in such vouchers, depending on the outcome of the meeting. These vouchers were issued by Jordan Petroleum Refinery Company. They were widely known in Jordan and could be exchanged for gasoline in petrol stations throughout the country. The value of the JD 10 voucher was equivalent to slightly more than the daily minimum wage, or about five days of participants' self-reported mean per capita household expenditure. Each invitee who agreed to participate was left with two receipts (one for the JD 5 voucher and the other for the JD 10 vouchers), which they were

of the transfer, the government also decided to set up a National Unified Registry (NUR) of the poor and vulnerable as a common platform for eligibility for social assistance programs, with several checks of living standards. Similar instruments (e.g. Cadastro Unico in Brasil, Ficha de proteccion social/Registro social de hogares in Chile) have been the backbone of transparent social assistance programs around the world (see Silva, Levin, and Morgandi 2013; Lindert et al. 2007; and Ministry of Social Development of Chile 2015). Many other developing countries have, in recent years, accompanied subsidy reforms by measures to enhance transparency. This includes, for example, earmarking increased funding for education and infrastructure linked to fiscal savings from subsidy reform, creating a website where each person could compute his/her score or enter their national ID and verify his/her (in)eligibility/information, or displaying the list of social assitance beneficiaries in a public place (IMF 2013).

encouraged to bring to the meeting to exchange for real vouchers.[8]

The sampling frame was based on the Government of Jordan's definition of "middle class": middle-class PSUs were identified as those whose households' average annual per capita expenditure was between twice and four times the poverty line (ESC 2008). Annex 1 provides a detailed explanation of the sample design and the selection protocols.

The decision to focus the experiment on middle-class behavior was driven by the need to study a population of highest relevance to the policy makers considering safety net reform. Such a reform would imply a redistribution of public funds away from the wealthy and the middle class, as both groups were capturing most benefits from universal food and fuel subsidies (Silva, Levin, and Morgandi 2013). The reform under consideration would benefit the poor, who would gain from the increased magnitude of transfers due to targeting and potentially from attaining a more optimal consumption basket, depending on the design of the new safety net.[9] The Jordanian middle class was the group that was likely to loose the most in relative terms from shifts of resources away from universal subsidies towards targeted social safety nets, and that could assemble a sufficiently large interest group to thwart the reform.

2.2 The redistributive proposals

The experiment was conducted on 20 participants in each randomly selected PSU. Upon arrival to the location of the experiment, participants were randomly allocated to either the control or treatment group and invited to enter a corresponding room. At the start of the experiment, each participant received the two vouchers that had been promised at recruitment stage: a JD 5 voucher as a show-up fee and a JD 10 voucher to use in the experiment.[10]

The experiment asked participants to make a series of decisions concerning whether to keep their JD 10 fuel voucher or to give it up in exchange for different scenarios ("proposals") of assistance to the poor. The exact wording of the proposals, intended to mimic the design of social safety net programs, was as follows:

P1 (Unconditional cash transfer): "You give up your JD 10 voucher. Our team gives JD 20 cash per family to 5 poor families in this community."

P2 (Unconditional food transfer): "You give up your JD 10 voucher. Our team gives a food basket worth JD 20 per family to 5 poor families in this community."

[8] To approximate the experience of subsidy reform, which entails the removal of what is often perceived as a citizen's right, the experiment activated an endowment effect (Kahneman, Knetch, and Thaler 1991) for the fuel vouchers by creating a sense of ownership using receipts with specified voucher values at the time of recruitment.

[9] For example, cash transfers would allow poor households to buy goods and services in the amounts providing the highest utility, whereas in-kind transfers (and price subsidies) distort such consumption patterns toward the provided or subsidized goods.

[10] To strengthen the endowment effect, initiated at recruitment via receipts, the vouchers were handed out at the very beginning of the experiment.

P3 (Unconditional cash transfer and school): "You give up your JD 10 voucher. Our team gives JD 20 cash per family to 2 poor families in this community and JD 60 cash goes to the local public school."

P4 (Cash transfer conditional on training): "You give up your JD 10 voucher. Our team gives JD 20 cash per family to 5 poor families in this community conditional on one family member completing a free training program on work-related skills."[11]

Proposals were revenue-neutral, since the amount to be disbursed in the proposals was equivalent to the total value of fuel vouchers in the room (i.e. 10 participants' JD 10 vouchers, a total of JD 100).

After each proposal was presented, participants were asked to fill their individual decision cards in silence and confidentially, marking whether they "accept" the proposal (indicating a preference to see the proposal implemented) or "reject" it (preference to keep the JD 10 voucher).[12] Participants were asked to write down their decision on each proposal before being presented with the next proposal. After all four proposals were presented and all four decisions were marked on decision cards, participants were asked to rank the four proposals in the order of preference. At the end, all decision cards were collected by the facilitator, who placed them in a glass bowl. This decision selection process was chosen to ensure that participants had a clear incentive to consider each proposal independently of what they had decided in preceding proposals. A second glass bowl contained numbers 1 through 4, corresponding to the proposal numbers. After all the cards were submitted, the facilitators drew one decision card from the first bowl and one number from the second bowl. The decision made regarding the selected proposal number on the selected decision card was implemented on the whole group. If the selected decision

[11] The different proposals correspond to the most common types of safety net schemes. There is an intense debate in the literature over the relative merits of each of these designs. Recent empirical evidence finds that cash transfers are generally as effective as food transfers in improving nutritional outcomes (Cunha 2012; Attanasio, Battistin, and Mesnard 2012; Hoddinott, Sandström, and Upton 2014), but they are more efficient when markets function well (that is, are not plagued by hyperinflation, conflicts, or supply constraints) (Busso and Galiani 2014). Several recent papers have discussed the marginal impact of attaching conditions to cash transfer programs. Although their administration is costlier relative to uncoditional transfers, they intend to address market failures that lead to underinvestment in education or health by imposing certain behaviors on recipient households (Hanlon, Barrientos, and Hulme 2010). Recent studies found that such schemes generally improve the conditioned-on outcome, but pose trade-offs with respect to gains in overall welfare, which can be particularly large in the presence of low quality (or accessibility) of conditioned services (Baird, McIntosh, and Ozler 2011; Attanasio, Veruska, and Marcos 2015; Blattman, Fiala, and Martinez 2015; Benhassine et al. 2013). On the other hand, transfers conditional on educational outcomes usually also provide a valuable mechanism to improve parents' monitoring over children's school attendance (Bursztyn and Coffman 2012). Finally, the literature discusses that accompanying cash transfers to the poor with financing of public goods with a broader user base (such as schools) promotes acceptance of public social assistance, and thus makes first-best redistribution (targeted safety nets) possible (Gahvari and Mattos 2007).

[12] To prevent peer pressure from biasing their decisions, the participants were not allowed to discuss their decisions with each other.

was “accept,” then the JD 10 voucher was collected from each participant and the selected proposal would be later implemented in the local community.[13] If the selected decision was "reject," all participants would keep their vouchers. The experiment was followed by a Becker, DeGroot, and Marschak (BDM) (1964) auction and the collection of basic demographic, socio-economic, and attitudinal characteristics of participants (via a written questionnaire).[14] To better understand the reasoning of study participants, debriefing focus groups discussions were also conducted. They showed that the proposals, decision mechanisms and consequences of the decisions in the context of the experiment were well understood by participants.

2.3 Audiovisual implementation of the experiment

Results of behavioral experiments can be biased by the heterogeneity of implementation. Biases can include accidental priming to values or anchoring to certain numbers, and they can also originate from the identity of the facilitator, see Brewer and Chapman (2002) and Furnham and Boo (2011) for a survey.

To ensure that the messages conveyed to participants were homogeneous, the experiment was implemented through an 18-minute video that featured a Jordanian woman with a neutral background explaining the purpose of the experiment and giving directions to participants at each stage. The video presented the decision cards and the proposals and illustrated graphically the proposal selection mechanics.[15] Participants were presented with

[13] In each selected PSU, on the day of the experiment, the facilitators arrived equipped to implement any of the potential outcomes of the experiment. Facilitators' cars contained the food baskets, training vouchers, and cash. Contact information of poor families that could be the recipients of these benefits was provided by the local community leader.

[14] In the BDM auction participants were told that they had the possibility of exchanging for cash the JD 5 fuel vouchers that they had received as a show-up fee. The video explained and illustrated the auction mechanism to ensure that all participants understood that their dominant strategy was to reveal their true preferences. They were then asked to write down the minimum cash amount, in denominations of JD 1, that they would need to receive in order to “sell back”their vouchers. Cards displaying different cash amounts (1 through 5) were then placed in a bowl, and one of them was randomly drawn. If the drawn value was above the value written by the participant, the participant would retain his or her voucher. If it was equal or lower, he or she would exchange the voucher for the cash amount drawn. The auction revealed that more than 95 percent of the participants considered the voucher to be equivalent to cash: that is, they wrote “5”, as they were not ready to exchange their JD 5 voucher for a lower monetary value than its nominal value. This is understandable given that 57 percent of respondents had a car in their households, and those who did not could also have had motorcycles or readily exchanged the voucher.

[15] During the pilot, the use of pictures emerged as important to improve participants' understanding of the experiment. To further ensure that participants had a good understanding of the experiment's mechanics, particularly of the fact that their decision, if selected, would affect everyone's payoffs, the experiment was preceeded by a mock trial (first part of the video). Participants were given a chocolate as an endowment, and they wrote down their preference between keeping their chocolate or getting a postcard (proposal 1), and between keeping their chocolate and having one of the facilitators recite a poem about Jordan (proposal 2). As with the actual experiment, one decision was randomly drawn and implemented on the whole group.

a sequence of four proposals. The order in which the the four proposals were presented was randomized at the PSU level by producing multiple versions of the same video. This procedure aimed to avoid any systematic anchoring effect due to a particular proposal order. The facilitator's role was to distribute and collect decision cards and questionnaires, answer questions according to a pre-developed answer script, and lead the focus group discussion that followed the experiment.

2.4 The transparency-enhancing treatment

Treatment status was assigned at the PSU level and the sample of 20 individuals in each PSU was randomly divided into two groups of equal size: treatment and control. After the random assignment, the experiment was started simultaneously in two separate rooms, one room with individuals in the control group and another room with individuals in the treatment group. In each PSU we implemented the experiment only once. Hence, the total sample contained 420 individuals in 21 PSUs: 210 individuals in the control group and 210 individuals in the treatment group.

The video for the treatment groups contained all the features of the video for the control groups, but it included additional information that would make the transfer delivery to the poor and content more transparent for participants. In particular, individuals in the treatment group were offered the option to accompany the facilitator after the experiment to witness the actual implementation of the proposal among poor families, if the randomly-selected decision was an acceptance of the proposal. To reinforce this message, right before participants were asked to make their decisions on each proposal, they were told that the facilitator would wait after the conclusion of the experiment for anyone who wanted to follow and witness the implementation of the proposal.[16]

In addition, participants in the treatment groups were shown in the video a basket of essential supplies worth JD 20, as in proposal 2 (unconditional in-kind transfer). Thus, the treatment increased transparency of the redistributive proposals by alleviating participants' uncertainty about the delivery of the transfer to the intended beneficiaries and about the value of the JD 20 in the case of an unconditional food transfer. The treatment was chosen as a result of a focus group on the perceived barriers to redistribution as well as consultations with Jordanian experts to offer concrete recommendations on measures to implement as part of a fuel subsidy reform.

2.5 Data

The data used in this paper were collected between late May and June 2012. The quantitative data from Jordan Gives includes decisions made by each individual participant

[16] Indeed, in some cases, participants in the treatment group did decide to follow the facilitator and, as highlighted by participants in the focus group discussion, the mere availability of this option sent a credible commitment signal of trustworthiness.

during the experiment, their valuation of fuel vouchers obtained via a post-experiment BDM auction, as well as basic demographic, socioeconomic, and attitudinal information on each participant collected via a short written survey administered after the experiment. Finally, we collected a rich qualitative dataset from structured in-depth focus groups that were conducted by facilitators after all quantitative data was collected.

3 Empirical model

We estimate by ordinary least squares (OLS) a set of treatment-effects models of the following form:

$$Y_i = \alpha + \delta T_i + \beta X_i + e_i \tag{1}$$

where: Y_i is the an outcome variable (mean giving rate in models using information from all the proposals, or the binary decision to accept or reject a specific proposal in models using information from a specific proposal) for individual i; T_i is an indicator variable equal to one if the individual was assigned to the treatment group and 0 otherwise; X_i is the vector of baseline characteristics; and e_i is the error term. The parameter of interest δ is the average treatment effect. Estimates are computed with a linear regression model even for binary dependent variables, such as decisions on specific proposals, as the coefficients are nearly identical to the marginal effects of a probit model (as discussed in Miguel, Satyanath, and Sergenti 2004). The advantage of using OLS is the availability of an established procedure to compute clustered wild bootstrap-t standard errors, which are more suitable for estimations with a small number of clusters. The standard errors are clustered at the PSU level, which accounts for the design effect of our PSU level treatment and for heteroscedasticity inherent in the linear probability model.[17]

We estimate results of equation (1) for six outcomes, two aggregating individual i's decisions across the four proposals (as described below) and four using decisions on each proposal at a time. "Mean giving rate" is the share of accepted proposals to give up the fuel voucher (out of possible four); "Frequent giver" is an indicator variable equal to one if individual i's mean giving rate is greater than 0.5, i.e. if the individual gave up the voucher in the majority of the proposals. The other four outcomes are binary indicator variables equal to one if individual i indicated that he or she would give up his/her voucher for that specific proposal (unconditional cash transfer, unconditional food transfer, unconditional cash transfer and school, or cash transfer conditional on training).

[17] Clustering at the PSU level was used to account for the first stage of the sampling strategy, which picked PSUs from the census sampling frame. Clustering thus adjusts the standard errors to take into account intra-cluster correlation, which could be relatively high for outcomes related to redistribution preferences. Given that Huber-White heteroskedastic-standard errors (commonly known as "cluster-robust") are potentially underestimated when the number of clusters is small (as discussed in Bertrand, Duflo, and Mullainathan 2004; Cameron, Gelbach, and Miller 2008; Cameron and Miller 2015), in this paper we recomputed all standard errors with wild cluster bootstrap-t statistics following the procedure by Cameron, Gelbach, and Miller (2008), which avoids standard error underestimation in the presence of few clusters.

In all regressions, we include the following variables as controls: gender, education level, residence in the capital city, and the number of cars in the household (the latter to proxy for relative wealth).[18] These variables were chosen because they are strongly predictive of outcomes and, as a result, they improve the precision of the impact estimates.[19]

4 Main Results

4.1 Sample balance

Sample balance statistics are presented in Table 1, testing the outcome of the randomization process at the PSU level, and thus ensuring that observable characteristics of participants in the treatment groups were similar to those in the control groups. The standard errors of the mean difference between treatment and control groups are corrected for intra-cluster correlation at the level of the 21 PSUs. Panel A shows balance on individual characteristics, Panel B shows balance on household attributes and Panel C shows balance on baseline giving behavior. Overall, the experiment appears well balanced between the treatment and control groups over a broad range of outcomes (see column 4).

4.2 Effect of the treatment

Table 2 describes the effect of the treatment on the probability of giving (i.e. choosing to accept a proposal and thus give up the fuel voucher), obtained via a treatment-effect regression controlling for participants' basic demographic and socioeconomic characteristics. The constant term in these regressions represents the mean in control groups, while the coefficient on the indicator variable for individuals' assignment to treatment groups represents the impact of the treatment. Table 2 reports the difference between treatment and control means in the giving rates (both aggregate and for each of the four proposals) as well as in the share of participants who gave up the voucher in more than two proposals. Column 5 shows full sample averages. Results indicate that mean giving (at the participant level) was 67 percent and that more than half of all participants in the experiment (61 percent) were frequent givers (i.e. opted to accept more than two proposals).[20] Across proposals, the unconditional food transfer proposal attained the highest acceptance rate (70 percent), closely followed by the unconditional cash transfer proposal (69 percent). Given the monetary value that the voucher represented for the subjects, the average giving rate of 67 percent was remarkable, compared with the giving rates found in other experiments, which ranged between 20 percent and 37 percent (DellaVigna, List, and Malmendier 2012;

[18] Appendix Table A.1 presents the summary statistics.

[19] Appendix Table A.2 presents more parsimonious results without control variables.

[20] About half of all participants in the experiment opted to accept all four proposals (i.e. to give up their vouchers to the poor in each of the presented scenarios), 17 percent decided to reject all the proposals (i.e. never to give up their vouchers), while the remaining one-third decide to give up their vouchers for some but not all the proposals.

List and Price 2009; Parra 2011). However, the design of the present experiment was unique in the literature. In contrast to Jordan Gives, classic dictator games allow the principal to define transfer size.[21] Fundraising experiments, on the other hand, do not provide participants with any endowment, while Jordan Gives took measures to enhance the endowment effect for the voucher.[22] Another potentially important distinctive feature of the present experiment is the identification of direct recipients of the transfer as local poor families rather than more abstract notions of giving to charities.[23] Columns 1 to 4 of Table 2 show the results from equation (1). The point estimates of giving rates suggest that individuals in the treatment groups are slightly more likely to give up their vouchers than individuals in the control groups. Nonetheless, none of the average treatment effects reaches statistical significance at conventional levels (with the lowest p-value of 0.11 for the unconditional cash transfer proposal).

4.3 Heterogeneity of effects

Although the results for the full sample are not significant at the conventional levels, the treatment appeared to have significant impacts on two specific subgroups of participants: individuals with low trust in the delivery of safety nets and young people. In fact, as shown in Table 3, the level of trust in the delivery of social safety nets, as measured with a post-experiment attitudinal question, appears to mediate the effect of treatment, particularly with regard to the two unconditional cash transfer proposals (i.e. unconditional cash transfer and cash transfer with school financing).[24] The results are obtained by estimating equation (1) above separately for individuals that reported being completely or somewhat

[21] In classic "dictator games" participants can determine the share of the received amount to distribute in a single-shot game. Parra (2011) found that Ghanaian participants shared 37 percent of the endowment in the baseline scenario. Forsythe et al. (1994) found a 25 percent giving rate in a dictator experiment where the donor knew the identity of the receiver. However, in our case participants had a discrete choice between giving up or retaining their vouchers, for a repeated number of proposals that were heterogeneous by design. We also had a full loss or full retention of the endowment in each proposal, approximating the experience of a subsidy reform which is also one shot. The design of both "whole versus part" and "one shot versus repeated" was adopted to approximate the experience of subsidy reform.

[22] In fundraising experiments List and Price (2009) and DellaVigna, List, and Malmendier (2012) found, respectively, a 20 percent and a 25 percent giving rate in the United States. Our findings would also be consistent with individuals being more generous when their endowmente depends solely on a random shock (Cappelen et al. 2007 and Cherry, Frykblom, and Shogren 2002).

[23] Interestingly, the proposal in which individuals could give up their voucher to both help the poor and to finance a public good (the local school) proved to be the least popular proposal among participants. Although other experiments have suggested that altruism could be enhanced by introducing a chance of personal gain (for instance, lotteries, as in Landry et al. 2006), in this case individuals may have thought that contributing such a limited amount of funding to the school was neither beneficial to themselves nor as impactful as a charitable transfer given directly to the poor.

[24] The question was "How confident are you that the public funds allocated for social assistance reach the poor?" The response scale had four options: "completely confident," "somewhat confident," "not very confident," and "not confident at all." Low-trust individuals are defined as those who responded with the two latter options.

confident that public funds for social assistance reach the poor (defined as “high trust”) and those who were not confident about this (defined as “low trust”). The treatment effect is always higher among low-trust individuals and is statistically significant for three outcomes: the mean giving, the propensity to be a frequent giver and the unconditional cash transfer.

The results point to two other important findings. First, the comparison of columns 1 and 4 reveals that among individuals in the control group, the mean rate of giving was 18 percentage points higher for high-trust individuals than for low-trust individuals, which is a statistically significant difference. It is also striking that this difference was driven essentially by the proposals involving unconditional cash transfers: in the control group, high-trust individuals were 31 and 23 percentage points more likely than low-trust individuals to give up their vouchers for, respectively, the unconditional cash transfers and cash transfer with school financing. Second, in the treatment group, we observe no statistically significant differences in mean giving rate between high-trust and low-trust participants. These relatively smaller differences are due to higher giving rates among low-trust individuals in the treatment groups compared to the control groups. For instance, in the case of unconditional cash transfer, 44 percent of low-trust participants gave up their vouchers in the control group, while 60 percent did so in the treatment group (implying a 16 percentage point treatment effect), whereas the treatment effect for high-trust participants was minimal (less than 2 percentage points). One exception was the proposal of giving cash both to the poor and to the local school; in this case the transparency treatment enhanced the overall giving rate for all participants, but did not reduce the gap in giving between low- and high-trust individuals.

The treatment effect was also heterogeneous based on participants’ age. Figure 1 summarizes the average treatment effect estimates for youth and older adults’ subsamples. Compared to older adults, young individuals (aged 18–29) were far more susceptible to changing their behavior as a result of the transparency-enhancing treatment. The treatment effects are always higher for young individuals and are statistically significant in two outcomes: mean giving and the unconditional cash transfer proposal.

Exploring the data further reveals a certain level of overlap between low trust and age, which explains why treatment impacts are highly heterogeneous on both dimensions. Figure 2 summarizes the local effect of age, as a continuous variable, on average giving behavior in both treatment and control groups according to the participants’ level of trust that public funds for social assistance reach the poor. Panel A presents results for aggregate/all proposals while Panel B presents results for each individual proposal. There is an obvious upward-sloping relationship in the control groups between age and giving rate, implying that youth are less likely to redistribute their endowment. However, for low-trust youth, the transparency-enhancing treatment flattens the age-giving curve, at least until around age 50, and makes these youth about as likely to give up their fuel vouchers as middle-aged individuals who have high trust in the provision of safety nets.

Panel B confirms this pattern at the level of specific proposals.

Taken together, Figure 3 shows that while young individuals are clearly those who are most affected by the treatment in three out of the four proposals, treatment increases the giving rate the most among those who are both low-trust and young. This result is formally confirmed in Table 4. Column 1 shows that trust is an important mediator for the transparency-enhancing treatment, with the interaction term of trust and treatment obtaining a significant negative coefficient in almost all model specifications. In other words, providing a signal that the redistributive transfer would reach the intended beneficiaries is most effective for "low-trust" individuals, i.e. those who ex ante have low confidence in the functioning of redistribution flows.

The heterogeneity of treatment effects is demonstrated further by controlling for participants' age. As mentioned above, young participants were less likely to give up their fuel vouchers; this is confirmed in column 2 in Table 4 by a consistently significant negative coefficient on the indicator for youth. The regressions were also repeated on the subsamples of young (ages 18–29) and older individuals separately, with results presented in columns 3 and 4 of Table 4. This analysis reveals that the treatment had a significant and larger impact among the youth, confirming the earlier findings of Figure 3.

4.4 Effects on preferred transfer modality: Cash versus in-kind assistance

In addition to the differences in the giving rates, treatment and control groups also differed in terms of the proposal that they favored the most.[25] Table 5 reports the favorite proposal among participants who gave up their voucher in response to at least one proposal. Because the control groups contained more individuals who never gave up their vouchers, the two samples are not of identical size, so we compare the distribution of responses. In the control group, the unconditional food transfer option was the most frequent favorite, with a third of respondents picking it as their most-preferred proposal. With food transfers being more visible than cash transfers, food could be considered to be more tractable and of lesser interest for capture by the better-off in the context of high potential fraud and corruption. However, after getting a transparency-enhancing signal, participants in the treatment group were much less likely (by nearly 13 percentage points) to pick the food transfer as their most-favored proposal. Thus the treatment appears to have enhanced the attractiveness of cash-based delivery options that may be perceived as more prone to capture but also commonly considered to be more efficient in poverty reduction than in-kind food transfers (Currie and Gahvari 2008).

[25] As a reminder, after all decisions were marked, participants were asked to rank the proposals in the order of preference. In order to focus on true preferences, the analysis that follows uses only responses by participants who chose to give up the voucher for at least one proposal, and for those who gave up the voucher for strictly one proposal, it imposes that proposal as the revealed favorite.

5 Alternative explanations and robustness checks

Although the trust channel discussed in the introduction is, in our view, the most plausible explanation for the increase in giving because of a transparency shock, it is not the only possible one. As discussed in section 2.4, increased awareness about the value of giving offers a plausible alternative mechanism linking treatment to giving rates. It is possible that in addition to increasing the ability to monitor delivery, the treatment has also corrected some informational asymmetries on the benefits of the transfer for poor families. In particular, better-off or more educated participants could be less familiar with the consumption basket of the poor and unaware that JD 20 could buy as many essential supplies. This could happen because participants' consumption basket differs from that of the poor either in terms of the products or their quality. Thus, the fact that participants in the treatment groups were shown in the video a basket of essential supplies worth JD 20, as in proposal 2 (unconditional in-kind transfer), could have increased awareness of the value of giving among the better-off participants. However, in this case, we would expect the treatment effect to vary according to the level of education or income. We check this formally in Table 6. Panel A describes the effect of the interaction between treatment and being skilled (defined as having completed high school or more) on giving, controlling for participants' basic demographic and socioeconomic characteristics (gender, location in the capital city and number of cars in the household).[26] Each column reports a regression on participants' decisions on that specific proposal. In all specifications, the interaction term between treatment and skilled is not significant. Panel B describes the effect of the interaction between treatment and being high income (a variable equal to one if the participant reported an above mean value of per capita income) controlling for gender, location in the capital city and education.[27] In all specifications, the interaction term is not significant. Panel C considers an alternative definition of high income using participants' responses on their subjective position on the income distribution of Jordan (self-identified income quartile). In particular, self-identified high income is a dummy variable equal to one if the participant declared to be in income quartiles three or four. Also in this case, the interaction term between treatment and high income is not significant in all model specifications. This evidence indicates that trust rather than information/awareness on the value of giving explains the increase in giving because of a transparency shock.

To provide additional evidence for the robustness of the trust-based channel, we consider how the effect of the interaction between treatment and trust on giving varies across groups of the population with different scope for giving. We consider two dimensions: economic distance from the poor (measured using education and income) and how much the participant values redistribution (measured using social norms that have been found

[26] Results without controlling for number of cars in the household are similar. These are available from the authors upon request.

[27] The results obtained controlling for number of cars in the household, or excluding the education variables are similar to the ones reported and are available from the authors upon request.

to be correlated with redistributive behavior in the literature).[28] To construct indicators of the second dimension we use information from three survey questions that participants answered after the experiment, asking about participants' agreement with the following statements: (1) People are poor in Jordan because of bad luck or injustice (rather than laziness or lack of willpower); (2) Successful careers are a matter of luck and connections (rather than hard work)[29]; and (3) A just society should make people's incomes more equal.

Tables 7 and 8 present the results. Table 7 is similar to Table 4 but focuses on distance from the poor rather than age. Results show that distance from the poor does not appear to mediate the effect of the transparency-enhancing treatment, with the interaction term of distance from the poor and treatment being not significant in most specifications. Table 8 focuses on social norms and adds to our baseline specification controls for the scope for giving using the three attitudinal questions described above (Columns 1 to 3) and a composite index, produced with polychoric principal-components analysis, for the importance of redistributing (Column 4). When controls for indicators of being in favor of redistribution are included, results on the effect of the interaction term between treatment and trust on giving remain largely unchanged. This suggests that the relationship of interest is not being driven by differences in these indicators.

Finally, in Table 9 we check the robustness of our results to a different age threshold for youth. Table 9 is similar to Table 4 but considers youth to be those aged 18 to 34. Results on the interaction term between trust and treatment are robust to this alterantive definition.

6 Conclusion

This paper analyzes income redistribution preferences and the effect of program design and enhanced transparency on willingness to give up a personal endowment. It uses data from a behavioral experiment conducted on a nationally representative sample of the Jordanian middle class. In contrast to opinion surveys, the experiment evaluated preferences using real trade-offs. It contributes to the literature on redistributive preferences by offering

[28] The literature has shown that the demand for redistribution varies according to personal beliefs about the causes of poverty and success, with those who believe that people are poor because of bad luck or injustice or that success is the result of individual effort rather than luck being more prone to redistribution (Alesina, Glaeser, and Sacerdote 2001; Alesina and Glaeser 2004; Alesina and Angeletos 2005; Alesina and La Ferrara 2005; Alesina and Giuliano 2011; Charness and Rabin 2002; Konow 2010).

[29] 66 percent of the participants expressed a general belief that poverty is the result of bad luck or injustice rather than laziness, 37 percent believed that hard work usually brings success and 80 percent agreed that society should make people's incomes more equal. These perceptions are in line with those in Latin America and Western Europe but in stark contrast with the United States, where government redistribution from the rich to the poor is less extensive. This might also be a factor behind the higher giving rates in our experiment compared to the classic dictator games or fundraising experiments in the United States.

experimental evidence and results on the influence of transparency in benefit delivery on the overall support for targeted social assistance and on program design preferences.

The paper shows that support for redistributive programs is sensitive to the level of trust in the system's ability to deliver benefits to intended beneficiaries. In fact, although transparency does not appear to significantly affect the overall rates of giving (controlling for program design), it does have a significant positive effect on the giving by youth and by those people who exhibit low trust in the existing delivery of safety nets, especially in the case of unconditional cash transfers. Moreover, among those low-trust individuals, enhanced transparency makes cash-based transfers more attractive than in-kind transfers. Because the latter are generally less efficient but may be perceived as less prone to elite capture, transparency could thus also enhance program efficiency by allowing policy makers to switch from in-kind to cash transfers without losing the support of their middle-class citizens.

Annex 1: Sample design and selection protocols

The experiment's sampling strategy adopted the definition developed by the Government of Jordan's study of the middle class (ESC 2008), which defined the middle class as those households that have per capita incomes between twice and four times the Jordan's national poverty line. This definition corresponded to the population between the 4th and the 8th income decile according to the 2004 Jordanian census, the latest available at the time. For this study, middle-class primary sampling units (PSUs) in the census were selected by a three-step process: 1) constructing a proxy means test regression using Jordan's 2010 Household Expenditure and Income Survey;[30] 2) applying coefficients from that regression to the 2004 census data; and 3) choosing PSUs with resulting average scores between 4th and 8th income deciles. Within the population of middle-class PSUs, 21 sampling units were selected for the experiment via random selection with probability proportionate to size.

Within each sampling unit, the following protocol was used to recruit the needed 20 individuals (10 for treatment, 10 for control) at the same time and place. The day before the experiment in a PSU, a team of enumerators would visit the selected PSU, and the team leader would use a random walk method to select households for recruitment. Enumerators then visited this sample of households, introducing themselves with an invitation letter from the Center for Strategic Studies in Jordan (CSS), and used a Kish (1949) table to identify one eligible person who was at least 18 years old to be invited to a meeting the next day at a the reserved location (usually a nearby public school). The purpose of

[30] The regression included the following variables, which appear in both the Household Expenditure and Income Survey and the census: average household size, owning a fixed phone, a computer, internet connection, central heating, microwave, home ownership, and having at least one family member with university education

the meeting was not directly explained to the invitees except to say that they have been randomly selected in their community to participate in a research study by the CSS, and that it is not related to market research.

To compensate participants for their time, the invitation letter explained that all participants would receive a fuel voucher of JD 5 (equivalent to about US$7.50) as a show-up fee and that there would be a chance to keep JD 10 more in such vouchers, depending on the outcome of the meeting. Each invitee who agreed to participate was left with two receipts, which they were encouraged to bring to the meeting to exchange for real vouchers: one for the show-up fee of JD 5, and the other for JD 10.

If the person selected by the Kish table was not present at the time of enumerators' first visit, enumerators would schedule an appointment and visit the household again in the evening to make the invitation in person. Based on extensive piloting, protocols were designed to replace households whose members refused the invitation and to ensure that two groups of 10 randomly assigned individuals could be constituted in each PSU. To ensure that 20 participants would show up at the set time to the next day's meeting, enumerators invited 30 individuals per PSU, emphasizing that it is very important to show up on time. At the start of the meeting, all present participants signed a consent form and were randomly assigned to either the treatment group classroom or the control group classroom. Despite the appeal to show up on time, the team had to delay the start of the experiment virtually every time to fill the quota of 20 participants. After 20 participants had assembled, randomization into control and treatment groups occurred. Any latecomers were assembled into a mock group, which received the show-up fee and completed a questionnaire.

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Table 1: Sample balance statistics

	Mean [s.d.]			Difference	Number of
	Control (C)	Treatment (T)	Full sample	(C-T) [p-value]	obs.
	(1)	(2)	(3)	(4)	(5)
Panel A: Individual characteristics					
% male	0.42	0.49	0.45	0.20	420
	[0.495]	[0.501]	[0.498]		
% with primary education	0.06	0.06	0.06	0.85	420
	[0.242]	[0.233]	[0.236]		
% with secondary education	0.61	0.57	0.59	0.15	420
	[0.488]	[0.497]	[0.492]		
% with tertiary education	0.32	0.38	0.35	0.17	420
	[0.469]	[0.486]	[0.477]		
% young (18-25 year old)	0.23	0.20	0.21	0.34	420
	[0.420]	[0.397]	[0.409]		
% young (18-29 year old)	0.31	0.25	0.28	0.11	420
	[0.465]	[0.435]	[0.451]		
% young (18-34 year old)	0.41	0.35	0.38	0.16	420
	[0.493]	[0.478]	[0.486]		
% currently employed	0.34	0.33	0.34	0.67	418
	[0.476]	[0.471]	[0.473]		
Panel B: Household caracteristics					
Per capita expenditure (JD per month)	63.02	63.37	63.19	0.90	406
	[44.67]	[44.810]	[44.682]		
% that has cars in the household	0.71	0.67	0.69	0.69	420
	[0.691]	[0.832]	[0.763]		
Household size	6.07	6.11	6.09	0.84	416
	[2.236]	[2.464]	[2.350]		
% with low ”subjective” income	0.18	0.23	0.21	0.15	412
	[0.388]	[0.421]	[0.405]		
% with middle "subjective" income	0.783	0.751	0.77	0.38	412
	[0.413]	[0 .433]	[0.423]		
% with high "subjective" income	0.03	0.02	0.03	0.48	412
	[0.181]	[0.139]	[0.161]		
Panel C: Giving behaviour					
% that gave to charity in the last 3 months?	0.65	0.58	0.62	0.14	413
	[0.477]	[0.495]	[0.487]		
% that can rely on kins' help if needed?	0.16	0.19	0.17	0.37	412
	[0.366]	[0.394]	[0.380]		

Notes: Columns 1 and 2 report the mean and standard deviation (in square brackets) of each variable for the control and treatment groups. Column 3 reports the mean and standard deviation (in square brackets) of each variable for the full sample (i.e. control + treatment groups). Column 4 reports the p-value of the t-test of the difference between the control and treatment groups (using clustered wild bootstrap-t statistics at the PSU level). Column 5 shows the number of observations used.

Table 2: Average treatment effect on giving rates

	Control (C)	Treatment (T)	ATE	Difference (C-T) [p-value]	Full Sample	Number of obs.
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Aggregate/all proposals						
Mean giving	0.58	0.63	0.05	0.22	0.67	420
	[0.396]	[0.377]			[0.387]	
Frequent giver	0.51	0.57	0.06	0.26	0.61	420
	[0.495]	[0.481]			[0.488]	
Panel B: Individual proposals						
Unconditional cash transfer	0.60	0.68	0.08	0.11	0.69	420
	[0.479]	[0.443]			[0.462]	
Unconditional food transfer	0.62	0.64	0.02	0.63	0.70	420
	[0.465]	[0.453]			[0.458]	
Unconditional cash transfer	0.59	0.66	0.07	0.15	0.63	420
and school	[0.493]	[0.473]			[0.483]	
Cash transfer conditional on	0.59	0.62	0.03	0.52	0.65	420
training	[0.483]	[0.471]			[0.476]	

Notes: Panel A uses information from all the proposals, while panel B uses information from each proposal at a time. Each line reports the results of a regression on giving in that specific proposal controlling for gender, three education levels, location in the capital city, and number of cars in the household. Mean giving is computed at the participant level and is the share total proposals in which the participant indicated he would give up his voucher. Columns 1 and 2 report the mean and standard deviation (in square brackets) of each variable for the control and treatment groups. Column 3 reports the average treatment effect and column 4 reports the p-value of the t-test of the difference between the two samples (using clustered wild bootstrap-t at the PSU level). Column 5 reports the mean and standard deviation (in square brackets) of each variable for full sample. Column 6 shows the number of observations used.

Table 3: Average treatment effect on giving rates among low- and high-trust participants

	Low trust (LT)			High trust (HT)			Difference (*p*-value)	
	Control (C)	Treatment (T)	*p*-value (C-T)	Control (C)	Treatment (T)	*p*-value (C-T)	(C in LT - C in HT)	(T in LT - T in HT)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Aggregate/all proposals								
Mean giving	0.48	0.57	0.08	0.66	0.70	0.45	0.02	0.15
	[0.403]	[0.371]		[0.378]	[0.367]			
Frequent giver	0.37	0.52	0.06	0.61	0.63	0.79	0.04	0.59
	[0.502]	[0.483]		[0.479]	[0.473]			
Panel B: Individual proposals								
Unconditional cash transfer	0.44	0.60	0.04	0.75	0.77	0.63	0.01	0.21
	[0.497]	[0.456]		[0.439]	[0.424]			
Unconditional food transfer	0.56	0.63	0.16	0.66	0.67	0.79	0.44	0.91
	[0.473]	[0.444]		[0.455]	[0.444]			
Unconditional cash transfer and school	0.39	0.49	0.18	0.62	0.67	0.35	0.06	0.10
	[0.501]	[0.489]		[0.472]	[0.444]			
Cash transfer conditional on training	0.51	0.56	0.44	0.63	0.67	0.48	0.46	0.27
	[0.489]	[0.479]		[0.476]	[0.456]			

Notes: Columns 1 and 2 report the mean and standard deviation (in square brackets) of each variable for the control and treatment group among low trust individuals. Column 3 reports the p-value of the t-test of the difference between the two samples among low trust individuals (using clustered wild bootstrap-t at the PSU level). Columns 4 and 5 report the mean and standard deviation (in square brackets) of each variable for the control and treatment group for high trust individuals. Column 6 reports the average treatment effect and column [8] reports the p-value of the t-test of the difference between the two samples among high trust individuals (using clustered wild bootstrap-t at the PSU level). Column 7 reports the p-value of the t-test of the difference between giving among low- and high-trust participants in the control group. Column 8 reports the p-value of t-tests of the difference between giving among low- and high-trust participants in the treatment group. All regressions include controls for gender, three education levels, location in the capital city, and number of cars in the household. Observations are 194 in the low trust group and 217 individuals in the high-trust group for a total of 411 participants (due to 9 missing responses to the trust question).

Table 4: Effect of the interaction between treatment and trust on giving for youth and adults

		All		Young (18-29)	Adults (30+)
		(1)	(2)	(3)	(4)
(A) Aggregate/all proposals					
Mean giving	Treatment	0.08	0.07	0.19	0.02
		[0.054]	[0.050]	[0.093]**	[0.050]
	Treatment*Trust	-0.05	-0.05	-0.10	-0.03
		[0.020]**	[0.020]**	[0.047]**	[0.031]
	Trust	0.11	0.11	0.11	0.10
		[0.038]***	[0.039]***	[0.063]	[0.042]**
	Young (18-29)		-0.10		
			[0.047]**		
Frequent giver	Treatment	0.13	0.11	0.21	0.07
		[0.077]	[0.074]	[0.132]	[0.082]
	Treatment*Trust	-0.12	-0.11	-0.20	-0.08
		[0.043]***	[0.042]***	[0.085]**	[0.050]*
	Trust	0.14	0.14	0.16	0.13
		[0.050]***	[0.051]***	[0.094]	[0.061]**
	Young (18-29)		-0.15		
			[0.063]**		
(B) Individual proposals					
Unconditional cash transfer	Treatment	0.14	0.13	0.25	0.07
		[0.075]*	[0.075]*	[0.134]*	[0.085]
	Treatment*Trust	-0.12	-0.11	-0.16	-0.11
		[0.038]***	[0.037]***	[0.071]**	[0.050]**
	Trust	0.18	0.18	0.17	0.18
		[0.000]***	[0.000]***	[0.088]*	[0.058]***
	Young (18-29)		-0.13		
			[0.047]***		
Unconditional food transfer	Treatment	0.07	0.06	0.21	0.00
		[0.050]	[0.048]	[0.000]***	[0.042]
	Treatment*Trust	-0.05	-0.05	-0.14	-0.02
		[0.027]*	[0.028]*	[0.057]**	[0.076]
	Trust	0.05	0.05	0.06	0.03
		[0.036]	[0.037]	[0.053]	[0.068]
	Young (18-29)		-0.03		
			[0.050]		
Unconditional cash transfer and school	Treatment	0.09	0.08	0.13	0.05
		[0.072]	[0.068]	[0.141]	[0.076]
	Treatment*Trust	-0.04	-0.03	-0.03	-0.03
		[0.027]	[0.027]	[0.038]	[0.039]
	Trust	0.15	0.15	0.08	0.17
		[0.063]**	[0.063]**	[0.097]	[0.078]**
	Young (18-29)		-0.12		
			[0.060]**		
Cash transfer conditional on training	Treatment	0.03	0.02	0.19	-0.05
		[0.064]	[0.058]	[0.170]	[0.090]
	Treatment*Trust	0.01	0.01	-0.09	0.06
		[0.020]	[0.047]	[0.054]	[0.056]
	Trust	0.05	0.06	0.11	0.02
		[0.055]	[0.059]	[0.084]	[3.495]
	Young (18-29)		-0.12		
			[0.056]**		
Number of observations		411	411	116	295

Notes: The estimation method is a linear probability model. All regressions include controls for gender, three education levels, location in the capital city, and number of cars in the household. The dependent variable is the giving rate. Standard errors clustered at the PSU level using wild bootstrap-t are reported in brackets. Trust is a dummy variable equal to one if the answer to the question "How confident are you that the public funds allocated for social assistance reach the poor?" is "completely confident" or "somewhat confident", and zero otherwise. ***, **, * significant at 1%, 5%, and 10% level.

Table 5: Distribution of the preferred proposal, by treatment status

	Control (C)	Treatment (T)	ATE	Difference (C-T) [p-value]
	[1]	[2]	[3]	[4]
Prefered proposal				
Unconditional cash transfer	0.21	0.24	0.03	0.56
	[0.409]	[0.429]		
Unconditional food transfer	0.34	0.21	-0.12	0.07
	[0.473]	[0.410]		
Unconditional cash transfer and school	0.13	0.20	0.07	0.18
	[0.337]	[0.397]		
Cash transfer conditional on training	0.32	0.35	0.03	0.62
	[0.469]	[0.478]		
Total	1	1		
Number of observations	161	174		

Notes: Columns 1 and 2 report the mean and standard deviation (in square brackets) of each variable for the control and treatment groups. Column 3 reports the average treatment effect and column 4 reports the p-value based on wild bootstrap-t cluster-robust standard errors. Results based on reported preferred proposal among those actually chosen by participants. Results control for gender, three education levels, location in the capital city, and number of cars in the household. For individuals who only decided to give up their voucher once, the preferred proposal is assumed to be the delivery method actually chosen.

Table 6: Effect of the interaction between treatment and education and between treatment and income

	A) Aggregate/all proposals		B) Individual proposals			
	Mean giving	Frequent giver	Uncond. cash transfer	Uncond. food transfer	Uncond. cash transfer and school	Cash transfer conditional on training
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Interaction with education						
Treatment	0.02	0.01	0.04	0.00	0.04	0.01
	[0.048]	[0.051]	[0.061]	[0.017]	[0.059]	[0.055]
Treatment*Skilled	0.09	0.14	0.12	0.06	0.11	0.06
	[0.093]	[0.101]	[0.129]	[0.083]	[0.151]	[0.147]
Skilled	0.00	-0.04	-0.07	0.03	-0.02	0.06
	[0.003]	[0.091]	[0.094]	[0.054]	[0.190]	[0.065]
Number of observations	420	420	420	420	420	420
Panel B: Interaction with income						
Treatment	0.06	0.04	0.08	0.07	0.05	0.02
	[0.041]	[0.055]	[0.058]	[0.050]	[0.050]	[0.052]
Treatment*High income	0.02	0.09	0.03	-0.10	0.08	0.05
	[0.045]	[0.164]	[0.070]	[0.063]	[0.228]	[0.124]
High income	0.04	0.04	0.03	0.10	0.01	0.03
	[0.033]	[0.046]	[0.023]	[0.062]	[0.019]	[0.052]
Number of observations	406	406	406	406	406	406
Panel C: Interaction with "subjective" income						
Treatment	0.04	0.03	0.05	0.00	0.08	0.01
	[0.050]	[0.058]	[0.051]	[0.027]	[0.057]	[0.036]
Treatment*High	0.11	0.13	0.17	0.11	0.04	0.13
"subjective" income	[0.144]	[0.159]	[0.101]*	[0.116]	[0.080]	[0.165]
High "subjective" income	-0.04	-0.07	-0.06	-0.05	-0.02	-0.04
	[0.219]	[0.120]	[0.194]	[0.103]	[0.048]	[0.146]
Number of observations	404	404	404	404	404	404

Notes: The estimation method is a linear probability model. Each column reports regression on participants' decisions on all (columns 1 and 2) or specific proposal (columns 3-6) controlling for gender, location in the capital city, and number of cars in the household (Panel A), and for gender, three education levels, location in the capital city and number of cars in the household (Panels B and C). Results are maintained if excluding education levels and number of cars in the household as control variables. Standard deviation reported below (in square brackets). Standard errors clustered at the PSU level using wild bootstrap-t are reported in brackets. Skilled is a dummy variable equal to one if the participant has completed high school or more. High income is a dummy variable equal to one if the participant lives in a household with an income per capita level above the sample mean. High "subjective" income is a dummy variable equal to one if the participant reports that his relative position on an income scale from one (lowest) to four (highest) is three or four. ***, **, * significant at 1%, 5%, and 10% level.

Table 7: Robustness of the interaction between treatment and trust to the inclusion of distance from the poor

Measure of distance from the poor used:		*Education*			*Per capita income*			*"Subjective" income*		
		All	Skilled	Unskilled	All	Above mean	Below mean	All	High	Low
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Mean giving	Treatment	0.08	0.10	0.07	0.10	0.13	0.09	0.09	0.37	0.05
		[0.055]	[0.079]	[0.073]	[0.057]*	[0.093]	[0.067]	[0.055]*	[0.177]**	[0.058]
	Treatment*Trust	-0.05	0.03	-0.07	-0.07	-0.12	-0.06	-0.06	-0.27	-0.03
		[0.021]**	[0.049]	[0.054]	[0.025]***	[0.048]**	[0.033]*	[0.022]***	[0.088]***	[0.023]
	Distance from the poor	0.04			0.05			-0.00		
		[0.044]			[0.034]			[0.031]		
Frequent giver	Treatment	0.12	0.14	0.11	0.15	0.23	0.11	0.13	0.48	0.09
		[0.076]	[0.099]	[0.102]	[0.082]*	[0.133]*	[0.090]	[0.077]*	[0.244]*	[0.079]
	Treatment*Trust	-0.11	0.03	-0.16	-0.15	-0.20	-0.14	-0.12	-0.35	-0.11
		[0.042]***	[0.030]	[0.088]*	[0.049]***	[0.065]***	[0.064]**	[0.046]***	[0.112]***	[0.043]**
	Distance from the poor	0.03			0.08			-0.02		
		[0.062]			[0.040]**			[0.210]		
Uncond. cash transfer	Treatment	0.14	0.22	0.09	0.16	0.21	0.14	0.15	0.49	0.10
		[0.077]*	[0.084]**	[0.098]	[0.078]**	[0.112]*	[0.102]	[0.077]*	[0.227]**	[0.078]
	Treatment* Trust	-0.12	-0.13	-0.09	-0.14	-0.19	-0.12	-0.13	-0.37	-0.09
		[0.038]***	[0.049]**	[0.063]	[0.045]***	[0.062]***	[0.054]**	[0.041]***	[0.118]***	[0.050]*
	Distance from the poor	-0.00			0.06			-0.00		
		[0.012]			[0.032]*			[0.009]		
Uncond. food transfer	Treatment	0.06	0.02	0.08	0.08	0.06	0.11	0.07	0.36	0.02
		[0.049]	[0.083]	[0.052]	[0.050]*	[0.094]	[0.073]	[0.049]	[0.197]*	[0.059]
	Treatment*Trust	-0.05	0.11	-0.11	-0.08	-0.15	-0.06	-0.06	-0.29	-0.02
		[0.027]*	[0.180]	[0.048]**	[0.028]***	[0.094]	[0.023]**	[0.027]**	[0.092]***	[0.039]
	Distance from the poor	0.05			0.04			-0.01		
		[0.053]			[0.056]			[0.231]		
Uncond. cash transfer and school	Treatment	0.09	0.08	0.09	0.11	0.15	0.10	0.11	0.31	0.09
		[0.072]	[0.124]	[0.105]	[0.079]	[0.141]	[0.090]	[0.076]	[0.233]	[0.084]
	Treatment*Trust	-0.03	0.10	-0.09	-0.06	-0.05	-0.08	-0.06	-0.24	-0.03
		[0.026]	[1.772]	[0.076]	[0.033]*	[0.043]	[0.060]	[0.033]*	[0.101]**	[0.031]
	Distance from the poor	0.04			0.05			-0.02		
		[0.051]			[0.037]			[0.129]		
Cash transfer cond. on training	Treatment	0.03	0.06	0.01	0.04	0.12	0.00	0.04	0.31	0.00
		[0.067]	[0.084]	[0.056]	[0.070]	[0.107]	[0.029]	[0.067]	[0.225]	[0.003]
	Treatment*Trust	0.01	0.05	0.00	-0.00	-0.08	0.03	-0.00	-0.19	0.02
		[0.030]	[0.268]	[0.054]	[0.004]	[0.047]	[0.259]	[0.001]	[0.104]*	[0.126]
	Distance from the poor	0.09			0.06			0.01		
		[0.050]*			[0.040]			[0.035]		
Number of observations		411	146	265	399	150	249	404	100	304

Notes: The estimation method is a linear probability model. All regressions include controls for trust, gender, location in capital city, and number of cars in household. Standard errors clustered at PSU level using wild bootstrap-t reported in brackets.***,**,* significant at 1%, 5% and 10% level.

Table 8: Robustness of the interaction between treatment and trust to the inclusion of social norms on redistribution

		Measure of "redistribution values"			
		People are poor because of bad luck or insjustice (not laziness)	Success is a matter of hard work	Society should make incomes more equal	Composite index
		(1)	(2)	(3)	(4)
(A) Aggregate/all proposals					
Mean giving	Treatment	0.12	0.09	0.08	0.11
		[0.065]*	[0.055]	[0.056]	[0.066]*
	Treatment*Trust	-0.10	-0.06	-0.03	-0.09
		[0.033]***	[0.022]**	[0.017]*	[0.036]**
	Redistribution values	0.05	0.11	-0.02	-0.03
		[0.044]	[0.037]***	[0.039]	[0.050]
Frequent giver	Treatment	0.18	0.13	0.13	0.17
		[0.091]*	[0.077]*	[0.078]	[0.097]*
	Treatment*Trust	-0.18	-0.12	-0.10	-0.17
		[0.060]***	[0.046]***	[0.038]***	[0.057]***
	Values redistribution	0.08	0.12	-0.07	-0.04
		[0.064]	[0.049]**	[0.058]	[0.099]
(B) Individual proposals					
Unconditional cash transfer	Treatment	0.17	0.14	0.14	0.16
		[0.089]*	[0.076]*	[0.080]*	[0.089]*
	Treatment*Trust	-0.17	-0.12	-0.10	-0.16
		[0.054]***	[0.041]***	[0.034]***	[0.051]***
	Redistribution values	0.07	0.10	-0.06	-0.03
		[0.045]	[0.041]**	[0.044]	[0.048]
Unconditional food transfer	Treatment	0.10	0.07	0.06	0.09
		[0.057]*	[0.049]	[0.050]	[0.056]
	Treatment*Trust	-0.10	-0.06	-0.04	-0.09
		[0.037]***	[0.028]**	[0.025]	[0.034]***
	Redistribution values	0.05	0.12	-0.06	-0.05
		[0.055]	[0.050]**	[0.053]	[0.056]
Unconditional cash transfer and school	Treatment	0.15	0.10	0.09	0.14
		[0.084]*	[0.076]	[0.071]	[0.092]
	Treatment*Trust	-0.11	-0.04	-0.01	-0.10
		[0.040]***	[0.029]	[0.013]	[0.043]**
	Redistribution values	0.08	0.10	0.04	-0.01
		[0.051]	[0.049]**	[0.053]	[0.196]
Cash transfer conditional on training	Treatment	0.06	0.04	0.03	0.06
		[0.083]	[0.068]	[0.068]	[0.089]
	Treatment*Trust	-0.03	0.00	0.02	-0.02
		[0.030]	[0.004]	[0.201]	[0.027]
	Redistribution values	0.01	0.10	-0.01	-0.03
		[0.038]	[0.044]**	[0.056]	[0.061]
Number of observations		346	405	407	340

Notes: The estimation method is a linear probability model. All regressions include controls for trust, gender, three education levels, location in the capital city, and number of cars in the household. The dependent variable is the giving rate. Results in column 1 control for believing that people are poor because of bad luck or injustice rather than laziness. Results in column 2 control for believing that success is a matter of hard work rather than luck or connections. Results in column 3 control for believing that society should redistribute income. Results in column 4 control for the composite index made of all three values constructed via polychoric principal-components model. Standard errors clustered at the PSU level using wild bootstrap-t are reported in brackets. ***, **, * significant at 1%, 5%, and 10% level.

Table 9: Robustness of interplay between treatment, trust and age to a different definition of youth

		All (1)	Young (18-34) (2)	Adults (35+) (3)
(A) Aggregate/all proposals				
Mean giving	Treatment	0.07	0.22	-0.01
		[0.051]	[0.088]**	[0.141]
	Treatment*Trust	-0.04	-0.19	0.03
		[0.018]**	[0.067]***	[0.054]
	Trust	0.11	0.17	0.08
		[0.039]***	[0.060]***	[0.064]
	Young (18-34)	-0.12		
		[0.045]**		
Frequent giver	Treatment	0.11	0.28	0.02
		[0.073]	[0.138]**	[0.063]
	Treatment*Trust	-0.10	-0.29	-0.01
		[0.038]***	[0.103]***	[0.034]
	Trust	0.14	0.21	0.11
		[0.051]***	[0.069]***	[0.078]
	Young (18-34)	-0.16		
		[0.064]**		
(B) Individual proposals				
Unconditional cash	Treatment	0.12	0.31	0.02
		[0.076]	[0.136]**	[0.069]
	Treatment*Trust	-0.11	-0.29	-0.02
		[0.037]***	[0.102]***	[0.038]
	Trust	0.18	0.29	0.12
		[0.000]***	[0.000]***	[0.069]*
	Young (18-34)	-0.13		
		[0.042]***		
Unconditional food transfer	Treatment	0.06	0.23	-0.03
		[0.049]	[0.076]***	[0.071]
	Treatment*Trust	-0.05	-0.22	0.03
		[0.027]*	[0.071]***	[0.045]
	Trust	0.05	0.12	0.01
		[0.039]	[0.062]*	[0.043]
	Young (18-34)	-0.06		
		[0.051]		
Unconditional cash transfer and school	Treatment	0.07	0.14	0.03
		[0.067]	[0.108]	[0.066]
	Treatment*Trust	-0.02	-0.04	-0.03
		[0.022]	[0.036]	[0.051]
	Trust	0.16	0.11	0.19
		[0.064]**	[0.087]	[0.092]**
	Young (18-34)	-0.14		
		[0.051]***		
Cash transfer conditional on training	Treatment	0.02	0.21	-0.08
		[0.050]	[0.147]	[0.101]
	Treatment*Trust	0.02	-0.22	0.14
		[0.451]	[0.086]**	[0.069]**
	Trust	0.06	0.16	-0.01
		[0.063]	[0.086]*	[0.017]
	Young (18-34)	-0.14		
		[0.056]**		
Number of observations		411	157	254

Notes: The estimation method is a linear probability model. All regressions include controls for gender, three education levels, location in the capital city, and number of cars in the household. The dependent variable is the giving rate. Standard errors clustered at the PSU level using wild bootstrap-t are reported in brackets. ***, **, *significant at 1%, 5%, and 10% level.

Table A.1: Summary statistics

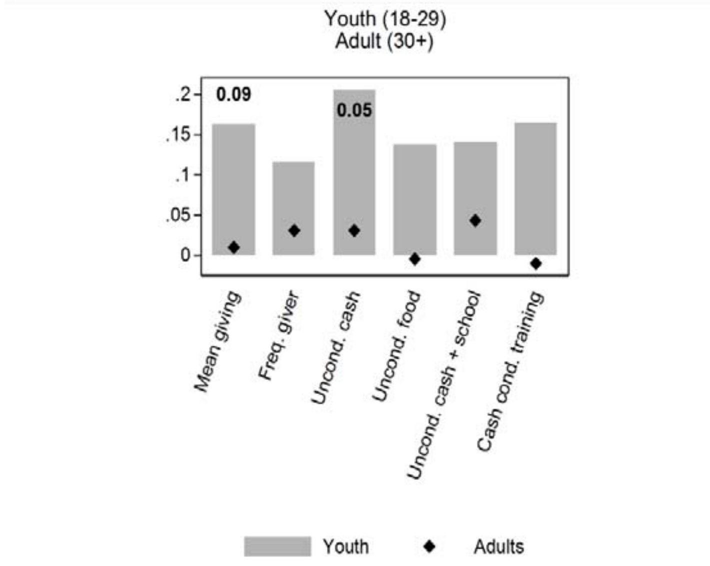
	Mean [s.d.] (1)	s.d. (2)	Number of observations (3)
Mean giving	0.67	0.39	420
% frequent giver	0.61	0.49	420
% "accepted" unconditional cash transfer (UCT)	0.69	0.46	420
% "accepted" unconditional food transfer (UFT)	0.70	0.46	420
% "accepted" unconditional cash transfer and school (UCT+School)	0.63	0.48	420
% "accepted" cash transfer conditional on training (CCT)	0.65	0.48	420
% high trust	0.53	0.50	411
% young (18-25 year old)	0.21	0.41	420
% young (18-29 year old)	0.28	0.45	420
% young (18-34 year old)	0.38	0.49	420
% skilled	0.35	0.48	420
Per capita expenditure (JD per month)	63.19	44.68	406
% high income	0.37	0.48	406
% that self-identified as high income	0.25	0.43	412
% that reported prefered proposal UCT	0.23	0.42	414
% that reported prefered proposal UFT	0.28	0.45	414
% that reported prefered proposal UCT+School	0.16	0.37	414
% that reported prefered proposal CCT	0.33	0.47	414
People are poor because of bad luck or insjustice (not laziness)	0.60	0.49	350
Success is a matter of hard work	0.63	0.48	408
Agrees that society should make incomes more equal	0.80	0.40	410

Table A.2: Average treatment effect on giving rates without control variables

	Mean [s.d.]			Difference (C-T) (p-value)	Number of obs.
	Control	Treatment	Full Sample		
	(1)	(2)	(3)	(4)	(5)
Panel A: Aggregate/all proposals					
Mean giving	0.65	0.71	0.67	0.19	420
	[0.395]	[0.377]	[0.387]		
Frequent giver	0.58	0.64	0.61	0.26	420
	[0.495]	[0.481]	[0.488]		
Panel B: Individual proposals					
Unconditional cash transfer	0.65	0.73	0.69	0.11	420
	[0.478]	[0.443]	[0.462]		
Unconditional food transfer	0.69	0.72	0.70	0.55	420
	[0.465]	[0.452]	[0.458]		
Unconditional cash transfer and school	0.59	0.67	0.63	0.13	420
	[0.492]	[0.472]	[0.483]		
Cash transfer conditional on training	0.63	0.67	0.65	0.47	420
	[0.483]	[0.476]	[0.476]		

Notes: Panel A uses information from all the proposals, while panel B uses information from each proposal at a time. Each line reports regression on participants' decisions on that specific proposal without any controls. Mean giving is computed at the participant level and is the share total proposals in which the participant indicated he would give up his voucher. Columns 1, 2 and 3 report the mean and standard deviation (in square brackets) of each variable for the control groups, treatment groups and full sample. Column 4 reports the p-value of the t-test of the difference between the two samples (using clustered wild bootstrap-t at the PSU level). Column 5 shows the number of observations used.

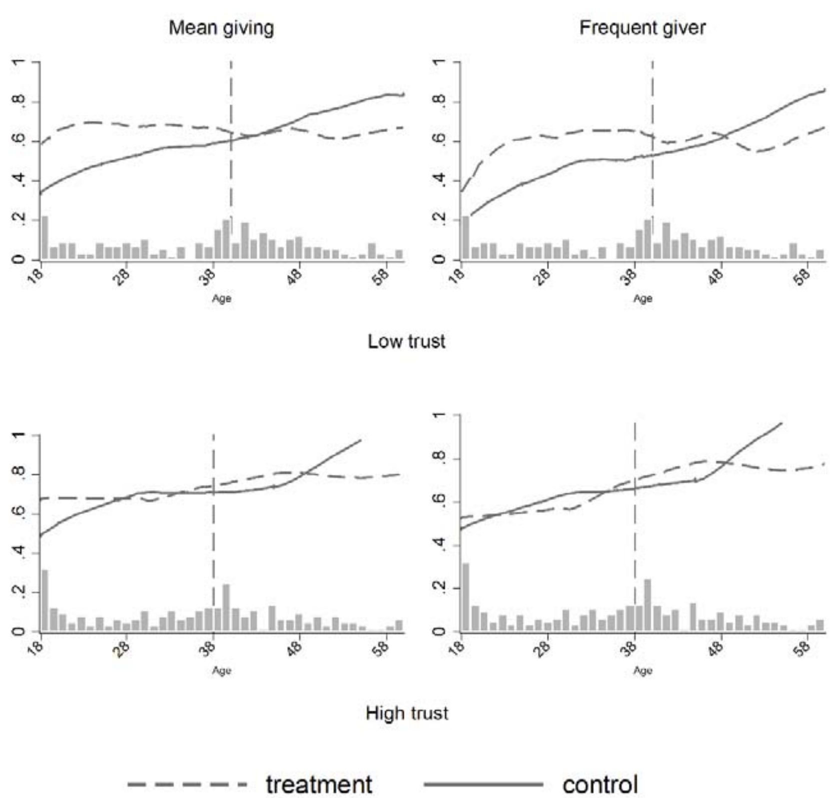
Figure 1: Average Treatment Effect on Giving Rates, by Age Group



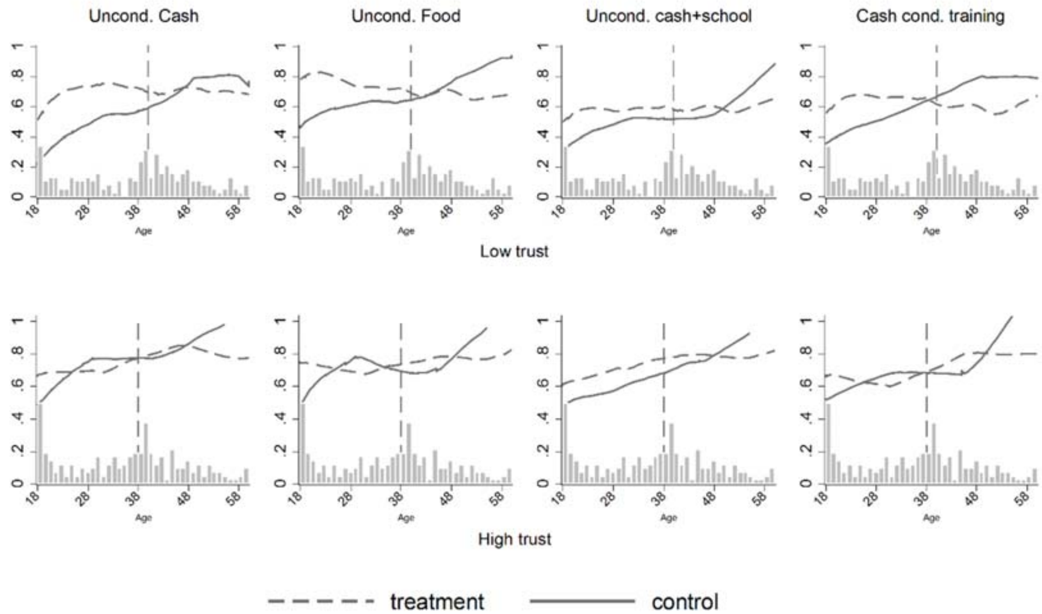
Notes: Linear effect of participating in treatment group on giving decisions in age group subsamples. P-values reported are based on wild-t bootstrap standard errors, N=420. "*Freq. giver*" means gave up the voucher in more than 2 proposals. Adults in the sample range up to 80 years old.

Figure 2: Treatment Impact, by Age and Trust Level

Panel A: Aggregate proposals

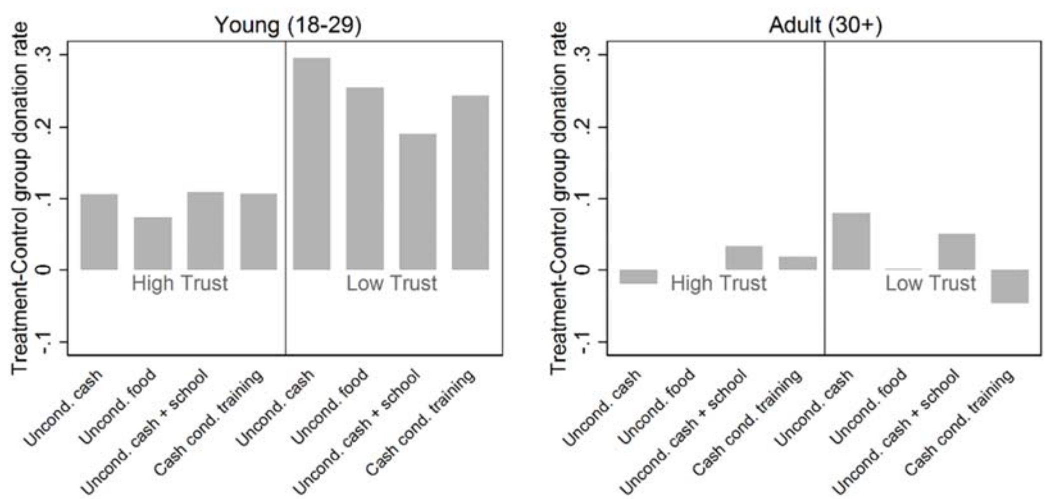


Panel B: Individual proposals



Notes: Lines produced with local polynomial smooth function on mediating variable age. Mean giving is the number of times a participant decided to donate his voucher out of 4. Vertical line represents the median of age in each sub-group. At the bottom of the graph the histogram of age is plotted. Bandwidth of 0.8.

Figure 3: Treatment Impact, by Age Group and Trust Level



Note: Figure shows the average treatment effect (difference between treatment and control groups' donation rates) for the nested subsamples of youth and adult with either high or low trust in the delivery of social safety nets. For the formal econometric results of the interaction between trust level and treatment for different age groups, see table 4.