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

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Abstract

We show that optimistic beliefs regarding the role of effort in success, while leading to success, diminish the individual's sympathy toward the unsuccessful. We generate random variation in the degree of optimism about the productivity of effort via an effective educational intervention. We find that treated children, holding significantly more optimistic beliefs, are no less likely than control to give to unlucky recipients, but significantly less likely to give to those who failed at a real effort task despite an opportunity to build skill. The results highlight possible unintended social effects of effort-focused optimism and have implications for political economy.

JEL Categories: D31, D64, I24, I28

Keywords: redistributive preferences; prosocial behavior; altruism; beliefs; fairness; field experiments

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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. 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, reciprocity and spite in dictator, ultimatum and trust games in the lab environment; 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; see Konow (2000), Cappelen et al (2007), Gill and Stone (2010). An individual's views on how one achieves success and wealth, and to what extent people are personally responsible for their financial well-being likely influence her willingness to donate to the poor, her choice of cause and her desire for redistribution in general.[2] Related to the latter, it has been shown that in societies where the majority of people believe that poverty is largely a self-imposed outcome, there tends to be less support for redistribution from the rich to the poor. In contrast, in societies where the majority perceives poverty 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), Alesina et al. (2011), Alesina and Guigliano (2009), Alesina et al. (2017) and the references therein.[3]

[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] In numerous lab experiments, the willingness to give in the dictator game declines considerably when subjects asked to 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. See also Levitt and List (2007).

[3] 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; 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). 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 Angeletos (2005)).

Underlying the worldview that poverty is largely the outcome of personal responsibility is the optimistic 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.[4] The adoption of this worldview is widely encouraged in educational settings for good reasons. There is now an emerging consensus on the need to develop grit, tenacity, and related non-cognitive skills by championing effort and instilling effort-oriented optimism in children; see Duckworth et al (2007), Heckman et al (2006), Almlund et al. (2001), Carol et al. (2016). Recent studies show that encouraging this optimistic mindset leads to higher perseverance, competitiveness, and consequently, higher achievement as measured by objective tests; see Alan, Boneva and Ertac (2016), Alan and Ertac (2017), Blackwell, Trzesniewski and Dweck (2007), Paunesku et al. (2015).[5]

In this paper, exploiting a unique field experiment, 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 continental Europe. Our setting allows us to generate random variation in the degree of optimism in beliefs about the role of effort in achievement via a unique and effective educational intervention, implemented in elementary schools. The intervention aims to impart to children the belief that ability, rather than being innately fixed, can be enhanced provided that the individual exerts sufficient effort. Using a well-designed curriculum, children's own trained teachers deliver the material and re-organize their teaching practices based on the ideas advocated by the intervention. General information on the curriculum is given in Section 2.1.

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.

[4]Max Weber, in his famous book "The Protestant Ethic and the Spirit of Capitalism", argues that the higher prosperity of Protestants stems from a particular worldview. This worldview emphasizes that achieving success is possible only through hard work and doing so is the individual's personal responsibility. Becker and Woessmann (2009) show that this worldview leads to economic prosperity through mainly better educational outcomes of Protestants.

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

This belief may affect her beliefs regarding how deserving the recipient is, and thereby how willing she is to give to that recipient.

The educational intervention that allows us to generate random variation in these beliefs is implemented in a randomized-controlled manner, in a sample of state-run elementary schools in Istanbul. It was first implemented in Fall 2014 and then replicated in Fall 2015 using an independent sample of schools for the purpose of testing the hypothesis put forward in this paper. To do this, we create an experimental setup that enables ability accumulation, using a specific real effort task with a performance target where children can practice over a one-week period. This involves physically visiting 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 opportunity to practice the task for a week at their own discretion, until the second visit. In the second visit, their success at meeting the performance target is measured again.[6]

Against this background, we implement our altruism experiment, where we manipulate 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 (gifts) 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, 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 patterns of altruism, 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 can play only a small 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 having been visited) and one where it may be due to a 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.[7]

The results show a striking difference in treated and untreated children's responsiveness to the

[6] 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 optimism regarding the malleability of ability, the educational intervention leads to higher achievement. In both randomized trials, we also estimate significantly higher math and verbal test scores for the treated children and show that the effect on math scores persists after over two years following the intervention in 2014.

[7] Eckel and Grossman (1996), Fong (2007), and Fong and Luttmer (2010) show that the recipient's worthiness is important for giving.

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 who 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 altruism 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 be successful 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 hold pessimistic views. To illustrate this, we propose a simple signal extraction model, where we show how differences in the perceived return to effort determine the response to donation context. Within the context of this model, we show that 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. Supporting this empirically, we find that the intervention indeed molded beliefs regarding the role of effort in achievement. Our formal mediation analysis indicates that about 25% of the estimated treatment effect on the response to donation context is mediated through this shift in beliefs toward more optimism.

The role of education in shaping a society's redistributive tendencies through instilling beliefs is stipulated in Alesina and Glaeser (2004). They argue that while optimistic beliefs are mainly shaped by capitalist indoctrination in the US, pessimistic European beliefs are likely instilled by teachers influenced by Marxism. Developing grit and tenacity in students by promoting goal-setting and an effort-oriented achievement/growth mindset has become an educational policy imperative in recent years, especially in the US. To this end, many policymakers, schools and organizations are now putting forward educational programs that aim to enhance these noncognitive skills through influencing the mindset of students. The US Department of Education, for example, is reportedly committed to promoting programs that enhance grit and tenacity in students (Scechtman et al. (2013), Boser and Baffour (2017)), and the Obama administration actively supported instilling a "growth mindset" in students to improve achievement (Shankar and Kalil (2013)).[8] Using a unique randomized educational

[8]There are also ongoing educational interventions across the world that are being carried out with similar objectives

intervention implemented with a similar goal, we manage to generate exogenous variation in mindset, that is, the degree of optimism in beliefs about the role of effort in success, and identify their effect on altruistic behavior. While this type of optimism is likely to indeed improve achievement outcomes, this paper is the first to show that it may also have unintended social effects. Specifically, it may alter altruistic patterns and, to the extent that impacts persist into adulthood, 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 and discusses potential mechanisms and Section 5 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 delivered 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 in any project of their choice, provided that these projects are approved by the local directorate of education. Activities proposed by these projects are implemented in allotted free hours (a maximum of 5 hours per week). In the absence of 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. It should be noted, however, that most teachers in our control group are involved in other projects (such as environmental awareness, health etc.). The educational program we evaluate in this paper received the Ministry's approval as an extra-curricular project to be implemented in state-run elementary schools in Istanbul.

The intervention is designed as a 12-week program, and recommended to be implemented for at least 2 hours per week. The program includes a novel curriculum, whose target concepts were conveyed by the authors to a large interdisciplinary team of education psychologists, a team of volunteer elementary

(e.g. Rustin (2016)).

school teachers, media artists and children's story writers. The resulting curriculum includes 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 were provided a detailed teacher kit to guide them in covering the material week by week in a structured way.

The main objective of the program is to expose children to a positive worldview 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. Along with this, children are strongly encouraged to set ambitious individualistic goals, work toward their goals, and most importantly, persevere in the face of setbacks. While the teachers were given clearly structured material to be covered 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, not only directly but also via influencing the mindset of their teachers. Teachers, while covering the curriculum in the way instructed in the teacher kit, are encouraged to internalize the ideas put forward in the curriculum in the training seminars as a general teaching philosophy. To this end, they are encouraged to review their style of giving feedback and praise, and the way they reward effort. Details of the summary curriculum and sample class activities are provided in the Appendix.

The impacts of this program on a novel 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 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. These behaviors then lead to increased willingness to exert effort to accumulate task-specific ability and consequently, higher success in the task.

It should be noted here that while we study the effect of the intervention on altruistic behavior, the intervention itself did not include any material related to giving, sharing or building empathy. Likewise, it did not involve any material that emphasized competing or doing better than others. Instead, the focus was squarely on individual achievement, and building an optimistic view on the achievement of personal success through sustained effort.

2.2 Timeline of the Field Experiment

The intervention was first implemented in Fall 2014 and then replicated in Fall 2015 using an independent sample of schools for the purpose of testing the hypothesis put forward in this paper. Figure 1 lays out the timeline of the field experiment conducted in Fall 2015. After all the paperwork 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.[9] After giving a minimal 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; however, they were told that they would be taking part in the program within the next two academic years, maybe immediately in the upcoming academic year, maybe in the following one.[10] 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 classrooms 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.[11] This meant a sample of 16 schools (42 classrooms), totaling over 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's 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.

[9] The Istanbul Education Directorate encouraged us to reach the elementary schools in lower-SES districts, where achievement concerns are greater.

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

[11] Our power calculations based on the 2014 intervention provide precise guidance for the sample size we use in this study.

As can be seen in row 2 of Figure 1, teachers were given the entire Fall 2015 term to implement the curriculum in the free hours allotted by the Ministry. As extra-curricular project activities are very common (and popular) among 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 experimental data collection strategy is composed of two visits to the classroom, one week apart. The set of tasks we implement aims to measure (1) giving to an anonymous recipient out of a windfall, (2) giving to an anonymous recipient out of earned wealth, part of which is accumulated through a real-effort task, in two different donation contexts. 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. [12]They are told that the amount of tokens they receive will depend on their decisions and performance 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

[12]These gifts are items of value to children, such as toys and stationary.

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. [13]

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 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.

The main purpose of this task is to explore whether the treatment had any impact on the time preferences of children, which is not part of the current study. What makes this elicitation relevant for the purpose of this paper is the fact that gifts received based on the decisions made constitute a significant portion of the overall experimental wealth of the children. This wealth, in turn, is relevant for all subsequent decisions, including those in donation experiments in week 1 and week 2.

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 larger reward. Put in the context of our purposes, experimental wealth generated by the time preference elicitation task in the first visit is not different across treatment status in the first week (and naturally in the second week). While 2.17 gifts on average are allocated to the earlier date in the control group, the corresponding value is 2.18 for the treated group (p-value=0.95). Incidentally, we do not reject the equality of the distributions across treatment status (p-value for Kolmogorov-Smirnov test=0.72).

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

[13] All experimental tasks were conducted with the approval of the local IRB.

we were unable to visit due to lack of time, and ask them whether they would like to donate (by writing discreetly on their decision sheets) some or none of their four gifts to the first graders in those schools.[14] The purpose of this experiment is to see if the treatment has any impact on general altruistic behavior using a version of the dictator game, a widely used measure 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.[15] 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. 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, they are given the chance to choose between the "4-gift game", which yields four (whole- token) gifts in the case of success and zero in the case of failure, and the "1-gift game", which yields one (whole-token) gift in the case of success and zero in the case of failure. Although in both games the goal is to find at least three pairs of numbers adding to 100, the 4-gift game is designed to be more difficult than the 1-gift game. In particular, in the 1-token game the grid of numbers is smaller, and the matching pairs are easier to spot.[16] In fact, the mean empirical success rate in the easy task ranges from 90% to 100% over the five rounds.

[14] 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.

[15] Please see the Appendix for instructions.

[16] See the Appendix, Figure 8 for examples of the two types of task.

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 this second visit, and that they need to decide now whether they would like to play the 4-gift (more difficult) game or the 1-gift (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 it home with them and practice. This exercise booklet and the one week period allow us to give the children an opportunity and technology for skill accumulation and help us measure the investment children make into improving their performance in the task. This is very important for our purposes, since a major component of our impact evaluation relies on being able to measure whether treated children can indeed put more effort and accumulate skill. The expectation here is that treated children, who believe in the productivity of effort, will make greater use of this booklet, and invest the time and effort necessary to practice and accumulate skill. 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. They are also informed about which game they are going to play in the second visit at the end of the first 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 are again given 1 minute and 45 seconds to find pairs of numbers that add up to 100. The game is played for only one round 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 elicitation task in the first week were brought to the classroom that day. This creates a setting where there is substantial variation in earned wealth levels.

3.5 Donation Experiment (Week 2)

Our main altruism experiment follows the feedback on the real effort task in Week 2. As shown in Figure 1, this experiment is conducted differently. First of all, children are asked to give out of their own earned wealth rather than a fixed, windfall endowment.[17] In addition to this, we change the donation context by randomly manipulating the reason for the 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 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 they would like to donate to these children. Figure 1 gives the (translated) wording of both cases. Note here that the randomization was done at the individual level. That is, in each class, a random subset of students got a booklet that contained the donation question within the "effort context", and a random subset of students got a booklet that contained the donation question within the "luck context". [18]

The donation context we experimentally manipulate here highlights 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 main contribution of the paper stems 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 pro-

[17] 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.

[18] We therefore did not provide spoken instructions for this part, but directed the children to the question on their booklet, and instructed them to read carefully and write in their choices.

duction process, will change the attribution of failure to a lack of innate ability versus a lack of effort in the task 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 the responsiveness to context across treatment status (difference-in-difference) allows us to identify how children's 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. As mentioned above, once we complete the random assignment of the teachers we visited all classrooms (both control and treatment) and collected baseline data. Among those are data on children's beliefs regarding the malleability of ability and their perceived return to effort. We construct a standardized measure, which we refer to as the "malleability score" by combining several related item-set questions.[19] Table 1, Panel 1 shows the balance of baseline variables across treatment status, including our malleability score. As can be seen in the table, no significant difference is detected in any of the variables with the exception of the baseline verbal (Turkish) test score. It appears that the average score in the verbal test is about 0.23 standard deviations lower in the treatment group. We do control for pre-treatment verbal test scores in our treatment effect regressions.

Panel 2 in the table presents the balance of some outcome variables measured in the first-week visit. This panel, as will be discussed 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. The latter is important to put our main findings in context. The fact that the success rate is higher for treated children in the second week provides evidence that treated children were more likely to seize the ability development opportunity given to them. We will revisit this and make use of the fact that initial ability in the real effort task is balanced across treatment status 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 donation behavior

[19] Translated questions are given in the Appendix.

as the outcome variable:

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

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 difference-in-difference 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 fractions (proportion of wealth given), 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. We also report permutated p-values, which account for the fact that we have a small number of clusters.

4.1 Treatment Effects on Giving

Due to the incentivized nature of our aforementioned in-class experiments, most children acquired a large number of gifts. Recall that in the first week, children received the gifts they chose to allocate to the earlier date (the today bowl) in the CTB task, which amounts to a minimum of 0 and a 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 performance. 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 a 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 2 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. The first thing to note in this table is that we estimate no significant treatment effect on any component of the experimental wealth in the first week. This holds true for both the rewards from the CTB task and earned income from the real effort task.

For the second week results, first note that treatment leads to significantly higher "earned wealth" 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). Overall, both the control and the treatment group own substantial experimental rewards: the mean number of quarter gifts in the second week is 23.4 and 25.9 for control and treatment, respectively. We now turn to comparing the giving patterns of the treatment and control groups and estimating the treatment effects on giving. 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 wealth.

4.1.1 Giving Out of Windfall (Week 1)

We first analyze whether the treatment had a general effect on giving, when there is no variation in the type of recipient and all recipients are just children in another school that was not visited. Figure 2 shows the proportion of givers and the fraction given for each treatment status. As presented previously in Panel 2 in Table 1 as well, while 66.6% of children give in the control group, the proportion in the treatment group is 64.4% and the difference is statistically insignificant (p-value=0.69). The fraction given is about 0.27 for both the control and treatment groups (p-value=0.98). These results suggest that the treatment had no impact on general altruistic behavior as measured by a standard dictator game.

4.1.2 Treatment Effects on Giving (Week 2)

Table 3 presents the estimated treatment effects on giving for the two contexts separately. Note first that giving out of earned wealth is much less than giving out of windfall (Week 1), with 36% of the children in the control group donating at least one gift in the effort context. The figure is 33% for the luck context.

We estimate that treated children are significantly less likely to give than those in the control group, but only within the effort context. Specifically, treated students are about 11 percentage

points less likely than those in control to give to a recipient who failed at the task. This amounts to an approximately 30% decline in giving due to treatment. We estimate no statistically significant treatment effect in the luck context. The behavioral difference between the two contexts is significantly larger for the treatment group relative to the control (difference-in-difference estimate of about 10 percentage points with the permutated p-value of 0.006). While the treatment has no impact on giving behavior when the recipient is poor because of not having had the chance to play the games, treated children are significantly less likely to give when the recipient is poor because he/she failed at the real effort task. The fact that the treatment has no impact within the luck context is actually consistent with the 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.

Table 4 presents the results for the fraction of wealth donated. Note first that unconditional means are very low, with only about 6 to 7% of gifts being donated in the control group. Consistently with the results on the probability of giving, here we also see that treated students donate a significantly lower fraction of their wealth, albeit, only in the effort context. When the recipients are poor because they failed at the task, the proportion of wealth donated is about 50% lower for the treated students. Again, no statistical difference is detected in the luck context. The behavioral difference between two contexts is again significantly higher for the treatment group (difference-in-difference estimate of about 3 percentage points with the permutated p-value of 0.004).

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 give similarly in both the luck and effort contexts, treated children exhibit a significant sensitivity to the reason behind the recipient's poorness. We will devote the rest of the paper to exploring the mechanisms that might have generated these results. To do this, we first analyze behavior in the control group. We then provide a formal mediation analysis where we propose beliefs about the malleability of ability as a mediating factor. Finally, we contemplate a theoretical framework that would yield implications consistent with our results.

4.1.3 Plausible Mechanisms of the Response to Donation 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)? Our proposed 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 who holds such beliefs might 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.

Before moving onto providing more formal analyses of our proposed mechanism, we consider some alternative channels. 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. This is exacerbated by the fact that the proportion of "earned income" is significantly higher for the treated children. 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 out of their earnings also to those who are unlucky, but this is not the case.[20]

The first piece of evidence that supports our proposed channel comes from the fact that beliefs regarding the role of effort in success indeed have an impact on context response in the control group. Table 5 presents the relationship between malleability beliefs and context response. Here, we do see that while maintaining these beliefs generally is positively related to giving, the stronger the beliefs in hard work, the stronger the context response. This is particularly pronounced in the probability of giving. Figure 3 presents the linear predictions from these regressions. As clearly depicted in the figures, as beliefs about the malleability increases, giving to a failed recipient declines.

Given these findings and the nature of the intervention, we are inclined to believe that a non-trivial portion of the estimated treatment effect on context response is likely mediated by the shift in beliefs caused by the treatment. In what follows, we will use our pre- and post-treatment malleability scores to provide further evidence to that effect. Serving as a backdrop, Figure 4 shows the strong treatment effect (a visible location shift) on malleability beliefs (controlling for pre-treatment beliefs) toward more optimism. Specifically we estimate a 0.33 standard deviation effect on this measure (p-value=0.001).

[20]We also conducted a formal mediation analysis (explained in the next subsection) using the proportion of earned income as a mediating factor and found that this variable does not mediate our treatment effects.

4.2 A Mediation Analysis

We provide two formal mediation analyses, both of which require usual strong assumptions. The first analysis assumes that the treatment effect is fully mediated by beliefs and there is no other channel. For this, we instrument post-treatment malleability beliefs with the binary treatment variable. The results are presented in Table 6. The effects are striking: under the (rather strong) exclusion restriction, a one standard deviation increase in malleability beliefs leads to on average a 21 percentage point decline in the probability of giving in the effort context, while we find virtually zero effect for the luck context. The average marginal effects are not as strong for the fraction given but still statistically significant, again, only for the effort context.

Assuming away other potential channels may not be appropriate. In particular, while it is plausible that a shift in beliefs explains part of the effects we estimate, we cannot rule out the presence of other post-treatment confounders and their possible interactions with beliefs. To address this, we perform a mediation analysis as proposed by Imai et al. (2010). While the random assignment of treatment is sufficient to identify the total effect, additional assumptions are required to identify the average causal mediated effect (ACME) and the average direct effect (ADE). Imai et al. (2010) show that the ACME and ADE can be nonparametrically identified under the "sequential ignorability" assumption, which constitutes two sequential conditions. The first one states that given the pre-treatment confounders, the treatment assignment is independent of the potential outcomes and potential mediators, which is satisfied by design in our case due to our random assignment. The second one states that the mediators are independent of the potential outcomes conditional on pre-treatment confounders and the treatment assignment. While we make use of our rich data on numerous pre-treatment confounders that may potentially affect both the mediators and the outcome, we still caution that the latter is a rather strong assumption.

We are essentially interested in finding what proportion of the treatment effect on the context response (difference-in-difference estimate) is mediated through the treatment effect on malleability beliefs. To estimate the average causal mediated effect and the average direct effect (ACME and ADE), we proceed in several steps. First, we posit and fit regression models for the mediator and the outcome of interest. The mediator model includes as predictors the treatment variable as well as any relevant pre-treatment covariates, including the pre-treatment mediator variable. The outcome is modeled as a function of the mediator, the treatment variable, as well as pre-treatment covariates. Based on the fitted mediator model, we then generate two sets of predicted mediator values for each child, one under the treatment and the other under the control. In our example, this amounts to generating predicted

malleability score levels for each child under each treatment status.

We then use the outcome model to impute potential outcomes. For each child, we first obtain the predicted value of the outcome corresponding to the treatment and the predicted mediator value for the treatment condition (obtained in the previous step). We then generate the predicted counterfactual outcome, i.e. the outcome where the treatment status is still set to 1 but the mediator is set to its predicted value under the control condition (also obtained in the previous step). Finally, we compute the average causal mediator effect by averaging the differences between the predicted outcome under the two values of the mediator across observations in the data. In our study, this corresponds to the average difference in difference-in-difference estimates (context response) under the treatment across the levels of malleability scores with and without participation in the program. Table 7 presents the estimates and their (bootstrapped) 95% confidence intervals. As can be seen, we estimate that about 25% of the total effect (context response difference-in-difference estimate) is mediated by the level shift in malleability beliefs when we consider the probability of giving as our outcome. The percentage of the mediated effect declines to about 16% when we consider the fraction of wealth given as our outcome. While these are nontrivial quantities, we acknowledge that a significant portion of the total effect is unexplained.[21] Nevertheless, this analysis shows that the belief channel is very important in generating the effects we estimate.

4.3 A Simple Signal Extraction Framework for Interpreting the Results

In addition to a statistical mediation analysis, one can put forward a plausible theoretical framework to give an intuitive explanation to the empirical results. We should note at the outset, however, that there may be different models that could generate similar insights.

Consider a utility function that incorporates other regarding preferences in a simple way as in Dellavigna, List and Malmendier (2012). Individual preferences are defined over experimental wealth W:

$$U(g) = u(W_i - g_i) + \kappa_i^d v(g_i) \tag{2}$$

where g_i is the amount donated by individual i to recipient j, where $W_i > W_j$.[22] Parameter κ_i^d captures the strength of the altruistic motive when treatment status is fixed at d, with a lower bound of zero.[23] Given the parameter κ, the amount of wealth and the functional form of the functions u

[21] We also considered success in the real effort task and proportion of earned income in total wealth as mediators but found statistically insignificant ACMEs in both cases.

[22] In the context of our experiment, $W_j = 0$.

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

and v, individual i decides on the amount of donation g_i that maximizes $U(.)$.

Using this preference structure, we postulate that κ, rather than being a parameter that captures the strength of altruism, is a rather complex function of several variables, conditional on the wealth signal received from the potential recipient j:

$$(\kappa_i \mid W_j) = f^d(e_i^d \mathbf{I^i}, e_j^d \mathbf{I^j}, \Theta_\mathbf{i}, \varepsilon_\mathrm{i}; \Psi)$$

where vector Θ represents factors such as culture, religion, personal history, family background, personal characteristics etc. (expected to be balanced across treatment status by design in our case). In this specification, the first two terms pertain to individual $i's$ sense of fairness. The first argument e_i represents the effort she perceives to have exerted to earn her own wealth, where the indicator $\mathbf{I}^i = 1$ when her wealth is earned via effort and $\mathbf{I}^i = 0$ when her wealth is the result of a windfall. The second fairness argument e_j represents the effort she perceives that recipient j exerted to earn her wealth. The indicator $I^j = 0$ if it is not possible to earn wealth for j, and $I^j = 1$ if j is given an opportunity to earn wealth, corresponding to the luck and effort contexts, respectively, in our experiment. Finally, ε captures all other omitted factors that may affect giving and, Ψ represents the parameter vector that maps all arguments into κ.

It follows easily that the degree of altruism in the effort context is:

$$(\kappa_i \mid W_j) = f^d(e_i^d, e_j^d, \Theta_\mathbf{i}, \varepsilon_\mathrm{i}; \Psi)$$

In this setting the treatment may affect i) the individual's perception of his/her own effort level, and/or ii) his/her perception of the recipient j's effort, and/or iii) the shape of the function itself (the way arguments interact with each other and map into κ). However, our results for the week 1 windfall experiment and the week 2 luck context do not square well with (i) and (iii).

Focusing solely on (ii), we postulate that, ceteris paribus, κ_i is an increasing function of e_j, meaning that individuals are more inclined to donate when they observe that for a given level of recipient wealth, the recipient exerted higher effort in wealth generating endeavors. Of course, like us experimenters, donors cannot directly observe e_j, but infer it based on their own perceived production function.

Now consider a simple discrete production function where the recipient's true ability a_j can be either high or low, and her effort e_j can be either 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 that the recipient put high effort α. We conjecture, as borne out by our survey data on malleability as well, that

the intervention altered the perceived marginal productivity of investment effort by instilling the idea that ability can be enhanced via effort. Assume, for a simple illustration, that control children believe ability is fully innate and it is not possible to increase it by studying (effort), that is, the perceived marginal productivity of effort is zero. It then follows that perceived success probabilities for control children are:[24]

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

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 who puts low effort may also fail.[25] 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 on the part of untreated children for the 4 ability-effort combinations of the recipient:

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

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:

[24] Recall that in the context of our experiment, success corresponds to finding at least 3 number pairs in the allotted time.

[25] 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.

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

That is, observation of the recipient's failure is completely uninformative about the recipient's effort, since from the perspective of the untreated child, week 2 ability cannot be different from week 1 ability, that is, ability cannot be enhanced. 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:

$$Pr\left(e_L|W_j=0\right)_T = Pr\left(a_L, e_L|W_j=0\right)_T + Pr\left(a_H, e_L|W_j=0\right)_T$$

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

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. As μ goes to 1, treated children believe that the failed 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. The ability attribution is also intuitive and given in the appendix.Invoking the assumption we made earlier that altruism is increasing in the recipient's effort for a given level of recipient wealth, it follows that the treated students would be less willing to give to failed recipients.

This simple model explains why treated and untreated children may 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.

5 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 clearly poor due to bad luck, treated children are more reluctant to donate to recipients who failed at the task. 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 that is 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 implies that failure will also be the responsibility of the individual, leading to less sympathy toward those who were given a chance to put effort but failed. However, such a change in worldview does not affect giving

when the recipient is known to be poor due to bad luck.

In addition to showing that other-regarding behaviors are malleable in childhood, the results have important implications for educational policies. Recently, it has become a clear objective for policy-makers 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 political economy in the longer run.

An important caveat is that we do not know the long-run effects of the educational intervention, that is, whether these views will be permanent. It is well-known in various other domains that childhood preferences correlate with adult behavior and outcomes. Even with short-term evidence, however, it is possible to conclude that if and when these views are instilled, they will result in changes in patterns of altruism. The evidence we provide can inform larger-scale educational policies that incorporate the ideas advocated in the intervention we evaluate.

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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)
Verbal Ability (normalized)	0.10	-0.23**
		(0.11)
Behavior Grade (teacher reported- Scale-1-5)	3.89	-0.14
		(0.14)
Wealth (teacher reported SES-Scale 1-5)	2.63	0.31
		(0.31)
Pre-Treatment Malleability Beliefs (normalized)	-.03	.06
		(.07)
PANEL 2: Week 1 Outcomes		
	Control	Difference
CTB Early Allocation	2.17	0.01
		(0.22)
% Give out of Windfall	66.6	-0.02
		(0.06)
Fraction Given out of Windfall	0.27	0.00
		(0.03)
Effort Task Ability	3.70	0.25
		(0.17)
Effort Task Earnings	3.54	-0.19
		(0.42)

Note: Each row reports coefficients from a regression of the variable shown in the first column on the treatment dummy. The second column reports the mean of the control, and the third 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). The student's wealth and behavior grade are reported by the teacher (1-5 scale). Panel 2 presents the balance for i) early allocation in the 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 earnings from real effort task in Week 1. Standard errors, obtained via clustering at the unit of randomization (school) level, are reported in parentheses.

Table 2: Treatment Effects on Experimental Wealth

	Week 1			Week 2		
	(1) CTB Early	(2) Earned Wealth	(3) Total Wealth	(4) CTB Delayed	(5) Earned Wealth	(6) Total Wealth
Treatment	-0.117	-0.188	-0.289	0.176	2.259***	2.580
	(0.99)	(0.42)	(1.08)	(1.48)	(0.56)	(1.55)
Control Mean	8.69	3.54	13.23	16.97	6.86	23.37
N	1329	1345	1329	1329	1334	1416

Reported estimates are coefficients from OLS regressions. Columns 1, 2 and 3 present the treatment effects on CTB early allocations, earnings from the real effort task and total earnings (CTB early+real effort task), respectively in the Week 1 visit. Columns 4, 5 and 6 present CTB delayed allocations, earnings from the real effort task and total earnings (CTB delayed+real effort task), respectively in the Week 2 visit. Pre-treatment variables used as covariates are gender, age, IQ score (Raven), math ability, verbal ability, teacher-reported socio-economic status and teacher-reported behavior grade. Clustered standard errors (at the school level) are in parentheses, $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Table 3: Treatment Effects on Probability of Giving-Week 2

	Effort	Luck	Effort	Luck
Treatment	-0.123***	-0.031	-0.108***	-0.008
	(0.04)	(0.04)	(0.04)	(0.04)
Student Characteristics			✓	✓
Control Mean	0.360	0.328	0.360	0.328
N	673	662	673	662
Permutated P-value	.007	.452	.018	.859
P-value (Effort vs Luck)		.026		.006

Reported estimates are average marginal effects from logit regressions. Pre-treatment variables (student characteristics) used as covariates in columns 3 and 4 are gender, age, IQ score (Raven), math ability, verbal ability, teacher reported socio-economic status and teacher reported behavior grade. Clustered standard errors (at the school level) are in parentheses, $*p < 0.10$, $**p < 0.05$, $***p < 0.01$. Permutated p-values, which account for small sample, are also provided. P-values for the equality of treatment effects across effort and luck context (difference-in-difference) are given in the last row.

Table 4: Treatment Effects on Fraction of Wealth Given-Week 2

	Effort	Luck	Effort	Luck
Treatment	-0.034***	-0.011	-0.037***	-0.008
	(0.01)	(0.01)	(0.01)	(0.01)
Student Characteristics			✓	✓
Control Mean	0.068	0.055	0.068	0.055
N	673	662	673	662
Permutated P-value	.001	.277	.001	.417
P-value (Effort vs Luck)		.023		.004

Reported estimates are average marginal effects from Tobit regressions. Pre-treatment variables (student characteristics) used as covariates in columns 3 and 4 are gender, age, IQ score (Raven), math ability, verbal ability, teacher reported socio-economic status and teacher reported behavior grade. Clustered standard errors (at the school level) are in parentheses, $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Permutated p-values, which account for small sample, are also provided. P-values for the equality of treatment effects across effort and luck context (diff-in-diff) are given in the last row.

Table 5: Relationship between Giving and Beliefs in the Control Group

	(1) Probability of Giving	(2) Fraction Given
Effort Context	0.037	0.010
	(0.11)	(0.01)
Malleability Beliefs	0.269***	0.013**
	(0.08)	(0.01)
Malleability Beliefs X Effort Context	-0.474***	-0.015
	(0.16)	(0.01)
Observations	710	669

Reported estimates are average marginal effects from a logistic regression for column 1 and a tobit regression for column 2. Pre-treatment variables used as covariates are gender, age, IQ score (Raven), math ability, verbal ability, teacher-reported socio-economic status and teacher-reported behavior grade. Clustered standard errors (at the school level) are in parentheses, $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 6: IV Estimates

	Probability of Giving		Fraction of Wealth Given	
	(1)	(2)	(3)	(4)
	Effort	Luck	Effort	Luck
Malleability Beliefs	-0.205***	-0.012	-0.079**	-0.029
	(0.06)	(0.13)	(0.04)	(0.03)
Control Mean	0.360	0.328	0.068	0.055
N	631	617	631	617
P-value (Effort vs Luck)		.043		.107

Reported estimates are average marginal effects from logit regressions and tobit regressions for columns 1-2 and 3-4, respectively. Pre-treatment variables used as covariates are gender, age, IQ score (Raven), math ability, verbal ability, teacher-reported socio-economic status and teacher-reported behavior grade. Clustered standard errors (at the school level) are in parentheses, $*p < 0.10$, $**p < 0.05$, $***p < 0.01$. P-values for the equality of the effect of malleability scores across the effort and luck contexts (difference-in-difference) are given in the last row.

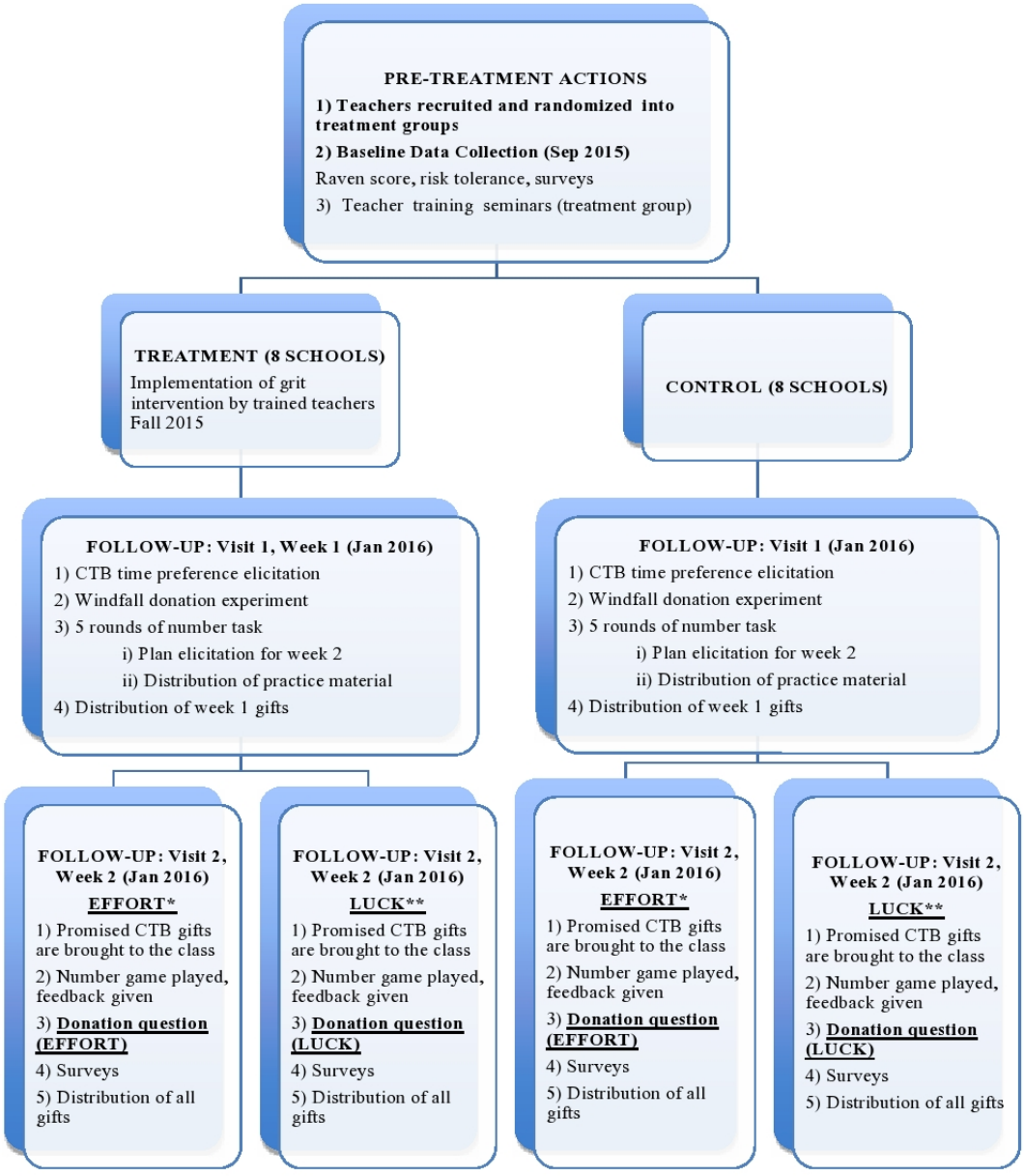
Table 7: Mediated Causal Effects

	Malleability Beliefs	
	Probability Donate	Fraction Donated
ACMEs	-0.021	-0.004
	(-0.008)	(0.001)
	[-0.035, -0.007]	[-0.006, -0.001]
ADEs	-0.067	-0.022
	(0.043)	(0.012)
	[-0.134, 0.0001]	[-0.003]
Average Total Effect	-0.082	-0.025
	(0.043)	(0.012)
	[-0.159, -0.021]	[-0.047, -0.009]

Reported estimates are obtained via the Imai, Keele and Tingley (2010) algorithm. Standard errors (in parentheses) and 95% confidence intervals (in square brackets) are obtained via 250 bootstrap replications. Pre-treatment variables used as covariates are gender, IQ score (Raven), math ability, verbal ability, pre-treatment malleability score, teacher-reported socio-economic status and teacher-reported behavior grade.

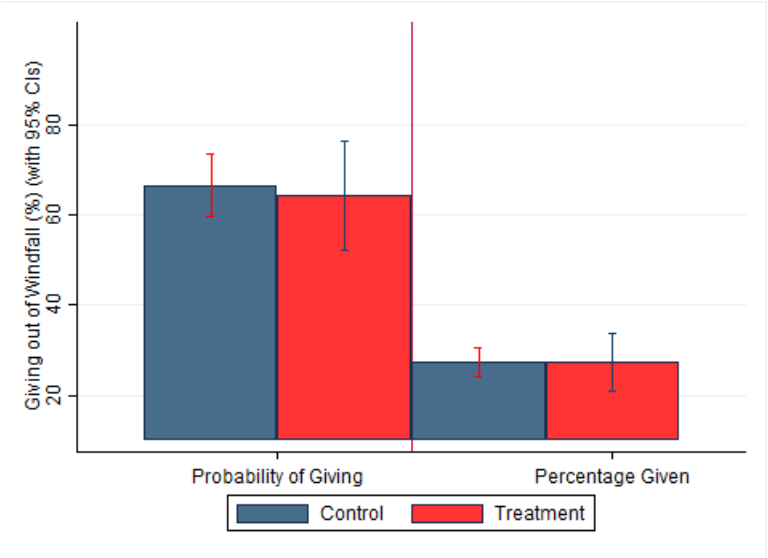
Figures

Figure 1: Timeline of the Field Experiment



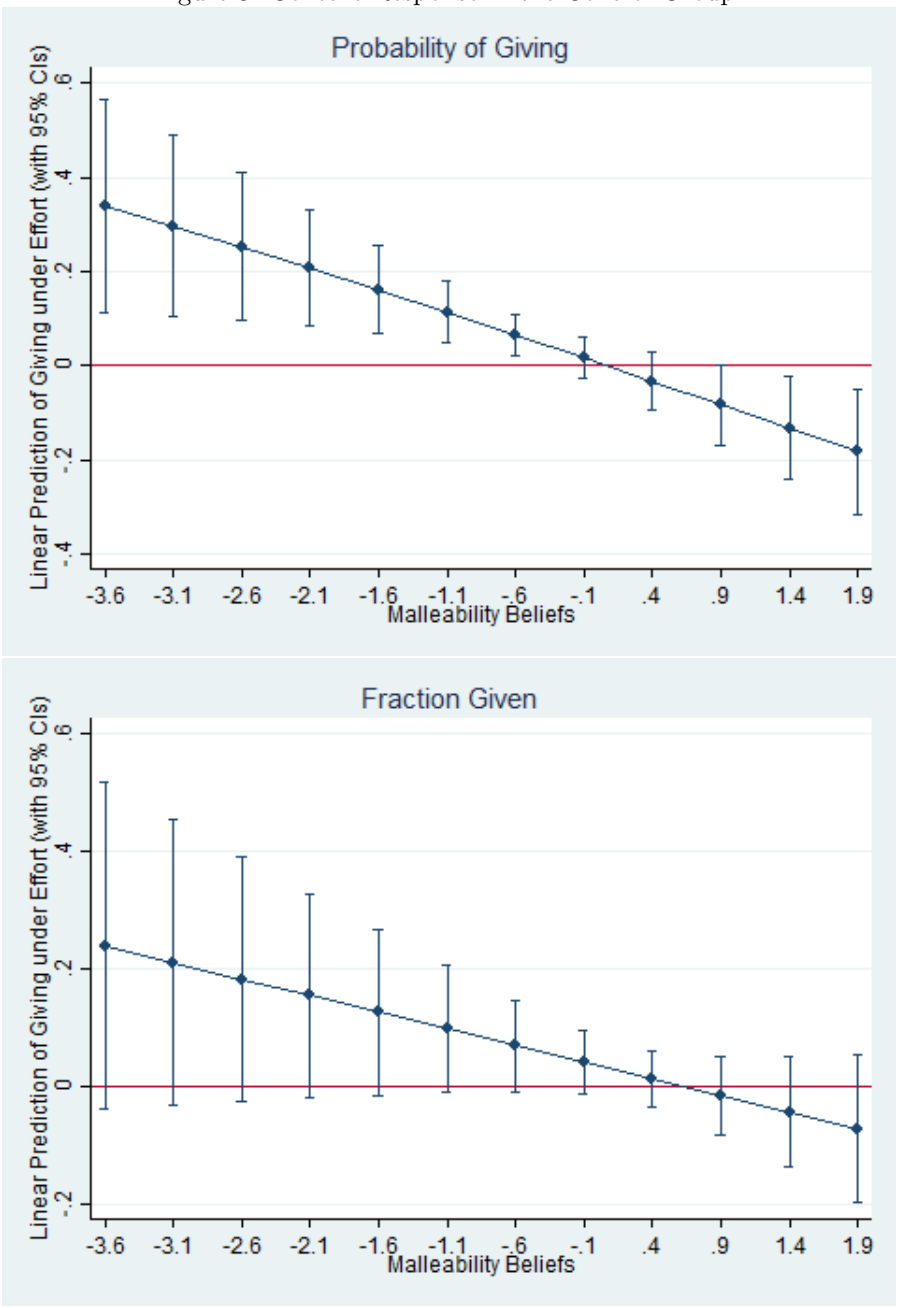
*EFFORT: *"There are schools where we played the number game (only this game, no other games) with 4th graders, in the same way as you. They also got a one-week period to practice, and then played one round of the game. Children who failed in this game could not get any gifts. If you want, you can give some (or none) of the gifts you will get today to those children."*
**LUCK: *"There are 4th graders in schools we could not visit and play the number game (or any other game). Those children did not get any gifts. If you want, you can give some (or none) of the gifts you will get today to those children."*

Figure 2: Giving Out of Windfall



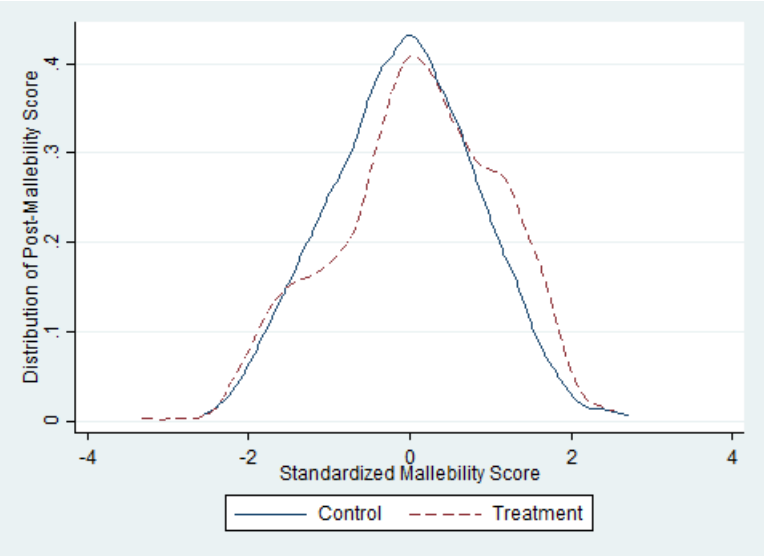
The figure depicts the proportion of children who donate at least one of their four gifts (probability of giving) and (unconditional) percentage of gifts given.

Figure 3: Context Response in the Control Group



Figures depict linear predictions obtained from the regressions presented in Table 5. Variables used as covariates are gender, age, IQ score (Raven), math ability, verbal ability, teacher-reported socio-economic status and teacher-reported behavior grade. Standard errors are clustered at the school level. Higher numbers of malleability beliefs represent more optimism.

Figure 4: Treatment Effect on Malleability Beliefs



The figure depicts density estimates of malleability belief scores, which are constructed based on children's answers to 4 item-set questions provided in the Appendix (4-item likert scale: Absolutely agree, Agree, Disagree, Absolutely disagree). The mean of the constructed score is 0.33 standard deviations higher for the treatment group and the difference is statistically significant (p-value=0.001), controlling for pre-treatment scores. Higher numbers of malleability beliefs represent more optimism.

Appendix

Proofs of the Results in Section 4.3

Ability Attribution

Ability attribution, given the simple discretized model presented in Section 4.3, is as follows. Since ability is paramount in success from their perspective, untreated children believe, when they see a bad outcome (low wealth), 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. It follows that:

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

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

Survey Questions for the Malleability Score

4-Point Likert scale: Absolutely Agree, Agree, Disagree, Absolutely disagree.

1) Intelligence is innate. You cannot change how smart you are.

2) If you don't have the talent for something, working hard cannot make you achieve it.

3) I can be the best in my class if I work hard enough.

4) The reason for getting a bad grade in an exam is because of not having enough intelligence.

Grit Curriculum and Class Activities

The grit curriculum consists of a range of topics to be covered in class during weekly project hours. Each week is dedicated to a specific topic, which is introduced by the teacher with the help of a pre-specified set of materials. The material is designed to initiate class discussions and activities, questions and homework. We recommend a minimum of 10 weeks to complete the curriculum. Based on the feedback we received from participating teachers, on average 12 weeks were necessary to complete the curriculum. Most teachers reported that they spent at least 2 hours/week on the project. In the following, we provide a summary of the material to be covered during each week, together with the recommended follow-up activities.

Week 1: The Plasticity of the Brain

Growth Mindset Video: The material used for this lecture is a 4-minute video on the concept of *growth mindset*. The focus of this week's lesson is the plasticity of the brain.

Follow-up Activities: Teachers initiate a class discussion based on questions we provide. Children are then asked to draw themselves while they struggle with an activity that they find very difficult and frustrating. They are asked to imagine their brain activities while they work on the task.

Activity for the Whole Semester: Every week the teacher selects a number of students to be the "students who exerted most effort during this week". The purpose of these announcements is to demonstrate how students make progress by putting in more effort. The announcements are followed by a class discussion on how valuable certain behaviors are. A round of applause is given to the selected students, after which the students' pictures are put on the board into a designated frame, and

the students are provided with certificates.

Week 2: Growth Mindset Messages

Growth Mindset Video: Children watch the 4-minute video on growth mindset one more time. The focus of the lesson is how children may differ in their mindsets. The video shows two children who engage in a dialog, one with a growth mindset and one with a fixed mindset.
Follow-up Activities: Teachers initiate a class discussion based on questions we provide. In addition, students are given homework in which they are asked to reflect on their current mindset.

Week 3-4: Failure and Praising

Reading a Letter: Students read a letter, which is written by a student to her parents. The student describes how she has been fascinated by the fact that famous scientists also experience failures on their way to success, and she asks her parents to understand that only because she sometimes fails on a test it doesn't mean that she is lacking the innate ability to perform better. She asks her parents to believe in her and support her when she is trying new things that she is not good at initially.
Follow-up Activity (Week 3): Teachers initiate a class discussion based on questions we provide. The children are then asked to write letters to their parents and teachers in which they explain how they would like to be praised/criticized.
Follow-up Activity (Week 4): Students are asked to form groups and to do research on a famous scientist, artist or athlete of their choice. They are asked to find out about this person's failures and frustrations. The students collect pictures and testimonies which they use to prepare a poster.

Week 5: Goals and Difficult Tasks

A Short Story on Mustafa - How to Deal with Difficult Tasks?: In the fifth week, children read a short story about a boy who has been given a homework assignment that is challenging for him. The story describes which strategies the boy uses to tackle the challenging homework task.
Follow-up Activity: Students are asked to write down a goal they themselves want to achieve in one month's time. They are asked to write down the goal on a post-it, and put it on the classroom board together with their name. During the following weeks, students can put a star on the post-it whenever they accomplish something which brings them closer to achieving their main goal.

Weeks 6-7 : Fear of Failure

A short story on Tugba - Who Likes Failing?: Students read and discuss another short story. The

story is about a girl who contemplates the meaning of the phrase *'Success consists of going from failure to failure without loss of enthusiasm'.* In the story, the girl asks the *'Wise Bird'* for advice. The girl and the Wise Bird engage in a conversation which centers around the idea that on the way to success people encounter numerous challenges and inevitably they fail many times before they succeed. The importance of not giving up is emphasized.

Follow-up Activities: In class, the teacher discusses these concepts on the basis of questions we provide and encourages students to think about their goals, what likely challenges they will encounter and how they are going to achieve them. In addition, the students are asked to write a letter to the Wise Bird in which they convey the goals they have.

Follow-up Games: In this game, the teacher draws a child on the board and asks students to imagine that this child has a goal (e.g. becoming a good football player). The teacher then draws a chronological chart, and complements it to include activities the child engages in to achieve this goal and challenges the child needs to overcome.

Homework: Students find out about the dream of their close friend and they create a similar chronological chart for him/her.

Week 8-9: Fear of Mathematics

A Mini-Cartoon Story - Dancing with Numbers: Students are presented with a story about a boy who is afraid of math. In his dreams, there are numbers that want to engage and play with him but he is afraid. The next morning the boy is in school and observes how other children attempt to find solutions to math problems that the teacher poses in class, even if they do not know the right answer from the start. The boy observes how the students get encouraged to praticipate even if they do not know the right answer, which eventually encourages him to participate as well.

Follow-up Activity: Students are asked to complete the same math problems and raise their hands to give their solutions to the math problems. The teachers initiate a class discussion based on questions we provide.

Week 10: Developing a Classroom Culture and Announcing Learned Outcomes

In the tenth week, students engage in a consolidating activity. In particular, students form groups and they prepare a poster which is then displayed on the classroom board. Each group covers a different topic: *1) We dream big. We have plans. 2) We are not afraid of failure. 3) We do not give up. 4) We champion effort. 5) We love difficult tasks.* Within each group, every student is expected to make a different contribution to the poster. Contributions ranged from making a painting, writing a short

essay or choosing a slogan for the poster to writing poems or songs. We awarded a large trophy to the top 3 materials produced in class. The selection was made by a committee of volunteer teachers. Follow-up activity: In all schools in which the project was carried out, the work was exhibited in the schools' corridors. This activity was carried out at the end of the semester, and it served the purpose to also inform the rest of the school about the project.

Examples of Classroom Activities

Figure 5: Screenshot of animated video

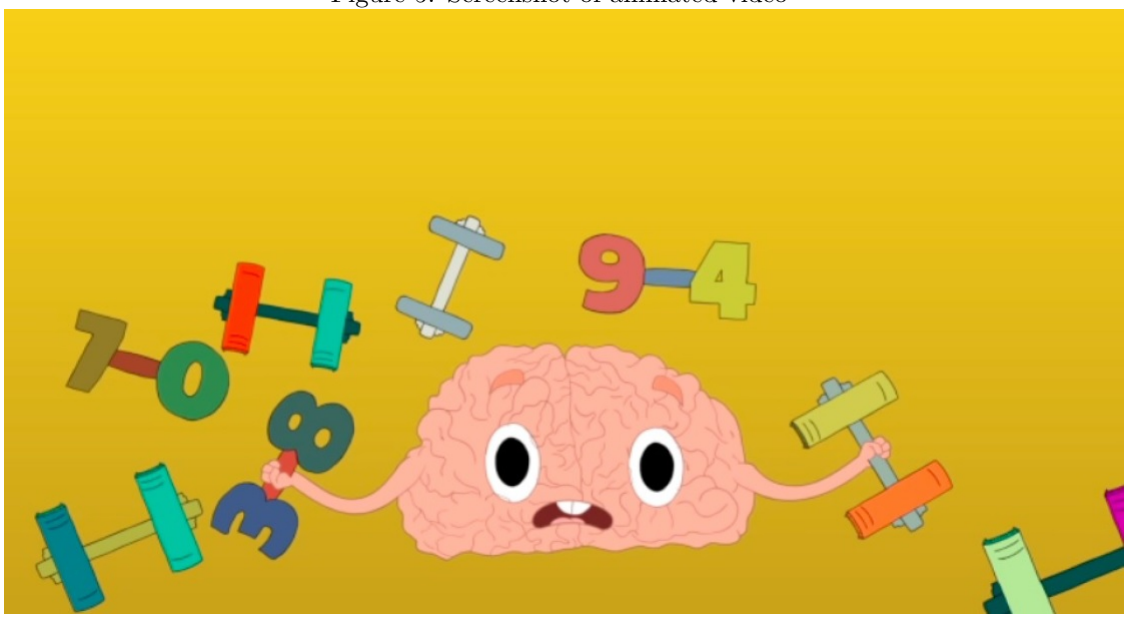


Figure 6: Example of a class activity (effort-success)

Figure 8: Examples of the Difficult and Easy Tasks

17	86	23	12
71	42	27	38
51	62	83	30
77	59	46	67
81	58	29	54

80	7
70	95
5	20
10	30
93	90

The grid on the left is an example of the 4-gift (more difficult) task and the one on the right is that of the 1-gift (easier) task. Students were given 1 minute and 45 seconds to find at least 3 pairs that add up to 100 at their chosen task.

Figure 7: Certificates students received for high effort

Grit Task Instructions

1st Week Instructions:

Hello all! Today we'll play a game. In this game, you'll do a task and make some decisions. Based on your performance and your decisions, you will earn gifts. [Show gift basket] There are no right or wrong decisions. Everyone can choose what they want. But, there's an important rule: not to comment at all on how the game is going, what you chose, whether you were successful... Keeping your choices a secret is very important. And you should not ask others what they are choosing, because everyone is different and everyone can make their own choices. OK? We will now introduce our game. [Draw a sample number grid on the board]

The game has 5 rounds. [Write R1, R2, R3, R4, R5 on the board] You will earn gifts according to whether you are successful in these rounds or not.

At the end of this class, we'll select one round randomly, by picking a number from a bag without looking. If you were successful in that round, you get gifts. For example, suppose I was unsuccessful in Round 1 and Round 2 but successful in rounds 3, 4, and 5 [Show by putting X and √ underneath R1...R5 on the board]. Suppose Round 2 was selected. Do I get gifts? No, because I was not successful. Do I get gifts if Round 4 is selected? Yes, because I was successful in that round. This means, gifts don't accumulate. You only get the gift you earned in the selected round. So, you should not think "I got enough gifts from the previous rounds, so I don't need to take these rounds seriously". Any round can be selected, so take all rounds seriously. OK?

Now, the game we will play is a number search game [Point to the grid on the board] In this game, you try to find number pairs that add up to 100 within a number grid. Look at the grid on the board. Here, there are number pairs whose sum is 100. For example: 15 and 85. Their total is 100. [Mark 15 and 85 with √, and write 15+85=100 beneath the grid on the board].

Are there any other pairs you can see? [Ask children, write on the board, mark the pairing numbers]. OK, so is there a number that matches 37? No! This shows that not all numbers have a matching number to sum up to 100. Also, numbers don't repeat, there is only one of each number. So, when you find a pair, you can mark those two numbers off. OK?

Now, before we start our 5 rounds, we will distribute a practice puzzle. Here, the goal is to find as many pairs summing up to 100 as you can, within 2 minutes. For each pair you find, you will get a small gift. [Distribute the practice sheets]

First, you'll write your name, last name and classroom on the front page, and wait. Do not turn the page yet. Everyone will start at the same time. You will write the pairing numbers in the blanks

underneath the grid, and mark them off on the grid. OK? Everyone ready? Start!

[Keep the time. Instruct children to turn the page immediately when time is up. Collect the sheets]

OK, now we'll start our main 5-round game. We have two different types of game here. One is the 4-gift game. If you are successful in this one, you get 4 whole-token gifts. If you fail, you get 0 gifts. [Write on the board: 4 gift game: √ =4, X=0]

We also have a one-gift game. Here, if you are successful, you get 1 whole-token gift. If you fail, you get 0 gifts. [Write on the board: One gift game: √=1, X=0]

The target is the same in both games: Finding at least 3 pairs that sum up to 100 [Write on the board: "AT LEAST 3"]. If you can't find 3, you are unsuccessful. OK?

But what is the difference between the 4-gift and the 1-gift game? The target is the same in both: Finding at least 3 pairs adding up to 100, within 1 minute and 45 seconds. But, the 4-gift game is more difficult than the other, because the grid is larger. You are looking for the numbers in a bigger space. [Open one sample puzzle from the 4-gift booklet, one sample puzzle from the 1-gift booklet, and show] See, the 1-gift game grid is smaller. That is why the 1-gift game is easier than the 4-gift game. Both grids have the same number of pairs, but in one they are harder to find because the grid is larger. OK? So, every round, you are going to play either the 4-gift or the 1-gift game.

First, I will explain the rules for Round 1. You will first make the decision only for Round 1. For the other rounds, you will decide later. We now ask you to think: Would you like to play the 4-gift, more difficult game in Round 1, or the 1-gift, easier game? This choice you make will count with half-half chance. Either your choice counts, or everyone will play the game written on this (folded) piece of paper in the first round. [Show a folded paper] OK? So think now: If the choice is up to you, what game would you like to play in Round 1? The 4-gift game or the 1-gift game? Think carefully because your choice may count! [Distribute 1st period choice sheets, collect]

Now, let's make our draw. We have two folded papers here. One says "free choice", the other says "no choice, difficult". Let's see if you will play the game of your choice in the first round. [Put both papers in a bag and make the class representative draw].

****

*****[If "No Choice, Play Difficult" is drawn]:

OK, so "no choice" was drawn. So, only in the first round, everyone will need to start with the more difficult task. In the other rounds, you will be free to make your own choices. OK?

*****[If "Free Choice" was drawn]:

OK, so "free choice" was drawn. So, you will be able to play the game you want in the first round. You will also be free to make your own choices in the other rounds.

****

Now, we'll give you both the 4-game booklet and the 1-game booklet. Look, in this booklet we have 5 rounds of the 4-gift game. That is, Round 1, Round 2, Round 3, Round 4, Round 5; 5 pages [show]. And in this one, we have 5 rounds of the 1-gift game. [Show both booklets] If you want to play the 4-gift game in a round, you use that booklet in that round. If in another round you want to play the 1-gift game, you use the 1-gift booklet in that round. OK? [Distribute the booklets] Now, please write your name on BOTH booklets, and wait. Please don't turn any pages.

****

*****[If "No choice" was drawn]:

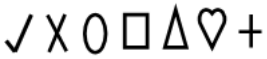
Now, since "no choice" was drawn, everyone will need to start with the 4-gift, more difficult task. After the 1st round, you can choose either the 4-gift game again, or choose the 1-gift game. OK? Now, everyone please take the 4-gift game booklet in their hand and wait. Is everyone ready? Do you see the 4-gift writing on the first page? OK. When we say so, you'll turn the first page, see that "Round 1" is written on the page.

*****[If "free choice" was drawn]:

Now, everyone please take the booklet you chose to work on for the first round. If you chose the 4-gift game, take the 4-gift booklet. If you chose the 1-gift game, take the 1-gift booklet and wait. OK? Everyone will turn the first page of their booklet at the same time. When we say so, you'll turn the first page, see that "Round 1" is written on the page.

****

When time is up, everyone will become a "flower".[26] If you are done before time is up, you just wait silently. And as we said, it is very important not to make any comments on the game, whether you were able to find numbers etc. OK? Finally, there is a difference in the way you'll mark the matching numbers. You won't have to write them underneath the table but mark them with matching marks. So as to not confuse two different pairs, you use different marks. For example, [show on the grid on the board] when 15 and 85 match, we circle both. For 20 and 80, we can no longer use the circle because it would get mixed up. We need to use something else. Let's say the check mark. You will have the signs you can use on your sheets [also write on the board]:



OK, ready? We start now [Hold the time] [When time's up: Fold your arms please! We'll check

[26] Becoming a flower is a commonly used concept in elementary school, which means folding arms and being dead silent.

everyone's sheet. If we've checked your sheet, close your booklet.] [Experimenters go around and mark Succeeded/Failed depending on whether child was able to find 3 pairs or not]

Now, for the 2nd round, we have new grids. Everyone is free to choose the type of game they want. Either the 4-gift game or the 1-gift game. Now, think carefully again, would you like to do the 4-gift, more difficult game in the 2nd round, or the 1-gift, easier game? Take the booklet of your choice. And open the page that says "2nd Round". Everyone ready? Start!

[Do this till 5 rounds are over]

Now, we will visit your classroom again and we will be playing this game again in a week. That day, you'll play one of the games again (either the 4-gift or the 1-gift game), but only for a single round.

Also, if you want, you can get practice games to work on them. [Show practice booklet] This booklet has practice games for the 4-gift, more difficult game, that can help you do well in the game. Whoever wants, can take this practice booklet and work on it at home and practice for the game next week. OK?

Now, we ask you: next week, would you like to play the 4-gift, more difficult game or the 1-gift, easier game?

An important point: You can't change your choice next week. With 50-50 chance, the choice you make now will count. Or, you will play the game written in this folded paper [show paper]. So, now please think and make a choice carefully, because this choice you are making can count. OK? [Distribute choice sheets]. Don't forget, you can also take practice booklets for home. Did everyone decide? [Collect choice sheets]

Now, let's see if your choice counts, or you play the game written in this paper [Make random draw]

****

*****[If "Free Choice" is drawn]:

OK, so "free choice" was drawn. So, next week you will play the game you have just chosen. OK? We will distribute your games next week according to the choice you made now. If you chose the 4-gift, more difficult game you will play that, if you chose the 1-gift, easier game you will play that. Remember, you cannot change your selection next week!

*****[If "No Choice" is drawn]

OK, so "no choice" was drawn. So, everyone will play the game written here. [Show the unfolded paper] Everyone will play the 4-gift, more difficult game next week. OK?

****

[Distribute practice booklets to those who want] Now, whoever wants, can take a practice booklet![27]

2nd Week Instructions:

Remember that we played a number game with you last week. And we said we'd play it again this week. Now let's remember what the rules of the number game were: [Write sample grid on board again, remind]

****

*****[If the class had "free choice" drawn the previous week]

Today we'll play a single round of the game. Remember that we had two types of games. One gave 4 gifts if you are successful, but was more difficult. The other gave 1 gift if you are successful, but was easier. In both, you needed to find at least 3 pairs to be successful. [Write on board: "AT LEAST 3"]. We asked you, last week, which game you wanted to play for this week. And you made a choice. Now, we'll distribute to you the game you will play, according to the choice you made last week. But you wait, and don't turn the page until everyone gets their sheets, OK?

[Distribute the difficult game to those who chose it and the easy game to those who chose it]

*****[If the class had "no choice" drawn the previous week]

Today we'll play a single round of the game. Remember that we had two types of games. One gave 4 gifts if you are successful, but was more difficult. The other gave 1 gift if you are successful, but was easier. In both, you needed to find at least 3 pairs to be successful. [Write on board: "AT LEAST 3"]. Remember that in your class, the draw was such that everyone plays the 4-gift, more difficult game. Now we'll distribute the sheets for the game. But you wait, and don't turn the page until everyone gets their sheet, OK?

[Distribute the difficult game to all]

****

Ready? Start! [Keep the time, mark success/failed. Collect sheets.].

[27] Then, we implement the procedures to select the chosen round for payment (out of the 5 rounds) and giving the gifts.