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Shaping Preferences for Redistribution: The Impact of Early-Age Education on Inequality

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Abstract

This paper examines the causal impact of early-age educational interventions on children's redistributive preferences. During the Covid-19 school closures, children in Bangladesh participated in free phone-based educational programs to mitigate learning loss. Upon completing these programs, we measured their preferences for equality and redistribution using three lab-in-the-field experiments, where children allocated candies between two anonymous peers. Our findings reveal that equality preferences were similar between treated and control children. However, treated children exhibited a significant inclination towards redistributing candies from advantaged to disadvantaged peers, particularly when the disadvantage arose from ill health or lack of opportunities. We also find suggestive evidence that these compassionate redistributive preferences were directly impacted by the educational interventions rather than indirectly through cognitive achievement or parental influence, highlighting the role of early life experiences in shaping individuals' compassionate redistributive behavior, underscoring the role of early life experiences in shaping behavior towards inequality.

Keywords: Redistribution, Social preferences, Lab-in-the-field experiment, Randomized experiment, Covid-19 pandemic, Rural children, Bangladesh.

JEL: C93, I21, I24

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1. Introduction

Governments have long employed redistributive measures such as taxes and transfers to reduce economic inequality in various societies. In recent decades, governments worldwide have faced mounting pressure to implement greater redistributive measures aimed at mitigating rising income inequality (Alvaredo et al., 2013; Alvaredo et al., 2017). This pressure reached new heights during and after the Covid-19 pandemic, which exacerbated income inequality in many countries (Angelov and Waldenström, 2023; World Bank, 2022a). However, redistributive policies often create winners and losers, raising concerns about whether there is sufficient political support for such measures. In democracies, where voters influence policy, understanding the determinants of preferences for equality and redistribution is critical for shaping effective policies(Alesina and Giuliano, 2011; García-Sánchez et al., 2022).

Identifying the factors that drive individuals' preferences for equality and redistribution can be a complex task. Previous studies have indicated various elements, such as personal history, cultural background, intergenerational transmission of beliefs, perceptions of justice, and self-interest, as potential determinants (Alesina and Giuliano, 2011; Andreoli and Olivera, 2020). These factors are often examined through correlations in survey-based studies conducted globally. On the other hand, a rich body of literature shows that individuals' social preferences are shaped significantly during childhood and adolescence (Eckel et al., 2011; Fehr et al., 2013; Gangadharan et al., 2022; Harbaugh et al., 2002; Sutter and Kocher, 2007; Sutter et al., 2013). Human social behavior theories posit that social preferences are influenced not only by genetic factors but also by lived experiences and social environments (Boyd and Richerson, 1988; Boyd et al., 2011; Fehr and Fischbacher, 2003; Henrich et al., 2004). For example, children's social preferences may develop by observing and imitating prosocial behavior from role models such as parents and other adults (Bandura, 1973). Moreover, causal evidence shows that children become more prosocial when exposed to role models (Kosse et al., 2020), and their behavior can change due to social environmental shifts (Abeler et al., 2024). Cognitive development (Batson and Shaw, 1991; Eisenberg et al., 2001; Hoffman, 2001) and early childhood education (Cappelen et al., 2020) can also shape social preferences, making early

educational interventions plausible candidates for influencing preferences toward equality and redistribution.

This paper investigates the causal impact of free 'low-tech' early-age distance educational interventions on children's preferences for equality and redistribution. The study focuses on two charitable educational interventions targeting young children, implemented during the significant disruptions of the Covid-19 pandemic. Our evidence comes from three lab-in-the-field experiments following two randomized controlled trials (RCTs) conducted in rural Bangladesh, where schools were closed for 18 consecutive months. The first RCT (Study 1) involved a volunteer-based telementoring program, in which university students provided free tutoring and mentoring to children aged 7-10 (grades 1-3) over basic feature phones for 13 weeks. The second RCT (Study 2) featured Interactive Audio Instruction (IAI) lessons delivered to children aged 6-11 (grades 1-4) using Interactive Voice Response (IVR) technology over 15 weeks.[5]

During the prolonged closure of schools, many countries, including Bangladesh, resorted to online education and home-schooling. However, children from lower socioeconomic backgrounds faced significant barriers to learning due to resource constraints, limited access to information and communication technology, and insufficient educational support at home. In this context, the Global Development and Research Initiative (GDRI), a local NGO in South-West Bangladesh, implemented the two interventions for primary school children from disadvantaged rural areas, providing them with educational resources free of charge.

To assess whether these early-age educational programs influenced children's preferences for equality and redistribution, we conducted three lab-in-the-field experiments at the end of each intervention. In these experiments, children acted as "spectators," making decisions that affected the earnings of two other children in scenarios designed to test redistributive behavior. Our field staff visited participants' homes and conducted the experiments one-on-one while adhering to health and safety protocols.

[5] Both studies included over 2,500 households in total. In Study 1, we recruited 838 mother-child dyads, which were randomly divided into two equal groups. In Study 2, we recruited 1,763 households, which were then divided almost equally among two treatment groups and a control group.

In the first experiment, children in both treatment and control groups were asked to choose between an equal (2:2) and an unequal (1:5) distribution of candies for two other anonymous children of the same gender and grade level. In this scenario, the decision involved a trade-off between equality and efficiency. In the second and third experiments, children were asked to redistribute an initially skewed allocation (8:0) of candies between two other anonymous children, with no cost to redistribution. In these two experiments, the skewed allocation of candies was selected by chance, and there was no cost associated with redistribution. In the second experiment, children were informed that one child had no candies due to bad luck, losing a fair coin toss. In the third experiment, children were informed that one child was unable to participate in a game providing candies due to illness. For payment purpose, only children recently experienced an illness were selected. The second and third experiments varied the source of others' disadvantage. The second experiment emphasized luck as a factor for inequality, while the third focused on unequal opportunities due to health. Generally, redistribution is motivated by dispositional compassion, dispositional envy, and the expectation of personal gain from redistribution (Sznycer et al., 2017). As the initial distribution of candies is identical in the second and third experiments, the difference in redistribution between them is more likely motivated by compassion. These designs allowed us to examine how children valued equality relative to fairness, and whether their decisions were motivated by compassion in the second and third experiments.

Our findings indicate that the interventions influenced children's redistributive preferences in the second and third experiments but had no impact on their preferences for equality (and efficiency) in the first experiment. Treated children were significantly more likely to redistribute candies to the unlucky child in the second experiment and the unwell child in the third experiment, with stronger effects observed in the third experiment. This suggests that compassionate redistribution was more salient when inequality arose from unequal opportunities, particularly due to illness. Children in the volunteer-based intervention (Study 1) exhibited stronger redistributive tendencies than those in the technology-based intervention (Study 2).

Our average causal mediation analysis indicates that both programs had a strong direct effect on children's inclination to redistribute candies from the fortunate child to the

unwell child in the third experiment. The programs also increased children's tendency to redistribute candies from the fortunate child to the unlucky child in the second experiment, but increased children's literacy and numeracy test scores appear to play a stronger mediating role in Study 2. We also conducted three experiments among the children's mothers to further assess whether the treatment effects are due to transmission of preferences from the mothers who also indirectly benefited from the charitable educational interventions. However, the average causal mediation analysis reveals small indirect effects channeled through the mothers. Thus, the role of cognition on the formation of compassionate redistributive preference is likely to be small and the treatment effects are unlikely to transmitted via parents' preferences.

Our findings indicate that early-age educational interventions can influence children's redistributive preferences without altering their preferences for equality and efficiency. This influence needs not depend on the effects on their cognition and parental preferences. To the extent that effect on redistribution is larger when the disadvantaged child's circumstance was due to ill health and lack of opportunities, compassion is more likely to motivate redistribution. Similarly, the stronger effects observed in Study 1, where beneficiary children experienced the compassion and altruism of volunteers, suggest that charitable interventions targeting children in times of need may strengthen their preferences for compassionate redistribution.

This study contributes to the literature on the determinants of social preferences, particularly related to childhood experiences. There has been a growing emphasis on the role of noncognitive or soft skills in human capital formation at an early age (Heckman, 2000, 2006). Individuals develop various skills and preferences at an early age, and lived experiences during this period play a critical role in their development (Cunha and Heckman, 2007). Our findings contribute to the research that demonstrated the causal impacts of early-age educational programs on children's social preferences in three important ways (Abeler et al., 2024; Alan et al., 2019; Cappelen et al., 2020; Jakiela et al., 2015; Kosse et al., 2020). First, we provide evidence on the causal effect of early-age educational programs on compassionate redistribution through differences in the second and third experiments. Second, because Study 1 directly involved volunteers who showed compassion and altruism while Study 2 did not involve human interaction, our results

suggest that benefitting from the compassion and altruism of role models are more likely to shape compassionate redistributive preferences of children. Third, we demonstrate that the effect of these early-age educational programs on compassionate redistribution needs not channel through its effect on cognition and parental preferences.

In summary, this study demonstrates the potential of early-age educational interventions to shape children's redistributive preferences, especially those driven by compassion. Our findings highlight the importance of human interaction in these interventions, as seen in Study 1, where the presence of compassionate role models led to stronger redistributive behavior. These results contribute to the growing literature on the development of social preferences and suggest that policies promoting early-age interventions can influence future support for redistributive measures aimed at addressing inequality and deprivation caused by unequal opportunities. The findings also suggest that promoting altruism in a society may help foster a sense of distributive justice among children. It is worth noting that our findings are based on experiments conducted immediately after the interventions; more research is needed to investigate whether the effects will persist in the longer term.

2. Background: 'Low-tech' Distance Learning

Bangladesh is home to nearly 17 million primary school-aged children, many of whom grapple with cognitive growth challenges (Hamadani et al., 2014). Unfortunately, 58% of these children struggle to read basic text in their native language by the age of 10, highlighting significant learning deficits (World Bank, 2022b). The Covid-19 pandemic exacerbated these pre-existing educational challenges. School closures persisted for 18 months (TBS, 2021), significantly impacting academic progress, especially among primary school students without access to distance learning technologies (Li et al., 2021). In response to the pandemic-induced school closures, the Bangladeshi government implemented educational programs via TV and radio, while urban private schools offered online classes. However, these initiatives encountered significant obstacles in rural areas, where access to TVs, radios, and smartphones was limited (BBS and UNICEF, 2019). An evaluation conducted by BRAC (2020) found that 56% of students did not participate in

any form of distance education. Among university students in rural areas, this non-participation rate increased to 60%

The prolonged closure of schools due to the Covid-19 pandemic heightened the risk of substantial learning losses, particularly for children in rural areas, where limited access to radio, television, and the internet, combined with a lack of parental understanding of their children's educational needs, exacerbated the situation. In response to these challenges, Hassan et al. (2024) and Wang et al. (2024) independently developed two 'low-tech' distance education interventions for primary school children and their caregivers in rural Bangladesh. These programs, provided free of charge—including the costs associated with phone calls—aimed to reach families through basic mobile devices, which are more widely used in these areas. A key feature of both programs was the active involvement of the children's primary caregivers, the majority of whom were mothers. These studies emphasize the critical role of contextually relevant and inclusive educational strategies, particularly in rural settings where access to technology is limited. By engaging caregivers and adapting interventions to local contexts, researchers and policymakers can contribute to more equitable learning outcomes for children in Bangladesh.

3. Research Design and Methodology

3.1. Study 1 intervention: Telementoring

Student volunteers from various local universities in Bangladesh were enlisted to serve as mentors, offering free learning support to primary school-age children and homeschooling advice to their mothers for 13 consecutive weeks in rural Bangladesh. Throughout the intervention period, each mentor contacted the mother at least once a week at a pre-determined time and day, providing remote support to the child's homeschooling over the phone. During these calls, mentors offered textbook solutions "on-demand" to the child, provided guidance to the mother in establishing weekly goals (such as time commitment and curriculum targets), and assisted both the child and the mother with homeschooling-related matters, including learning plans and solution keys. The mentors focused exclusively on two core subjects: mathematics and English. Each mentoring session had a duration of approximately 30 minutes. To facilitate these interactions, mentors received a

weekly top-up of 100+ minutes of phone credit, ensuring they could initiate the necessary weekly calls. Additionally, mothers received text messages every week containing tips, advice, and ideas to enhance parental engagement.

From a pool of over 7,500 children identified by GDRI, 968 mother-child dyads across 206 villages were randomly selected for participation. Out of this cohort, 838 satisfied the specific eligibility criteria, encompassing children in grades 1, 2, or 3, and households with consistent and reliable access to a phone. Within the group of 838 mother-child dyads, they were randomly divided into two equal halves: 419 were allocated to the treatment arm, receiving weekly telementoring, while the remaining 419 comprised the control arm, without any telementoring. Subsequently, during the endline phase, we conducted surveys, assessments, and experiments involving 814 mother-child dyads. For greater details of the intervention and its impact on the cognitive and homeschooling domains, refer to the Study 1's research report (Hassan et al., 2024).

3.2. Study 2 intervention: IVR-based education

In this intervention, a series of audio lessons utilizing the Interactive Audio Instruction (IAI) methodology were provided to participating children free of charge. IAI facilitates learners to pause, respond to questions and exercises verbally, and engage in physical and intellectual activities with a 'special helper,' mostly the mother in this case, while the program is in progress. To deliver these lessons to children in primary grades through basic mobile phones over 15 weeks, two toll-free numbers based on Interactive Voice Response (IVR) were established.

Comprising three modules divided into 75 lessons, each lesson has a duration of 16 to 18 minutes. In collaboration with two international organizations and a team of local curriculum experts and researchers, these modules, focusing on literacy, numeracy, and leadership, were exclusively designed for this program. The overarching goal of these modules is to complement the national curriculum and facilitate learning in out-of-school settings.

In contrast to Study 1, this intervention employed village-level randomization. GDRI, had previously operated in 222 villages across five sub-districts in the South-West

region of Bangladesh in a prior project. 90 of these villages were randomly selected and equally divided into three treatment arms. The Standard treatment (T1) group received a literacy and numeracy module, the Extended treatment (T2) group included an additional leadership module alongside the literacy and numeracy module, and the pure control (C) group did not receive any intervention.

Approximately 16-22 children were randomly chosen from each of these 90 villages, resulting in a final sample size of 1,763 children from 1,756 households. This included 596 children in T1, 586 children in T2, and 581 children in C. The educational intervention spanned 15 weeks, and endline data collection took place in November 2021. More details of the intervention and its effectiveness on the cognitive, noncognitive, behavioral, homeschooling, and parenting domains are discussed in Study 2's research report (Wang et al., 2024). For our analysis here, we pool T1 and T2 given that both groups yield similar treatment effects on a wide range of cognitive, noncognitive, behavioral, homeschooling, and parenting domains (Wang et al., 2024). Fig. 1 provides an overview of the activities in both studies.

3.3. Outcomes

Children's preferences are measured using standard lab-in-the-field economic experiments. We use three experiments where children decide as a spectator so that the experiments can be executed in a one-on-one setting while keeping a physical distance (to minimize the potential spread of Covid-19). These three tasks are related to the notion of distributive justice. The decisions made in the distributive justice experiments do not affect the earnings of the participants but have real consequences for the gift received by "other" non-participating children. These distributive tasks are adapted from previous studies with some modifications (Cappelen et al., 2020; Huppert et al., 2019). See in Section S1 of the Supplementary Information of this paper for a detailed script of these experiments. The other children receiving the gift are from villages/schools not in the sample or locale of the study. We distributed these candies after completing the survey and experiments. The specific choice scenarios are:

A. **Experiment 1 – Distributive preferences under unequal conditions:** Experiment 1 description of Section S1 of the Supplementary Information depicts

the two choice options in this experiment. Participants were briefed about two mutually exclusive options for distributing candies between two other anonymous children. Choice 1 represents an equal distribution, while Choice 2 represents an unequal distribution as one child gets fewer candies. The outcome variable that measures preference for equality is a dummy variable that takes the value of 1 for choice 1 and 0 for choice 2.

B. **Experiment 2 – Distributive preferences under unlucky condition:** Experiment 2 description of Section S1 shows the choice options in this experiment. Participants were informed of an existing allocation (8:0) between two other anonymous children and explained that skewed distribution was because one child was unlucky in a coin toss that resulted in the child not winning any of the candies while the other child won all the candies. Participants were then asked either to keep the current distribution or redistribute the candies according to their preferences. We use the number of candies redistributed to the unlucky child ($Y_i^{unlucky} \in \{0, 1, 2, \dots, 8\}$) to measure preference for redistribution (to the unfortunate by chance).

C. **Experiment 3 – Distributive preferences under unwell condition:** Experiment 3 description of Section S1 shows the choice options in this experiment, which is identical to panel B. The main difference here is that participants were informed of an existing allocation (8:0) between two other children and explained that skewed distribution was because one child had fallen sick, which prevented the child from participating in a task/game that resulted in the child not having any candies. We use the number of candies redistributed to the unwell child ($Y_i^{unwell} \in \{0, 1, 2, \dots, 8\}$) to measure preference for redistribution (to the unfortunate by ill health or the lack of opportunities).

We also conducted the same set of experiments with the mothers, but they made decisions about distribution/redistribution using different experimental currencies for two other anonymous non-participating mothers, i.e., mothers who are not part of these two studies. Instead of candies, the mothers distributed money, i.e., 20:20 BDT (Bangladeshi Taka) vs 10:50 BDT in Experiment 1, and 80:0 BDT in Experiments 2 and 3. After completing the experiments, GDRI identified local school children who met the experimental conditions to receive the earnings distributed by the participating children. For payment purposes, other children and mothers from villages or schools outside the study sample were selected only if they met the experimental criteria. For instance, in

Experiment 3, only those who had recently experienced an illness were chosen for payment.

3.4. Hypotheses

We make several hypotheses regarding the anticipated effects of the interventions on the distributive preferences of participating children as follows:

1. **Equality preference:** As Experiment 1 provides a neutral framing about the two anonymous children, we expect the majority of children to choose equality over inequality despite the efficiency tradeoff. As past research has shown that early-age educational interventions enhanced children's tendencies to split earned allocation equally (Cappelen et al., 2020), we anticipate that the interventions to increase treated children's preference for equality in Experiment 1.
2. **Redistributive preference:** Both interventions were provided free of charge to children in a time of crisis. The compassion and altruism provided by others could provide children with role models that instilled them a sense of distribution justice. The effect of role models might then translate into children's preference to redistribute to the unfortunate by chance (Experiment 2) and the unfortunate by ill health or unequal opportunities (Experiment 3). Similarly, these early-age educational interventions increased treated children's cognitive achievement (Wang et al., 2024). As children's social preferences can also be shaped by cognitive development (Batson and Shaw, 1991; Eisenberg et al., 2001; Hoffman, 2001), the interventions could also increase the treated children's preference to redistribute to the disadvantaged children through their impacts on children's cognition.
3. **Compassionate redistribution:** The redistribution in Experiment 2 and Experiment 3 both elicit showing compassion for others. The extent of compassion elicited is expected to stronger in Experiment 3 than in Experiment 2 as the circumstance under which the disadvantaged child in Experiment 3 suffer is arguably more salient and worse – ill health and unequal opportunities as opposed to a fair coin toss. We anticipate that the treatment will exert a more pronounced effect on participating children's redistribution to the unfortunate by ill health or unequal opportunities (Experiment 3) than their redistribution to the unfortunate by chance (Experiment 2).
4. **Compassionate role model:** We expect Study 1 to yield stronger treatment effects on redistribution than Study 2. The strong interpersonal interaction between the child and the mentor in Study 1 could facilitate stronger role model effects through

the compassion and altruism shown by the volunteer mentor. In contrast, the intervention in Study 2 was also provided by the NGO out of compassion and altruism, but it did not involve volunteer mentors in the process.

5. **Parental preference transmission:** The mothers in both studies also participated in the interventions and any impacts on the mothers might then transmit to their children through role model effects. The mothers in Study 1 received direct help from the mentors, albeit to a lesser extent than children. The mothers in Study 2 did not receive any direct help, but indirectly participated in the program as a special helper and also benefited from the intervention given that the intervention was provided to them for free.

3.5. Sample Characteristics, Balance, and Attrition

Table S1 and Table S2 in the Supplementary Information present the characteristics and inter-group balance for the baseline and endline samples of Study 1, respectively. As of January 1, 2021, the participants had an average age of 7.73 years, with an age range of 7.00 to 9.95 years. Approximately 51% of the children were girls. The mean years of schooling for fathers and mothers were 6.01 and 6.98 years, respectively. These households predominantly belonged to a low socio-economic status, with an average monthly income of BDT 11,409 (USD 134) and an average homestead size of 8.40 decimals during the baseline survey in July 2020. The characteristics of the endline sample closely resembled those of the baseline sample.

Table S3 and Table S4 in the Supplementary Information detail the characteristics and inter-group balance for the baseline and endline samples of Study 2. The average age of participants in Study 2 was slightly lower than that of Study 1 participants, at 7.39 years as of January 1, 2021. Fathers' and mothers' education in number of years were 6.09 and 7.17 years, respectively. Overall, our sample predominantly represented a low socio-economic status.

In addition to presenting characteristics for the full sample, Table S1-Table S4 also demonstrate balanced characteristics across the treatment arms in both studies. Given the implementation of a randomized controlled trial, achieving inter-cluster balance was crucial for accurately identifying the treatment effect. Columns 1 and 2 in these tables showcase the characteristics of the sample in different treatment arms, revealing mostly similar

profiles. To further verify this balance, OLS regressions were conducted to examine whether baseline characteristics differed across treatment groups. In Column 4, p-values from F-tests are presented, and the values ($p > 0.05$) indicate no significant differences between treatment and control groups for observable characteristics. Also, these characteristics are not significant in the joint F-test as reported in the very last row of each table.

Despite the interventions being implemented during the Covid-19 crisis, we observe a low level of survey attrition between baseline and endline surveys of these two studies. Table S5 depicts the rate of survey attrition, which is about 3% and 4% in Study 1 and Study 2, respectively. These low rates of survey attritions are not different across treatment arms of the two studies as indicated by Pearson Chi-Square Test.

We also do not find differential attrition rates for both studies, according to the OLS regressions exhibited in Table S6. None of the treatments, household characteristics and interaction terms are significant when regressed with the attrition dummy (takes the value of 1 if any household missed endline survey and experiments).

3.6. Empirical Strategy

We estimate the following OLS regression specifications to examine the effect(s) of treatment(s) on participants' preferences for equality and redistribution:

$$\text{[Study 1]} \qquad \boldsymbol{Y_i = \alpha + \beta TS1_i + X_i'\theta + \varepsilon_i} \ \ldots\ldots\ldots \text{[1]}$$

where Y_i is an outcome of child i measured at the endline (outcomes are explained earlier); $TS1$ is an indicator for the telementoring treatment; β captures the intent to treat (ITT) effect – the average causal effect of receiving the treatment on an outcome; X is a vector of individual and household-specific characteristics that are included as controls.

$$\text{[Study 2]} \qquad Y_i = \alpha + \beta TS2_i + X_i'\theta + \varepsilon_i \ \ldots\ldots\ldots \text{[2]}$$

where Y_i is an outcome of child i measured at the endline; The variable $TS2_{ij}$ in Equation 2 is a pooled treatment indicator, which takes the value of 1 if a participant is in either treatment arm 1 or treatment arm 2 in Study 2, and 0 otherwise. β in Equation 2 informs the average of the causal effect of receiving any treatment on an outcome.

[Study 1 & 2] $Y_i = \alpha + \beta T_i + X_i'\theta + \varepsilon_i$ ……. [3]

Equation 3 is estimated by pooling both studies, Study 1 and Study 2. Here, T_i takes the value of 1 if the child participated in any of the treatment arms of any studies. We cluster the standard errors at the individual-level in Study 1 and at the village level in Study 2.

We estimate equations 1, 2, and 3 for the mothers too, where Y_i is an outcome of mother i, measured at the endline.

In all equations, we control for age of the children (in years), gender, baseline literacy and numeracy score, parents' education (number of years of education), household monthly income, household assets (homestead and farmland), and religion.

To explore the direct impact of interventions and the impacts channeled through better reading and numeracy performance or changes in mothers' preferences, two sets of equations are evaluated using ACME (Imai et al., 2010):

$$\boldsymbol{M_i = \alpha + \beta T_i + \Gamma' X_i + \varepsilon_i} \quad [4]$$

$$Y_i = \rho + \pi T_i + \delta M_i + \Gamma' X_i + \omega_i \quad [5]$$

where M_i is a mediator that captures test performance or mothers' preference. If test performance or mothers' preference is an important channel through which the intervention leads to an improvement in the child's learning, then $\beta\delta \neq 0$. ACME is carried out under the assumption of sequential ignorability, i.e., error terms from equations (4) and (5) are independent.

4. Results

We present our main results in three sections. First, we exhibit the distributions of choices in all three experiments. Second, we describe the treatment effects on the choices of the children. Third, we report the average causal mediation analysis.

4.1. Choice Overview

Fig. 2 provides an overview of the allocation decisions across all three experiments made by children in both the treatment and control groups. In Panel A1 and A2 of Fig. 2 we

present the distribution of preferences regarding equal (but inefficient, 2:2) versus unequal (but efficient, 1:5) distributions in Study 1 and Study 2, respectively. Notably, in Study 1, approximately 69% and 71% of children in the treatment and control groups, respectively, favored an equal distribution in the first experiment. A similar pattern is observed in Study 2, with 73% and 69% of children in the treatment and control groups, respectively, expressing a preference for equal distribution. This consistent inclination toward equality contrasts with findings from prior spectator experiments involving children of similar age groups, where a 51:49 ratio of equal versus unequal choices was observed (Cappelen et al., 2020).

Panel B1 and B2 of Fig. 2 present the percentage of children redistributing candies from the lucky child to the unlucky child in Study 1 and Study 2, respectively. In Study 1, around 60% and 51% of children in the treatment and control groups, respectively, favored an equal split by distributing four candies from the lucky child (who initially had eight candies) to the unlucky child (who initially had zero candies). In Study 2, the pattern of redistribution is similar, where approximately 55% and 51% of children in the treatment and control groups, respectively, chose an equal distribution. Strikingly, in both interventions, children in the treated group tended to distribute more candies to the unlucky child than children in the control group.

Panel C1 and C2 of Fig. 2 depict the percentage of children who distributed candies to the unwell child in Study 1 and Study 2, respectively. Here, the unwell child originally received zero candies due to their inability to participate in a game, while the other child originally received eight candidates. In Study 1, roughly 64% and 55% of children in the treatment and control groups, respectively, favored an equal split, distributing four candies to the unwell child. In Study 2, about 57% and 50% of children in the treatment and control groups, respectively, preferred an equal distribution. Once again, in both interventions, children in the treatment group exhibit a preference for distributing candies to the unwell child who had unequal opportunities due to illness.

Table S7 in the Supplementary Information provides further analysis of the correlation between the choices made by children across different experiments. Notably, a discernible pattern emerges: children displaying a preference for equal distribution in the first experiment tend to distribute more candies to both the unlucky and unwell children

in the subsequent experiments. This positive correlation underscores the consistency in children's decision-making across these three experimental scenarios.

Overall, the results indicate that children in the treatment and control groups had similar equality and efficiency preferences, but treated children were more likely to redistribute candies to the disadvantaged children. In particular, children in the treatment group exhibit a heightened tendency to redistribute more candies to the unwell child than the unlucky child.

4.2. Children's Equality and Redistributive Preferences

Table 1 presents the ordinary least squares (OLS) regression estimates of the treatment effects. Each regression estimates the choices made by children in an experimental condition against the treatment dummy. All regressions control for demographic characteristics and pre-intervention literacy and numeracy skills. Given that not all parents and children attended the full length of the interventions, these regressions offer intent-to-treat (ITT) estimates, capturing the average causal effects of the educational intervention on the equality and redistributive preferences of the program children. In the Supplementary Information, we show that our estimates are not sensitive to potential social desirability bias.

Table 1 shows that participation in the educational interventions did not affect children's preference for equality (and efficiency), as the estimated treatment effects are not statistically different from zero in columns 1, 4, and 7). However, treated children in both educational programs consistently redistributed a significantly greater number of candies to the unlucky child compared to children in the control group. The treatment effect is 0.21 in Study 1 (column 2) and 0.20 in Study 2 (column 5). Combining both studies, treated children redistributed an average of 0.16 candies more than control children did (column 8).

Table 1 also shows that treated children in both studies significantly redistributed more candies to the unwell child compared to children in the control group. The treatment effect is 0.23 in Study 1 (column 3), 0.20 in Study 2 (column 6), and 0.18 when we pool both studies (column 9). Notably, the magnitude of redistribution in this third experiment

(unwell condition) surpasses that in the second experiment (unlucky condition). Thus, the treatment effect on redistribution is stronger when compassion is more salient in both the educational program and redistribution rationale.

In summary, the participation in the educational programs amid the Covid-19 pandemic had led to increased inclination among children to redistribute from the fortunate to the unfortunate, especially when the initial disadvantage was the result of ill health and an unequal opportunity and when the program children had directly gained from the compassion and altruism of others. These findings suggest that educational interventions during early childhood can cultivate compassionate redistributive preferences.

4.3. Heterogenous Treatment Effect

Table 2 presents the effects of different treatments on children's redistributive preferences. Several interaction models are estimated based on Equation 1, including a binary variable representing either the child's gender, median baseline test scores, median household income, or median combined parental education level. Additionally, an interaction between the binary variable and treatment status is included to capture heterogeneous treatment effects. Panel A and Panel B of Table 2 show the estimates for Study 1: Telementoring and Study 2: IVR-based education programs, respectively.

The evidence in Panel A indicates that the impact of the Telementoring intervention on distributive preferences, across three different experimental conditions, is statistically similar for both boys and girls. Furthermore, the treatment effects are statistically comparable across family income levels and parental education levels. However, children with higher baseline scores tend to distribute less to the unlucky or unwell child compared to those with lower baseline scores. This finding aligns with the idea that high-achieving individuals may favor meritocracy, associating rewards with personal effort and ability, and consequently may oppose significant wealth redistribution. Despite this, we did not find a significant effect of endline test performance on redistributive preferences.

In Panel B, the IVR-based education programs exhibit no evidence of heterogeneous treatment effects on redistributive preferences. One exception arises under

the "unlucky" condition, where children from households with below-median income show a tendency to increase the distribution of candies. However, this effect is inconsistent across other experimental conditions and studies. The overall analysis of both the Telementoring and IVR-based education programs suggests that heterogeneous treatment effects are unlikely to emerge in non-personalized distance education programs.

4.4. Causal Mediation Analysis

It is plausible that the effects of the early-age educational interventions on children's redistributive preferences were present only because treated children also experienced significant improvement in their cognitive achievement (Hassan et al., 2024; Wang et al., 2024). It then follows that simply benefitting from the compassion and altruism of others in the absence of improved cognition, children's redistributive preferences will not be altered. To assess the potential role of cognition in mediating the effect, we employ average causal mediation analysis (ACME) to investigate the extent to which the total effects of the interventions are contributed by their direct effects and indirect effects through improved literacy and numeracy test scores (Imai et al., 2010). Panel A in Table 3 presents the ACME results of assessment test performance. In Study 1, the total effects are primarily due to the direct effects of the intervention. Specifically, less than 30% of the total effects on redistributive preferences can be attributed to improved cognitive test scores. In comparison, in Study 2, the findings are more mixed. 64% of the intervention's impact on children's tendency to redistribute candies to the unlucky child is indirectly channeled through improved cognitive test scores, whereas 37% of the intervention's impact on children's tendency to redistribute candies to the unwell child is indirectly channeled through improved cognitive test scores. Given that the indirect effect is generally less than 50% in the third experiment (unwell condition), the treatment effect on children's compassionate redistributive preference is not primarily channeled through improved cognition.

It is also plausible that the effects of the early-age educational interventions on children's redistributive preferences were present because the redistributive preferences of their mothers were altered as a result of their involvement in the interventions. Panel B in Table 3 presents the ACME results. In both studies, the indirect effects due to changes in

the mothers' preferences are no more than 20%. In the Supplementary Information, Table S8 shows that the treatment effects on mothers are also generally smaller. Thus, early-age educational interventions can influence children's redistributive preferences without necessarily altering parental preferences.

5. Discussion and Conclusion

This study investigates the effects of early-age educational interventions on children's preferences for wealth distribution. Two distinct interventions, implemented as randomized controlled trials (RCTs) during the Covid-19 pandemic, provided children with either direct mentoring or technology-based assistance, both free of charge. Analyzing data from three lab-in-the-field experiments, we found that while children in both treatment and control groups showed similar tendencies to choose equal (and less efficient) distributions, those in the treatment groups exhibited a greater propensity to redistribute resources, particularly to children facing misfortune due to chance or illness, compared to control children.

Our findings differ from prior research in some aspects. Earlier study reports that children are generally more willing to accept inequalities resulting from unequal opportunities rather than those arising from luck in outcomes (Cappelen et al., 2024). However, in our study, we have found the opposite. Children distributed more candies to other children who faced unequal opportunities due to illness compared to the unlucky children. Another notable difference is the high percentage of children opting for equal distribution (that is less efficient) and the absence of treatment effect on equality preference in the first experiment (Cappelen et al., 2020). However, the differences are likely due to the differences in experimental conditions. In prior research, children were asked to split allocation they had earned between themselves and an anonymous child (Cappelen et al., 2020). Thus, prior research examined whether children were altruistic and willing to share more equally with another child from what they had earned. In contrast, children in our experiment participated in a spectator experiment where they were asked to choose an equal or unequal distribution between two anonymous children without any effect on themselves. Importantly, previous research found that the early-age educational

intervention studied led to increased redistribution from the lucky child to the unlucky one, a result that our study also confirmed.

Significantly, previous research found that early-age educational interventions led to increased redistribution from lucky children to unlucky ones, a finding that our study also confirmed. However, we extend this by showing that treated children consistently redistributed more candies to both unlucky and unwell peers across the second and third experiments, compared to control children. These results indicate that early-age educational interventions can shift children's redistributive preferences and foster a stronger sense of distributive justice. Notably, the treatment effect was more pronounced when children redistributed to the unwell child with unequal opportunities than to the unlucky child. This effect was also stronger when program children interacted with volunteers, suggesting that educational interventions involving role models who demonstrate compassion and altruism are likely to have a greater impact on children's compassionate redistributive preferences. Our causal mediation analysis further suggests that early-age educational interventions can enhance children's compassionate redistributive preferences even without improving their cognitive achievement or altering parental preferences.

These findings contribute to the literature on the determinants of social preferences and have important policy implications. The immediate treatment effects on children's redistributive preferences highlight the potential of educational interventions, particularly those promoting altruism and compassion through direct human interaction, to strengthen children's sense of distributive justice. Given that these interventions occurred during the Covid-19 pandemic, the findings suggest that charitable interventions in times of crisis may shape future support for redistributive policies. Policies that promote such interventions at an early age, especially when children are in need, may influence their long-term support for measures addressing inequality and deprivation caused by unequal opportunities. Further research, particularly longitudinal studies, will be valuable in understanding the persistence and broader implications of these effects.

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Author Contributions

HH, AI, and LCW designed and performed the research. HH analyzed the data with input from AI and LCW. HH and LCW wrote the manuscript with input from AI. All authors approved the final paper for submission. Authors' names appear in alphabetical order.

Competing Interest

The authors declare no competing interest.

Figures and Tables

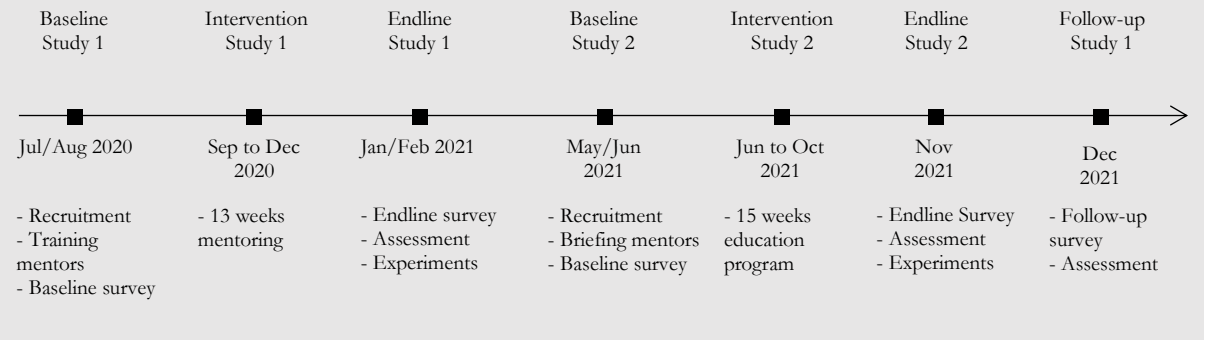

Fig. 1. Timeline of interventions and evaluations. In Bangladesh, schools closed on 17 March 2020 and reopened on 12 Sep 2021. Again, in 2022, schools were closed from 21 January to 28 February 2022. Now schools are fully operational.

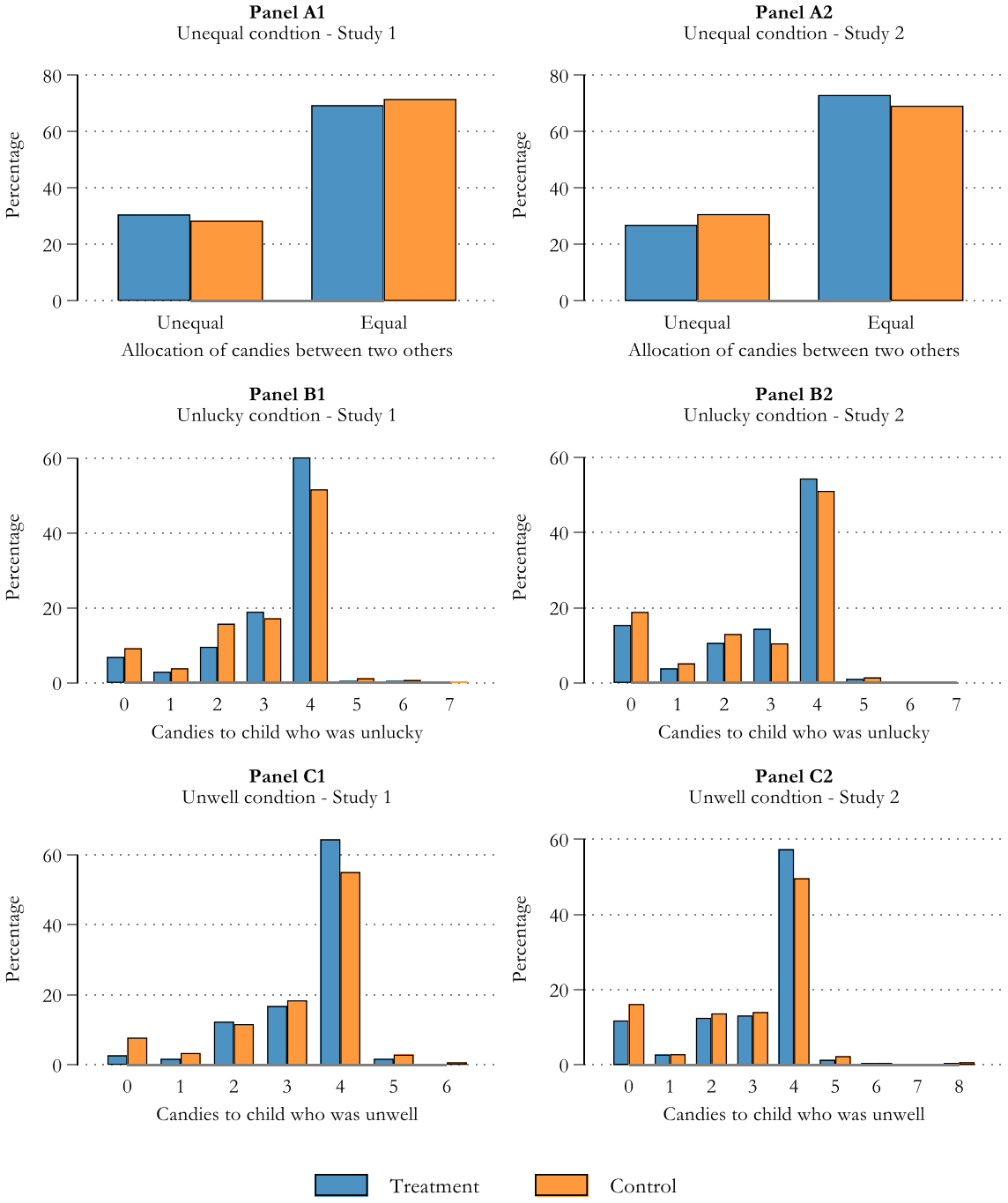

Fig. 2. Distributive preference under different conditions: allocation overview. This figure exhibits histograms of the choices made by the children in each of the three experiments of Study 1 and Study 2.

Table 1

Treatment effects on children's distributive preferences.

Variables	**X: Study 1**			**Y: Study 2**			**Z: Pooled**		
	(1) Unequal	(2) Unlucky	(3) Unwell	(4) Unequal	(5) Unlucky	(6) Unwell	(7) Unequal	(8) Unlucky	(9) Unwell
Treatment	-0.02	0.21**	0.23***	0.03	0.20*	0.20**	0.01	0.16*	0.18**
	(0.03)	(0.09)	(0.08)	(0.03)	(0.12)	(0.10)	(0.02)	(0.08)	(0.07)
Child age	0.00	0.11	0.17**	0.01	0.11	0.17**	0.01	0.18***	0.23***
	(0.04)	(0.10)	(0.08)	(0.02)	(0.08)	(0.07)	(0.02)	(0.06)	(0.05)
Child gender (Boy = 1)	0.02	0.09	-0.03	0.01	-0.08	0.01	0.01	-0.03	-0.00
	(0.03)	(0.09)	(0.08)	(0.02)	(0.07)	(0.08)	(0.02)	(0.06)	(0.06)
Baseline literacy score	0.01*	0.03*	0.02	-0.00	0.02	0.01	0.00	0.02*	0.01
	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)
Baseline numeracy score	0.01	0.01	0.01	0.01**	0.00	0.01	0.01**	0.01	0.01
	(0.01)	(0.02)	(0.02)	(0.00)	(0.02)	(0.02)	(0.00)	(0.01)	(0.01)
Father's education in years	0.00	0.01	-0.00	0.00	0.00	0.00	0.00	0.01	0.00
	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)
Mother's education in years	0.00	0.00	0.01	0.00	0.01	-0.00	0.00	0.01	-0.00
	(0.01)	(0.02)	(0.01)	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)
Family's monthly income	-0.00	-0.00	0.00	-0.00	-0.00	-0.00	-0.00	-0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Market value of household asset	0.00	0.00	-0.00	0.00	-0.00	-0.00	0.00*	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Religion (Islam = 1)	0.03	-0.30***	-0.10	0.03	-0.03	-0.04	0.03	-0.12	-0.07
	(0.04)	(0.10)	(0.10)	(0.04)	(0.12)	(0.09)	(0.03)	(0.08)	(0.07)
Constant	0.48	1.87**	1.52**	0.47***	1.51***	1.42***	0.49***	1.25***	1.13***
	(0.29)	(0.82)	(0.67)	(0.13)	(0.55)	(0.47)	(0.12)	(0.47)	(0.40)
Observation	814	814	814	1690	1690	1690	2504	2504	2504
R-Squared	0.01	0.03	0.02	0.01	0.01	0.01	0.01	0.02	0.02

Notes: This table reports regression results of treatment effects of the telementoring intervention (Study 1), IVR-based educational intervention (Study 2), and any intervention on the children (Study 1 & 2 combined). The treatment variable is a dummy variable that takes a value of 1 if a child is from any of the treatment groups. Unequal experiment – a binary choice between an unequal (0) and equal distribution (1). Unlucky experiment – number of candies given to the unlucky child. Unwell experiment - number of candies given to the unwell child. Robust standard errors, presented in parentheses, are clustered at the individual level for the Study 1 and at the village level for the Study 2. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 2

Heterogenous treatment effects of interventions.

Variables	W: Gender			X: Baseline score			Y: Household income			Z: Parental education		
	(1) Boy	(2) Girl	(3) Interaction	(4) Above median	(5) Below median	(6) Interaction	(7) Above median	(8) Below median	(9) Interaction	(10) Above median	(11) Below median	(12) Interaction
Panel A: Study 1												
Unequal	-0.01	-0.04	0.04	-0.01	-0.04	0.04	0.00	-0.06	0.07	-0.01	-0.03	0.02
	(0.04)	(0.05)	(0.06)	(0.04)	(0.05)	(0.06)	(0.04)	(0.05)	(0.07)	(0.04)	(0.05)	(0.06)
Unlucky	0.31**	0.12	0.18	0.03	0.41***	-0.40**	0.16	0.25*	-0.08	0.22*	0.20	0.02
	(0.13)	(0.13)	(0.18)	(0.12)	(0.13)	(0.18)	(0.11)	(0.15)	(0.19)	(0.12)	(0.14)	(0.18)
Unwell	0.35***	0.12	0.24	0.09	0.40***	-0.32**	0.24**	0.20	0.06	0.24**	0.24**	0.01
	(0.12)	(0.11)	(0.16)	(0.10)	(0.12)	(0.16)	(0.10)	(0.13)	(0.17)	(0.11)	(0.12)	(0.16)
Panel B: Study 2												
Unequal	0.02	0.05	-0.03	-0.00	0.07	-0.08	0.07*	-0.00	0.07	0.03	0.04	-0.00
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.06)	(0.04)	(0.04)	(0.05)	(0.04)	(0.04)	(0.05)
Unlucky	0.08	0.29**	-0.19	0.23	0.15	0.11	0.01	0.39***	-0.41**	0.16	0.22	-0.06
	(0.14)	(0.13)	(0.16)	(0.14)	(0.14)	(0.16)	(0.14)	(0.14)	(0.17)	(0.14)	(0.15)	(0.19)
Unwell	0.14	0.23*	-0.09	0.09	0.32**	-0.21	0.15	0.24**	-0.09	0.32**	0.03	0.28
	(0.13)	(0.13)	(0.17)	(0.12)	(0.14)	(0.16)	(0.14)	(0.12)	(0.17)	(0.13)	(0.14)	(0.18)

Notes: This table presents the heterogeneous treatment effects of the interventions on children's distributive preferences. Coefficients are estimated using OLS regressions. The dependent variable for each regression is listed in the first column. All specifications include a set of controls for household and child characteristics. Boy = dummy variable for boy participant; above-median = dummy (1 if the corresponding value is above the median); interaction = interaction term between treatment and gender or above median variable. Robust standard errors, presented in parentheses, are clustered at the individual level for the Study 1 and at the village level for the Study 2. *** p<0.01, ** p<0.05, * p<0.1.

Table 3

Average causal mediation effect on children's redistributive preference.

	Panel X: Study 1			Panel Y: Study 2			Panel Z: Pooled		
	(1) Unequal	(2) Unlucky	(3) Unwell	(4) Unequal	(5) Unlucky	(6) Unwell	(7) Unequal	(8) Unlucky	(9) Unwell
Panel A: Test performance									
Direct effect	-0.04	0.16	0.17*	0.00	0.06	0.13	-0.02	0.05	0.11
	(0.03)	(0.10)	(0.09)	(0.03)	(0.11)	(0.10)	(0.02)	(0.08)	(0.07)
Indirect effect (ACME)	0.02	0.06	0.07	0.04*	0.14*	0.08	0.03*	0.11*	0.07
	(0.01)	(0.04)	(0.04)	(0.01)	(0.03)	(0.03)	(0.01)	(0.03)	(0.02)
Total effect	-0.02	0.22*	0.24*	0.04	0.20	0.21*	0.02	0.16*	0.18*
	(0.03)	(0.09)	(0.08)	(0.03)	(0.11)	(0.10)	(0.02)	(0.08)	(0.07)
Percentage of total effect mediated via academic achievement of the children	-0.45	0.27	0.28*	0.83	0.64	0.37	1.11	0.66	0.39
	(6.94)	(0.24)	(0.14)	(3.74)	(2.32)	(0.59)	(10.40)	(1.24)	(0.31)
Corr$(\omega, \varepsilon \mid T)$ for ACME = 0	0.05	0.06	0.07	0.11	0.12	0.07	0.09	0.09	0.06
Panel B: Mothers' redistributive preference									
Direct effect	-0.02	0.21*	0.23*	0.03	0.18	0.17	0.02	0.15*	0.16*
	(0.03)	(0.09)	(0.08)	(0.03)	(0.11)	(0.10)	(0.02)	(0.08)	(0.07)
Indirect effect (ACME)	0.00	0.00	0.01	0.00	0.02	0.04*	0.00	0.01	0.02*
	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)	(0.02)	(0)	(0.01)	(0.01)
Total effect	-0.02	0.21*	0.24*	0.04	0.20	0.21*	0.02	0.16*	0.18*
	(0.03)	(0.09)	(0.08)	(0.03)	(0.11)	(0.10)	(0.02)	(0.08)	(0.07)
Percentage of total effect mediated via academic achievement of the children	0.08	0.00	0.02	0.03	0.11	0.20	-0.03	0.05	0.14
	(0.84)	(0.00)	(0.01)	(0.2)	(0.3)	(0.27)	(0.19)	(0.06)	(0.12)
Corr$(\omega, \varepsilon \mid T)$ for ACME = 0	0.15	-0.02	0.03	0.07	0.10	0.11	0.09	0.07	0.09

Notes: ACME is the average causal mediation effect. Robust standard errors are calculated using 95% CI, presented in parentheses, and are clustered at the individual level for the Study 1 and at the village level for the Study 2. * $p<0.05$, which is calculated manually from the 95% CI.

Supplementary Information

Shaping Preferences for Redistribution: The Impact of Early-Age Education on Inequality

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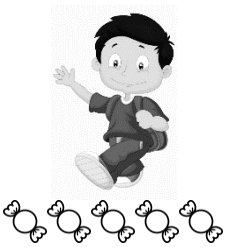
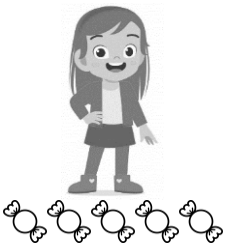
Supplementary Text

Section S1. Stimuli and scripts for the children's distributive justice games

[Explain to the children] Now we will play some fun games with you. We have selected a school from your Upazila (sub-district). We will give some candies to some children from that school. Each child is part of a pair. We have selected those pairs randomly and we will tell you how many candies each child from that pair will get. Our game has a fun twist. That is, you can decide who will get how many candies. Meaning, you can give more or fewer candies to anyone of that pair of children, if you want. We will write down what you decide. In that way, we will deliver candies to the children. We will not tell you the children's real names. So here we use some pseudonyms. Do you understand me? Do you have any questions? [Answer any questions] So let's start the game.

Experiment 1: Distributive preferences under equality condition

[Explain to the children with appropriate pictures] The two children of the school we have selected are [boy/girl names]. They are your age and study in the same grade. Candies can be distributed in two ways between them. We will give them in any way you want to give them. The distributions are–

Distributions	**Boy**		**Girl**	
	Bijoy	Akash	Pori	Sara
1 (2:2) - Equal				
2 (1:5) – Unequal				
Question	How do you distribute? option 1 or option 2		How do you distribute? option 1 or option 2	

Experiment 2: Distributive preferences under the unlucky condition

Two more such children from that school are [boys or girls]. They are your age and study in the same grade. To give them some candies, we did a toss to choose any one of them. [boy 1 or girl 1] won the toss and [boy 2 or girl 2] lost. As a result, we will give 8 candies to [boy 1 or girl 1]. And we will not give any candies to [boy 2 or girl 2]. As we said earlier, we will do whatever you decide. That's how we will give them the candies. According to our toss, the distributions are–

Boy		**Girl**	
Turya	Nabil	Ela	Korobi
	 0		 0
How do you share? How many candies do you want to give to Nabil?		How do you share? How many candies do you want to give to Korobi?	

Experiment 3: Distributive preferences under the unwell condition

Two other such children of that school are [boys or girls]. They are your age and study in the same grade. We organised a game [football for boys or skipping rope for girls] between them. But on the day of the game, [boy 2 or girl 2] had a high fever. So, they could not come to the game. [boy 1 or boy 2] was adjudged the winner of the game despite not playing. So, we want to give all 8 candies to [boy 1 or girl 1] as the winner. And we have decided not to give any candies to [boy 2 or girl 2] as a loser. As we said earlier, we will do whatever you decide. According to our decision, the distributions are–

Boy		**Girl**	
Sayem	Arif	Sayra	Juthi
 	 0	 	 0
How do you share? How many candies do you want to give to Arif?		How do you share? How many candies do you want to give to Juthi?	

Supplementary Tables

Table S1. Baseline sample characteristics of the telementoring program (Study 1)

Variable	(1)	(2)	(3)	(4)
	Treatment	Control	Total	F-test
Child age (1/1/2021)	7.72	7.73	7.73	0.94
	(0.02)	(0.02)	(0.02)	
Child gender (Boy = 1)	0.49	0.49	0.49	0.85
	(0.02)	(0.02)	(0.02)	
Baseline literacy score	16.12	16.24	16.18	0.83
	(0.19)	(0.20)	(0.14)	
Baseline numeracy score	14.78	14.75	14.76	0.78
	(0.14)	(0.15)	(0.10)	
Father's education in years	6.01	6.01	6.01	0.98
	(0.21)	(0.21)	(0.15)	
Mother's education in years	6.98	6.73	6.85	0.41
	(0.16)	(0.17)	(0.11)	
Family's monthly income	11409.31	11342.00	11375.66	0.96
	(278.70)	(226.46)	(179.45)	
Homestead land size in decimal	8.40	9.03	8.72	0.50
	(0.48)	(0.54)	(0.36)	
Market value of household asset	828242.19	726756.85	777499.52	0.61
	(152060.06)	(88455.66)	(87923.26)	
Religion (Islam = 1)	0.79	0.79	0.79	0.67
	(0.02)	(0.02)	(0.01)	
Observation	419	419	838	-
Village	159	171	200	-
Joint F-test p-value on individual/ household characteristics		0.99		

Notes: This table reports the background characteristics of the participants of the telementoring intervention during the baseline (Study 1). The rightmost column reported the *p-value* from the F-test of significance. Robust standard errors are in parentheses.

Table S2. Endline sample characteristics of the telementoring program (Study 1)

Variable	(1)	(2)	(3)	(4)
	Treatment	Control	Total	F-test
Child age (1/1/2021)	7.72	7.72	7.72	0.99
	(0.02)	(0.02)	(0.02)	
Child gender (Boy = 1)	0.50	0.50	0.50	1.00
	(0.02)	(0.02)	(0.02)	
Baseline literacy score	16.03	16.23	16.13	0.59
	(0.20)	(0.21)	(0.14)	
Baseline numeracy score	14.75	14.75	14.75	0.66
	(0.15)	(0.15)	(0.10)	
Father's education in years	6.00	5.99	5.99	0.91
	(0.21)	(0.21)	(0.15)	
Mother's education in years	6.98	6.73	6.86	0.39
	(0.16)	(0.17)	(0.12)	
Family's monthly income	11429.46	11348.29	11388.57	0.92
	(287.40)	(229.57)	(183.51)	
Homestead land size in decimal	8.17	9.08	8.63	0.28
	(0.45)	(0.55)	(0.36)	
Market value of household asset	826111.22	688063.71	756578.69	0.39
	(157316.04)	(76464.73)	(87040.73)	
Religion (Islam = 1)	0.79	0.80	0.79	0.68
	(0.02)	(0.02)	(0.01)	
Observation	404	410	814	-
Village	154	170	197	-
Joint F-test p-value on individual/ household characteristics		0.77		

Notes: This table reports the background characteristics of the participants of the telementoring intervention during the endline (Study 1). The rightmost column reported the p-value from the F-test of significance. Robust standard errors are in parentheses.

Table S3. Baseline sample characteristics of the IVR-based education program (Study 2)

Variable	(1)	(2)	(3)	(4)
	Treatment	Control	Total	F-test
Child age (1/1/2021)	7.38 (0.02)	7.38 (0.03)	7.38 (0.02)	0.90
Child gender (Boy = 1)	0.49 (0.02)	0.48 (0.02)	0.48 (0.01)	0.74
Baseline literacy score	16.81 (0.20)	16.86 (0.35)	16.82 (0.10)	0.89
Baseline numeracy score	14.76 (0.13)	14.84 (0.17)	14.79 (0.07)	0.70
Father's education in years	6.09 (0.18)	5.79 (0.26)	5.99 (0.11)	0.34
Mother's education in years	7.17 (0.16)	7.05 (0.21)	7.13 (0.09)	0.66
Family's monthly income	11084.84 (232.96)	11284.02 (435.12)	11150.48 (128.91)	0.69
Homestead land size in decimal	9.35 (0.52)	11.26 (1.11)	9.98 (0.43)	0.12
Market value of household asset	654344.92 (77587.57)	756346.89 (116339.17)	687959.86 (50718.23)	0.47
Religion (Islam = 1)	0.81 (0.03)	0.77 (0.05)	0.80 (0.01)	0.51
Observation (Households)	1182 (1178)	581 (578)	1763 (1756)	-
Village	60	30	90	-
Joint F-test p-value on individual/ household characteristics		0.66		

Notes: This table reports the background characteristics of the participants of the IVR-based education intervention during baseline (Study 2). The rightmost column reported the p-value from the F-test of significance. Robust standard errors are clustered at the village level in parentheses.

Table S4. Endline sample characteristics of the IVR-based education program (Study 2)

Variable	(1)	(2)	(3)	(4)
	Treatment	Control	Total	F-test
Child age (1/1/2021)	7.39	7.38	7.39	0.98
	(0.02)	(0.04)	(0.02)	
Child gender (Boy = 1)	0.49	0.49	0.49	0.96
	(0.02)	(0.02)	(0.01)	
Baseline literacy score	16.85	16.87	16.86	0.97
	(0.21)	(0.35)	(0.10)	
Baseline numeracy score	14.81	14.85	14.83	0.88
	(0.14)	(0.17)	(0.07)	
Father's education in years	6.07	5.76	5.97	0.35
	(0.19)	(0.26)	(0.11)	
Mother's education in years	7.16	7.04	7.12	0.66
	(0.16)	(0.21)	(0.09)	
Family's monthly income	11051.24	11293.97	11131.81	0.63
	(233.92)	(441.10)	(132.77)	
Homestead land size in decimal	9.27	11.12	9.89	0.14
	(0.51)	(1.12)	(0.44)	
Market value of household asset	651082.63	754860.15	685531.85	0.48
	(81057.20)	(120182.50)	(52713.60)	
Religion (Islam = 1)	0.82	0.77	0.80	0.46
	(0.03)	(0.05)	(0.01)	
Observation	1129	561	1690	-
	(1126)	(559)	(1685)	
Village	60	30	90	-
Joint F-test p-value on individual/ household characteristics		0.62		

Notes: This table reports the background characteristics of the IVR-based education intervention during endline 1 (Study 2). The rightmost column reported the *p-value* from the F-test of significance. Robust standard errors are clustered at the village level in parentheses.

Table S5. Survey attrition rate

Particulars	(1)		(2)		(3)	
	Treatment		Control		Total	
	N	%	N	%	N	%
Panel A: Study 1						
Not attrited	404	96.42	410	97.85	814	97.14
Attrited	15	3.58	9	2.15	24	2.86
Total	419	100	419	100	838	100
Panel B: Study 2						
Not attrited	1,129	95.52	561	96.56	1,690	95.86
Attrited	53	4.48	20	3.44	73	4.14
Total	1,182	100	581	100	1,763	100

Notes: This table reports the frequency of attrition at the endline surveys of two studies. [Study 1] Pearson's Chi-squared test: χ^2 = 1.54, P = 0.21. [Study 2] Pearson's Chi-squared test: χ^2 = 1.06, P = 0.30.

Table S6. Attrition, by treatment

Variables	Panel Y: Study 1		Panel Z: Study 2	
	(1) Attrition	(2) Attrition	(3) Attrition	(4) Attrition
Treatment dummy	0.01	0.02	0.01	-0.05
	(0.01)	(0.26)	(0.01)	(0.13)
Constant	0.02***	-0.23	0.03***	0.13
	(0.01)	(0.20)	(0.01)	(0.10)
Controls	No	Yes	No	Yes
Interaction terms (treatment dummy × controls)	No	Yes	No	Yes
Observations	838	838	1763	1763
R-squared	0.00	0.03	0.00	0.01
Joint F-test p-value on characteristics	-	0.70	-	0.71
Joint F-test p-value on interactions	-	0.56	-	0.52

Notes: All columns present estimates using OLS, where the dependent variable is a dummy variable for attrition (1 if the parent-child dyad did not participate in the endline survey and experiment). Control variables are – children's age, gender, baseline literacy score, baseline numeracy score, parents' education in years, family income, market value of household asset, and religion. Robust standard errors, presented in parentheses, are clustered at the individual level for the Study 1 and at the village level for the Study 2. *** p<0.01, ** p<0.05, * p<0.1.

Table S7. Correlations between experiments' responses

	Unequal	Unlucky	Unwell
Panel A: Study 1			
Unequal	1.00		
Unlucky	0.08*	1.00	
Unwell	0.10*	0.58*	1.00
Panel B: Study 2			
Unequal	1.00		
Unlucky	0.01	1.00	
Unwell	0.04	0.55*	1.00
Panel C: Pooled			
Unequal	1.00		
Unlucky	0.03	1.00	
Unwell	0.05*	0.56*	1.00

Notes: This table exhibits the pair-wise correlation between experiments' responses by the children. * $p<0.05$.

Table S8. Treatment effects on mothers' distributive preferences

Variables	X: Study 1			Y: Study 2			Z: Pooled		
	(1) Unequal	(2) Unlucky	(3) Unwell	(4) Unequal	(5) Unlucky	(6) Unwell	(7) Unequal	(8) Unlucky	(9) Unwell
Treatment	-0.02	-0.03	0.14**	0.01	0.14*	0.22***	-0.01	0.07	0.17***
	(0.02)	(0.07)	(0.07)	(0.04)	(0.07)	(0.08)	(0.03)	(0.05)	(0.05)
Education in years	0.01	-0.01	-0.02*	0.00	-0.00	-0.00	0.00	-0.01	-0.01
	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)
Spouse's education in years	0.00	0.00	0.01	-0.00	-0.01*	-0.01	0.00	-0.01	-0.01
	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)
Family's monthly income	-0.00	-0.00	-0.00	0.00	0.00	-0.00	-0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Market value of household asset	0.00**	-0.00	0.00	0.00	0.00	-0.00*	0.00	0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Religion (Islam = 1)	0.05*	-0.04	-0.03	0.01	-0.04	0.02	0.02	-0.04	0.00
	(0.03)	(0.09)	(0.09)	(0.03)	(0.07)	(0.08)	(0.02)	(0.06)	(0.06)
Constant	0.80***	0.21	0.22	0.79***	0.12	0.12	0.81***	0.14*	0.15*
	(0.04)	(0.14)	(0.13)	(0.05)	(0.11)	(0.10)	(0.04)	(0.09)	(0.08)
Observation	814	814	814	1690	1690	1690	2504	2504	2504
R-Squared	0.02	0.01	0.01	0.00	0.01	0.02	0.00	0.00	0.01

Notes: This table reports regression results of treatment effects of the telementoring intervention (Study 1), IVR-based educational intervention (Study 2), and any intervention (Study 1 & 2 combined) on the mothers of the participating children. Unequal experiment – a binary choice between an unequal (0) and equal distribution (1). Unlucky experiment – the amount of money given to the unlucky mother. Unwell experiment - the amount of money given to the unwell mother. Latter two variables are standardised i.e. [(y_i – mean of the control group)/standard deviation of control group]. Robust standard errors, presented in parentheses, are clustered at the individual level for the Study 1 and at the village level for the Study 2. *** p<0.01, ** p<0.05, * p<0.1.