Belief in Hard Work and Prosocial Behavior: Evidence from a Randomized Field Experiment *

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Abstract

We provide evidence that optimistic beliefs regarding the malleability of ability, while leading to individual success, diminish the individual's sympathy toward the unsuccessful. We generate random variation in beliefs via an educational intervention, which imparts to elementary school children the idea that ability, rather than being innately fixed, can be developed through effort. To evaluate the impact of the intervention we create an experimental setting where both control and treatment students are given the opportunity to accumulate ability in a real effort task and earn rewards. Against this background, we implement an altruism experiment where we manipulate the donation context in terms of the potential reasons for the anonymous recipient's poorness. We find that while treated students are no less likely than control to give to recipients who are poor because of bad luck, they are significantly less likely to give to those who failed at the task despite having had a chance to practice. The results point to the importance of beliefs about the role of hard work in shaping redistributive preferences.

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1 Introduction

Most people exhibit some degree of generosity toward other people. This generosity surfaces quite early in childhood and marks our fundamental ability to live and cooperate with others in almost all areas of life. Giving to others, reflected in a multi-billion dollar global charity industry, has been a productive area of research in economics; see List (2011). On the theory side, standard preferences have been extended in useful ways to account for the apparent concern people have for others (see Fehr and Schmidt (2005), Fehr and Schmidt (1999), Bolton and Ockefels (2000), Charness and Rabin (2002) among others). On the empirical side, ample evidence has been accumulated on the viability of social preferences and their heterogeneity across individuals as well as ethnic and religious groups. Methods of experimental economics have been transformative in understanding social preferences and their implications for economic policy. It has been shown that a significant portion of people exhibit altruism, fairness and reciprocity by giving to others, being fair, rewarding fair behavior and punishing unfair behavior in dictator, ultimatum and trust games (see Cooper and Kagel (2016) and Fehr and Schmidt (2006) for extensive reviews of this large literature).

What compels people to concern themselves with others' well-being is thought to be in part the notion of fairness and social justice.[1] In many contexts, people tend to consider an outcome fair, be it positive or negative, when the circumstances that generated that outcome were under the individual's control, and responsibility can be attributed to the individual (Konow (2000), Cappelen et al (2007), Gill and Stone (2010)). In particular, an individual's views on what generates wealth, and to what extent individuals are personally responsible for their outcomes are crucial for how willing she would be to donate to the poor. These views are partly shaped by factors such as culture, religion, family and education (Guiso et al. (2006)). In societies where people have a tendency to believe that poverty is largely a self-imposed state, as in the United States, there tends to be less support for redistribution from the rich to the poor. In contrast, in societies where poverty is perceived as a result of circumstances outside of one's control, a stronger desire for redistribution is likely to be observed; see Piketty (1995), Alesina and Angeletos (2005), Bénabou and Tirole (2006). Empirically, people support redistribution from the rich to the poor more when they believe that poverty is caused by exogenous circumstances, as opposed to being a result of active choices.[2] These views on the part of a society, in equilibrium, shape redistributive policies in that society, in addition to shaping patterns of charitable giving (Alesina and Angletos (2005)).

[1]See Alesina and Guilino (2009) for an in-depth review on the determinants of preferences for redistribution. Also see Luttmer and Singhall (2011).

[2]See Williamson (1974), Heclo (1986), Gilens (1999), Alesina, Glaeser, and Sacerdote (2001), Fong (2001), Corneo and Grüner (2002), Durante and Putterman (2008), Fong and Luttmer (2011).

Underlying the worldview that poverty is largely the outcome of personal responsibility is the belief that success and wealth are within the reach of the hard-working individual. Such individualism, which pervades the American culture to a greater extent, is often considered to be the main force behind the mass wealth accumulation and technological superiority of the United States. In this paper, using a unique artefactual setting, we show that such optimistic beliefs not only lead to individual success and wealth but also affect patterns of altruistic behavior in a way consistent with the much debated differences in redistributive preferences between the USA and the Continental Europe. Our setting allows us to generate random variation in beliefs about the role of effort in achievement via a unique and effective educational intervention, implemented in elementary schools.

Building on the conjecture (which is corroborated in the data) that the intervention will significantly shift beliefs toward more optimism about the role of effort in achievement, the objective of the paper is to study the effect of this optimism on altruistic behavior of the individual. The reason why believing that anyone can achieve success by working hard enough may impact patterns of giving is as follows. An individual who is a firm believer in the optimistic worldview of achievement may see others' failure from a different perspective than someone who believes that outcomes reflect innate ability (or lack thereof) and effort plays little role. In particular, if an individual believes that opportunities for ability accumulation and eventual success are available to everyone who is willing to work hard and it is up to the individual to seize this opportunity, a bad outcome may be perceived as more the fault of the failed individual himself, rather than as an unlucky turn of events. This belief may affect her beliefs regarding how deserving the recipient is, and thereby how willing she is to give to that recipient. The ideas advocated by the intervention go against the belief that those who are lucky to have been born able (and well-connected) tend to be successful.

The educational intervention is implemented in a randomized-controlled manner, in a sample of state-run elementary schools in Istanbul. The impact of the intervention on the behaviors and outcomes of the children is evaluated using an incentivized experiment as well as surveys. The experiment is designed to create a setup that enables ability accumulation, using a specific real effort task with a performance target where individuals can practice over a one-week period. Therefore, we physically visit each classroom twice, one week apart, to implement the experiment. In the first visit, students are introduced to the real effort task, and their task-specific ability as well as their success in terms of meeting a performance target are measured. Students are then given the option to practice on the task for a week, until the second visit. In the second visit, their success at meeting the performance target is measured again. As we report in detail in Alan, Boneva and Ertac (2016), treated children are significantly more likely to take up the opportunity to invest in their ability and they follow through with this commitment, which translates into significantly higher success rates and experimental earnings in the second visit. That is, causing a significant increase in the optimism regarding the malleability of ability, the educational intervention leads to higher achievement.

Against this background, we implement our altruism experiment, which manipulates the donation context. Using a variant of the dictator game, we ask children whether they would like to give a fraction of their experimental earnings to anonymous children. A random half of both treated and control children are given the information that the recipient has no gifts because he/she failed at the real-effort task despite the chance to practice, while the other half are given the information that the recipient has no gifts because his/her school was not visited. The motivation behind this design is that an individual's worldview will manifest itself in her altruistic patterns through her perception of the recipient's "deservingness". In particular, those who believe that the skill needed for success can be developed through effort will likely perceive others' failure and low wealth as a fair outcome rather than bad luck. In contrast, those who believe that outcomes largely reflect innate abilities and effort will play little role will likely focus on the role of bad luck in the same situation. By implementing one context where the recipient's poorness is certainly due to bad luck (not being visited) and one where it may be due to lack of effort, we compare the responsiveness of giving on the part of treated and untreated children to the potential reasons for the recipient's poorness.

The results show a striking difference in treated and untreated childrens' responsiveness to the donation context: While there is no effect of the treatment on the probability or amount of giving when the recipient is perceived to be unlucky, we estimate a large and significant effect when the recipient is known to have failed in the real effort task. In particular, treated students are about 10 percentage points less likely than control students to donate to those failed at the task. Consistently with the results in the luck context, we also do not find any effect of the treatment on giving to anonymous recipients out of a windfall in the first visit, before the real effort task is introduced. We interpret these results to mean that the intervention did not have an impact on the overall social preferences of children, that is, it did not make treated children more selfish–rather, children who were exposed to the worldview where enough effort will surely bring success, are more reluctant to donate when the recipient is known to have had a chance to study but failed.

Our preferred explanation for these results is that children who embrace the optimistic view of achievement will have a different responsibility attribution of failure compared to children who believe in luck. Our survey evidence strongly suggests that the intervention indeed moulded beliefs regarding the malleability of ability and role of effort in achievement. These beliefs do not seem to translate into less giving when the recipient's bad outcome is not under her control, but does so in a context where effort could play a role. With the help of a theoretical model, we propose a mechanism that involves differences in malleability beliefs and the perceived return to effort across treated and control children, to explain the strong treatment effect we estimate on the response of giving to context. In particular, treated children, who believe that even low innate ability can be enhanced with sufficient effort, are likely to view failure as the recipient's own responsibility. In contrast, children in the control group, who have a more pessimistic view of what can be achieved with effort, are more likely to consider the failed recipient as unlucky.

There is by now a large literature that studies giving and shows the associations of redistributive preferences with factors such as culture, education, race, gender, social context etc. Literature shows that the social norms evoked by the decision context determines what is perceived to be fair (Levitt and List (2007)). This is evident in the findings that sources of income (for both sides), perceived entitlements, and attributions of responsibility are important in other-regarding behavior ranging from altruism to negative reciprocity.[3] The worldview on fairness/social justice, that is, what level of personal responsibility poor individuals have is of paramount importance in giving behavior, and has redistributional implications as well. Using a randomized educational intervention that aims to instill an "achievement mindset", we manage to generate exogenous variation in worldview/fairness beliefs and identify their causal role in determining altruistic behavior. By manipulating the donation context, we manipulate the potential perceived reasons for the recipient's poorness, and are able to observe how different perceptions across treatment and control influence giving. In addition to the literature on other-regarding preferences, the paper also informs a growing literature on the importance of non-cognitive skills in achievement (see for example Duckworth et al (2007), Heckman et al (2006), Almlund et al (2001)). Developing grit and tenacity in students by promoting goal-setting and an effort-oriented achievement/growth mindset has been an important focus of both educational policymakers and families[4]. While this type of mindset is likely to indeed improve achievement outcomes, we show that these beliefs may also affect patterns of altruistic behavior and, to the extent that impacts are persistent, the desire for redistribution in the society.

The rest of the paper is organized as follows: Section 2 presents the background, Section 3 provides the experimental design and procedures, Section 4 presents results, Section 5 puts forward a potential

[3]In numerous lab experiments, the willingness to give in the dictator game declines considerably when subjects donate out of their earned experimental rewards; see Hoffman et al (1994), Ruffle (1998), Cherry et al (2002), Cherry and Shogren (2008), Oxoby and Spraggon (2008), Carlsson et al (2012), Erkal et al (2011), Rey-Biel et al (2011). In addition, List (2007) shows that when recipients have earned their income, dictators are less willing to take from them. In trust and ultimatum games, the first mover is punished less when she has no control over the sent amount in a trust game.

[4]Dweck (2006) puts forward the importance of growth or achievement mindset on success. Blackwell et al (2007) show that the students' mindset with regard to the malleability of intelligence has an effect on the trajectory of mathematics grades among 7th graders. More evidence on the relationship between students' mindsets and achievement is provided by Aronson (2002) and Good (2003).

mechanism and Section 6 concludes.

2 Background

In this section, we provide a brief review on the educational intervention we evaluate in the paper. We first give a summary of the educational content and the way in which it was covered in the classroom. Then, we provide a detailed timeline of the entire experiment from baseline data collection to the implementation of follow-up experiments and surveys.

2.1 Educational Intervention

The Turkish Ministry of Education encourages socially useful projects offered by reputable organizations– such as international organizations, state departments, universities and reputable private companies-- to be implemented in schools. Teachers are allowed to participate any projects of their choice provided that these projects are approved by the local education directorate. Activities proposed by these projects are implemented in the free-allotted hours (maximum of 5 hours per week). Absent projects, the way in which these hours are used is at the discretion of the teacher. They can be used for crafts and arts or they can be given to students as unstructured play/activity time. The educational program we evaluate in this paper received the Ministry's approval as an extra-curricular project to be implemented in the state-run elementary schools in Istanbul.

The intervention is designed as a 12-week program, and recommended to be implemented at least 2 hour per week. The program includes a newly designed curriculum. The target concepts for the curriculum is conveyed by the authors to a large interdisciplinary team of education psychologists, a team of volunteer elementary school teachers, media artists and children story writers. The resulting curriculum include animated videos, mini stories and case studies that aim to impart critical concepts to children in a fun and effective way. Involved (volunteer) teachers were required to master the curriculum via intensive teacher training seminars, and provided a detailed teacher kit to cover the material week by week in a structured way.

The main objective of the program is to expose children to a positive world view regarding the means of achievement. The specific aim is to impart to children that ability, rather than being fixed and innate, can be developed through sustained effort. To this end, children are strongly encouraged to set ambitious goals, work toward their goals, and more importantly, persevere in the face of setbacks. While the teachers were given a clearly structured material to be covered for each week along with a large variety of suggested class activities, the program is not confined to mechanically covering the curriculum. Rather, it aims to influence the mindset of the students regarding the malleability of ability and intelligence and therefore emphasize the role of effort in achieving success, via influencing the mindset of their teachers. Teachers, while covering the curriculum in the way instructed in the teacher kit, are encouraged to internalized the ideas put forward in the curriculum in the training seminars as a general teaching philosophy. To this end, they were encouraged to review their feedback giving style, style of praising and the way they reward effort and outcome[5].

The impacts of this program on a unique experimental measure that aims to capture the main pillars of grit and on actual test scores are evaluated in Alan, Boneva and Ertac (2016). Using a novel real effort task with a performance target, the paper shows that the program is highly effective in encouraging challenge seeking and perseverant behavior as well as increasing the propensity to set ambitious goals. They then show that these behaviors lead to heightened willingness to exert effort to accumulate task-specific ability and consequently higher success in the task[6].

2.2 Timeline of the Field Experiment

Figure 1 lays out the timeline of the field experiment. After all the paper work required by the Ministry of Education was completed, our field partner began contacting 4th grade teachers in random sequence. In order to be able to sample a large number of districts across Istanbul, we stratified our calls based on districts.[7] After giving a minimum amount of information about the content of the program, teachers were asked if they would be willing to participate in the program. All willing teachers were promised to be invited to the training seminars and given the training material eventually but they were told that they will be involved in the program within the next two academic years, maybe immediately in the upcoming academic year, maybe in the following one. This way, we made sure that control students would never receive the treatment, although their teachers would after this cohort of students moved on to middle school, giving ourselves the chance do a longer-term follow-up later[8]. About 80% of the contacted teachers stated their willingness, which is a very large percentage that gives us comfort in terms of the external validity of our results.

Random assignment of schools was carried out in the following manner: Upon several positively-ended phone calls in a given district we assigned these schools to treatment or control. Since data collection involves very labor intensive experimental procedures that require physically visiting class-

[5] Details of the curriculum can be found at https://drive.google.com/file/d/0Bwo3BHO1RC19eDZjQktoSHFZaWM/view

[6] The paper also shows that treated children receive significantly higher test scores in math and language.

[7] The Istanbul Education Directorate encourages us to reach the primary schools in lower SES districts where achievement concerns are greater

[8] Turkish mandatory education spans 12 years, with 4 years is spent in primary school, the next 4 in middle school and the final 4 in high school.

rooms multiple times and spending considerable time in each classroom, we stopped the calls when we hit our logistical constraint of being able to visit classrooms. This meant a sample of 16 schools (42 classrooms), totaling about 1300 students. While the involvement of a school depends on the willingness of a teacher in that school, the unit of randomization is set to be the school and not the classroom/teacher, in order to prevent potential spillover effects. It is also worth noting that Istanbul is a big metropolitan city with 15 million residents; therefore, any communication among teachers or students across treatment status is highly unlikely.

Once we complete the random assignment of the teachers we visited all classrooms (both control and treatment) and collected baseline data. This was done via teacher assessment surveys, student surveys, a cognitive ability test (Raven Progressive Matrices), an incentivized risk tolerance task and standardized mathematics and language tests. We then invited teachers who were assigned to the treatment group in the upcoming academic year to a day-long seminar where a team of education consultants introduced the curriculum and trained the teachers.

As can be seen in row 2 of Figure 1, teachers were given the entire Fall 2015 term to implement the curriculum in the allotted free hours by the Ministry. As extra-curricular project activities are very common (and popular) among the elementary school teachers, teachers in the control group were also engaged in other projects (e.g. environmental awareness) at the time of the implementation of our program.

As detailed in row 3 of Figure 1, we collected all follow-up data in January 2016, just before the Fall term ended in that particular academic year. All experiments were conducted by the authors by physically visiting all classrooms with the assistance of graduate and undergraduate students along with well-trained professional survey staff.

3 Experimental Measures and Procedures

Our data collection strategy for testing whether the intervention influences prosocial behavior is composed of two visits to the classroom, one week apart. The set of tasks we implement aims to measure (1) giving out of a windfall, (2) giving out of earned wealth, part of which is accumulated through a real-effort task, in two different donation contexts, (3) time preference. In the first visit, children are told that they will play some games for two hours and at the end of these two hours they will receive tokens that correspond to gifts. They are told that the amount of tokens they receive will depend on their decisions and performances in the games. The tokens they can earn are of 3 different values, whole, half and quarter. A whole token corresponds to larger/more valuable gifts, a half token relatively smaller and a quarter token corresponds to the smallest gifts. Children are told that they will receive their gifts at the end of all the games that day and we would revisit their classrooms to play more games exactly one week later. After this introduction, we first elicit children's time preferences. We then conduct a version of the dictator game to elicit giving out of windfall, and complete the first visit by implementing the first part of our two-week real effort task which was briefly mentioned above in Section 2.1 and will be explained in detail below. This task sets the background for our main altruism experiments in the second week.

In the second visit, we first implement the second part of the real effort task. Because of differential amounts of gifts earned as a result of success/failure in the real effort task and the delayed gifts to be received from the time preference task the previous week, children have different amounts of experimental wealth at the beginning of the altruism experiment. We then implement our main altruism experiment, which manipulates the reason for the recipient's poorness. We complete the second visit with follow-up math and language tests and student surveys, which, as we explain below include a battery of questions aiming to elicit children's beliefs. In January 2016, we physically visited all classrooms twice, one week apart, to collect these post-intervention data. Each classroom visit took two complete lecture hours. We now give a detailed account of each incentivized experimental task.

3.1 Time Preference Elicitation (Week 1)

We begin our experiments with the elicitation of time preferences using a version of the Convex Time Budget (CTB) task, adapted from Andreoni and Sprenger (2012). The main purpose of this task is to explore whether the treatment had any impact on the time preferences of children. Somewhat secondary to this purpose, we also aim to establish a possible relationship between impatience and giving behavior. The implementation of the task is as follows: Children are asked to allocate 5 whole tokens between an earlier and a later option, with the earlier date being the day of the first visit and the later date the day of the second visit. In order to facilitate comprehension, we introduce two bowls, an "earlier" bowl that gives gifts today and a "later" bowl that gives gifts one week later. Children are told that tokens placed in the "later" bowl "give birth", that is, each token placed in this bowl generates an extra half token (an interest rate of 50%). After graphically presenting all 6 options on the blackboard while explaining the task, students are distributed choice sheets that include all the options, and then they are asked to pick one.

As shown later in the text, we estimate a virtually zero treatment effect with respect to time preferences, suggesting that the educational intervention had no impact on children's willingness to wait for a higher reward. We do, however, observe a very strong negative association between impatience and giving out of a windfall, with more impatient children donating less.

3.2 Elicitation of Giving out of Windfall (Week 1)

Following the elicitation of time preferences, we implement our windfall altruism experiment. For this, we give children four quarter tokens as an endowment. We then tell them that there are many schools we were unable to visit due to lack of time, and ask them whether they would like to donate some of their four gifts to the first graders in those schools.[9] They were asked to write down in their decisions sheets discretely whether they would like to donate some or none of their quarter tokens to these children. The purpose of this experiment is to see if the treatment has any impact on general altruistic behavior using a version of a measure (dictator game) widely used to study social preferences and giving behavior.

3.3 The Real Effort Task (Week 1)

The real effort task is conducted over two weeks, and is designed to experimentally evaluate the impact of the program on major components of grit–challenge seeking, perseverance in the face of failures, goal setting and engaging in skill accumulation. For the purposes of the current paper, the task serves well to provide us with a context where the recipient's poorness may have been caused by low effort, as will be explained below[10]. The task involves two consecutive visits to the classroom, a week apart from each other. In the first visit, after eliciting time preferences and giving out of a windfall, we implement the first part of the task, which is designed to elicit goal-setting and perseverance. In this first part, children go through five rounds of a mathematical real effort task. In particular, they are presented with a grid which contains several two-digit numbers where the goal is to find pairs of numbers that add up to 100. At the end of the five rounds, one of the rounds is selected at random and subjects get rewarded based on their performance in that round. Rewards depend on meeting a performance target, which is to find three pairs of numbers which sum up to 100, within 1 minute and 45 seconds.

Children are presented with two versions of the same task: Before each round begins, children are

[9] We chose first-graders as the recipient sample here in order to have a recipient group that was not similar to the subjects in terms of age or predicament. Given that we use 4th graders (similar to our subjects) as our recipient group in the second week experiment, this also prevents repetition of the same task with the same subjects. Since we are not interested in comparing windfall vs. earned-income giving directly but rather in documenting treatment effects within windfall and within earned income, the difference in the recipient group in windfall vs. earned income is not an issue for our purposes.

[10] As mentioned in Section 2.1, the evaluation results on grit are detailed in ABE. In ABE we do the evaluation by using two independent samples and the sample we use in the current paper constitutes the replication sample (Sample 2) in that study. We implemented this second field study to (1) replicate the results we obtained in the first study, and (2) to test the hypothesis regarding the effect of the grit treatment on altruism, which is the goal of this paper.

given the chance to choose between (1) the “4- token game”, which yields four (whole token) gifts in the case of success and zero in the case of failure, and (2) the “1- token game”, which yields one (whole token) gift in the case of success and zero in the case of failure[11]. Although in both games the goal is to find at least three pairs of numbers adding to 100, the 4-token game is designed to be more difficult than the 1-token game. In particular, in the 1-token game the grid numbers is smaller, and the matching pairs are easier to spot[12]. In fact, the mean empirical success rate in the easy task ranges from 90% to 100% over the five rounds.

After each round, experimenters go around the class and circle either “Succeeded” or “Failed” on the students’ sheets for that round, based on whether at least 3 pairs were correctly found. As mentioned above, students have the opportunity to switch back and forth between the two types of tasks as the rounds progress. After the five rounds are completed, we inform the children that we will visit their classrooms once more in exactly a week’s time. The children are told that they will play the game one more time (for only one round) during the second visit, and that they need to decide now whether they would like to play the 4-token (more difficult) game or the 1-token (easier) game in a week’s time. Crucially, they are also told that an "exercise booklet", which contains examples and practice questions that have a similar difficulty level to the 4-token game, is available if they would like to take home with them and practice. Just as in the first round, in order to get a subsample to play the difficult game free of selection, the students’ choices are implemented with 50% chance, and with 50% chance they play the challenging game in the next visit. Students are aware that their choices will have a 50% chance of counting when they make their choices, They are also informed about which game they are going to play in the 2nd visit at the end of the 1st visit.

3.4 Real Effort Task (Week 2)

In the second visit, children either do the task that they had committed to in the first visit or the difficult task, depending on whether the difficult task was imposed in their classroom or not. They were again given 1 minute and 45 seconds to find pairs of numbers that add up to 100. The game is played for one round only this time, and children receive immediate feedback at the end of the game, so they are fully aware of how many gifts they earned from this game. They are also told at the beginning of the second visit that all the gifts they had allocated to Week 2 during the time preference

[11] There is one exception to children’s freedom to choose between the tasks. Only in the first round, after children choose between the 4-token and 1-token game, in about half of the classes (randomly selected), children are imposed the 4-token game in the first round. That is, children’s 1st round choices count with 50% chance, which they are aware of before they make a choice. This is to observe the response to failure in the difficult task in a sample that is free of selection. In rounds 2-5, there are no constraints on children’s choices.

[12] See Appendix for examples of the two types of task.

elicitation task in the first week were brought to the classroom that day. This creates a setting where there is variation in earned wealth levels.

3.5 Donation Experiment (Week 2)

Our main altruism experiment follows the feedback on the real effort task. As shown in Figure 1, the donation experiment is conducted differently. First of all, children are asked to give out of their own earned wealth rather than a fixed, windfall endowment[13]. In addition to this, we change the donation context by randomly manipulating the reason for anonymous recipient's poorness: bad luck or personal failure. In particular, a random subset of the students (both in control and treatment) were told that there are fourth-graders in another school who played the number game just as they did, with a chance to practice for a week, but did not play any other games, so children who failed in the number game did not receive any gifts. Students are then asked whether and how many of the gifts they earned that day they would like to donate to these children.

In contrast, a random subset of the students were told that there are fourth-graders in another school who did not get to play these games and who, therefore, did not get any gifts. They are then asked whether and how many of the gifts they earned that day they would like to donate to these children.

The donation context we experimentally manipulate here gives the reason behind the recipient's poorness. In the "effort context", the recipient played the number game in similar conditions, she/he was given the same amount of time to study for the second week game and failed. In the "luck context", there was nothing the recipient could do to receive any gifts since the experimenters did not visit them and play this game.

It is worthwhile to pause here and explain what we expect to achieve with this design. While the first visit is important for establishing the effect of the treatment on general altruistic preferences with a measure used widely in the literature, the core message of the paper and the main contribution stem from the data collected in the second visit. This visit provides us with the background necessary to identify whether the treatment has an effect on how individuals perceive the deservingness of recipients and how much they donate in two different contexts: one where the recipient's poorness is clearly due to bad luck, and one where it can potentially be attributed to personal responsibility.

We hypothesize that the treatment, which emphasizes the high productivity of effort in the production process, will change the attribution of failure between innate ability and effort in the task

[13] With the gifts they had delayed in the time preference task and the gifts they may have earned in the real effort task, many students had a considerable number of gifts at their disposal.

context. Still, it may be that the treatment group has higher earned income because of skill accumulation, and/or they may value their earned income differently than the control group. Implementing the luck context along with the effort context helps us control for these potential effects, by observing the differences in the luck vs. effort contexts within treatment and control. In particular, the difference in responsiveness to context across treatment status (difference-in-difference) allows us to identify how the views on the role of effort in success affect donations, purged of differences in potential earned income effects.

4 Data and Results

We have data on over 1300 students from 16 schools (42 classrooms), where 8 are assigned to treatment, and 8 to control. Table 1 Panel 1 shows the balance of some baseline variables across treatment status. As can be seen in the table, no significant difference is detected in any of the variables with the exception of the baseline Turkish test score. It appears that the average score of the Turkish test is about 0.23 standard deviations lower in the treatment group. Such differences may have emerged by chance and their effect on the estimated treatment effects can be eliminated by using unbalanced variables in the regressions to estimate treatment effects.

Panel 2 in the table presents the balance of some outcome variables measured in the first week visit. This panel, as will be discussed later again below, shows that the program had no impact on time preferences, donation behavior out of windfall and earnings from the real effort task. It also establishes the fact that measured ability in the real effort task prior to the one-week practice period is balanced across treatment status. We will revisit and make use of these results later in our analysis.

In order to test the null hypothesis that the treatment had no impact on the responsiveness to the donation context, we estimate the following empirical model using the second week's donation behavior as the outcome variable:

$$y_{ij} = \alpha_0 + \alpha_1 Treatment_j + \alpha_2 Effort_{ij} + \alpha_3 TreatmentXEffort + X_{ij}\gamma + \varepsilon_{ij} \tag{1}$$

where the dependent variable y_{ij} is a dummy variable which equals 1 if student i in school j chose to donate (out of earned wealth) in week 2. The estimated coefficient of the interaction $\hat{\alpha}_3$ is the coefficient of interest, the diff-in-diff estimate that captures the differential response to the donation context due to the treatment. Estimates are obtained via logit regressions when the outcome considered is binary (donating or not). When we estimate the treatment effect using levels and fractions, we use tobit regressions due to the large number of zeros in the outcome variables. In all empirical analyses where we estimate treatment effects, standard errors are clustered at the level of the school, which is the unit of randomization.

Before we move on to estimating treatment effects, we provide an analysis of giving in the control sample. This analysis will i) place our work in the literature by providing new evidence on the determinants of giving in childhood, ii) set the benchmark for giving, against which the treatment is hypothesized to operate on, iii) help us identify potential mechanisms through which effects are achieved.

4.1 Giving in the Control Group

We first note that about 65.5% of the students in the control group stated their willingness to donate some amount out of their 4 small gifts in the first week. And conditional on giving, about 41% of the windfall is given. These values are strikingly consistent with what has been found in the lab environment (Engel (2011), Cooper and Kagel (2016)). Table 2 presents the determinants of giving out of windfall (column 1 and column 2) and giving out of earned wealth (columns 3-5) in the baseline. It appears that impatience is a significant predictor of giving out of windfall, with one additional token allocated to the earlier date lowering the probability of giving by 4.3 percentage points. This is consistent with Angerer et al. (2015), who show that patience increases donations. Notice that the more impatient kids have higher wealth "today", and if giving is a normal good, wealth effects would predict more giving. That said, the discounted total wealth of more patient kids is higher. While impatience remains as a significant predictor, risk tolerance emerges as a significant determinant when we consider the fraction of the endowment given (see column 2). This is also consistent with Angerer et al. (2015). It could, however, also be that more risk tolerant kids are more certain of receiving the payoffs from the time task in the next visit and therefore feel more wealthy, and more able to give more. Interestingly, we estimate no relationship of giving out of windfall with cognitive ability and we estimate no gender difference in this type of giving behavior.

The results are slightly different for the second week donation experiment where, this time, giving is out of earned wealth rather than out of a windfall. Recall that this earned wealth consists of delayed gifts from the CTB task implemented in week 1 and the gifts earned from the single-round number task. In the second week, about 35% of the students in the control group stated their willingness to donate some amount, which is a much lower proportion than ın the windfall experiment. The observed reluctance to give out of earned wealth relative to out of windfall is consistent with the literature mentioned earlier[14]. A number of findings are worth noting in this table. First, while we estimate no gender difference in giving out of windfall, we find that boys are about 8 percentage points less likely to donate than girls out of their earned wealth. Second, the single most important predictor of the second week donation is the donation made in the first week. One extra gift given out of a windfall in the first week is associated with about a 12 percentage-point increase in the propensity to give out of earned wealth. The strong positive correlation suggests that second week donations are not crowded out by the first week donation. Finally, although this does not hold true for the probability of giving, cognitive ability seems to be negatively correlated with the amount of giving as well as the fraction of wealth given. Interestingly, total wealth is not a significant predictor of giving behavior (see columns 2 and 3), and consistently with this, the wealth profile of fraction of wealth given takes a declining convex shape, as shown in the estimated coefficients of total wealth and total wealth-squared.

Perhaps most importantly for the main point of the paper, we estimate no significant response to donation context in any of our measures in the control group. The estimated coefficients are in fact positive, suggesting more giving in the effort context (giving to failed children) but none of the coefficients reaches statistical significance. After establishing that there is no context response in the control group, that is, children do not give differentially based on the reason for the recipient's poorness, we now turn to examine whether the treatment has altered this behavior toward the hypothesized direction.

4.2 Treatment Effects on Giving

4.2.1 Giving Out of Windfall (Week 1)

We first analyze if the treatment had a general effect on giving prior to creating a cross-sectional variation in the wealth position of children. Figure 2 shows the proportion of givers and fraction given, conditional on giving across treatment assignment. As presented previously in Panel 2 in Table 1 as well, while 65.5% of children give in the control group, the proportion in the treatment group is 62.3% and the difference is statistically insignificant (p-value=0.55). Conditional on giving, the fraction given is 0.41 and 0.40 for the control and treatment groups, respectively. These results suggest that the treatment had no impact on general altruistic behavior as measured by a standard dictator game. This result is confirmed in Table 3, where we estimate Equation 1 and control for various baseline covariates.

[14]Note, however, that our main focus is not to compare windfall and earned wealth giving; rather, it is to compare giving to different types of recipients out of earned wealth in treatment and control.

4.2.2 Treatment Effects on Earned Wealth

Due to the incentivized nature of our aforementioned in-class experiments, most children acquired a large number of gifts, which we refer to as "earned wealth". Recall that in the first week, children received the gifts they chose to allocate to the earlier date (today bowl) in the CTB task, which amounts to a minimum of 0 and maximum of 20 quarter gifts depending on the decision. Then, they were given 4 quarter gifts as an endowment for the donation question, which amounts to a minimum of 0 and a maximum of 4 quarter gifts to be kept. Finally, they had the opportunity to earn gifts from the number game, which could result in a minimum of 0 and maximum of 16 quarter gifts depending on their choices and performances. In week 2, they received the delayed gifts from the time task (minimum of 0, maximum of 30 due to 50% interest rate) and the gifts from the number task played in the second week (a minimum of 0 and a maximum of 16). Given that we also promised everyone a one quarter participation gift in each visit, the maximum total number of gifts that could be earned in the first and the second week is 41 and 47, respectively, with a minimum of 1 quarter gift. Therefore at the time of the second week donation experiment, we have substantial cross-sectional variation in the number of gifts owned by the children.

As detailed in Alan, Boneva and Ertac (2016), the main impact of the program we evaluate is that it leads to higher ability accumulation efforts on the part of the treated children within the context of the real effort task. In particular, while there is no difference between the treatment groups in terms of task-specific ability, success rate and consequently in terms of "earned income" in the first week, treated students exhibit significantly higher success rate in the difficult version of the task in the second week. This higher success in the difficult task translates into higher earned income for the treated students. Table 4 presents the estimated treatment effects on the number of gifts obtained from the number task, gifts obtained from CTB task and total gifts in the first and the second week, as well as the grand total of all gifts received in both weeks. The first thing to note in this table is that we estimate no significant treatment effect on earned wealth in the first week. This holds true for both the rewards from the time task and earned income from the real effort task.

The two most notable findings when we look at the second week results are the following: first, treatment leads to significantly higher "earned income" from the number task, and second, its effect on total wealth is imprecisely estimated (see column 5 for the former and 6 for the latter). We are now in the position to compare the giving patterns of the treatment and control groups, which both own substantial amount experimental rewards: the mean number of quarter gifts in the second week is 22.7 and 24.5 for control and treatment, respectively. It is worth noting once again that our methodology is to analyze differences-in-differences: comparing the effort context with the luck context for both the treatment and control groups and looking at the responsiveness of giving to context allows us to control for any differences in earned income.

4.2.3 Treatment Effects on Giving (Week 2)

Table 5 presents the estimated treatment effects on giving and on context response. The estimated coefficient on the interaction dummy, which is denoted as α_3 in Equation 2, gives the treatment effect on the differential response to donation context, i.e.:

$$\underbrace{\alpha_3 = [E(y|Ef=1,Tr=1) - E(y|Ef=0,Tr=1]}_{context\ response-treated} - \underbrace{[E(y|Ef=1,Tr=0) - E(y|Ef=0,Tr=0]}_{context\ response-control} \tag{2}$$

As is clear in Table 5, giving is significantly less likely in the treatment group, but only within the effort context, meaning that the behavioral difference between the two contexts is significantly larger for the treatment group relative to the control. While the treatment has no impact on giving behavior when recipients are poor because of not having had the chance to play the games, treated children are significantly less likely to give when the recipients are poor because they failed at the real effort task. The fact that the treatment has no impact within the luck context is actually consistent with week 1 windfall results: It appears that the program did not affect general altruistic behavior toward a generic recipient who is perceived to be unlucky.

The estimated coefficient of the interaction dummy is sizable and statistically significant both for the probability of giving and the fraction of wealth given. It is still large and negative but does not reach statistical significance for the amount given. Treated students are estimated to be about 10 percentage points less likely to donate than students in the control group, when the recipients are those who failed at the real effort task. They also donate a significantly smaller fraction of their wealth (about 40% less than the control group).

Overall, we estimate a significant treatment effect on the way that giving responds to the donation context in terms of the reason behind the recipient's poorness. While children in the control group do give similarly in both luck and effort contexts, treated children exhibit a significant sensitivity to the reason behind the recipient's poorness.

4.3 Treatment Effect Heterogeneity

Before setting out to explore the potential mechanisms behind our main result, we ask whether the effect we estimate on context response is heterogeneous across some dimensions. The first dimension to explore is gender. A major strand of the literature on gender and economic behavior focuses on gender differences in other-regarding behavior. While overall women seem to be more generous in dictator games (Engel (2011)), gender differences tend to be context-specific and indeed, women are found to be more responsive to the social context in giving (Croson and Gneezy (2009)). Given this evidence, it is interesting to analyze whether treated women will give less in general, and respond more to the manipulation of the reason for the recipient's poorness. We find no significant heterogeneity either in overall treatment effects on giving or in the treatment effect on context response with respect to gender.

Another dimension to explore is cognitive ability. A positive relationship between cognitive skills and social preferences has been documented in the literature (Chen et al. (2013), Angerer et al. (2015), Houser and Schunk (2009)). Apart from influencing giving behavior, cognitive skills may potentially influence the response of donations to the giving context. In the control group, we do not find any evidence of differential response to context by cognitive ability levels. However, if the ideas put forward by the treatment are likely to be internalized more easily and more strongly by cognitively more able children–somewhat akin to the idea that soft skills can be more easily acquired if cognitive ability is higher, we might expect a stronger influence of the treatment on children with higher levels of cognitive ability. Put differently, if an aspect of the program indeed influences giving patterns, and this aspect is more likely to be internalized by children who are more able, treatment effects may be heterogeneous along the cognitive ability dimension.

In order to understand whether the treatment effect on the responsiveness to context is moderated by cognitive ability, we estimate Equation 1 for different ability quartiles: those who are lower than the 25th percentile, those who are between 25th and 75th and, those 75 and higher[15]. Panels 1 and 2 in Figure 3 depict the estimated coefficients (marginal effects) of the interaction term, which captures the difference in context response across treatment status, for the probability of donating and fraction donated, respectively. We see a remarkably monotonic increase (in absolute value) in the size and the precision of the interaction effect (context responsiveness) for both outcomes. It is clear from this picture that the higher the cognitive ability, the stronger the treatment effect on context response. Next, we will try to understand the mechanism behind these results with the help of a theoretical

[15]Estimation of quantile treatment effects and estimation of average treatment effects based on smaller intervals such as 5, were not possible due to sample size issues and the high proportion of zeros.

model as well as our data.

5 Plausible Mechanisms of the Response to Context

Which aspect of the treatment was effective in making treated children give much less to failed children without affecting giving in other contexts (windfall giving and giving out of earned wealth when the recipient is unlucky)? Although somewhat far-fetched, an intervention of this sort, by emphasizing personal achievement, may directly change altruistic preferences and generally reduce how much a person considers others' well-being. The fact that we do not detect any treatment effect in the luck context or in the context of giving out of windfall does not square well with this explanation.

Another explanation may be that treated children may now be attaching higher value to a given level of earnings than similar control students, since they value their own effort differently. However, the luck context provides a checkpoint for this explanation. If the treated children as a group attach more value to their earnings because they value their own effort more or are more likely to have put more effort themselves, they should be more reluctant than the control group to give also to those who are unlucky, but this is not the case[16].

Given these findings and the nature of the intervention, our preferred mechanism is that the treatment changes beliefs regarding the role of effort versus innate intelligence in achieving success. The treatment aims to instill an optimistic worldview where intelligence is to a great extent malleable and success (and all the benefits it brings, such as wealth) can be achieved by hard work and effort. An individual with such a worldview will have a different interpretation of the recipient's failure in the effort context. Specifically, a child who believes that success can be achieved with enough effort is more likely to view a recipient who had the chance to study but still failed as personally responsible for his/her own poorness, whereas such responsibility is absent in the luck context. These views would then lead to lower donations to failed children in the effort context on the part of treated children, as compared to the luck context.

Suggestive evidence supporting this mechanism comes from the surveys, where students are asked, before and after the treatment, about their beliefs regarding the malleability of innate ability and what can be achieved with effort. We construct a standardized score by combining these questions. The estimated treatment effect on this measure, controlling for the pre-treatment score is 0.31 standard deviations with p-value=0.001. Panel 1 of Figure 4 shows the strong treatment effect on beliefs in visual clarity. We next look at whether the heterogeneity in the treatment effect with respect to cognitive

[16]It should also be noted that in the control group, there are no differences between successful and unsuccessful children in terms of their response to recipient type.

ability is also reflected in the beliefs regarding the malleability of ability. Given that the treatment effect on the differential response to the donation context was found to be stronger for children with high cognitive ability, it may be that the malleability ideas advocated by the program may also have been internalized better by students who learn faster, i.e., those who are cognitively more able. Figure 4 Panel 2 depicts the estimated treatment effects (standard deviation effects) across cognitive ability quartiles. While we do not find statistically heterogeneous treatment effects on malleability beliefs across cognitive ability scores, the figure, which essentially mirrors Figure 3, suggests larger and more precise average treatment effects on beliefs as cognitive ability increases. In light of these results, our conjecture is that treated children who now believe that success in the task could be achieved by working over a week are less likely to feel sympathy towards those who failed at the task. That is, failure in the task may now be interpreted differently across treatment groups.

We now use a theoretical framework to show how the belief mechanism we contemplate may generate the results we obtain. To do this, we use the model of other regarding preferences in Dellavigna, List and Malmendier (2012). Individual preferences are defined over experimental wealth W:

$$U(g) = u(W_i - g_i) + \kappa_i v(g_i) \tag{3}$$

where g_i is the amount donated by individual i to a cause. Parameter κ captures the strength of the altruistic motive and it is heterogeneous, with a lower bound of zero[17]. Given the parameter κ, the amount of wealth and the functional form of the functions u and v, individual i decides on the amount of donation g_i that maximizes $U(.)$.

Given this preference structure, we postulate that the parameter that captures the strength of altruism may be a rather complex function of several variables:

$$\kappa_i = f^i(e_i\mathbf{I^i}, e_j\mathbf{I^j}, \mathit{\Theta}_\mathbf{i}, \varepsilon_\mathrm{i}; \Psi)$$

where vector $\mathit{\Theta}$ represents factors such as culture, religion, personal history, family background etc. (expected to be balanced across treatment status in our sample). In this specification, the first two terms pertain to individual $i's$ sense of fairness. The first argument e_i represents the effort she exerted to earn her wealth, where the indicator $\mathbf{I}^i = 1$ when her wealth is earned via effort and $\mathbf{I}^i = 0$ when her wealth is a result of a windfall. The second fairness argument e_j represents the effort recipient j exerted to earn her wealth from the point of view of individual i. The indicator $I^j = 0$ if it is not possible to earn wealth for j, corresponding to luck context in our experiment. Finally, ε captures all other

[17]We rule out negative κ, which captures spite, since we do not measure it in our experiments.

omitted factors and Ψ denotes the parameter vector. It follows easily that our windfall experiment implies:

$$\kappa_i = f^i(\Theta_{\mathbf{i}}, \varepsilon_{\mathrm{i}}; \theta)$$

and the intervention may be expected to change parameter vector θ. Given that we do not estimate a significant effect of the treatment on giving, we conclude that the treatment had no impact on how these underlying factors affect giving out of windfall.

It also follows that in the second week all subjects earn their wealth so that the only difference across the two donation contexts is the fact that in the effort context $I^j = 1$ and in the luck context $I^j = 0$. In both cases, it is possible that treatment may have changed the way "earned income" affects altruism (parameter change) or effort to earn income is valued differently across treatment status. For example, given that more students were successful in the effort task in the treatment due to more studying, the treatment group's sense of entitlement may have increased. If this was the case, however, we would estimate a significantly different giving across treatment and control in the luck case as well as in the effort case (less giving in treatment). We do not.

Now, we explore the possibility that the treatment may have affected the sense of fairness through the recipient's perceived effort (which is an input into the assessment of the recipient's worthiness). To articulate this further, we first calibrate the model presented in Equation 3. Given that we observe W and g for everybody, we can reverse-engineer the distribution of κ for each donation context, assuming a functional form for u and v. For the luck context the altruism parameter is $\kappa_i = f^i(e_i\mathbf{I^i}, \Theta_i, \varepsilon_{\mathrm{i}}; \Psi)$ and for the effort context it is $\kappa_i = f^i(e_i\mathbf{I^i}, e_j\mathbf{I^j}, \Theta_{\mathbf{i}}, \varepsilon_{\mathrm{i}}; \Psi)$. We calibrate the cross-sectional distribution of κ using our data from the two donation contexts and assuming that u and v are concave functions of their arguments such that $u = \sqrt{W-g}$ and $v = \sqrt{g}$. Figure 5 depicts the distribution of κ for both contexts and tests the equality of distributions across treatment and control within each context (Panel 1 depicts effort context, Panel 2 luck context). It is clear from the figure that while the distributions are statistically the same across treatment status in the luck context, they are significantly different in the effort context, suggesting that in accordance with the posited model, treatment may have affected giving through its effect on the perception of the recipient's effort.

Now assume that the donor cares about whether the recipient works hard or not, that is, whether he/she is deserving based on the perceived study effort in the given study period. Abstracting from all other arguments:

$$\kappa_i = f^i(e_j, \varepsilon_{\mathrm{i}}). \tag{4}$$

where again, e_j is the investment effort of recipient j as perceived by donor i and κ_i is a decreasing function of e_j. Here, effort is considered as the "study/investment effort" exerted in the given one week period. Of course, like us experimenters, donors cannot directly observe e_j, but infer it based on their own perceived production function.

Using the simplest production function, assume that the number of pairs found in week 2 is generated via following simple production function:

$$y_i = a_{2,i} + \epsilon_i \tag{5}$$

where y_i is the number of correct pairs found by i, a_2 is her task ability in week 2, and ϵ is iid random shocks to her productivity.[18] Assume that ability in the context of our real effort task is malleable, so that practicing in the allotted time of one week can enhance it. Week 2 ability follows a simple accumulation technology:

$$a_2 = a_1 + \lambda e \tag{6}$$

where a_1 is week 1 ability (measured before subjects knew nothing about the task), e is unobserved investment effort (studying) and parameter λ is the marginal productivity of investment effort. We conjecture that the intervention altered the perceived marginal productivity of investment effort by instilling the idea that ability can be enhanced via effort so that (using the potential outcomes framework):

$$\lambda_{i,d=1} > \lambda_{i,d=0}$$

where d indicates the treatment status of the subject. For a child who does not believe that his ability can be improved, $\lambda = 0$.

It follows that the perceived production function of recipients (denoted as j) from the point of view of donor i is

$$y_j = a_{1,j} + \epsilon_j + \lambda_i e_j \tag{7}$$

In the context of our experimental task, when y_j reaches a threshold, the recipient succeeds. Upon learning the recipient's failure, the donor then updates her beliefs about $a_{1,j}$ and e_j, given her own perceived production function.

We now illustrate how a treated child would interpret the recipient's failure differently than a control child, using a discretized version of this model. For simplicity, we assume that the recipient's true ability a can be high or low, and her effort ej can be high or low. That is, $a_j \epsilon \{a_L, a_H\}$ and $e_j \epsilon \{e_L, e_H\}$. Let the prior probability of the recipient having high ability be p, and the prior probability

[18]We assume that the true marginal productivity of ability and effort are the same for all i.

that a given recipient put high effort is α. Assume, for simplicity, that control children believe ability to be solely responsible for the outcome and effort to play no role in success (this would correspond to $\lambda_i = 0$ in the above continuous formulation). That is, perceived success probabilities for control children are as follows:

$$Pr\left(Success|a_H, e_H\right)_C = 1, Pr\left(Success|a_H, e_L\right)_C = 1, Pr\left(Success|a_L, e_H\right)_C = 0, Pr\left(Success|a_L, e_L\right)_C = 0$$

Treated children, like control children, believe that a high effort-high ability combination will surely lead to success and low-effort low ability will lead to failure. In contrast to untreated children, however, they believe that someone with low ability can succeed with high effort and someone with high ability but who puts low effort may also fail.[19] That is,

$$Pr\left(Success|a_H, e_H\right)_T = 1, Pr\left(Success|a_H, e_L\right)_T = q, Pr\left(Success|a_L, e_H\right)_T = \mu, Pr\left(Success|a_L, e_L\right)_T = 0$$

Given this structure, in response to the observation of failure in the task (zero wealth, $W_j = 0$), we have the following posterior probabilities for the 4 ability-effort combinations for untreated children:

$$Pr\left(a_H, e_H|W_j = 0\right) = 0, Pr\left(a_H, e_L|W_j = 0\right) = 0, Pr\left(a_L, e_H|W_j = 0\right) = \alpha, and Pr\left(a_L, e_L|W_j = 0\right) = 1-\alpha$$

In contrast, treated children have:

$Pr\left(a_H, e_H|W_j = 0\right) = 0$,

$Pr\left(a_H, e_L|W_j = 0\right) = \frac{(1-q)p(1-\alpha)}{(1-q)p(1-\alpha)+(1-p)(1-\alpha)+(1-\mu)(1-p)\alpha}$,

$Pr\left(a_L, e_H|W_j = 0\right) = \frac{(1-\mu)(1-p)\alpha}{(1-q)p(1-\alpha)+(1-p)(1-\alpha)+(1-\mu)(1-p)\alpha}$,

$Pr\left(a_L, e_L|W_j = 0\right) = \frac{(1-p)(1-\alpha)}{(1-q)p(1-\alpha)+(1-p)(1-\alpha)+(1-\mu)(1-p)\alpha}$,

Given this, for an untreated child, the posterior probability that the recipient's effort is low given failure is:

$$Pr\left(e_L|W_j = 0\right)_C = Pr\left(a_L, e_L|W_j = 0\right)_C + Pr\left(a_H, e_L|W_j = 0\right)_C = 1 - \alpha$$

That is, observation of the recipient's failure is completely uninformative about the recipient's

[19] The probabilistic nature of the outcome in the case of high ability-low effort and high effort-low ability captures the possibility that there are idiosyncratic shocks to the production process.

effort, since from the perspective of the untreated child, success is solely driven by ability. Therefore, the prior probability for effort is unchanged.

In contrast, for a treated child, the posterior probability that the recipient put low effort given failure is:

$$Pr\left(eL|W_j=0\right)_T = Pr\left(aL, eL|W_j=0\right)_T + Pr\left(aH, eL|W_j=0\right)_T = \frac{(1-q)p(1-\alpha)+(1-p)(1-\alpha)}{(1-q)p(1-\alpha)+(1-p)(1-\alpha)+(1-\mu)(1-p)\alpha}$$

The probability of the recipient having put low effort is always higher for a treated child (see Appendix for the proof). In fact, the difference between the low effort perceptions of treated and untreated children is increasing in μ and decreasing in q (see Appendix). This suggests that as the probability of success of a low-ability recipient that puts high effort increases in the donor's mind, the more likely she is to attribute the recipient's failure to low effort. And as μ goes to 1, treated children believe that the recipient must surely have put low effort. Similarly, the higher the probability of success of a high-ability recipient that puts low effort in the donor's mind, the less likely she is to believe that the failed recipient put low effort.

Invoking the assumption we made earlier that

$$\frac{\partial \kappa_i}{\partial e_j} = \frac{\partial f^i(e_j, \varepsilon_\mathrm{i})}{\partial e_j} > 0$$

it follows that the treated students would be less willing to give to failed recipients. The ability attribution is also intuitive and given in the appendix.

This simple model explains why treated and untreated children end up with different views about the effort the recipient must have put, when they observe a failure. Untreated children, who believe that effort does not help, conclude that regardless of the effort the recipient might have put, the outcome reflects the underlying (Week 1) low ability, which can be thought of as another component of bad luck. This can explain why control children do not respond to context: if effort has a small role in the outcome, not having enough ability and not having the chance to play the game are both similar components of bad luck that lead to poorness. From the perspective of a treated child, however, failure is informative about effort (the lack thereof, rather) and initial ability matters much less, since treated children believe that whatever the underlying ability was, the recipient could have enhanced it by practicing. That is, since the perceived marginal productivity of investment is higher for the treated donor (as we observe in reported beliefs about the malleability of ability and the role of effort), the perceived investment effort of recipient j is lower from the perspective of treated donor i.

6 Conclusion

This paper provides causal evidence that personal views about what generates success and wealth influence the altruistic patterns of individuals. We evaluate the effects of a unique educational program that aims to impart to children the mindset that ability is malleable, and that in almost every task, it is possible to achieve success if one puts sustained effort. Against the background of a real-effort task that allows children to practice and build skill over a one week period, we implement our main altruism experiment, which manipulates the reason for the recipient's poorness in a random manner: not having had the chance to play the games at all (bad luck) or having failed in the real-effort task despite a chance to practice (due potentially to a lack of effort, or low ability).

We find that while there is no difference between treated and untreated children in a context where the recipient is poor due to bad luck, treated children are more reluctant to donate to recipients who did the task and failed. We explain this result through a mechanism that postulates changes in the perceived production technology on the part of treated children. Survey evidence indicates that the program, which has been shown to have significant positive impact on goal-setting and skill accumulation, indeed changes children's views about the role of effort versus innate ability in performance and success. A treated child, who has a more positive belief about the productivity of effort than an untreated child, is more likely to attribute a failure outcome to insufficient effort rather than to low innate ability. Perceiving a bigger personal responsibility behind the failure, the treated child would then be less willing to donate to the failed recipient. We provide a simple model and calibration exercise that are consistent with this mechanism.

It is well-known that worldviews about whether the poor can rise out of poverty with enough effort differ across countries, and differences in these views are correlated with differences in redistributive policies. Our paper provides causal evidence that educating children to believe in the power of effort for achieving success will not only make them work harder themselves, but also will affect their views of and sympathy towards others who have been unsuccessful, and thereby their desire for redistribution. The optimistic mindset that highlights individual control over outcomes through effort means that failure will also be the responsibility of an individual. However, such a change in the worldview does not affect giving when the recipient is known to be poor due to bad luck.

In addition to showing that other-regarding preferences are malleable in childhood, the results have important implications for educational policies. Recently, it has become a clear objective for policymakers to improve non-cognitive skills starting from early education. Building an achievement mindset, praising effort rather than outcomes, and downplaying the role of innate ability to promote grit and tenacity are a major focus of such efforts on the part of schools and families alike. Our results show that such policies, through changing the interpretation of what generates wealth, may have effects on the patterns of altruistic behavior as well. Given the literature that shows the equilibrium link between citizens' worldviews and redistributive policies, these policies, if widely implemented, may have effects on the macroeconomy in the longer run.

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Tables

Table 1: Baseline Variables and Week 1 Balance

PANEL 1: Baseline Variables	Control	Difference
Male	0.49	0.03
		(0.02)
Age	9.45	0.01
		(0.04)
Raven Score (normalized)	0.059	-0.20
		(0.12)
Math Ability (normalized)	-0.05	0.11
		(0.09)
Language Ability (normalized)	0.10	-0.23**
		(0.11)
Risk Tolerance	2.13	0.07
		(0.22)
Wealth (teacher reported)	2.63	0.31
		(0.31)
Malleability Score (normalized)	-.03	.06
		(.07)
PANEL 2: Week 1 Outcomes		
	Control	Difference
CTB Early Allocation	2.18	0.01
		(0.23)
% Give out of Windfall	66	-0.03
		(0.05)
Amount Given out of Windfall	1.08	-0.09
		(0.11)
Effort Task Ability	3.74	0.24
		(0.19)
Effort Task Earnings	3.65	-0.34
		(0.47)

Note: Each row reports coefficients from a regression of the variable shown in the first column on the treatment dummy. The first column reports the mean of the control, the second one reports the difference between the treatment and control. Panel 1 presents the balance for demographic variables and baseline attitudes either reported by the child or the teacher. The variable "Malleability Score" is an extracted factor from questionnaire items in the pre-treatment student survey. The Raven score is measured using a progressive Raven's matrices test (Raven et al., 2004). Risk tolerance is elicited using the incentivized Gneezy and Potters (1997) task. The student's wealth is reported by the teacher (1-5 scale). Panel 2 presents the balance for i) early allocation in CTB task, ii) the proportion of students who donate out of windfall in week 1, iii) amount donated out of windfall in week 1, and iv) amount of earning from real effort task in week 1. Standard errors, obtained via clustering at the school level, are reported in parentheses.

Table 2: Giving in the Control Group

	Windfall Giving		Income Giving		
	(1) Probability	(2) Fraction Given	(3) Probability	(4) Amount Given	(5) Fraction Given
Male	-0.017	0.006	-0.103***	-0.536***	-0.022***
	(0.03)	(0.01)	(0.03)	(0.20)	(0.01)
Raven-IQ Score	-0.013	-0.003	0.004	-0.160**	-0.007*
	(0.02)	(0.01)	(0.02)	(0.07)	(0.00)
Risk Tolerance	0.002	0.008***	-0.008	-0.068	-0.002
	(0.01)	(0.00)	(0.01)	(0.07)	(0.00)
Age	-0.018	-0.001	0.016	0.161	0.006
	(0.03)	(0.01)	(0.02)	(0.26)	(0.01)
Number of Early Tokens in CTB	-0.035**	-0.010*	-0.014*	-0.137	-0.010**
	(0.02)	(0.01)	(0.01)	(0.10)	(0.00)
Math Ability	-0.018	-0.002	0.004	0.007	0.005
	(0.02)	(0.01)	(0.02)	(0.11)	(0.00)
Language Ability	0.018	0.005	0.013	-0.050	-0.002
	(0.03)	(0.01)	(0.03)	(0.15)	(0.01)
Effort Context			0.018	0.146	0.011
			(0.03)	(0.22)	(0.01)
Malleability Beliefs			0.050**	0.286**	0.011***
			(0.02)	(0.12)	(0.00)
Malleability X Effort Context			-0.045	-0.427	-0.015
			(0.03)	(0.31)	(0.01)
Amount of Week 1 Donation			0.132***	1.077***	0.031***
			(0.02)	(0.20)	(0.01)
Total Wealth-Week2			-0.004	-0.040	-0.008***
			(0.01)	(0.04)	(0.00)
Total Wealth-Week2-Square			0.000	0.001	0.000***
			(0.00)	(0.00)	(0.00)
Observations	687	687	652	652	652

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Treatment Effect on Donation out of Windfall-Week1

	(1) Probability of Giving	(2) Fraction Given
Treatment	-0.025	-0.012
	(0.05)	(0.01)
Male	-0.009	0.006
	(0.02)	(0.01)
Raven-IQ Score	-0.009	-0.001
	(0.01)	(0.01)
Risk Tolerance	-0.016	0.003
	(0.01)	(0.00)
Age	0.005	0.004
	(0.03)	(0.01)
Number of Early Tokens in CTB	-0.022*	-0.009*
	(0.01)	(0.00)
Math Ability	0.002	0.003
	(0.01)	(0.00)
Language Ability	0.025	0.008
	(0.02)	(0.01)
Control Mean	0.66	1.08
N	1263	1263

Standard errors in parentheses

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

Table 4: Treatment Effect on Total Wealth

	Week 1			Week 2			Week 1+2
	(1) CTB Early	(2) Earned Income	(3) Total Wealth	(4) CTB Delayed	(5) Earned Income	(6) Total Wealth	(7) Grand Total
Treatment	-0.200	-0.357	-0.549	0.300	1.896***	2.358	1.842*
	(0.98)	(0.44)	(1.07)	(1.46)	(0.58)	(1.61)	(1.00)
Male	0.271	0.839***	1.127***	-0.406	1.103***	0.006	1.574**
	(0.38)	(0.22)	(0.37)	(0.57)	(0.36)	(0.63)	(0.54)
Raven-IQ Score	-1.060***	0.309	-0.734**	1.589***	1.065***	2.393***	1.771***
	(0.21)	(0.20)	(0.32)	(0.31)	(0.23)	(0.42)	(0.36)
Risk Tolerance	0.066	-0.013	0.069	-0.099	-0.058	-0.210	-0.066
	(0.14)	(0.14)	(0.18)	(0.20)	(0.17)	(0.31)	(0.33)
Age	-1.113***	0.314	-0.820	1.670***	1.012*	2.085***	1.034*
	(0.30)	(0.31)	(0.50)	(0.45)	(0.49)	(0.70)	(0.54)
Math Ability	-0.703***	0.927***	0.205	1.055***	2.154***	3.219***	3.431***
	(0.18)	(0.17)	(0.27)	(0.27)	(0.16)	(0.28)	(0.19)
Language Ability	-0.417	0.172	-0.252	0.626	0.621***	1.835***	1.065***
	(0.34)	(0.18)	(0.43)	(0.51)	(0.15)	(0.51)	(0.22)
Control Mean	8.71	3.65	13.37	16.93	7.07	22.70	37.10
N	1268	1284	1268	1268	1274	1352	1268

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Treatment Effect on Probability of Giving-Week 2

	(1) Probability	(2) Amount Given	(3) Fraction Given
Treatment	-0.011	-0.082	-0.010
	(0.04)	(0.07)	(0.01)
Effort Context	0.024	0.062	0.009**
	(0.02)	(0.06)	(0.00)
Treatment X Effort Context	-0.092**	-0.158	-0.026**
	(0.04)	(0.10)	(0.01)
Male	-0.035	-0.024	-0.004
	(0.03)	(0.06)	(0.01)
Raven-IQ Score	0.020	-0.002	-0.006
	(0.02)	(0.04)	(0.00)
Risk Tolerance	-0.006	-0.004	-0.001
	(0.01)	(0.02)	(0.00)
Age	0.001	0.005	-0.003
	(0.02)	(0.07)	(0.01)
Math Ability	0.008	0.016	-0.001
	(0.01)	(0.03)	(0.00)
Language Ability	0.016	0.006	-0.004
	(0.01)	(0.03)	(0.00)
Amount of Week 1 Donation	0.129***	0.303***	0.028***
	(0.01)	(0.03)	(0.00)
Control Mean	0.35	0.70	0.06
N	1275	1275	1275

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figures

Figure 1: Timeline of the Field Experiment

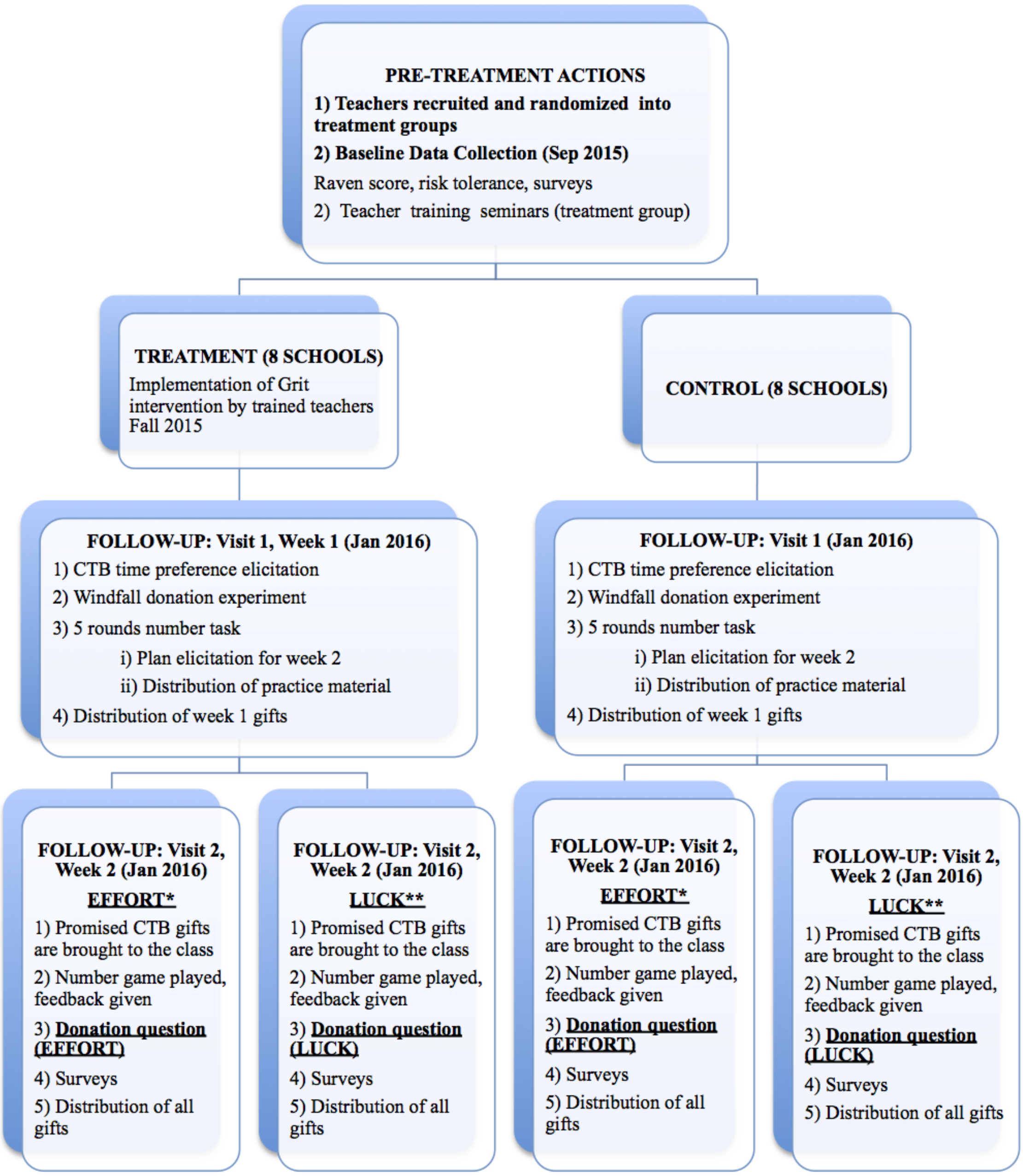


Figure 2: Giving Out of Windfall

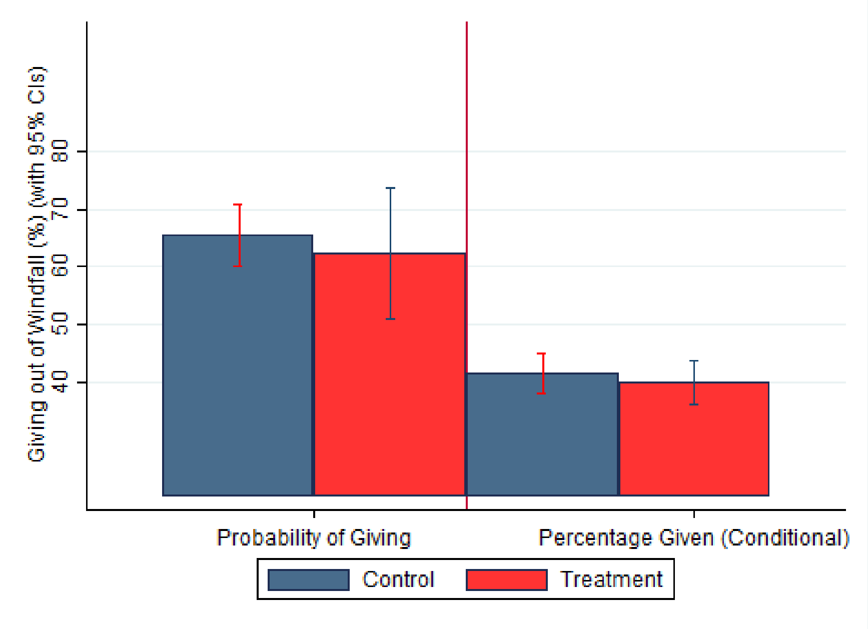


Figure 3: Heterogeneous Treatment Effect on Context Response: Cognitive Ability

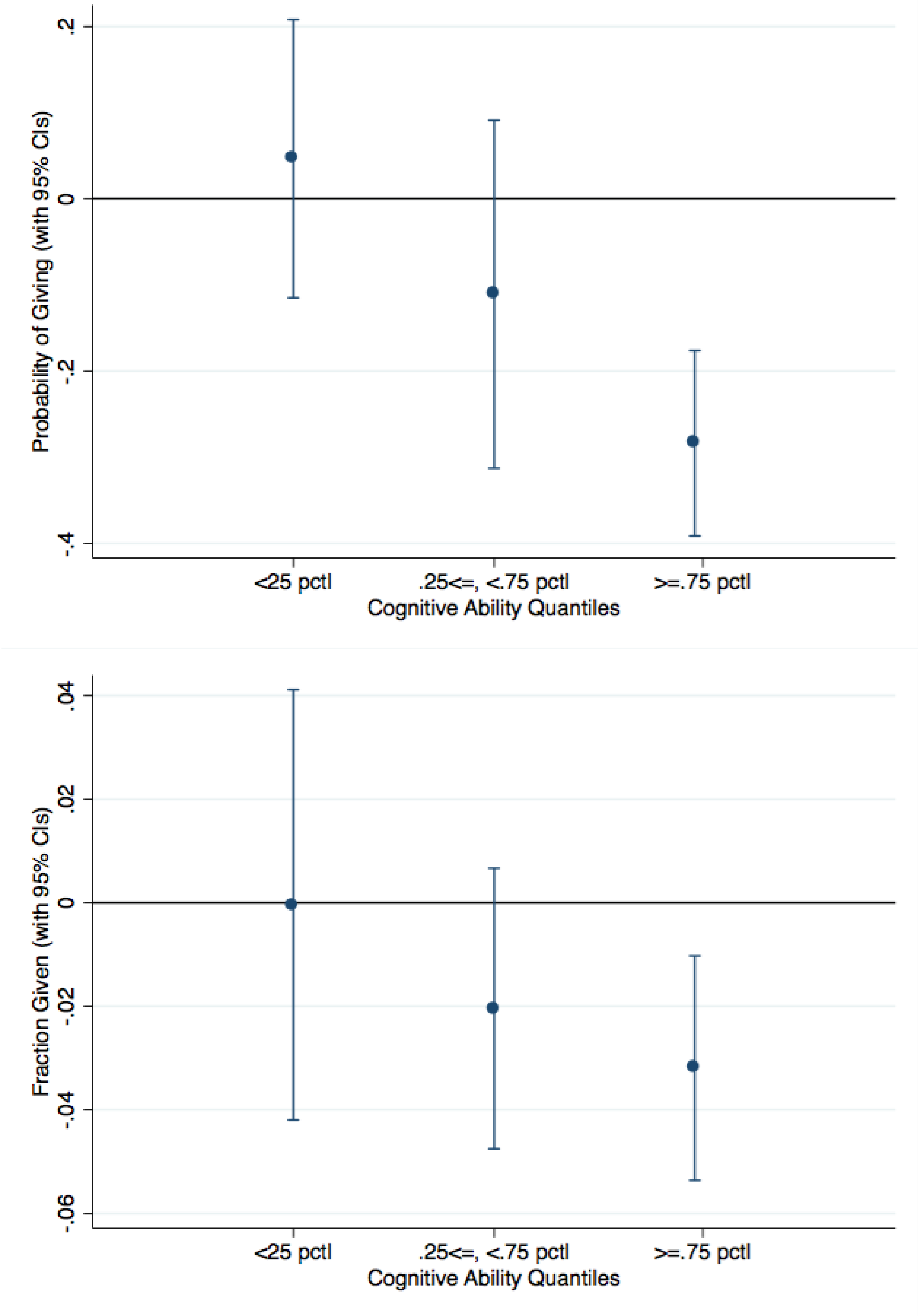


Figure 4: Treatment Effect on Malleability Beliefs

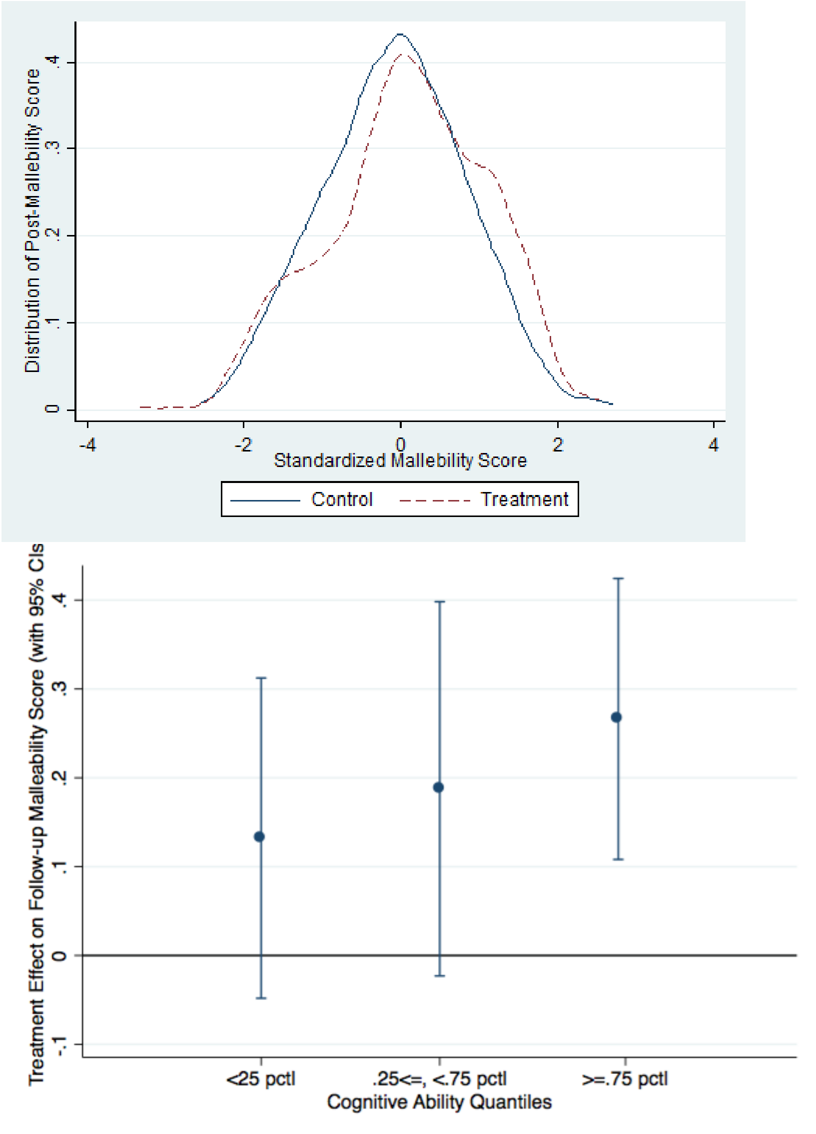
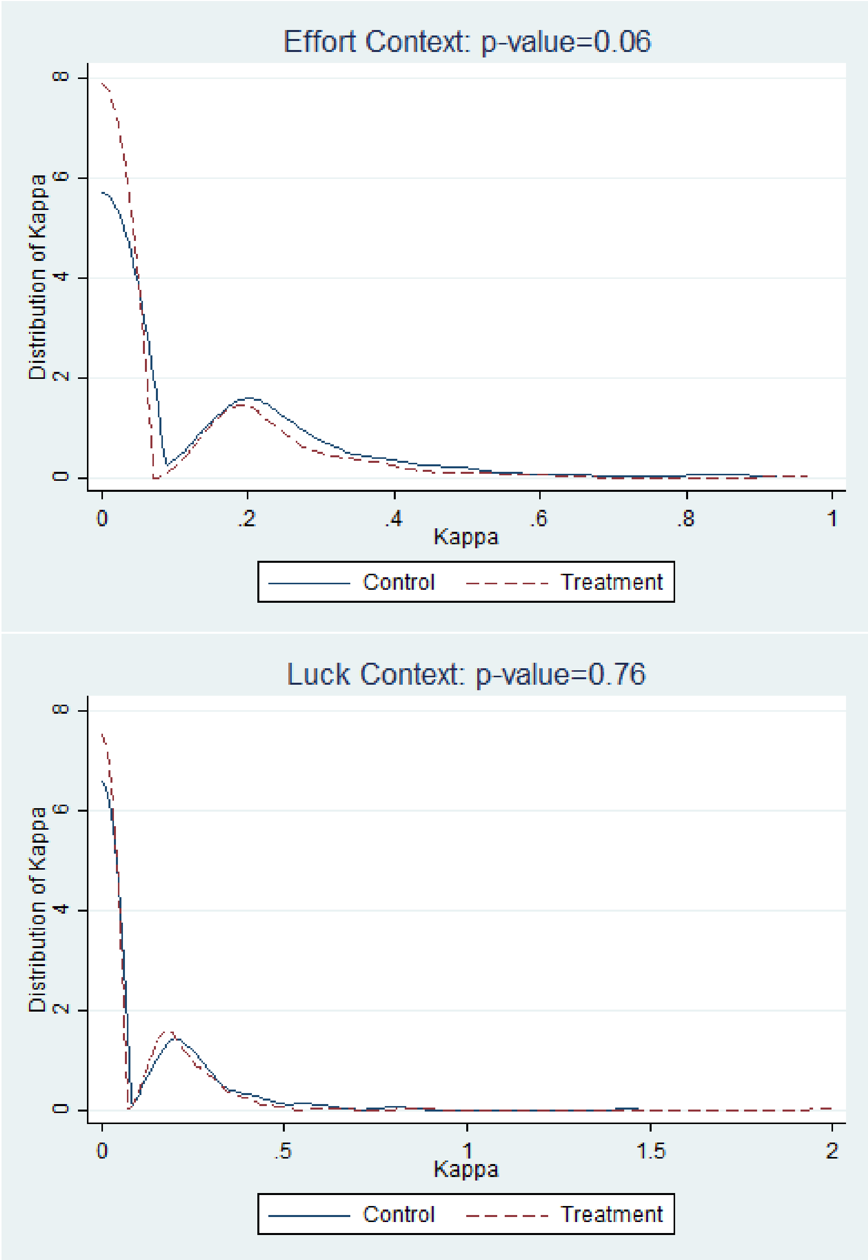


Figure 5: Cross-sectional Distribution of Implied Altruism Parameter (κ)



Appendix

Ability Attribution

Ability attribution, given the simple discretized model presented in Section 5, is as follows. Since ability is paramount in success from their perspective, untreated children believe, when they see a bad outcome, that the recipient's ability is for sure low.

$Pr\left(a_L|W_j=0\right)_C = Pr\left(a_L,e_L|W_j=0\right)_C + Pr\left(a_L,e_H|W_j=0\right)_C = 1.$

In contrast, for treated children:

$Pr\left(a_L|W_j=0\right)_T = Pr\left(a_L,e_L|W_j=0\right)_T + Pr\left(a_L,e_H|W_j=0\right)_T = \frac{(1-\mu)(1-p)\alpha+(1-p)(1-\alpha)}{(1-q)p(1-\alpha)+(1-p)(1-\alpha)+(1-\mu)(1-p)\alpha}.$

For treated children, the updated probability that the recipient has low ability is lower than that for untreated children. This is because for treated children, the failure outcome is not as informative about ability as it is for untreated children, since effort also plays a role. As q, the probability that a high ability recipient succeeds with low effort, goes to 1, the informativeness of failure on ability increases and treated children also believe that the failed child must be of low ability (in addition to having put low effort).

Donor's Perceived Low Effort of Recipient

Proposition: i) The probability that the treated children believe the recipient put low effort is higher than that of control children, (ii) the difference between the low effort perceptions of treated and untreated children is increasing in μ and decreasing in q

Proof:

$Pr\left(e_L|W_j=0\right)_T - Pr\left(e_L|W_j=0\right)_C = ((1-q)p(1-\alpha)+(1-p)(1-\alpha))/((1-q)p(1-\alpha)+(1-p)(1-\alpha)+(1-\mu)(1-p)\alpha)-(1-\alpha)>0$

$((1-q)p(1-\alpha)+(1-p)(1-\alpha))/((1-q)p(1-\alpha)+(1-p)(1-\alpha)+(1-\mu)(1-p)\alpha)>(1-\alpha)$

$((1-q)p+(1-p))/((1-q)p(1-\alpha)+(1-p)(1-\alpha)+(1-\mu)(1-p)\alpha)>1$

$(1-q)p+(1-p)>(1-q)p(1-\alpha)+(1-p)(1-\alpha)+(1-\mu)(1-p)\alpha$

$(1-q)p\alpha+(1-p)\alpha>(1-\mu)(1-p)\alpha$

$(1-q)p\alpha+(1-p)\alpha\mu>0$, which is always true.

$$Derivative\ w.r.t\ \mu = -((\alpha-1)\alpha(p-1)(pq-1))/(-\alpha\mu+p(\alpha(\mu-1)+(\alpha-1)q)+1)^2>0$$

$$Derivative\ w.r.t\ q = ((\alpha-1)\alpha(\mu-1)(p-1)p)/(-\alpha\mu + p(\alpha(\mu-1) + (\alpha-1)q) + 1)^2 < 0$$