Distrust in State Capacity and Low Support for Redistribution: Is Transparency the Solution?*

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Abstract

Citizens' support for redistributive policies can be influenced by their perceptions of state capacity for effective implementation of pro-poor programs. Evidence from a novel representative survey in four countries in the Middle East and North Africa demonstrates that most citizens support redistribution yet lack trust in the capacity of their governments to deliver programs that benefit the poor. Using a lab-in-the-field experiment on a representative sample of middle-class households in Jordan, this study provides causal evidence that increasing transparency in program delivery makes citizens (particularly low-trust individuals and youth) more willing to redistribute from themselves to the poor.

Keywords: redistribution, trust, state capacity, altruism, transparency, development, experiments.

JEL: C93, D31, H23, I38.

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1 Introduction

Preferences and trust are the two main factors that explain citizens' support for redistribution, from the richest to the poorest (Alesina, Glaeser and Glaeser, 2004).[1] Low support for redistribution might simply be explained by inequity aversion. Alesina and Angeletos (2005) find for instance that countries whose citizens believe wealth is mostly the outcome of luck, rather than work and effort tend to prefer redistributive policies, a mechanism that likely explains observed differences between the United States and Europe. But even in countries where citizens favor more equal income distribution, high levels of corruption and inefficiencies at the state level jeopardize support for redistribution as citizens distrust their government. Indeed, even when the median voter is in favor of redistribution, they face an agency issue: the principal (that is, the median voter) can choose to support a redistributive policy led by the agent (the government or any representative), but can only observe partially the agent's action.

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. A government's failure to attain such credibility could affect the provision of public goods in different forms: in more mature democracies, through voting outcomes (Ferraz and Finan, 2011), and in other cases, through tax evasion (Barone and Mocetti, 2011; Friedman et al., 2000; Johnson et al., 2000; Silverman, Slemrod and Uler, 2014), exercise of corruption, or public protest (Acemoglu, Hassan and Tahoun, 2018). Without any opportunity to prove good will, the government may be stuck in a distrust trap: citizens do not support redistributive policies and the government cannot prove its potential efficiency.

To obtain citizens' support, an optimal strategy is to provide credible signals that the transferred resources will be used as the principal intends. For example, to ensure that unemployment and pension payments are delivered to intended beneficiaries, the Government of India has created a biometrically authenticated payments infrastructure

[1] A third important factor is social mobility: citizens might accept more inequity when social mobility is high because no one is stuck in a given position on the social ladder (Alesina, Glaeser and Glaeser, 2004). Even though we acknowledge the existence of this mechanism, this paper focuses on the roles of preferences and trust.

("smartcards"), which delivers more predictable and less corruption-prone benefits without reducing program access (Muralidharan, Niehaus and Sukhtankar, 2016). Thanks to transparency, public trust in government can be increased by expanding knowledge about the government's activity (Cook, Jacobs and Kim, 2010). But while there are high expectations of the positive effects of increased transparency on citizens' support for redistributive policies, there is a dearth of rigorous evidence on this topic.

This paper investigates how trust in state capacity to deliver welfare programs can be key for eliciting more altruistic decisions and offers evidence on the power of transparency-enhancing measures to increase public support for redistributive policies. We focus on countries in the Middle East and North Africa (MENA), as the region is known for high levels of inequality and weak social safety net programs (Silva, Levin and Morgandi, 2013). Using unique survey data on representative samples of adults from four MENA countries (Arab Republic of Egypt, Jordan, Lebanon and, Tunisia), we are the first to provide evidence that the majority of citizens in these particual countries want redistribution to the poor, preferably in cash rather than in-kind, and believe that it is the role of the state to support the most fragile individuals. However, citizens tend to think that the government is relatively ineffective in this role. These results rule out the hypothesis that weak welfare policies can be explained by low aversion to inequality in MENA countries, and instead indicate that trust in state capacity to deliver desired redistribution is an important channel to explore.

We build a simple decision model to show how an inequity-averse representative citizen might reject redistributive policies when exposed to uncertainty about the government's benevolence, that is the extent to which the government aims to maximize social welfare. Our model provides sufficient conditions so that a society that is stuck in a bad equilibrium (no support for redistributive policies when the government is benevolent) can move to a good equilibrium once a transparency-enhancing device is introduced. The transparency-enhancing device operates as a signal to the representative nonpoor citizen that the government can be trusted to help the poor. In addition to shifting society out of a bad equilibrium, the transparency-enhancing device is also shown to generate a positive effect on support for more efficient, but riskier, redistributive cash-based policies, rather than less efficient but safer in-kind policies.

To study empirically the causal link between transparency-enhancing design and support for redistributive policies, we conducted a behavioral experiment on a nationally representative sample of middle-class adults in Jordan—the Jordan Gives experiment. Specifically, the experiment aimed to identify the effect of enhanced transparency on support for and the preferred design of social safety nets.[2] The experiment involved a sample of 420 participants recruited from 21 randomly drawn middle-class primary sampling units (PSUs) 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. Participants in the control group had to decide whether to give up their fuel voucher in favor of a transfer to people in need. Participants in the treatment group were told, in advance of the same decision, about the opportunity to verify in person whether these transfers would actually reach poor individuals.

Our experimental design simulates at the micro level the decision faced by middle-class citizens on whether to support a shift of public resources from universal benefits (such as fuel subsidies) to targeted programs that benefit the poor, under different designs and degrees of certainty about their delivery to the intended recipients. The experiment thus allows us to provide rigorous evidence on: (a) the propensity for redistribution and relative support for different redistributive methods and (b) the 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 three main sets of results. First, the transparency-enhancing treatment had a statistically significant overall effect on altruism. Second, it caused larger and significant increases in support for redistribution among two groups of participants: low-trust individuals and youth. The first group—people who were suspicious about the capacity of the state to deliver welfare programs to the poor—used the experiment's credible signal that payments would be delivered to targeted beneficiaries to exhibit higher altruism.

[2]Social safety nets, 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 (such as conditional cash transfers) or provided without any conditions (such as unconditional cash transfers) (Grosh et al., 2008; Fiszbein and Schady, 2009).

In the control group without such a signal, low-trust individuals were significantly less likely to give up their voucher in favor of the poor than individuals who trusted that social safety nets would reach the intended beneficiaries. In the treatment group, the giving rate of low-trust individuals matched that of high-trust participants, suggesting that the transparency-enhancing treatment mitigated the effect of their distrust on altruistic behavior. The second group—youth, defined with different age thresholds— when exposed to the treatment experienced the highest increase in the rate of giving. Youth are the age group among whom distrust in MENA countries is more prevalent (OECD, 2016).[3]

Third, 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 as 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 the transparency of delivery increased support for the delivery option—cash—that is generally considered more efficient in reducing poverty relative to in-kind benefits (see, for example, Haushofer and Shapiro, 2016). However, in the absence of a credible signal of trustworthiness from the state, cash transfers are also likely to be perceived as carrying the highest risk of capture, because delivery of in-kind transfers is easier to monitor and conditional transfers often involve an element of self-targeting (Nichols and Zeckhauser, 1982).[4]

Our paper contributes to several strands of the literature. 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 lab-in-the-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

[3]Youth also played a leading role in sparking the protests leading up to the Arab Spring, where the calls for more transparent, fair, and accountable government were a central demand of the protesters.

[4]Such self-targeting may come in the form of fulfilling a time-consuming condition (such as showing up at an unemployment office or completing a training course) or providing supporting evidence for fulfilling eligibility requirements.

investigates altruistic behavior using samples of university students in laboratory settings (for example, 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 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, Joseph and Wodon, 2016; Johansson-Stenman, Mahmud and Martinsson, 2009).

The causal estimates we offer based on a nationally representative sample contribute to informing pressing policy issues in developing countries. Governments in many of these countries 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; Bank, 2015). In 2012, the dollar value of global fuel subsidies was $110 billion, and the associated deadweight loss was $44 billion (, n.d.; IMF, 2013*a*; Davis, 2014). Although the impact of subsidies on government budgets is often very large, sometimes exceeding (and crowding out) public spending on health and education, governments usually find it politically difficult to replace easy-to-monitor commodity subsidies with a targeted income transfer that can deliver better impacts for the poor and vulnerable at a lower fiscal cost.

In general, there are two explanations for opposition to such reform. First, the losers from subsidy reforms are often the middle and upper classes, who have more resources at their disposal to protect their interests than the poor who stand to gain from these reforms. Second, there is usually a lack of trust in the state's capacity to implement effective targeted transfers, that is, to deliver the intended benefits to the intended recipients. This second justification is particularly prominent among potential supporters of redistribution who nonetheless do not trust the existing state capacity to deliver safety nets.

Muralidharan, Niehaus and Sukhtankar (2016) argue that building state capacity for the implementation of welfare programs may expand the state's long-term set of feasible policy choices, including replacing subsidies with targeted income transfers. However, perceptions about state capacity can prevent sensible reforms from being enacted. Specifically, a larger set of policy choices will only be possible if citizens believe in that higher capacity for program implementation. Because positive returns to higher state capacity may accrue only in the long run, citizens' *perceptions* will matter more in the political calculus (Besley and Persson, 2009, 2010). Certain mechanisms, such as technology-enabled solutions, can ensure program accountability and good governance, but would such measures affect support for redistribution?[5] While there are reasons to believe this is the case, this paper is, to our knowledge, the first to provide direct experimental evidence on this topic.

Indeed, governments' ability to manage the political economy of subsidy reforms has been key to the success and sustainability of such reforms.[6] 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, 2017). A similar situation occurred during energy subsidy reform episodes in many other countries (see IMF, 2013*b*). These episodes were usually triggered by youth and soon augmented by more sizable crowds. In several cases, the reforms became the critical catalyst for larger revolutions or regime changes (Gutner, 2002).

In these contexts, gaining support among those who are least likely to trust the state's

[5] To ensure transparent administration of income transfers, several governments have created national unified registries of the poor and vulnerable (for example, Cadastro Ãnico in Brazil, and Ficha de proteccion social/Registro social de hogares in Chile) as a common platform for eligibility and payments for social assistance programs, with several checks of living standards and links to secure payment systems (see Silva, Levin and Morgandi, 2013; Lindert et al., 2007; Ministry of Social Development of Chile, 2016). Similarly, to monitor teachers and nurses, time-stamped photos and other technological solutions have been introduced (Duflo, Hanna and Ryan, 2012; Banerjee, Duflo and Glennerster, 2008).

[6] In most successful subsidy reforms, 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. The survey module on subsidies fielded as part of the Gallup World Poll in the spring 2012 found that most respondents in all four countries preferred that the savings from fuel or diesel subsidy reform 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. 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 of the transfer, the government also decided to set up a National Unified Registry of the poor and vulnerable as a common platform for eligibility for social assistance programs, with several checks of living standards. In recent years, many other developing countries have accompanied subsidy reforms with measures to enhance transparency. These measures include for example, earmarking increased funding for education and infrastructure linked to fiscal savings from subsidy reform, creating a website where each person could compute their score or enter their national ID and verify their (in)eligibility/information; or displaying the list of social assistance beneficiaries in a public place (see IMF, 2013*b*).

capacity to deliver safety net programs may be crucial for political acceptability, and thus for the success of welfare reform. Transparency-enhancing mechanisms such as the one introduced in our experiment are likely to have stronger effects on those who exhibit a higher level of distrust in the state's capacity than on the general population.

The main results of this paper show that by increasing transparency in the delivery of benefits to targeted beneficiaries, the political acceptability of welfare reform can increase, and the effects are particularly large among the key groups whose opposition can most endanger the reform's prospects. We also show that the impact of transparency-enhancing measures on support for redistribution is larger among these groups than the mean impact on the overall population. This is not surprising, as in the overall population those whose decisions are driven only by the implications of redistribution are not expected to be affected by transparency-enhancing measures, which attenuates the average treatment effect. Our conclusions hold even when we allow the treatment effects to vary according to other key variables, such as income and education.

The remainder of the paper proceeds as follows. Section 2 explores preferences for redistribution in four MENA countries (Egypt, Jordan, Lebanon, and Tunisia) using a survey that was fielded by Gallup in 2012. Section 3 presents a simple model to identify the mechanisms underlying the causal impact of transparency on support for redistributive policy. In the model, the transparency-enhancing device acts as a signal to identify the benevolence of the government in charge of implementing the policy. Section 4 describes the experiment, which aimed at testing the predictions of our model. Section 5 presents the empirical model. Section 6 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, how trust and age affected the impact of the treatment, and the relative preferences across the different program designs. Section 7 discusses alternative explanations for the increase in giving because of a transparency shock. Section 8 concludes.

2 The Survey

In this section, we explore survey data from four MENA countries (Egypt, Jordan, Lebanon and Tunisia). We chose to focus on these countries given the global discussion on the Arab Spring and the role of state capacity in those debates. The goal of this section is to explore, using unique representative and comparable surveys, citizens' preferences for redistribution in MENA countries and their trust in the state's capacity to implement redistributive policies. By doing so, we are able to probe which mechanism (preferences versus trust) is at play in the lack of support for redistributive policies.

2.1 Data

The Middle East and North Africa Social Protection Evaluation of Attitudes, Knowledge, and Support (MENA SPEAKS) surveys are four nationally representative and comparable surveys that were conducted as part of the spring 2012 wave of Gallup's World Poll in Egypt, Jordan, Lebanon, and Tunisia. The surveys collected data from 1,000 randomly selected adults in each country on their subjective income, perceptions on existing inequality, the role of the state as the main provider of social safety nets; knowledge of existing social safety net programs, preferences on social safety net design features (cash versus in-kind, categorical versus poverty targeting, conditional versus unconditional transfers, and the acceptability of different types of conditionality), knowledge of existing subsidies, and preferences for subsidy removal and different compensation packages. Descriptive statistics on the samples by country are reported in Table B.1, in Appendix B.

Redistributive policy preferences. Preferences for redistribution are measured by using a hypothetical situation of a policy reform. Respondents were initially asked to name at least one product they think the government should stop subsidizing. They were then presented with the following statement: "Instead of spending money on subsidizing the price of [the good you were most willing for the government to stop subsidizing], the government could spend that money on something else." The respondents were asked to choose among the following four options: (a) distribute that money to the poor, (b) distribute that money to all families except the wealthy, (c) distribute that money to all families including the wealthy; or (d) distribute a portion of that money to the poor and spend the rest on health care and educational programs for all.

The respondents were also asked whether social safety net programs should help the poor or specific groups in need, such as widows, orphans, the sick, and the elderly: "Ideally, do you think that a social assistance project should focus mainly on serving the poor OR mainly on serving specific groups of people, such as widows, orphans, the sick, and the elderly, whether or not they are poor?"

Preferences for policy type. The respondents were also asked about the type of redistribution that would be better: "Do you think it would be better for recipients of government social safety net programs to receive assistance in the form of cash OR to receive assistance in the form of goods, such as food or clothes?".

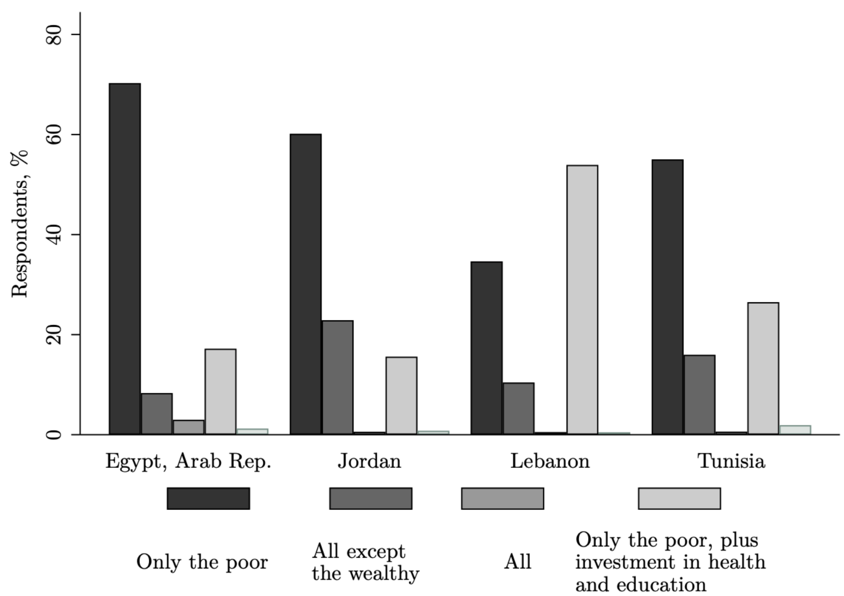
Views on the state's role and capacity. Respondents were asked to state which group or institution is responsible for helping the poor. The options included (a) the government, (b) family and friends, (c) religious organizations, (d) charitable organizations, or (e) no group.

State capacity refers to the government's ability to implement redistributive policies successfully. To measure citizens' perception of state capacity, we use two questions in the MENA SPEAKS questionnaire. First, respondents were asked to state how effective the government is in providing social assistance for the poor (from "very effective"=1 to "not effective at all"=4). Second, respondents were asked whether they were satisfied with the country's efforts the country is putting to deal with the poor (1="satisfied", 2="dissatisfied", 3="don't know").

2.2 Preferences for Redistribution

Figure 1 summarizes the redistributive policy preferences of the citizens surveyed in the four countries. The results show strong support for targeted redistribution to the poor in Egypt, Jordan and Tunisia. In Egypt, reallocating the money only to the poor was chosen by around 70 percent of the respondents, while in Jordan and Tunisia, approximately 60 percent of the respondents chose this option. In Lebanon, although a non-negligible proportion of the respondents chose to reallocate the money only to the poor, the majority of the citizens surveyed also wanted more investment in public goods such as health and education. Although the citizens could opt for redistributing the money to all groups in society or to all groups except the rich, the fact that the majority chose to help specifically those at the bottom of the income distribution points toward strong redistributive preferences.

FIGURE 1: PREFERRED DESTINATION OF SAVINGS FROM SUBSIDY REFORM, BY COUNTRY



Citizens' preferences for redistribution were also showcased when the respondents were asked whether social safety net programs should help the poor or specific groups in need, such as widows, orphans, the sick and the elderly. The results show that an important majority of the respondents across the four countries sampled answered that social safety net programs should help the poor, and not specific groups. This number ranges from around 80 percent support for helping the poor in Lebanon to more than 90 percent in Egypt (see Figure 2). In addition, the survey results show overwhelming support for in-cash (in contrast to in-kind) transfers to the poorest across all the countries surveyed (see Figure 3). Support for in-cash benefits ranges from 68 percent in Lebanon to more than 85 percent in Jordan.

FIGURE 2: PREFERENCE FOR SOCIAL ASSISTANCE TARGETS, BY COUNTRY

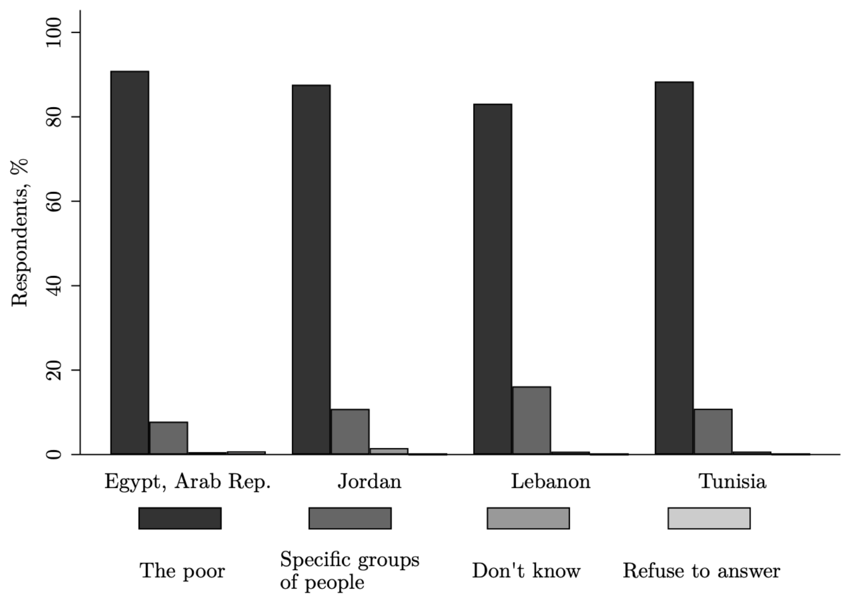
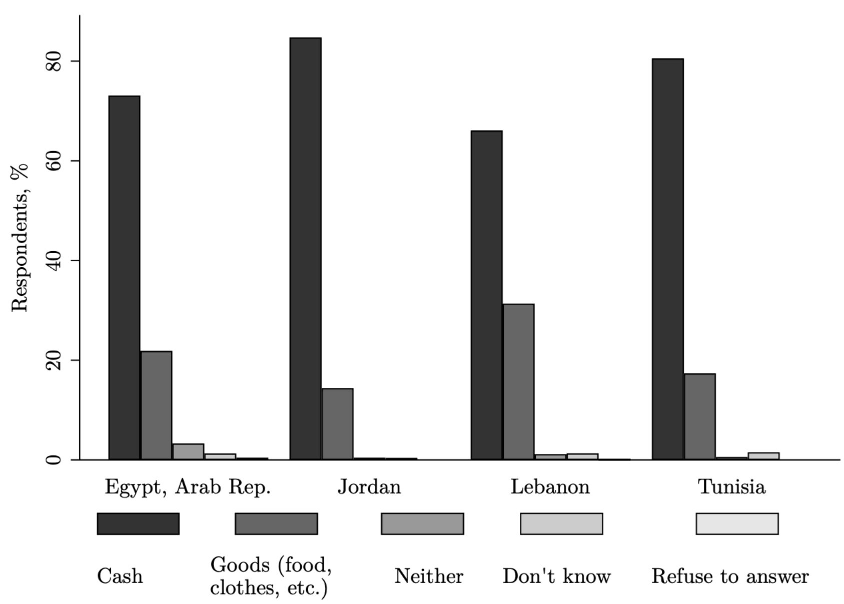


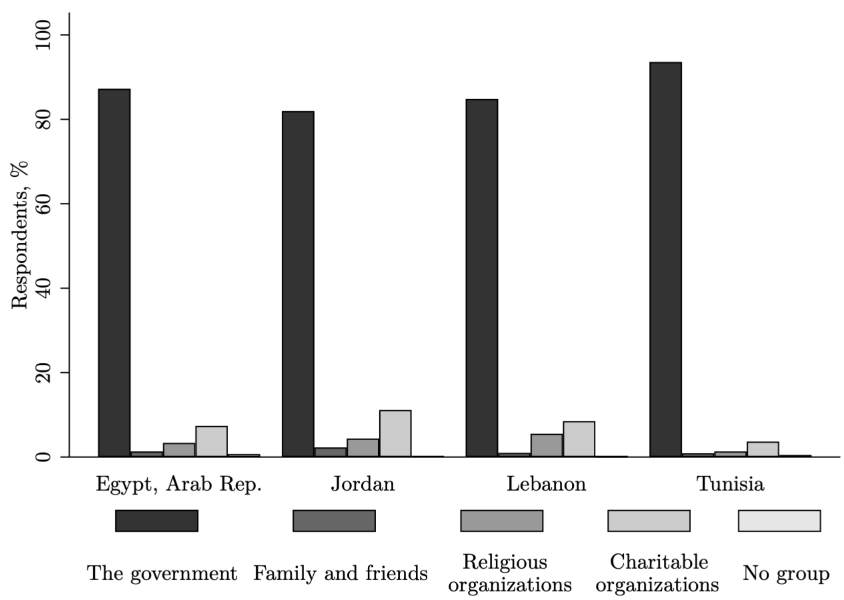
FIGURE 3: PREFERRED SOCIAL ASSISTANCE POLICY, BY COUNTRY



2.3 Beliefs about the State's Role and Effectiveness

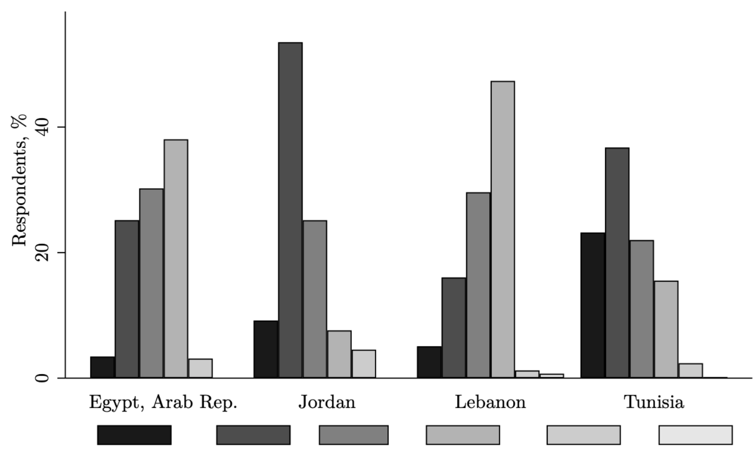
Lack of support for redistributive public policies could be due to a belief that the government should not be the actor implementing such redistribution. Figure 4 illustrates citizens' opinions on the entity that should be responsible for helping the poor. The results show that an overwhelming majority of the respondents believe that the government should be the main actor responsible for helping the poor. This number ranges from 79 percent of the respondents in Jordan to more than 90 percent in Tunisia.

FIGURE 4: PREFERENCE FOR THE MOST RESPONSIBLE GROUP FOR HELPING THE POOR, BY COUNTRY

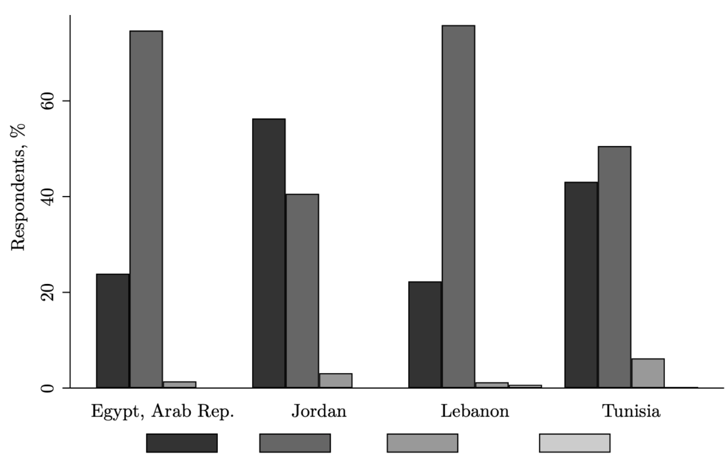


Finally, even if citizens support redistribution in principle, and consider the state to be primarily in charge of supporting the poor, belief in the state's capacity to implement redistributive programs may ultimately affect citizens' support for such policies. As illustrated in Figure 5, panel a, citizens' opinions suggest that there is significant distrust in the state's ability to provide social safety net programs to the poorest effectively. In two of the countries surveyed, namely Egypt and Lebanon, we observe that only a small percentage of the respondents believe that the government is very effective or somewhat effective in providing social safety nets. In Egypt, that share is around 31 percent while in Lebanon, it is around 21 percent. From another perspective, this implies that a significant number of the respondents in MENA countries question the government's ability to provide social safety nets to those in need. Although higher proportions of the respondents view the government as being very or somewhat effective in Tunisia and Jordan (60 and 63 percent, respectively), an important share of the citizens still question the government's ability to provide social safety net programs effectively. A significant share of the population also seems to be dissatisfied with the way their government deals with the poor. In Egypt and Lebanon, about three-quarters of the citizens reported they are dissatisfied, while in Tunisia and Jordan, it is 51 and 38 percent, respectively (see Figure 5, panel b).

FIGURE 5: BELIEFS ABOUT THE GOVERNMENT EFFECTIVENESS



(a) Effectiveness of the government



(b) Efforts to deal with the poor

3 The Model

How would a transparency-enhancing device operate in convincing citizens to support redistributive policies? In this section, we present a simple model of the interplay between distrust and support for redistributive policies and we explore the impact of a transparency-enhancing device. The model consists of a principal-agent framework where the principal chooses a redistributive rule and the agent implements it. For the purpose of this paper, we assume the principal to be a representative nonpoor citizen who has a preference for redistributing their income while the government is the agent implementing it. Following the literature (see Nannicini et al., 2013; Ferraz and Finan, 2011; Dincer, Ellis and Waddell, 2010), the government is represented by a single decision maker.

The model assumes a society with two risk-neutral representative citizens with two income levels: the nonpoor citizen with income x_{np} and the poor citizen with income x_p with $x_p < x_{np}$. Both citizens are averse to inequity such that there is a demand for redistribution.[7] What people consider as inequitable may depend on many contextual factors. Evidence shows that inequity aversion can be reasonably approximated by inequality aversion as formalized by Fehr and Schmidt (1999) (Tyran and Sausgruber, 2006). We hence set the citizens' respective utilities as $U_{np} = x_{np} - \beta_{np}(x_{np} - x_p)$ and $U_p = x_p - \alpha_p(x_{np} - x_p)$ where $\beta_{np} < 1$ is the disutility of earning more than others for the nonpoor citizen while α_p is the disutility of earning less than the others for the poor citizen.

The starting point of the model is a government setting up a redistributive policy that consists of a transfer t from the nonpoor to the poor, subsequently reducing income inequality. To implement the redistributive policy, a politician is randomly drawn from a pool of potential individuals. Each politician is characterized by a level of civility: a politician is either benevolent ($b = 0$) or nonbenevolent ($b = 1$), with a proportion θ of benevolent politicians within the government. If the politician is benevolent, they are in line with the government's objective and care about the social welfare (the sum of citizens' utility U_i, with $i = np, p$). If the politician is nonbenevolent, they care about the amount

[7] Preferences for redistribution may root in several factors, including perceptions on relative merits (Alesina, Glaeser and Glaeser, 2004), indoctrination (Alesina and Fuchs-Schündeln, 2007), social norms (Le Garrec, 2018) or, ethnic diversity (Dahlberg, Edmark and Lundqvist, 2012; Luttmer, 2001). In this model, we assume that the inequity aversion parameters is the result of all mechanisms identified in the literature.

of money they can capture for their own interest (elite capture). In other words, even a nonbenevolent politician does not want to set state's capacity to zero.

This redistributive policy consists of an unconditional cash transfer ("cash policy") or an unconditional in-kind benefit ("in-kind policy"). For the cash policy, the non-benevolent politician can capture the transfer t entirely, while for the in-kind policy, they cannot.[8] In other words, with the in-kind policy, the transfer will surely reach the poor citizen. It is however less efficient than the cash policy as suggested in the literature (Currie and Gahvari, 2008): the poor citizen only receives st where $s < 1$ is the level of efficiency.

The nonpoor citizen decides to accept either one of the redistributive policies (cash or in-kind) or to reject both. Rejection can take the form of voting punishment[9], tax evasion or public protest.

In our framework, what the poor will actually receive, denoted $r(t)$, depends on the nonpoor citizen's decision, the transfer mode (cash or in-kind), and the benevolence of the politician in charge of the public policy:

$$r(t) = \begin{cases} (1-b)t & \text{if the nonpoor citizen accepts the cash policy} \\ st & \text{if the nonpoor citizen accepts the in-kind policy} \\ 0 & \text{if the nonpoor citizen rejects any policy} \end{cases}$$

3.1 The Citizens' Distrust Trap

By transferring money to the government, the nonpoor citizen can benefit from redistribution. Yet, with the cash policy, there is a risk that the politician is nonbenevolent ($b = 1$) and the transfer does not reach the intended beneficiaries. With the in-kind policy, even the nonbenevolent politician cannot embezzle the transfer, but the transfer is subject to inefficiency.

As shown in Section 2, citizens in MENA countries are averse to inequity and would prefer their government to implement redistributive policy. We thus consider here the

[8] For simplicity, we assume that the politician is not interested in the in-kind transfer. The results hold as long as in-kind transfers limit the share of the transfer the politician can capture.

[9] In a study using (random) audit reports on municipal governments in Brazil, Ferraz and Finan (2011) show, for instance, that corruption disclosure is punished by voters in terms of decreased reelection probability.

case where $\beta_{np} > \frac{1}{2}$, meaning the nonpoor citizen cares about redistribution. A nonpoor citizen will prefer to support the in-kind policy over no policy if $\beta_{np} > \frac{1}{1+s}$ (mathematical proofs are provided in Appendix C).

Proposition 1. *Under full information about the politician's type, the cash policy is always preferred to the in-kind policy when the official in charge of implementing the policy is benevolent while preferences are reversed when the official is non-benevolent.*

Under asymmetric information, we assume that the nonpoor citizen has a belief $\hat{\theta}$ about the probability that the politician is benevolent. This belief can be accurate ($\hat{\theta} = \theta$) or not. The nonpoor citizen will reject the cash policy if $\beta_{np} < \frac{1}{1+\hat{\theta}}$. In other words, even though a nonpoor citizen might be averse to inequality, they may not accept the cash policy if their belief that the official is benevolent is too low. Finally, the cash policy is preferred to the in-kind one if the risk of money capture is lower than the inefficiency of the in-kind transfer, that isif $(1 - \hat{\theta}) < (1 - s)$.

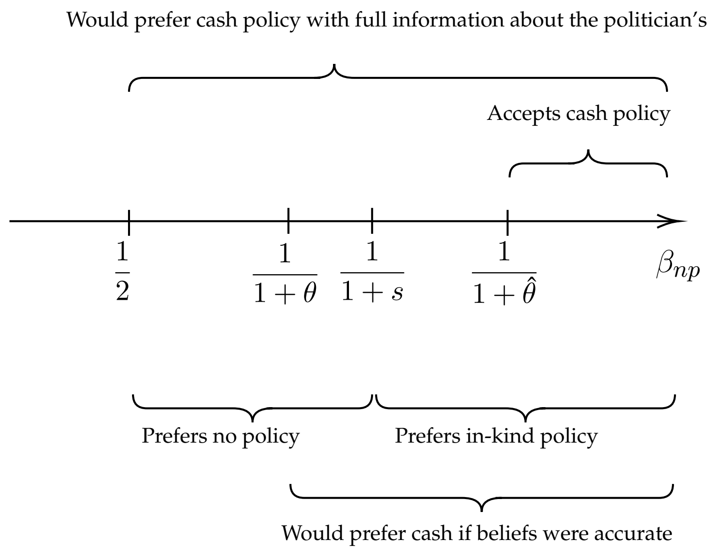
Proposition 2. *A nonpoor citizen will prefer to support an in-kind redistributive policy over a cash policy if the perceived risk of money capture is larger than the inefficiency of the in-kind policy, that is, if* $(1 - \hat{\theta}) < (1 - s)$.

As a consequence of asymmetric information, the nonpoor citizen may opt for in-kind or no redistributive policy even when the politician is benevolent, resulting in a welfare loss. In addition, if beliefs about the government's composition are inaccurate (specifically if $\hat{\theta} < \theta$), there exist equilibria where the nonpoor citizen will support the in-kind policy, or worst no policy at all, while they would have supported the cash policy if their beliefs had been accurate. In other words, misconceptions about the proportion of nonbenevolent politicians also lead to a welfare loss.

Proposition 3. *(Negative) misconception about the government composition ($\hat{\theta} < \theta$) may lead the nonpoor citizen to prefer the in-kind policy or no policy instead of the cash policy.*

Figure 6 summarizes the nonpoor citizen's decision on the policy they wish to support under the specific case where $\hat{\theta} < s < \theta$. This situation illustrates the risk of a distrust trap: even when a government intends to operate a redistributive policy properly, if citizens do not believe in the relative benevolence of the politicians composing the government, they would most likely not support it. However, one way to overcome the problem of distrust would be for the government to provide a credible signal of its benevolence.

FIGURE 6: NONPOOR CITIZEN SUPPORT FOR REDISTRIBUTIVE POLICIES



3.2 Transparency as a Signal

The nonpoor citizen is willing to support cash redistribution if they are reassured that it will go to the intended beneficiaries. As already mentioned, this type of reassurance could be given by a new technology. In the regulatory realm, regulations can have a similar effect, as they help to allow citizens to control some aspects of what governments do. Aghion et al. (2010) show that when people do not have trust (including in the government), they have a higher demand for regulation.

We consider now that the politician can invest in a transparency-enhancing device. It consists of a mechanism that increases the probability of an audit of the policy implementation. The conduct of the audit has a fixed cost, c_0 and, if the audit detects nonbenevolence, politicians are subject to a loss (for example, a fine). The expected loss, F, occurs only for nonbenevolent politicians and may consist, for instance, of an actual legislative fine or the costs of public disapproval. The politician can decide to invest (T) or not (NT) in this device. Investing in the transparency-enhancing device is associated with a cost $c(T)$ such that:

$$c(T) = \begin{cases} c_0 & \text{if } b = 0 \\ c_0 + F & \text{if } b = 1 \end{cases}$$

with $F > 0$ such that it is more costly for a nonbenevolent politician to invest in transparency. Can the transparency-device move society from a bad equilibrium to a good equilibrium?

Proposition 4. *The good separating equilibrium, where a government represented by a benevolent politician invests in the transparency-enhancing device while a government represented by an non-benevolent politicians does not, arises if the fixed cost of the transparency-enhancing device is not too high ($c_0 < (1 + \alpha_p + \beta_{np})(1 - s)t$ when $\beta_{np} > \frac{1}{1+s}$ and $c_0 < 2(\alpha_p + \beta_{np})t$ when $\beta_{np} < \frac{1}{1+s}$) and the expected fine for the non-benevolent official is high enough ($F > t - c_0$).*

Proposition 4 states that a separating equilibrium can arise. In addition, this equilibrium is more likely to arise, other conditions remaining the same, in countries where inequity aversion is higher or in countries where punishment of nonbenevolent politicians is higher. In addition to convincing reluctant citizens, the transparency-enhancing device may improve future adherence to public policies in general. Indeed, by learning about the type of the official for a given policy, citizens can update their beliefs $\hat{\theta}$ for future policies.

4 Research Design and Data

This section describes the Jordan Gives experiment, which was designed to test the predictions of the model.

4.1 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 Jordanian middle-class adults. 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 a fuel subsidy and social safety net reform. Such a reform would imply a redistribution of public funds away from the wealthy and the middle class, as both groups captured most of the 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.[10] The Jordanian middle class was the group that was likely to lose the most in relative terms from shifts of resources away from universal subsidies and toward targeted social safety nets, and the middle class could assemble a sufficiently large interest group to thwart the reform.

Participants in the experiment were identified through a three-stage process: (a) 21 PSUs were randomly drawn from a sampling frame of middle-class enumeration areas in Jordan based on the 2004 Census;[11] (b) within each PSU, households were selected using a random walk method; and (c) adults were recruited (one per household) for 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 could be constituted in each PSU (10 for treatment, 10 for control) at the same time and place (see Appendix D for more details).

At the recruitment stage, an invitation letter explained that all participants who appeared 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 there would be a chance to receive JD 10 more in such vouchers, depending on the outcome of the meeting. These vouchers were issued by the Jordan Petroleum Refinery Company. They were widely known in Jordan and could be exchanged for gasoline at 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 given two receipts (one for the JD 5 voucher and the other for the JD 10 voucher), which they were

[10] 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.

[11] 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 (Tabbaa, 2008). Appendix D provides a detailed explanation of the sample design and selection protocols.

encouraged to bring to the meeting to exchange for the vouchers.[12]

4.2 The Redistributive Proposals

The experiment was conducted on groups of 20 participants in each randomly selected PSU. Upon arrival at the location of the experiment, participants were randomly allocated to 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 the recruitment stage: a JD 5 voucher as a show-up fee and a JD 10 voucher to use in the experiment.[13]

The experiment asked participants to make a series of decisions concerning whether to keep their JD 10 fuel voucher or give it up in exchange for different scenarios ("proposals") of assistance to the poor. At the start of the experiment, each participant received a decision card for recording their decisions on each proposal; the card only included proposal numbers and did not describe the proposals themselves, which were revealed, one at a time, during the experiment. The exact wording of the proposals, intended to mimic the design of different 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."

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

[12] 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 for the fuel vouchers by creating a sense of ownership using receipts with specified voucher values at the time of recruitment (Kahneman, Knetsch and Thaler, 1991).

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

family member completing a free training program on work-related skills."[14]

The proposals were revenue-neutral, since the amount to be disbursed in the proposals was equivalent to the total value of all the fuel vouchers in the room (that is 10 participants' JD 10 vouchers, a total of JD 100). To avoid any systematic anchoring effect due to particular proposal order, the order in which the proposals were presented was randomized at the PSU level.

After the first proposal was presented, participants were asked to mark on their individual decision cards whether they "accept" the proposal (indicating a preference to see the proposal implemented in lieu of keeping their JD 10 voucher) or "reject" it (preference to keep the JD 10 voucher). Participants were asked to write down their decision on the specific proposal before being presented with the next proposal. After all four proposals were presented and all four decisions were marked on the decision cards, the participants were asked to rank the four proposals in their order of preference. At the end, all the decision cards were collected by the facilitator, who placed them in a clear glass bowl. No names were included on the decision cards and participants were asked to mark their decision cards in silence and confidentially to prevent peer pressure. The subjects were assured when making their decisions that they would not be revealed to the group during or after the experiment.

A second clear glass bowl contained the numbers 1 through 4, corresponding to the proposal numbers. After all the cards were submitted, the facilitators drew one deci-

[14] 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, 2014; Attanasio, Battistin and Mesnard, 2012; Hoddinott, Sandström and Upton, 2018), but they are more efficient when markets function well (that is, not plagued by hyperinflation, conflicts, or supply constraints) (Busso and Galiani, 2019). Several recent papers have discussed the marginal impact of attaching conditions to cash transfer programs. Although their administration is costlier relative to unconditional transfers, conditional cash transfers intend to address market failures that lead to underinvestment in education or health by imposing certain behaviors on recipient households (Hanlon, Barrientos and Hulme, 2012). Recent studies have found that such schemes generally improve the conditioned-on outcome, but they 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 Özler, 2011; Attanasio, Oppedisano and Vera-Hernández, 2015; Blattman, Fiala and Martinez, 2014; Benhassine et al., 2015). On the other hand, transfers conditional on educational outcomes usually also provide a valuable mechanism to improve parents' monitoring of their 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).

sion card from the first bowl and one number from the second bowl. The decision made for the selected proposal number on the selected decision card was implemented on the whole group. If the selected decision was "accept," then the JD 10 voucher was collected from each participant and the selected proposal would later be implemented in the local community.[15] If the selected decision was "reject," all the participants would keep their vouchers. This decision selection process was chosen to ensure that participants had a clear incentive to consider each proposal independently of what they had decided for preceding proposals.[16] The experiment was followed by a Becker, DeGroot, and Marschak (BDM) 1964 auction and collection of the basic demographic, socio-economic, and attitudinal characteristics of the participants (via a written questionnaire).[17] For better understanding the reasoning of the study, debriefing focus group discussions were also conducted.[18] They showed that the proposals, decision mechanisms, and consequences of the decisions in the context of the experiment were well understood by the participants.

[15]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 for poor families that could be the recipients of these benefits was provided by the local community leader.

[16]To 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 payoff, and that participants understood that they should consider each proposal independently of the next one, the experiment was preceded 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.

[17]The BDM auction is a mechanism commonly used in the literature to induce individuals to reveal their willingness to pay for a given good (Noussair, Robin and Ruffieux, 2004). 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 the 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 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 their voucher. If it was equal or lower, they 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 the respondents had a car in their households, and those who did not could also have had motorcycles or readily exchanged the voucher.

[18]Participants did not know about the focus group discussions until after they completed the experiment. At the beginning of the focus group discussion, participants were told that they did not need to discuss their personal decisions in the experiment.

4.3 Audiovisual implementation of the experiment

Results of behavioral experiments can be affected by the heterogeneity of implementation, which can introduce noise in the estimation of treatment effects. Such noise can arise due to accidental priming to values or anchoring to certain numbers (Brewer and Chapman, 2002; Furnham and Boo, 2011). Moreover, noise from facilitator characteristics or quality of delivery can complicate estimation of the impact of the treatment.

To ensure that the messages conveyed to participants were homogeneous across sessions, 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 proposals and illustrated graphically the proposal selection mechanics.[19] Participants were presented four proposals one at a time. The order in which the proposals were presented was randomized at the PSU level by producing multiple versions of the same video. The facilitator's role was to distribute and collect the decision cards and questionnaires, answer questions according to a pre-developed answer script, implement the randomly-drawn decision in the group and lead the focus group discussion that followed the experiment.

4.4 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, the experiment was implemented only once, obtaining a total sample of 420 individuals in 21 PSUs: 210 individuals in the control group and 210 individuals in the treatment group.

The video for the treatment group contained all the features of the video for the control group, but it included additional information that would make the delivery and content of

[19] During the piloting stage, the use of pictures emerged as important to improve participants' understanding of the experiment.

the transfer to the poor 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 the 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.[20]

In addition, participants in the treatment group 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 on measures that could raise the political feasibility of a fuel subsidy reform.

4.5 Data

The data used in this paper were collected between late May and June 2012. The quantitative data from Jordan Gives include decisions made by each individual participant during the experiment, their valuation of fuel vouchers obtained via a post-experiment BDM auction, as well as basic demographic, socioeconomic, and attitudinal information collected from each participant via a short written survey administered after the experiment. The estimation sample includes a total of 402 participants who provided information on all the variables used in the estimation.

Finally, we collected a rich qualitative dataset from structured in-depth focus groups that were conducted by facilitators after all the quantitative data were collected to ensure that the mechanics of the experiment had been well understood by the participants.

[20]Indeed, in some cases, participants in the treatment group decided 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.

5 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 an outcome variable (the mean giving rate using information from all the proposals or the binary decision to accept or reject 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.[21]

We estimate the results of equation (1) for five outcomes: one 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 (of a possible four). The other four outcomes are binary indicator variables equal to one if individual *i* indicated that they would give up their voucher for that specific proposal (unconditional cash transfer, unconditional food transfer, unconditional

[21] 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 account for intra-cluster correlation, which could be relatively high for outcomes related to redistribution preferences. Given that Huber-White heteroskedasticity-consistent 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 the 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.

cash transfer and school, or cash transfer conditional on training).[22]

For each outcome variable, we estimate two specifications of equation (1). In the "unadjusted" regressions, we do not include any control variables. In the "adjusted" regressions, we include the collected controls to improve the precision of the treatment effect. These variables were included in the questionnaire because they are commonly considered to be correlated with mean giving in the altruism literature (Eckel and Grossman, 1998; Andreoni and Vesterlund, 2001; Yen, 2002; List, 2004; Rooney et al., 2005; Andreoni, 2006; Andreoni and Payne, 2013). They include personal characteristics, such as gender, education level, employment status, number of cars in the household (a proxy for household wealth), household size, residence in the capital city, and whether the participant gave to charity in the past three months.[23] In the results section that follows, we focus, in particular, on the estimation of equation (1) that includes interaction terms between the treatment and the two key groups for whom the transparency-enhancing measures tested in the experiment were expected to have the most significant effects: (a) those who have low trust in the state's capacity to deliver effective targeted transfers and (b) youth.

6 Main Results

6.1 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 the observable characteristics of the participants in the treatment group were similar to those in the control group. The standard errors of the mean difference between the 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).

[22] Table B.2 in Appendix B presents the summary statistics.

[23] The results are not sensitive to the set of controls included. Estimates using alternative sub-sets of controls are available upon request.

TABLE 1: SAMPLE BALANCE STATISTICS

	Mean [s.d.]			Difference	Number
	Control	Treatment	Full	(C-T)	of obs.
	(C)	(T)	sample	[p-value]	
	(1)	(2)	(3)	(4)	(5)
Panel A: Individual characteristics					
Male (%)	0.42	0.47	0.45	0.28	402
	[0.494]	[0.500]	[0.497]		
Primary education (%)	0.05	0.06	0.06	0.78	402
	[0.227]	[0.238]	[0.232]		
Secondary education (%)	0.61	0.56	0.58	0.12	402
	[0.489]	[0.497]	[0.493]		
Tertiary education (%)	0.34	0.38	0.36	0.26	402
	[0.473]	[0.486]	[0.480]		
Young (ages 18-29) (%)	0.31	0.25	0.28	0.12	402
	[0.464]	[0.434]	[0.450]		
Young (ages 18-34) (%)	0.42	0.35	0.38	0.15	402
	[0.494]	[0.478]	[0.486]		
Currently employed (%)	0.35	0.33	0.34	0.66	402
	[0.477]	[0.471]	[0.473]		
Panel B: Household characteristics					
Number of cars in the household	0.70	0.67	0.69	0.73	402
	[0.684]	[0.808]	[0.748]		
Household size	6.07	6.08	6.08	0.93	402
	[2.239]	[2.449]	[2.343]		
	[0.184]	[0.122]	[0.156]		
Residence in the capital city	0.72	0.72	0.72	0.88	402
	[0.451]	[0.450]	[0.450]		
Panel C: Giving behavior					
% that gave to charity in the past three months	0.64	0.57	0.61	0.19	402
	[0.480]	[0.495]	[0.488]		

Note: 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 (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 primary sampling unit level). Column 5 shows the number of observations used.

6.2 Effect of the Treatment

The transparency-enhancing intervention is aimed at increasing giving rates (the rates at which participants decide to accept proposals in lieu of their JD 10 voucher). We focus on the average treatment effects on mean giving as well as on giving rates for each individual proposal. We analyze average treatment effects for the overall population and among two subsamples: low- versus high-trust participants, and youth versus older adults.

Average treatment effect. Table 2 describes the effect of the transparency-enhancing treatment on the probability of giving (choosing to accept a proposal and thus give up the fuel voucher), obtained via a treatment-effect regression. The constant term in these regressions represents the control group mean, while the coefficient on the indicator variable for individuals' assignment to treatment groups represents the impact of the treatment on the giving rates, or the average treatment effect (reported in column 3) for both the aggregate (mean giving rate) and each of the binary decisions (the four proposals). Column 4 reports the p-value of the average treatment effect when controls are not included ("unadjusted" regressions), and column 5 reports the p-value when controls are included ("adjusted" regressions). Column 6 shows the full sample averages of the giving rates.

The results indicate that mean giving (at the participant level) was 67 percent.[24] Across proposals, the unconditional food transfer proposal attained the highest acceptance rate (71 percent), closely followed by the unconditional cash transfer proposal (69 percent). Given the monetary value that the voucher represented for the subjects, the mean giving rate was remarkable, compared with the giving rates found in other experiments, which ranged between 20 and 37 percent (List and Price, 2009; DellaVigna, List and Malmendier, 2012; Parra, Joseph and Wodon, 2016). However, the design of the present experiment was unique in the literature.[25] Fundraising experiments do not provide participants any token,

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

[25] In contrast to Jordan Gives, classic dictator games allow the principal to determine the share of the received amount to distribute in a single-shot game. Parra, Joseph and Wodon (2016) 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

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)	(7)
Panel A: Aggregate/all proposals							
Mean giving	0.63	0.71	0.08	0.08	0.08	0.67	402
	[0.367]	[0.396]				[0.383]	
Panel B: Individual proposals							
Unconditional cash transfer	0.64	0.75	0.11	0.04	0.04	0.69	402
	[0.436]	[0.481]				[0.462]	
Unconditional food transfer	0.68	0.73	0.05	0.24	0.25	0.71	402
	[0.442]	[0.466]				[0.454]	
Unconditional cash transfer and school	0.59	0.69	0.10	0.05	0.06	0.64	402
	[0.465]	[0.493]				[0.481]	
Cash transfer conditional on training	0.63	0.69	0.06	0.30	0.36	0.66	402
	[0.465]	[0.484]				[0.475]	
Controls included				No	Yes		

Note: 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. Mean giving is computed at the participant level and is the share of total proposals in which the participant indicated they would give up their 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. Column 4 reports the p-value of the t-test of the difference between the two samples in the unadjusted regression (that does not include any control variables). Similarly, column 5 also reports the p-value of the t-test of the difference between the two samples in the adjusted regression which controls for gender, three education levels, employment status, number of cars in the household, number of people living in the household, residence in the capital city and if the person donated to charity in the past three months. In columns 4 and 5, standard errors are clustered at the primary sampling unit level using wild bootstrap-t. Column 6 reports the mean and standard deviation (in square brackets) of each variable for the full sample. Column 7 shows the number of observations used.

while Jordan Gives provided a voucher as the token but also took measures to enhance the endowment effect, by explicitly framing the 10 JD voucher at the time of participants' recruitment as potentially theirs to keep.[26] Another potentially important distinctive feature of the experiment was the identification of direct recipients of the transfer as local poor families rather than more abstract notions of giving to charities.[27]

Columns 1 to 6 in Table 2 show the results from estimating equation (1). The point estimates of giving rates suggest that the transparency-enhancing treatment increases support for redistribution by around eight percentage points. The average treatment effects reach statistical significance at conventional levels for mean giving and the two proposals that contain unconditional cash transfers (i.e. unconditional cash transfer and cash transfer with school financing), suggesting that transparency may be most important for cash-based redistributive programs, as cash transfers may be more prone to elite capture.[28]

Effect among low-and high-trust participants. Among the overall population, individuals with low trust in state capacity to deliver welfare programs are the ones for whom the transparency-enhancing measures tested in the experiment were expected to have a larger impact. Indeed, we find that the treatment had (larger and) significant impacts on low-trust individuals. As shown in Table 3, the level of trust in the delivery of social safety nets, as measured by a post-experiment attitudinal question, appears to mediate the effect of the treatment, particularly for the unconditional cash transfer proposal. The results are obtained by estimating equation (1) separately for individuals who reported being

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 that 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.

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

[27]Interestingly, the proposal in which individuals could give up their voucher both to help the poor and to finance a public good (the local school) proved to be the least popular proposal among the 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.

[28]In Table B.3 in Appendix B, we follow Imbens and Rubin (2015) and Lin and Green (2016) to check the robustness of our results to multiple hypothesis testing. For this purpose, we include the covariates (the control variables used in Tables 2) and their interactions with the treatment indicator in this fully-adjusted specification. The results confirm the robustness of the effect of the treatment.

completely or somewhat 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").[29] The treatment effect is always higher among low-trust individuals (except for the cash transfer conditional on training) and is statistically significant for two outcomes (columns 4 and 5): mean giving and unconditional cash transfer, with the treatment effect for unconditional food transfer among low-trust participants reaching statistical significance as well.

[29] The exact question measuring trust in state capacity 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.

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)	ATE	Difference (C-T) [p-value]		Control (C)	Treatment (T)	ATE	Difference (C-T) [p-value]		(C in LT - C in HT)	(T in LT - T in HT)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Aggregate/all proposals												
Mean giving	0.59	0.68	0.09	0.09	0.06	0.69	0.74	0.05	0.32	0.44	0.08	0.34
	[0.404]	[0.366]				[0.382]	[0.367]					
Panel B: Individual proposals												
Unconditional cash transfer	0.56	0.72	0.16	0.04	0.02	0.73	0.76	0.03	0.49	0.42	0.01	0.44
	[0.498]	[0.453]				[0.445]	[0.425]					
Unconditional food transfer	0.66	0.74	0.08	0.11	0.12	0.71	0.73	0.02	0.72	0.66	0.46	0.88
	[0.475]	[0.440]				[0.456]	[0.445]					
Unconditional cash transfer and school	0.52	0.61	0.09	0.22	0.20	0.67	0.73	0.06	0.26	0.23	0.04	0.09
	[0.501]	[0.489]				[0.473]	[0.445]					
Cash transfer conditional on training	0.61	0.65	0.04	0.57	0.40	0.65	0.71	0.06	0.33	0.48	0.66	0.44
	[0.488]	[0.478]				[0.481]	[0.457]					
Controls included	No	No		No	Yes	No	No		No	Yes		

Note: 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 average treatment effect among low-trust individuals. Column 4 reports the p-value of the t-test of the difference between the two samples of the unadjusted regression (that does not include any control variables) among low-trust individuals. Columns 6 and 7 report the mean and standard deviation (in square brackets) of each variable for the control and treatment groups among high-trust individuals. Column 8 reports the average treatment effect among high trust individuals. Column 9 reports the p-value of the t-test of the difference between the two samples of the unadjusted regression among high trust individuals. Column 11 reports the p-value of the t-test of the difference between giving among low- and high-trust participants in the control group. Column 12 reports the p-value of t-tests of the difference between giving among low- and high-trust participants in the treatment group. Columns 5 and 10 report the p-value of the t-test of the difference between the two samples of the adjusted regression controlling for gender, three education levels, employment status, number of cars in the household, number of people living in the household, residence in the capital city and if the person donated to charity in the past three months. In columns 4, 5, 9, and 10, standard errors are at the primary sampling unit level using wild bootstrap-t. There are 190 observations in the low-trust group and 212 in the high-trust group for a total of 402 participants.

The results point to two other important findings. First, comparison of columns 1 and 6 reveals that, among individuals in the control group, the mean rate of giving was 10 percentage points higher for high-trust individuals than for low-trust individuals, which is a statistically significant difference (see column 11 in Table 3). It is also striking that this difference was driven essentially by the unconditional cash transfer proposal: in the control group, high-trust individuals were 17 percentage points more likely than low-trust individuals to give up their vouchers for this proposal. Instead, in the treatment group, we observe smaller and statistically insignificant differences in mean giving rate between high-trust and low-trust participants. This occurs because giving rates among low-trust individuals are higher in the treatment group than in the control group. For instance, in the case of an unconditional cash transfer, 56 percent of the low-trust participants gave up their vouchers in the control group, while 72 percent did so in the treatment group (implying a 16 percentage point treatment effect), on the other hand, the treatment effect for high-trust participants was minimal (less than 3 percentage points). One exception was the proposal to give cash to the poor conditional on training; in this case, the transparency treatment enhanced the overall giving rate for all participants, but it did not reduce the gap in giving between low- and high-trust individuals.

The larger effect of the transparency-enhancing treatment on low-trust participants is confirmed in Table 4. Columns 1 and 2 show that the interaction of trust and treatment has a significant negative coefficient for mean giving rates, unconditional cash and unconditional food transfers. The result show that providing a signal that the redistributive transfer will reach the intended beneficiaries is most effective on "low-trust" individuals, who doubted the capacity of the state to implement redistributive schemes. Notably, the treatment had less of an impact on preference for redistributive designs that include benefits for the wider community (funding for a school) or conditionalities, suggesting that such design elements can also alleviate concerns with state capacity (even though they may be more complex to implement or less efficient).

Effect among youth and adult participants. A mechanism that demonstrates effective implementation of targeted income transfers is also likely to have a more significant impact among youth compared with other age groups, as youth in MENA exhibit higher distrust

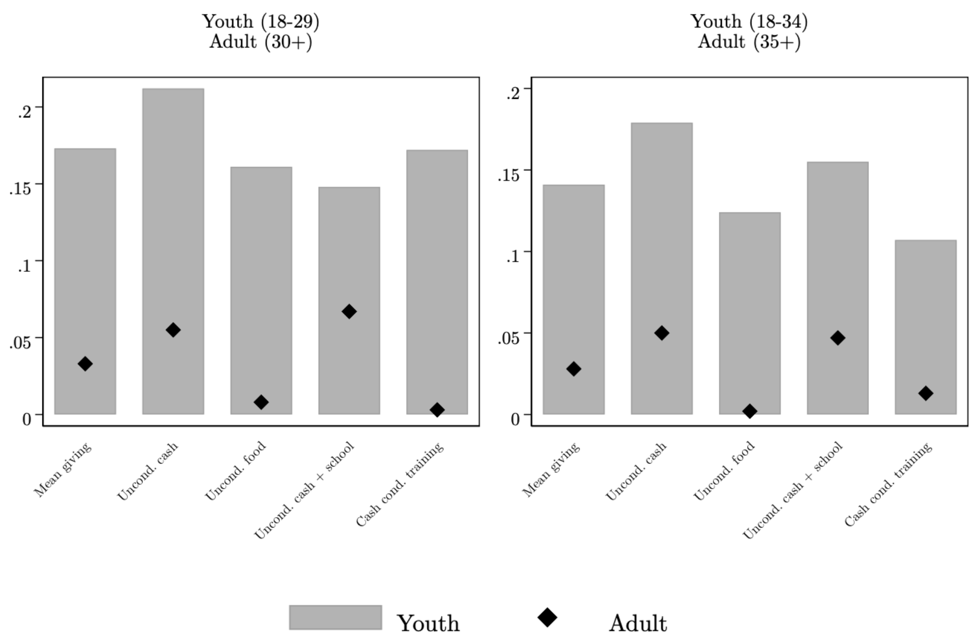
TABLE 4: EFFECT OF THE INTERACTION BETWEEN TREATMENT AND TRUST AND BETWEEN TREATMENT AND AGE

		(1)	(2)	(3)	(4)
(A) Aggregate/all proposals					
Mean giving	Treatment	0.09*	0.09*	0.26**	0.25**
		[0.054]	[0.053]	[0.104]	[0.104]
	Treatment*Trust	-0.05***	-0.05**		
		[0.017]	[0.019]		
	Trust	0.10***	0.10***		
		[0.032]	[0.035]		
	Age			0.01***	0.01***
				[0.000]	[0.000]
	Treatment*Age			-0.01***	-0.01***
				[0.002]	[0.002]
(B) Individual proposals					
Unconditional cash transfer	Treatment	0.16**	0.15**	0.30**	0.30*
		[0.075]	[0.071]	[0.151]	[0.150]
	Treatment*Trust	-0.12***	-0.11***		
		[0.040]	[0.037]		
	Trust	0.17***	0.16***		
		[0.000]	[0.000]		
	Age			0.01***	0.01***
				[0.000]	[0.000]
	Treatment*Age			-0.01***	-0.01***
				[0.002]	[0.002]
Unconditional food transfer	Treatment	0.08	0.08	0.32***	0.32***
		[0.050]	[0.049]	[0.106]	[0.113]
	Treatment*Trust	-0.06**	-0.06**		
		[0.026]	[0.028]		
	Trust	0.05	0.05		
		[0.032]	[0.035]		
	Age			0.01***	0.01***
				[0.000]	[0.000]
	Treatment*Age			-0.01	-0.01
				[0.002]***	[0.002]***
Unconditional cash transfer and school	Treatment	0.09	0.10	0.27*	0.26*
		[0.077]	[0.076]	[0.142]	[0.142]
	Treatment*Trust	-0.03	-0.03		
		[0.025]	[0.025]		
	Trust	0.14**	0.14**		
		[0.067]	[0.059]		
	Age			0.01***	0.01***
				[0.000]	[0.000]
	Treatment*Age			-0.01	-0.00
				[0.002]***	[0.002]***
Cash transfer conditional on training	Treatment	0.04	0.04	0.14	0.14
		[0.069]	[0.061]	[0.141]	[0.149]
	Treatment*Trust	0.02	0.01		
		[0.085]	[0.046]		
	Trust	0.03	0.04		
		[0.042]	[0.048]		
	Age			0.01***	0.01***
				[0.000]	[0.000]
	Treatment*Age			-0.00*	-0.00*
				[0.001]	[0.001]
Controls included		No	Yes	No	Yes
Number of observations		402	402	402	402

Note: The estimation method is a linear probability model. Columns 1 and 3 report the unadjusted regressions (that do not include any control variables) on participants' decisions on all proposals (panel A) or a specific proposal (panel B). Columns 2, and 4 report the adjusted regressions on participants' decisions on all proposals (panel A) or a specific proposal (panel B) controlling for gender, three education levels, employment status, number of cars in the household, number of people living in the household, residence in the capital city and if the person donated to charity in the past three months. Standard errors clustered at the primary sampling unit 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 the 1%, 5%, and 10% level.

in government (OECD, 2016). This is particularly important given the role of youth in driving protest actions in the region at the time. The results confirm the prior that the treatment effect would differ according to participants' age. Figure 7 summarizes the average treatment effect estimates for the youth and older adult subsamples. Compared with older adults, young individuals (ages 18-29 or 18-34) 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 for the mean giving and unconditional cash transfer proposals.

FIGURE 7: AVERAGE TREATMENT EFFECT ON GIVING RATES, BY AGE GROUP

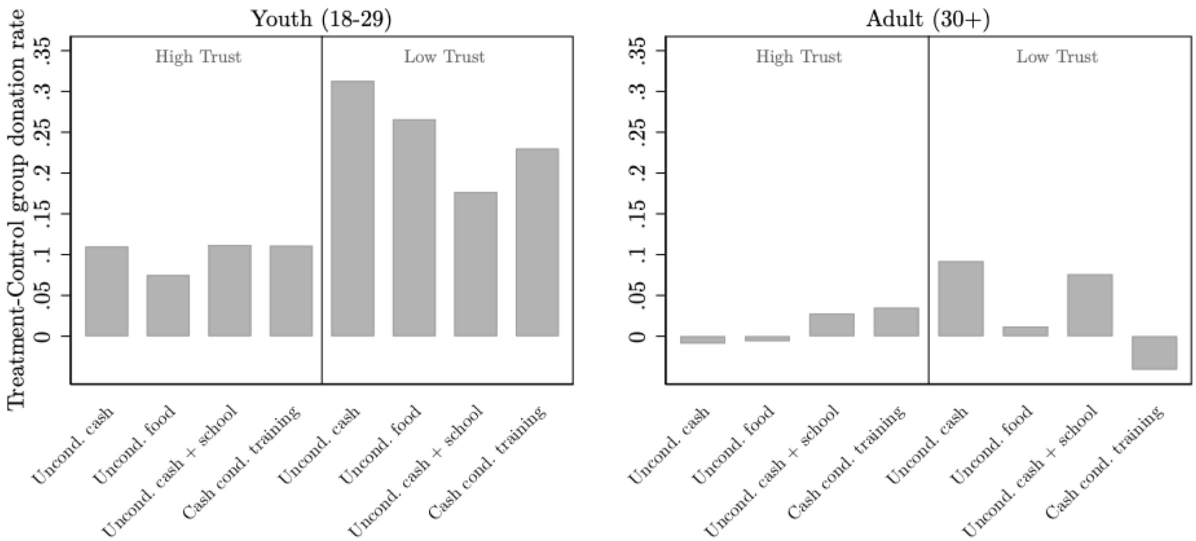


Note: Linear effect of participating in treatment group on giving decisions in age group subsamples. N=402. Adults range up to age 80 years.

Exploring the data further reveals a certain level of overlap between low trust and age, which explains why the treatment impacts are highly heterogeneous in both dimensions. Figure 8 shows that while young individuals are clearly the most affected by the treatment for three of the four proposals, treatment increases the giving rate the most among those who are both low-trust and young.[30]

[30]Figure A.1 in Appendix A summarizes the local effect of age, as a continuous variable, on average giving behavior in both the treatment and control groups according to the participants' level of trust that

FIGURE 8: TREATMENT IMPACT, BY AGE GROUP AND TRUST LEVEL



Note: The figure shows the average treatment effect (difference between treatment and control groups' donation rates) for the nested subsamples of youth and adults with 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 B.4.

The robustness of the heterogeneity of the treatment effects between young and older participants is demonstrated further in Table 4, columns 3 and 4. Young participants were less likely to give up their fuel vouchers, yet they were more positively impacted by the transparency-enhancing device; this is confirmed by a consistently significant negative coefficient for the interaction term of treatment and age in the specifications with and without controls.[31]

public funds for social assistance reach the poor. Panel A presents the results for aggregate/all proposals while panel B presents the 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.

[31]Table B.4 in Appendix B. confirms that the interaction terms of treatment and trust and treatment and age remain negative and statistically significant when included simultaneously in the same regression for mean giving and the unconditional cash transfer proposals (columns 1 and 2). In columns 3 to 6, the regressions considering solely the interaction term of treatment and trust (similar to columns 2 and 3 in Table 4) were repeated on the separate subsamples of young (ages 18-29) and older individuals. This analysis reveals that the treatment and the treatment and trust interaction had significant and larger impact among the youth. Table B.5, we check the robustness of our results to a different age threshold for youth. Table B.5 is

6.3 Effects on Preferred Design: Cash versus In-Kind assistance

In addition to the differences in giving rates, the treatment and control groups differed in terms of the proposal they favored the most.[32] Table 5 reports the favorite proposal among participants who gave up their voucher in response to at least one proposal.[33] In the control group, the unconditional food transfer option was the most preferred, being chosen by about one-third of the respondents. As food transfers are more visible and less fungible than cash, this can be considered the option with the least risk of capture. On the other hand, participants in the treatment group were 11 percentage points less likely to pick food transfer as their preferred design. Thus, the treatment appears to have enhanced the attractiveness of cash-based delivery, which may be perceived as more prone to capture but is considered to be more efficient for poverty reduction than in-kind food transfers (Currie and Gahvari, 2008; Haushofer and Shapiro, 2016).

similar to Table B.4 but extends the definition of youth to ages 29 to 34 years. The results on the interaction term between trust and treatment are robust to this alternative definition. Finally, in Table B.6, we check the robustness of the interaction terms of treatment and trust and of treatment and age to other interaction terms considered. The regression specifications presented in Table B.6 include trust, skilled, high income, an index of redistributive values and their interactions with treatment. The results on the interaction of treatment effects with trust and age are very similar to the effects reported in Tables 4 and B.4, which implies that the treatment effects reported earlier are robust. The results hold when using subjective income as shown in Table B.7.

[32] As a reminder, after all the decisions were marked, participants were asked to rank the proposals in the order of preference. 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. For those who gave up the voucher for strictly one proposal, the analysis imposes that proposal as the revealed favorite.

[33] Because the control group contained more individuals who never gave up their vouchers, the two samples are not of identical size, so we compare the distributions of responses.

TABLE 5: DISTRIBUTION OF THE PREFERRED PROPOSAL, BY TREATMENT STATUS

	Control (C)	Treatment (T)	ATE	Difference [p-value] (C-T)	
	(1)	(2)	(3)	(4)	(5)
Preferred proposal					
Unconditional cash transfer	0.20	0.24	0.03	0.49	0.48
	[0.402]	[0.425]			
Unconditional food transfer	0.33	0.22	-0.11	0.06	0.09
	[0.472]	[0.414]			
Unconditional cash transfer and school	0.14	0.20	0.06	0.16	0.20
	[0.344]	[0.401]			
Cash transfer conditional on training	0.33	0.35	0.02	0.59	0.76
	[0.472]	[0.477]			
Total	1	1			
Controls included	Yes	Yes		No	Yes
Number of observations	154	170			

Note: 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. Column 4 reports the p-value of the t-test of the difference of the unadjusted regression (that does not include any control variables) between the two samples. Column 5 reports the p-value of the t-test of the difference of the adjusted regression between the two samples. Column 4 and 5 standard errors are clustered at the primary sampling unit level using wild bootstrap-t. The results are based on the reported preferred proposals among those actually chosen by the participants. The adjusted regressions control for gender, three education levels, employment status, number of cars in the household, number of people living in the household, residence in the capital city and if the person donated to charity in the past three months. For individuals who only decided to give up their voucher once, the preferred proposal is assumed to be the delivery method actually chosen.

7 Alternative Explanations

Although the trust channel 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 4.4, 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 participants could be less familiar with the consumption basket of the poor and unaware that JD 20 could buy many essential supplies. This could happen because the participants' consumption basket differs from that of the poor in terms of the products or their quality. Thus, the fact that participants in the treatment group 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. In this case, we would expect the treatment effect to vary according to the participants' level of income or education, as these would be proxies for the distance between the participant and the potential beneficiaries of the transfer. 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.[34] Each column reports a regression of participants' decisions on that specific proposal. In all the specifications, the interaction term between treatment and skilled is not significant. Panel B describes the effect of the interaction between treatment and having a high income (a variable equal to one if the participant reported an above-mean value of per capita income).[35] In all the specifications except the unconditional food transfer, the interaction term is not significant. Panel C considers an alternative definition of high income using participants' responses on their subjective position in the income distribution of Jordan (subjective income quartile). In particular, subjective high income is a dummy variable equal to one if the participant declared that they were in income quartile three or four. In this case, in all the model specifications, the interaction term between treatment and high income is not significant. This evidence indicates that trust rather than information/awareness of 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 on giving of the interaction between treatment and trust varies across groups of the population with different scopes for giving. We consider two dimensions: economic distance from the poor (measured using education and income, as shown above) and how much the participant values redistribution (measured using beliefs that have been found to be correlated with redistributive behavior in the literature).[36] To construct

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

[35] Results obtained without controls are similar to the ones reported and are available from the authors upon request.

[36] The literature has shown that support for redistribution varies according to personal beliefs about the

indicators of the second dimension we use information from three survey questions that participants answered after the experiment, asking about their agreement with the following statements: (a) A just society should make people's incomes more equal; (b) Successful careers are a matter of luck and connections (rather than hard work); and (c) People are poor in Jordan because of bad luck or injustice (rather than laziness or lack of willpower).[37]

Tables 7 and 8 present the results. Table 7 is similar to Table 4 but focuses on distance from the poor rather than age. The 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, the 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 beliefs.

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 luck rather than individual effort being more prone to redistribution (Alesina, Glaeser and Sacerdote, 2001; Alesina, Glaeser and Glaeser, 2004; Alesina and Angeletos, 2005; Alesina and La Ferrara, 2005; Charness and Rabin, 2002; Konow, 2010).

[37] In our sample, 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 with the classic dictator games or fundraising experiments in the United States.

TABLE 6: EFFECT OF THE INTERACTION BETWEEN TREATMENT AND EDUCATION AND BETWEEN TREATMENT AND INCOME

	A) Aggregate /all proposals	*B) Individual proposals*			
	Mean giving	Uncond. cash transfer	Uncond. food transfer	Uncond. cash transfer and school	Cash transfer conditional on training
	(1)	(2)	(3)	(4)	(5)
Panel A: Interaction with education					
Treatment	0.05	0.07	0.04	0.07	0.04
	[0.052]	[0.066]	[0.045]	[0.064]	[0.072]
Treatment*Skilled	0.06	0.10	0.03	0.08	0.03
	[0.723]	[0.452]	[0.308]	[0.510]	[0.320]
Skilled	-0.04	-0.09	-0.02	-0.10	0.05
	[0.148]	[0.131]	[1.329]	[0.117]	[0.077]
Controls included	Yes	Yes	Yes	Yes	Yes
Number of observations	402	402	402	402	402
Panel B: Interaction with income					
Treatment	0.07	0.10	0.09*	0.07	0.02
	[0.045]	[0.062]	[0.054]	[0.056]	[0.056]
Treatment*High income	0.02	0.03	-0.12**	0.08	0.06
	[0.026]	[0.052]	[0.056]	[0.399]	[0.117]
High income	0.05	0.05	0.11	0.02	0.03
	[0.044]	[0.033]	[0.069]	[0.036]	[0.056]
Controls included	Yes	Yes	Yes	Yes	Yes
Number of observations	396	396	396	396	396
Panel C: Interaction with "subjective" income					
Treatment	0.05	0.07	0.02	0.09	0.01
	[0.053]	[0.051]	[0.046]	[0.058]	[0.035]
Treatment*High "subjective"	0.13	0.16	0.12	0.05	0.18
income	[0.148]	[0.134]	[0.137]	[0.128]	[0.162]
High "subjective" income	-0.06	-0.08	-0.06	-0.04	-0.08
	[0.165]	[0.188]	[0.135]	[0.156]	[0.122]
Controls included	Yes	Yes	Yes	Yes	Yes
Number of observations	398	398	398	398	398

Note: The estimation method is a linear probability model. Each column reports the adjusted regression on participants' decisions on all proposals (column 1) or a specific proposal (columns 2 to 5) controlling for gender, three education levels, employment status, number of cars in the household, number of people living in the household, residence in the capital city, and if the person donated to charity in the past three months. Standard deviations are reported in square brackets. Standard errors clustered at the PSU level using wild bootstrapt 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 their relative position on an income scale from one (lowest) to four (highest) is three or four. ***, **, * significant at the 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								
		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	0.09*	0.10	0.09	0.10*	0.09	0.12	0.09*	0.05	0.36**
		[0.053]	[0.084]	[0.073]	[0.057]	[0.067]	[0.096]	[0.054]	[0.062]	[0.170]
	Treatment*Trust	-0.05**	0.01	-0.08*	-0.06***	-0.05*	-0.10**	-0.05***	-0.02	-0.26***
		[0.019]	[0.018]	[0.047]	[0.023]	[0.028]	[0.047]	[0.018]	[0.020]	[0.082]
	Distance from the poor	0.00			0.06			-0.01		
		[0.024]			[0.043]			[0.115]		
Uncond. cash transfer	Treatment	0.15**	0.23***	0.11	0.17**	0.14	0.20*	0.15**	0.10	0.50**
		[0.071]	[0.086]	[0.092]	[0.076]	[0.101]	[0.119]	[0.076]	[0.072]	[0.213]
	Treatment*Trust	-0.11***	-0.14**	-0.10*	-0.13***	-0.11**	-0.17***	-0.11***	-0.08*	-0.36***
		[0.037]	[0.057]	[0.056]	[0.043]	[0.051]	[0.054]	[0.036]	[0.046]	[0.115]
	Distance from the poor	-0.02			0.07			-0.01		
		[0.036]			[0.043]			[0.062]		
Uncond. food transfer	Treatment	0.08	0.04	0.11*	0.09*	0.12	0.05	0.07	0.03	0.35*
		[0.049]	[0.091]	[0.058]	[0.050]	[0.073]	[0.096]	[0.050]	[0.061]	[0.188]
	Treatment*Trust	-0.06**	0.07	-0.15***	-0.07**	-0.06**	-0.14	-0.06**	-0.02	-0.28***
		[0.028]	[0.311]	[0.052]	[0.030]	[0.025]	[0.095]	[0.028]	[0.039]	[0.090]
	Distance from the poor	-0.00			0.05			-0.01		
		[0.013]			[0.065]			[0.089]		
Uncond. cash transfer and school	Treatment	0.10	0.08	0.11	0.11	0.09	0.14	0.10	0.08	0.30
		[0.076]	[0.126]	[0.116]	[0.080]	[0.088]	[0.145]	[0.079]	[0.086]	[0.212]
	Treatment*Trust	-0.03	0.09	-0.10	-0.05*	-0.06	-0.03	-0.04	-0.02	-0.24**
		[0.025]	[3.821]	[0.073]	[0.030]	[0.052]	[0.035]	[0.029]	[0.023]	[0.102]
	Distance from the poor	-0.04			0.07*			-0.02		
		[0.057]			[0.036]			[0.094]		
Cash transfer cond. on training	Treatment	0.04	0.06	0.02	0.04	-0.01	0.10	0.04	-0.01	0.31
		[0.061]	[0.087]	[0.072]	[0.067]	[0.631]	[0.103]	[0.065]	[0.054]	[0.214]
	Treatment*Trust	0.01	0.04	0.01	0.01	0.04	-0.06	0.02	0.03	-0.14*
		[0.046]	[0.151]	[0.092]	[0.020]	[0.159]	[0.041]	[0.058]	[0.082]	[0.081]
	Distance from the poor	0.08			0.06			-0.00		
		[0.103]			[0.055]			[0.016]		
Controls included		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations		402	144	258	396	247	149	398	301	97

Note: The estimation method is a linear probability model. All adjusted regressions include controls for trust, gender, three education levels, employment status, number of cars in the household, number of people living in the household, residence in the capital city, and if the person donated to charity in the past three months. Skilled is a dummy variable equal to one if the participant has completed high school or more. Per capita income above the mean 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 their relative position on an income scale from one (lowest) to four (highest) is three or four. Standard errors clustered at primary sampling unit level using wild bootstrap-t are reported in brackets. ***, **, * significant at the 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 "redistributive values"			
		Society should make incomes more equal	Success is a matter of hard work	People are poor because of bad luck or injustice (not laziness)	Composite index
		(1)	(2)	(3)	(4)
(A) Aggregate/all proposals					
Mean giving	Treatment	0.09	0.10*	0.13*	0.12*
		[0.054]	[0.056]	[0.066]	[0.069]
	Treatment*Trust	-0.03**	-0.06***	-0.09***	-0.09***
		[0.014]	[0.020]	[0.031]	[0.030]
	Redistribution values	-0.03	0.10***	0.06	-0.02
		[0.040]	[0.034]	[0.046]	[0.055]
(B) Individual proposals					
Unconditional cash transfer	Treatment	0.15**	0.16**	0.18**	0.17*
		[0.076]	[0.074]	[0.085]	[0.090]
	Treatment*Trust	-0.10***	-0.12***	-0.16***	-0.15***
		[0.032]	[0.040]	[0.053]	[0.050]
	Redistribution values	-0.08	0.10**	0.08*	-0.03
		[0.047]	[0.042]	[0.044]	[0.050]
Unconditional food transfer	Treatment	0.08	0.08	0.11**	0.10*
		[0.050]	[0.049]	[0.057]	[0.059]
	Treatment*Trust	-0.04*	-0.07**	-0.10***	-0.09**
		[0.025]	[0.030]	[0.035]	[0.036]
	Redistribution values	-0.06	0.12**	0.06	-0.04
		[0.055]	[0.050]	[0.056]	[0.055]
Unconditional cash transfer and school	Treatment	0.09	0.10	0.15*	0.14
		[0.074]	[0.079]	[0.085]	[0.089]
	Treatment*Trust	-0.01	-0.04	-0.10**	-0.09**
		[0.009]	[0.027]	[0.038]	[0.039]
	Redistribution values	0.04	0.09*	0.09*	0.00
		[0.056]	[0.049]	[0.053]	[0.012]
Cash transfer conditional on training	Treatment	0.03	0.04	0.06	0.06
		[0.062]	[0.067]	[0.083]	[0.085]
	Treatment*Trust	0.03	0.01	-0.02	-0.01
		[0.782]	[0.024]	[0.019]	[0.014]
	Redistribution values	-0.01	0.08*	0.02	-0.01
		[0.054]	[0.044]	[0.048]	[0.121]
Controls included		Yes	Yes	Yes	Yes

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

8 Conclusion

It is widely believed that increasing transparency in the delivery of welfare programs is a critical step in improving their political acceptability. This paper sheds light on this important relationship. Using representative survey data from four MENA countries, we show that preferences are not the reason for the lack of support of redistributive policies, rather distrust in the state's capacity seems to be the key determinant. Our theoretical model illustrates how distrust (and wrong beliefs) about the government's intention (whether it is benevolent or nonbenevolent) may lead to a state capacity trap: even when citizens are in favor of redistribution, asymmetric information and biased beliefs result in lack of support for redistributive policies, leaving no room for potential belief updates. One possible solution is to provide a credible signal of the state capacity's by increasing transparency. Using data from a behavioral experiment conducted on a nationally representative sample of the Jordanian middle class, this paper shows that transparency-enhancing measures make middle-class citizens more willing to forgo their own welfare to benefit the poor. This finding is robust to multiple hypothesis testing and does not appear to be explained by a variety of alternative hypotheses. We find that the main result is driven by an increase in support for unconditional cash transfers, which are considered to be the most efficient instrument of poverty reduction. The results suggest that the design of successful social policy reform (for example, subsidy reform) should include elements to enhance monitoring of welfare programs' delivery. This is particularly important for unconditional cash transfer schemes, the public support for which appears to be more sensitive to transparency of delivery than that for in-kind or conditional transfer schemes.

The paper also shows that the effect of transparency on support for redistributive programs is sensitive to the baseline level of trust in state capacity to deliver benefits to the intended beneficiaries. The transparency-enhancing treatment caused larger and significant increases in support for redistribution among individuals who had been suspicious about the capacity of the state to deliver welfare programs to the poor. This result suggests that measures to enhance transparency are particularly important for the success of social policy reform in countries where perceived state capacity to implement effective targeted transfers is low. Popular mechanisms, such as technology-enabled solutions, can not only ensure program accountability and good governance, but also increase support for redistribution, and therefore allow for an expansion of the state's long-term choice set of feasible policies, including replacing subsidies with targeted income transfers.

The results also point to an important conclusion: enhanced transparency increases the attractiveness of cash-based relative to in-kind transfers. Because the latter are generally less efficient but may be perceived as less prone to elite capture, transparency could thus enhance program efficiency by allowing policy makers to switch from in-kind to cash transfers without losing the support of middle-class citizens.

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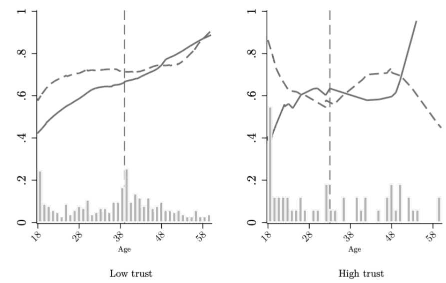
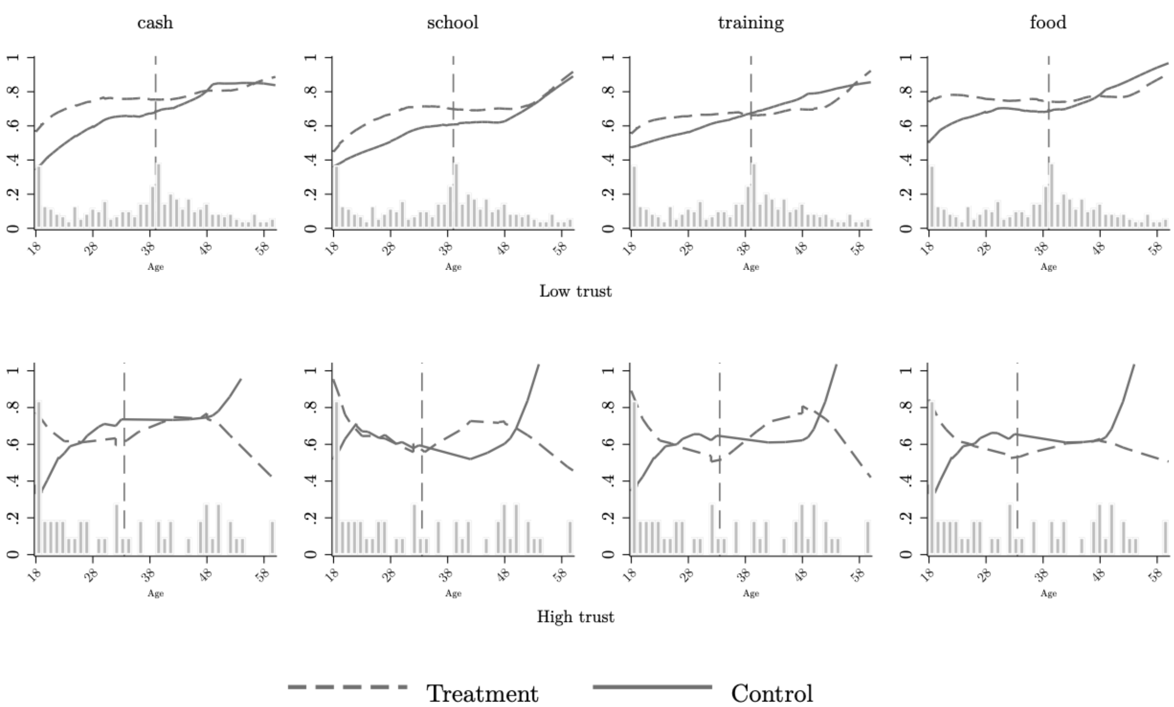
Appendices

A Figure

FIGURE A.1: TREATMENT IMPACT, BY AGE AND TRUST LEVEL



(a) Aggregate Proposals



(b) Individual proposals

Note: The lines produced by a local polynomial smooth function on mediating variable age. Mean giving is the number of times a participant decided to donate their voucher out of four. The vertical line represents the median of age in each sub-group. At the bottom of each graph, the histogram of age is plotted. The bandwidth is 0.8.

B Tables

TABLE B.1: MENA SPEAK DEMOGRAPHICS, BY COUNTRY

	Country				
	Egypt Arab Rep.	Lebanon	Jordan	Tunisia	Total (Pooled)
Male (%)	0.52	0.45	0.50	0.51	0.50
Younger than 18 years (%)	0.04	0.03	0.08	0.04	0.05
18-29 years (%)	0.27	0.24	0.33	0.32	0.29
30-39 years (%)	0.21	0.22	0.26	0.24	0.23
40-49 years (%)	0.19	0.22	0.17	0.17	0.19
50-59 years (%)	0.15	0.14	0.09	0.11	0.12
60-69 years (%)	0.09	0.11	0.04	0.06	0.08
Older than 70 years (%)	0.05	0.05	0.03	0.05	0.04
Income quintile 1 (%)	0.22	0.16	0.18	0.20	0.19
Income quintile 2 (%)	0.22	0.24	0.25	0.21	0.23
Income quintile 3 (%)	0.17	0.17	0.19	0.23	0.19
Income quintile 4 (%)	0.18	0.19	0.19	0.17	0.18
Income quintile 5 (%)	0.21	0.23	0.20	0.19	0.21
Employed (%)	0.42	0.52	0.36	0.39	0.42
Married (%)	0.70	0.63	0.64	0.47	0.61
Children	1.43	0.98	1.86	0.91	1.30
Native (%)	1.00	0.98	0.89	1.00	0.97

TABLE B.2: JORDAN GIVES EXPERIMENT SUMMARY STATISTICS

	Mean [s.d.] (1)	s.d. (2)	Number of observations (3)
Mean giving	0.67	0.38	402
"Accepted" unconditional cash transfer (UCT) (%)	0.69	0.46	402
"Accepted" unconditional food transfer (UFT) (%)	0.71	0.45	402
"Accepted" unconditional cash transfer and school (UCT+School) (%)	0.64	0.48	402
"Accepted" cash transfer conditional on training (CCT) (%)	0.66	0.48	402
High trust (%)	0.53	0.50	402
Male (%)	0.45	0.50	402
Age	38.24	12.96	402
Young (ages 18-29 years) (%)	0.28	0.45	402
Young (ages 18-34 years) (%)	0.38	0.49	402
Primary education (%)	0.06	0.23	402
Secondary education (%)	0.58	0.49	402
Tertiary education (%)	0.36	0.48	402
Currently employed (%)	0.34	0.47	402
Number of cars in the household	0.69	0.75	402
Household size	6.08	2.35	402
Low "subjective" income (%)	0.21	0.40	398
Middle "subjective" income (%)	0.77	0.42	398
High "subjective" income (%)	0.03	0.16	398
Residence in the capital city	0.72	0.45	402
Gave to charity in the past three months (%)	0.61	0.49	402
Skilled (completed high school or above) (%)	0.36	0.48	402
High income (%)	0.38	0.48	402
Self-identified as high income (%)	0.25	0.43	398
Reported prefered proposal UCT (%)	0.23	0.42	397
Reported prefered proposal UFT (%)	0.28	0.45	397
Reported prefered proposal UCT+School (%)	0.17	0.37	397
Reported prefered proposal CCT (%)	0.33	0.47	397
Believe people are poor because of bad luck or injustice (%)	0.60	0.49	340
Believe success is a matter of hard work (%)	0.63	0.48	396
Agree that society should make incomes more equal	0.79	0.40	398

Note: CCT = conditional cash transfer; UCT = unconditional cash transfer; UFT = unconditional food transfer.

TABLE B.3: ROBUSTNESS OF THE AVERAGE TREATMENT EFFECT TO MULTIPLE HYPOTHESIS TESTING

	A) Aggregate /all proposals	*B) Individual proposals*			
	Mean giving	Uncond. cash transfer	Uncond. food transfer	Uncond. cash transfer and school	Cash transfer conditional on training
	(1)	(2)	(3)	(4)	(5)
Treatment	0.52**	0.56**	0.20	0.63**	0.68**
	[0.241]	[0.238]	[0.299]	[0.251]	[0.263]
Male	0.10	0.08	0.10	0.15	0.08
	[0.072]	[0.071]	[0.083]	[0.099]	[0.076]
Completed secondary	-0.00	0.03	-0.13	-0.09	0.17***
	[0.004]	[0.020]	[0.373]	[0.167]	[0.055]
Completed tertiary	-0.01	-0.07	-0.10	-0.10	0.24***
	[0.008]	[0.107]	[0.755]	[0.181]	[0.077]
Employment status	0.02	0.12*	-0.01	-0.00	-0.03
	[0.043]	[0.072]	[0.166]	[0.046]	[0.110]
Number of cars in the household	0.03	0.06	0.01	0.08	-0.01
	[0.042]	[0.059]	[0.044]	[0.078]	[0.067]
Household size	0.01**	0.01**	0.00	0.03***	-0.00
	[0.006]	[0.006]	[0.011]	[0.000]	[0.001]
Residence in the capital city	-0.06	-0.09	-0.11	-0.08	0.04
	[0.352]	[0.167]	[0.099]	[0.230]	[0.046]
Gave to charity in the past three months	0.08	0.12	0.14**	0.03	0.04
	[0.054]	[0.073]	[0.066]	[0.067]	[0.055]
Treatment*Male	-0.13	-0.08	-0.07	-0.23**	-0.13
	[0.084]	[0.084]	[0.101]	[0.104]	[0.099]
Treatment*Completed secondary	-0.12**	-0.15**	0.10	-0.05*	-0.37***
	[0.051]	[0.061]	[1.363]	[0.027]	[0.122]
Treatment*Completed tertiary	-0.02*	0.02	0.15	0.05	-0.32***
	[0.014]	[0.014]	[3.141]	[0.032]	[0.105]
Treatment*Employment status	-0.03	-0.13	-0.01	0.02	-0.00
	[0.082]	[0.128]	[0.085]	[0.096]	[0.128]
Treatment*Number of cars in the household	-0.05	-0.11*	-0.01	-0.10	0.04
	[0.059]	[0.062]	[0.062]	[0.091]	[0.101]
Treatment*Household size	-0.02**	-0.02**	-0.01	-0.05***	-0.01*
	[0.009]	[0.008]	[0.014]	[0.016]	[0.004]
Treatment*Residence in the capital city	-0.09*	-0.03	-0.04	-0.09	-0.21**
	[0.050]	[0.032]	[0.048]	[0.056]	[0.082]
Treatment*Gave to charity in the past three months	-0.11*	-0.11	-0.23**	0.01	-0.12
	[0.064]	[0.081]	[0.093]	[0.123]	[0.088]
Controls included	Yes	Yes	Yes	Yes	Yes
Number of observations	402	402	402	402	402

Note: The estimation method is a linear probability model. The dependent variable is the giving rate. Completed secondary is a dummy variable equal to one if the participant has completed high-school. Completed tertiary is a dummy variable equal to one if the participant has completed tertiary education. Standard errors clustered at the primary sampling unit level using wild bootstrap-t are reported in brackets.***, **, * significant at the 1%, 5%,and 10% level.

TABLE B.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)	(5)	(6)
(A) Aggregate/all proposals							
Mean giving	Treatment	0.29***	0.29***	0.25**	0.20**	0.03	0.03
		[0.109]	[0.106]	[0.106]	[0.099]	[0.055]	[0.052]
	Treatment*Trust	-0.06**	-0.06**	-0.14**	-0.10**	-0.02	-0.04
		[0.025]	[0.024]	[0.062]	[0.046]	[0.019]	[0.029]
	Trust	0.12***	0.12***	0.11*	0.10*	0.10**	0.09***
		[0.000]	[0.041]	[0.064]	[0.056]	[0.038]	[0.034]
	Age	0.01***	0.01***				
		[0.000]	[0.000]				
	Treatment*Age	-0.01***	-0.01***				
		[0.002]	[0.002]				
(B) Individual proposals							
Unconditional cash transfer	Treatment	0.38**	0.37**	0.31**	0.28**	0.09	0.09
		[0.157]	[0.150]	[0.154]	[0.136]	[0.085]	[0.082]
	Treatment*Trust	-0.14***	-0.13**	-0.20***	-0.18**	-0.10**	-0.11**
		[0.046]	[0.049]	[0.074]	[0.069]	[0.045]	[0.045]
	Trust	0.19***	0.19***	0.18**	0.19**	0.18***	0.16***
		[0.000]	[0.000]	[0.083]	[0.084]	[0.000]	[0.000]
	Age	0.01***	0.01***				
		[0.000]	[0.000]				
	Treatment*Age	-0.01***	-0.01***				
		[0.002]	[0.002]				
Unconditional food transfer	Treatment	0.37***	0.37***	0.27***	0.21***	0.01	0.00
		[0.000]	[0.000]	[0.000]	[0.067]	[0.049]	[0.050]
	Treatment*Trust	-0.08***	-0.08***	-0.19***	-0.13**	-0.02	-0.04
		[0.027]	[0.028]	[0.067]	[0.052]	[0.044]	[0.079]
	Trust	0.07*	0.07**	0.09	0.06	0.03	0.03
		[0.035]	[0.035]	[0.059]	[0.047]	[0.058]	[0.071]
	Age	0.01***	0.01***				
		[0.000]	[0.000]				
	Treatment*Age	-0.01***	-0.01***				
		[0.003]	[0.003]				
Unconditional cash transfer and school	Treatment	0.29*	0.28*	0.18	0.14	0.06	0.06
		[0.162]	[0.151]	[0.153]	[0.133]	[0.080]	[0.087]
	Treatment*Trust	-0.04	-0.05	-0.07	-0.04	-0.03	-0.05
		[0.039]	[0.037]	[0.063]	[0.042]	[0.034]	[0.044]
	Trust	0.16**	0.17***	0.08	0.06	0.18**	0.17**
		[0.067]	[0.060]	[0.077]	[0.073]	[0.079]	[0.072]
	Age	0.01***	0.01***				
		[0.000]	[0.000]				
	Treatment*Age	-0.01**	-0.01**				
		[0.002]	[0.002]				
Cash transfer conditional on training	Treatment	0.13	0.13	0.23	0.18	-0.04	-0.05
		[0.150]	[0.150]	[0.156]	[0.172]	[0.095]	[0.098]
	Treatment*Trust	0.01	0.01	-0.12*	-0.08	0.07	0.05
		[0.060]	[0.025]	[0.063]	[0.048]	[0.072]	[0.051]
	Trust	0.04	0.06	0.11	0.09	-0.00	0.01
		[0.054]	[0.055]	[0.077]	[0.074]	[0.009]	[0.037]
	Age	0.01***	0.01***				
		[0.002]	[0.000]				
	Treatment*Age	-0.00	-0.00				
		[0.002]	[0.002]				
Controls included		No	Yes	No	Yes	No	Yes
Number of observations		402	402	113	113	289	289

Note: The estimation method is a linear probability model. Columns 1, 3 and 5 report the unadjusted regressions (that do not include any control variables) on participants' decisions on all proposals (panel A) or a specific proposal (panel B). Columns 2, 4, and 6 report the adjusted regressions on participants' decisions on all proposals (panel A) or a specific proposal (panel B) controlling for gender, three education levels, employment status, number of cars in the household, number of people living in the household, residence in the capital city and if the person donated to charity in the past three months. Standard errors clustered at the primary sampling unit 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 the at the 1%, 5%, and 10% level.

TABLE B.5: ROBUSTNESS OF THE INTERPLAY BETWEEN TREATMENT, TRUST, AND AGE TO A DIFFERENT DEFINITION OF YOUTH

		Young (18-34)		Adults (35+)	
		(1)	(2)	(3)	(4)
(A) Aggregate/all proposals					
Mean giving	Treatment	0.25***	0.22**	-0.00	-0.01
		[0.088]	[0.090]	[0.053]	[0.384]
	Treatment*Trust	-0.21***	-0.18***	0.04	0.02
		[0.073]	[0.060]	[0.072]	[0.042]
	Trust	0.14**	0.15***	0.08	0.06
		[0.056]	[0.053]	[0.055]	[0.060]
(B) Individual proposals					
Unconditional cash transfer	Treatment	0.34**	0.32**	0.04	0.03
		[0.153]	[0.139]	[0.079]	[0.073]
	Treatment*Trust	-0.32***	-0.29***	-0.01	-0.01
		[0.112]	[0.095]	[0.013]	[0.030]
	Trust	0.27***	0.29***	0.11**	0.09
		[0.000]	[0.000]	[0.056]	[0.063]
Unconditional food transfer	Treatment	0.27***	0.23***	-0.02	-0.03
		[0.098]	[0.000]	[0.073]	[0.073]
	Treatment*Trust	-0.24***	-0.20***	0.04	0.01
		[0.080]	[0.066]	[0.045]	[0.020]
	Trust	0.11*	0.10*	0.01	0.01
		[0.060]	[0.054]	[0.739]	[0.052]
Unconditional cash transfer and school	Treatment	0.17	0.13	0.04	0.04
		[0.119]	[0.107]	[0.078]	[0.075]
	Treatment*Trust	-0.04	-0.02	-0.04	-0.05
		[0.037]	[0.024]	[0.049]	[0.055]
	Trust	0.07	0.06	0.20***	0.19**
		[0.065]	[0.064]	[0.076]	[0.072]
Cash transfer conditional on training	Treatment	0.23	0.22	-0.08	-0.08
		[0.143]	[0.152]	[0.093]	[0.101]
	Treatment*Trust	-0.22***	-0.22**	0.16**	0.14**
		[0.082]	[0.084]	[0.073]	[0.064]
	Trust	0.12	0.15*	-0.03	-0.02
		[0.076]	[0.084]	[0.038]	[0.028]
Controls included		No	Yes	No	Yes
Number of observations		154	154	248	248

Note: Column 1 and 3 report the unadjusted regressions (that do not include any control variables) on participants' decisions on all proposals (panel A) or a specific proposal (panel B). Columns 2 and 4 report the adjusted regressions on participants' decisions on all proposals (panel A) or a specific proposal (panel B) controlling for gender, three education levels, employment status, number of cars in the household, number of people living in the household, residence in the capital city, and if the person donated to charity in the past three months. The dependent variable is the giving rate. Standard errors clustered at the primary sampling unit level using wild bootstrap-t are reported in brackets. ***, **, * significant at the 1%, 5%, and 10% level.

TABLE B.6: ROBUSTNESS OF THE INTERACTION BETWEEN TREATMENT AND TRUST TO MULTIPLE HYPOTHESIS TESTING

	A) Aggregate /all proposals	*B) Individual proposals*			
	Mean giving	Uncond. cash transfer	Uncond. food transfer	Uncond. cash transfer and school	Cash transfer conditional on training
	(1)	(2)	(3)	(4)	(5)
Treatment	0.32**	0.36*	0.52***	0.27	0.14
	[0.131]	[0.204]	[0.000]	[0.165]	[0.217]
Trust	0.12***	0.20***	0.08**	0.17***	0.05
	[0.040]	[0.000]	[0.038]	[0.063]	[0.059]
Age	0.01***	0.01***	0.01***	0.01***	0.01***
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Skilled	0.11*	0.08	0.09	0.08	0.19**
	[0.058]	[0.078]	[0.075]	[0.084]	[0.088]
High income	0.05	0.05	0.10	0.02	0.02
	[0.057]	[0.061]	[0.083]	[0.070]	[0.086]
Redistributive values index	0.01	-0.06	0.02	0.05	0.02
	[0.010]	[2.210]	[0.018]	[0.031]	[0.045]
Treatment*Trust	-0.06**	-0.13**	-0.08***	-0.04	0.02
	[0.025]	[0.057]	[0.030]	[0.040]	[0.186]
Treatment*Age	-0.01***	-0.01***	-0.01***	-0.01**	-0.00
	[0.002]	[0.002]	[0.002]	[0.003]	[0.002]
Treatment*Skilled	0.04	0.07	0.02	0.04	0.02
	[0.108]	[1.308]	[0.026]	[0.496]	[0.143]
Treatment*High income	0.01	0.02	-0.11	0.07	0.06
	[0.087]	[0.135]	[0.104]	[0.115]	[0.090]
Treatment*Redistributive values index	-0.07*	-0.04	-0.16***	-0.01	-0.06
	[0.035]	[0.026]	[0.053]	[0.011]	[0.083]
Controls included	Yes	Yes	Yes	Yes	Yes
Number of observations	392	392	392	392	392

Note: The estimation method is a linear probability model. All adjusted regressions control for gender, three education levels, employment status, number of cars in the household, number of people living in the household, residence in the capital city, and if the person donated to charity in the past three months. The dependent variable is the giving rate. Standard errors clustered at the primary sampling unit 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. Redistributive value is a dummy variable equal to one for believing that society should make incomes more equal. ***, **, * significant at the 1%, 5%, and 10% level.

TABLE B.7: ROBUSTNESS OF THE INTERACTION BETWEEN TREATMENT AND TRUST TO MULTIPLE HYPOTHESIS TESTING USING "SUBJECTIVE" INCOME RATHER THAN PER CAPITA INCOME

	A) Aggregate/all proposals	*B) Individual proposals*			
	Mean giving	Uncond. cash transfer	Uncond. food transfer	Uncond. cash transfer and school	Cash transfer conditional on training
	(1)	(2)	(3)	(4)	(5)
Treatment	0.28**	0.30	0.45***	0.27	0.11
	[0.137]	[0.193]	[0.158]	[0.183]	[0.199]
Trust	0.13***	0.19***	0.08**	0.18***	0.05
	[0.000]	[0.000]	[0.039]	[0.064]	[0.057]
Age	0.01***	0.01***	0.01***	0.01***	0.01***
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Skilled	0.12**	0.08	0.10	0.09	0.19**
	[0.054]	[0.073]	[0.076]	[0.083]	[0.092]
High "subjective" income	-0.03	-0.04	-0.02	0.00	-0.04
	[1.788]	[0.239]	[0.142]	[0.009]	[0.153]
Redistributive values index	0.00	-0.06	0.02	0.05	0.01
	[0.001]	[0.346]	[0.015]	[0.030]	[0.022]
Treatment*Trust	-0.05**	-0.12**	-0.08**	-0.03	0.01
	[0.026]	[0.057]	[0.033]	[0.035]	[0.114]
Treatment*Age	-0.00***	-0.01***	-0.01***	-0.01**	-0.00
	[0.002]	[0.002]	[0.002]	[0.002]	[0.002]
Treatment*Skilled	0.04	0.08	-0.01	0.06	0.02
	[0.215]	[0.434]	[0.006]	[0.358]	[2.788]
Treatment*High "subjective" income	0.10	0.14	0.11	0.01	0.16
	[0.123]	[0.083]	[0.179]	[0.037]	[0.161]
Treatment*Redistributive values index	-0.06*	-0.03	-0.14***	-0.02	-0.05
	[0.035]	[0.022]	[0.054]	[0.024]	[0.078]
Controls included	Yes	Yes	Yes	Yes	Yes
Number of observations	394	394	394	394	394

Note: The estimation method is a linear probability model. All adjusted regressions control for gender, three education levels, employment status, number of cars in the household, number of people living in the household, residence in the capital city, and if the person donated to charity in the past three months. The dependent variable is the giving rate. Standard errors clustered at the primary sampling unit 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 "subjective" income is a dummy variable equal to one if the participant reports that their relative position on an income scale from one (lowest) to four (highest) is three or four. Redistributive value is a dummy variable equal to one for believing that society should make incomes more equal. ***, **, * significant at the 1%, 5%, and 10% level.

C Mathematical Proofs

Proof of Proposition 1. Under full information, the nonpoor citizen compares the utility associated with each alternative:

$$\begin{aligned}
U(\text{no policy}) &= x_{np} - \beta_{np}(x_{np} - x_p) \\
U(\text{in-kind}) &= x_{np} - t - \beta_{np}(x_{np} - x_p - (1+s)t) \\
U(\text{cash}) &= x_{np} - t - \beta_{np}(x_{np} - x_p - (1-b)t)
\end{aligned}$$

Consequentially, under full information and with $\beta_{np} > \frac{1}{2}$:

- If $b = 0$, the cash policy is preferred to the in-kind policy. The cash policy is preferred to no policy. The in-kind policy is preferred to no policy if $\beta_{np} > \frac{1}{1+s}$.
- If $b = 1$, the in-kind policy is preferred to the cash policy. No policy is preferred to the cash policy. The in-kind policy is preferred to no policy if $\beta_{np} > \frac{1}{1+s}$.

Proof of Proposition 2. Under asymmetric information, the nonpoor citizen compares the utility associated with each alternative:

$$\begin{aligned}
&U(\text{no policy}) = x_{np} - \beta_{np}(x_{np} - x_p) \\
&U(\text{in-kind}) = x_{np} - t - \beta_{np}(x_{np} - x_p - (1+s)t) \\
&EU(\text{cash}) = \hat{\theta}[x_{np} - t - \beta_{np}(x_{np} - x_p - 2t)] + (1-\hat{\theta})[x_{np} - t - \beta_{np}(x_{np} - x_p - t)]
\end{aligned}$$

Consequentially, under asymmetric information and with $\beta_{np} > \frac{1}{2}$, the cash policy is preferred to the in-kind policy if $s > \hat{\theta}$. The in-kind policy is preferred to no policy if $\beta_{np} > \frac{1}{1+s}$. The cash policy is preferred to no policy if $\beta_{np} > \frac{1}{1+\hat{\theta}}$

Proof of Proposition 3. Trivial.

Proof of Proposition 4. In the good separating equilibrium, the non-benevolent politician does not invest in the transparency-enhancing device, while the benevolent politician does.

If the citizen observes the transparency-enhancing device, they believe they are in the presence of a benevolent politician. The cash policy is preferred to the in-kind and no policy as long as $\beta_{np} > \frac{1}{2}$. The in-kind policy is preferred to no policy if $\beta_{np} > \frac{1}{1+s}$.

If the citizen does not observe the transparency-enhancing device, they believe they are in the presence of a nonbenevolent politician. No policy is preferred to the cash policy. The in-kind policy is preferred to no policy if $\beta_{np} > \frac{1}{1+s}$.

A nonbenevolent politician's gains depend on the money they can capture. If they do not invest in the transparency-enhancing device, they receive 0. If they invest, they receive $t - c_0 - F$. Hence, the nonbenevolent politician does not invest if $F > t - c_0$.

A benevolent politician's gains depend on the citizens' welfare. If they invest in the transparency-enhancing device, they receive $x_{np} + x_p - (\alpha_p + \beta_{np})(x_{np} - x_p - 2t) - c_0$. If they do not invest, they receive $x_{np} + x_p + (s-1)t - (\alpha_p + \beta_{np})(x_{np} - x_p - (1+s)t)$ when $\beta_{np} > \frac{1}{1+s}$ and $x_{np} + x_p - (\alpha_p + \beta_{np})(x_{np} - x_p)$ when $\beta_{np} < \frac{1}{1+s}$. Hence, the benevolent politician invests in the transparency-enhancing device if $c_0 < (1 + \alpha_p + \beta_{np})(1-s)t$ when $\beta_{np} > \frac{1}{1+s}$ and $c_0 < 2(\alpha_p + \beta_{np})t$ when $\beta_{np} < \frac{1}{1+s}$.

In conclusion, the good separating equilibrium is feasible if the fixed cost of the transparency-enhancing device is not too high ($c_0 < (1 + \alpha_p + \beta_{np})(1-s)t$ when $\beta_{np} > \frac{1}{1+s}$ and $c_0 < 2(\alpha_p + \beta_{np})t$ when $\beta_{np} < \frac{1}{1+s}$) and the expected fine for the nonbenevolent politician is high enough ($F > t - c_0$)

D Jordan Gives Sample Design and Selection Protocols

The Jordan Gives 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 households that have per capita incomes between twice and four times Jordan's national poverty line. This definition corresponded to the population between the fourth and the eighth income deciles 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;[38] (2) applying coefficients from that regression to the 2004 Census data; and (3) choosing PSUs with resulting average scores between fourth and eighth 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 age 18 years to be invited to a meeting the next day at the reserved location (usually a nearby public school). The purpose of 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

[38] 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

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 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 the quota of 20 participants was filled, those who showed up later were turned away, after filling out a questionnaire and being paid the promised show-up fee. This turn-away policy allowed us to enlist 20 participants per session in all cases.