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Behavioral Consequences of Religious Education

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Behavioral Consequences of Religious Schooling*

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Abstract

I investigate how long-term exposure to religious schooling affects economic behavior of children. To identify the effect of religious schooling, I study residential schools for orphans in Bangladesh that differ in terms of religious curriculum and social environment, limits transmission of beliefs and preferences from parents to children following being orphaned, makes social learning by children limited after school enrolment, and mitigates issues concerning endogenous school choice by parents. Using a lab-in-the-field experiment in this natural setting, I measure children's behavior and find that (i) children receiving religious education are more altruistic and honest relative to children receiving secular education; (ii) religious schooling does not affect risk aversion, cooperation, trust, and trustworthiness of children; and, (iii) behavioral differences are driven by children who are around puberty and completed primary education. My findings provide useful insights into how long-term exposure to religious schooling can affect behavior–possibly by shifting preferences–during childhood and adolescence.

Keywords: Economic behavior, preference formation, religious schooling, selection bias, lab-in-the-field experiment, Bangladesh.
JEL Classification: C9, D91, I21, Z12

1 Introduction

A growing literature in economics on child development highlights how important preferences, personalities, and skills are formed during childhood (Heckman, 2006; Fehr et al., 2008) and how that can be further shaped by nurture (Kosse et al., 2019; Cappelen et al., 2019). Childhood is considered a critical period, as appropriate investments, environment, and experiences can fabricate the foundation for adult skills, personality, and preferences that would consequently affect many adult outcomes (Knudsen et al., 2006; Heckman, 2007; Cunha & Heckman,

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2007). While family environment is considered a primary factor for healthy child development and preference formation during childhood (Kosse et al., 2019), school environment is also crucial for behavior and preference formation of children (Booth & Nolen, 2012a,b; Eckel et al., 2012; Lee et al., 2014). Despite being an important factor, we know little about how long-term exposure to an important and rapidly growing school category in developing countries – religious schooling – can affect economic behavior of children.[1] Because religious schools closely follow religious ideology and scriptures to teach students to be prosocial, honest, and forgiving, it is an important institution that can shape preferences of students and affect decision-making later in life. Thus, investigating such behavioral impacts have important implications for clarifying the mechanisms through which religious schooling may affect longer-term outcomes.[2]

In this paper, I study the effect of long-term exposure to religious schooling on economic behavior of children by exploiting a unique school setting in Bangladesh that allows me to mitigate selection bias. I carry out a lab-in-the-field experiment to measure behaviors, namely altruism, dishonesty, risk aversion, cooperation, and trust, of *orphaned* children from religious (i.e., Islamic) and secular *residential schools for orphans*. I focus on these five economically relevant behaviors because religious instructions in almost all major religions teach people to be other-regarding and honest, and the associations between religion and these five behaviors have been widely studied and empirically tested in social sciences.

Children in orphan schools reside and receive education within their school premises that are segregated from everyday social activities. Thus, children go through a restricted social learning process that relies largely on learning from the school curriculum, teachers, peers (who are also orphans), and school environment, with no learnings from family, relatives, and neighbors following being orphaned (on social learning, see Bandura (1977)). Moreover, the difference between religious and secular schools is more than just nominal – students in religious schools get much more rigorous religious teachings than students in secular schools. For instance, students in religious orphan schools devote a significant amount of their time to studying scripture and rituals; they also observe the rules of proper Muslim behavior such as reciting the *Quran* and *Hadith* daily, praying five times a day, fasting, following the Prophet's lifestyle closely, wearing religious outfits, and so on. However, students in secular schools are not bound by such restrictions.[3]

Another strength of this natural setting is that it allows me to mitigate selection bias.

[1] Religion and individual behavior formation have been widely studied in evolutionary anthropology, sociology, psychology, and, recently, in economics. For instance, Max Weber studied how Protestant work ethics drove economic progress in Northern Europe (Weber, 1930). Later, Becker & Woessmann (2009) and Cantoni (2015) empirically test Weber's hypothesis and find contradicting results (this divergence in results is thought to be due to differences in sample settings).

[2] Establishing the causal effect of religious schooling is difficult due to the non-random enrolment of children into religious schools (Altonji et al., 2005; Hungerman, 2014; Iyer, 2016). However, one could circumvent this endogeneity issue by either designing a large-scale randomized experiment (see Bryan et al. (2020)) or by exploiting a natural experiment, where children are randomly assigned to different institutions that happen to vary in religiosity (e.g., admission lottery). But conducting the former to explore the effect of long-term exposure to religious schooling would be challenging due to various constraints. Similarly, natural experiments of the sort are rare, particularly in developing countries. Thus, estimating how long-term exposure to religious schooling can shape economic behaviors—possibly by shifting preferences—of children that are already enrolled in schools can be a challenge.

[3] See Asadullah & Chaudhury (2009) and Asadullah (2016) on traditional Islamic education and how it differs from secular education in Bangladesh.

This endogeneity problem arises because children in orphan schools are not randomly admitted. Therefore, to address issues associated with selection, I exploit information on where children lived during their pre-school years. If children grew up in non-family environments, such as in infant orphanages, then they must have had minimal interaction with their biological parents and siblings at an early age, lowering possibilities of learning (behaviors) from their biological family.

In this setting, orphan schools are composed of two types of children: (i) *family* children – children who spent their pre-school years with their own family but were eventually abandoned and admitted to orphanages by their family, and (ii) *non-family* children – children who did not have a family before joining schools and, hence, they were brought up under a non-family environment during their pre-school years, such as in orphanages for infants or the street. Since parents pass on their behaviors, beliefs, and preferences to their children (Eisenberg & Mussen, 1989; Bisin & Verdier, 2001; Dohmen et al., 2011), it is possible that behaviors of children who grew up in a *non-family* environment prior to starting school would have weak or no correlation with behaviors of their parents.[4] Therefore, among the *non-family* children, factors such as individual and family behaviors, preferences, or characteristics, parental pressure, endorsed norms within neighborhoods, and so on, are uncorrelated or, at least, weakly correlated with school participation. Moreover, this unique information also allows for testing the existence and magnitude of selection bias. To my knowledge, this is the first study to exploit such a unique natural setting to mitigate selection bias.

To measure behaviors, I ran a battery of controlled experiments on orphan children to capture their level of altruism, dishonesty, risk aversion, cooperation, and trust. These five behaviors that I measure are both directly and indirectly associated with religious teachings, and also explain a plethora of economic decisions and outcomes. To collect data, I recruited male orphans from six different orphan schools in the Rajshahi district of Bangladesh that either follow an Islamic or a secular curriculum and social environment. In each orphan school, children between the age of 8 and 18 participated in experimental sessions that involved completing simplified versions of five established experimental games suitable for all school-going children: donation (Eckel & Grossman, 1996), dishonesty (Hanna & Wang, 2017), risk aversion (Gneezy & Potters, 1997), cooperation (Goette et al., 2006), and trust (Berg et al., 1995). Children were randomly assigned to one of the two sessions that always ran simultaneously where they made decisions in private. Since religious and secular orphan schools differ in terms of religious teachings, I expect children to behave in accordance with the type of education they receive.

My first set of results focuses on the *non-family* children sample only. I find that children from religious schools donated significantly more to the charity relative to children from secular schools and this result is in accordance with Islamic teachings on charity (*Sadaqah* and *Zakat*).

[4]Children develop various characteristics and skills by learning from their parents (Eisenberg & Mussen, 1989; Maccoby, 2000), mostly around their school joining age (Alan et al., 2017). Similarly, family also plays a crucial role in shaping individual economic behaviors and attitudes (Alesina & Giuliano, 2010). Although children could still be influenced by their *non-family* caregivers at infant orphanages, the transmission of behaviors and preferences from caregivers should not be as strong as that from parents. This is because children are biologically prepared to develop a strong attachment with their mothers during the first year of their lives and are unlikely to form principal attachments with other secondary figures, such as babysitters, caregivers, or teachers, later in life (Bowlby, 1969; Ricciuti, 1974; Ainsworth, 1979; Farran & Ramey, 1977). Moreover, some studies suggest parents do not influence behaviors of their children, rather peer groups play a major role in shaping individual behavior (Harris, 1995; Cipriani et al., 2013).

Moreover, on in-group bias in charitable giving, I find no evidence for children from religious (secular) schools donating more to religious (secular) recipients. Additionally, as per the belief that religion teaches people to be honest and truthful, I find that children from religious schools cheat significantly less in a private task relative to children from secular schools. However, I find no robust evidence on religious schooling having any statistically significant effect on risk-taking, cooperation, trust, and trustworthiness among children.

My next set of results focuses on the mechanisms. I find that the average behavioral differences captured are driven entirely by children who have hit puberty (thirteen years or older) and have completed primary education. Therefore, it seems, the main source of difference in behaviors is in the period when children are exposed to distinct religious rules (i.e., when strict religious practices such as *Ramadan* fasting and praying five-times daily become obligatory for adolescents in religious schools) and different educational pathways (i.e., when children after their primary education in religious schools begin training to memorize the *Quran* to be an Islamic scholar).

My final set of results focuses on selection and other biases. First, to quantify selection, I compare behaviors of *non-family* to that of *family* children. I find that *family* children are more altruistic, honest, and trusting relative to *non-family* children in religious schools. Moreover, among *family* children, I find evidence for positive selection of children into religious schools. Thus, excluding *family* children from the main analysis successfully mitigates selection bias. Second, I isolate behaviors of *non-family* children who are relatively 'new' incomers. That is, I zoom in on behavior of relatively young children (12 or younger) and those in their early years of schooling. If selection is driving the main behavioral differences between religious and secular school children, then I would observe a sharp difference in behaviors among this subsample. For this group, I find no difference in behaviors. Then, to show that selection could prevail at an early age, I compare behaviors of young *non-family* to young *family* children. I find that young *family* children differ from young *non-family* children within religious schools in terms of altruism, honesty, and trust, implying that selection can prevail even among the young. Therefore, insignificant differences between religious and secular young children who are *non-family* imply that *non-family* children must have been behaviorally similar when they were enrolled in orphan schools. Finally, I also assess whether *non-family* children's behaviors are influenced by *family* children. I find that such peer effects are only strong and sizeable in the case of altruism. However, the main results on *non-family* children do not change when I control for *family* children's influences in a formal regression framework. Thus, the main differences reported in this study are possibly causal.

To summarize, using controlled experiments in a natural setting, I show in this paper how long-term exposure to rigorous religious schooling can affect behavior—possibly by shifting preferences–during childhood and adolescence. Therefore, findings from this study corroborate findings from the existing literature on preference formation during childhood as well as on religion and its impact on economic decision-making and outcomes.[5] Besides, in this paper, I

[5]Various studies have shown how religion/religiosity influences economic outcomes, such as contributions to public goods (Benjamin et al., 2016); educational outcomes (Oosterbeek & van der Klaauw, 2013); discrimination (Chuah et al., 2016); economic growth and happiness (Campante & Yanagizawa-Drott, 2015); trust and ethics (Guiso et al., 2003); charitable giving (Shariff & Norenzayan, 2007); competition with social groups Norenzayan et al. (2016); views toward out-group members (Clingingsmith et al., 2009); and, mental health (Fruehwirth

also show how selection issues can be mitigated by carefully designing experiments in a natural setting. My study, thus, relates to the literature that incorporates controlled experiments in natural settings to study how habitat can endogenously shape preferences (Bowles, 1998), such as cooperation (Gneezy et al., 2016), competitiveness (Leibbrandt et al., 2013; Siddique & Vlassopoulos, 2020), altruism, risk, and time preferences (Voors et al., 2012), and antisocial behavior (Prediger et al., 2014). This study also relates to studies on the influence of education and the social environment in changing attitudes. For instance, by exploring individuals' media use and education data from nine Muslim countries, Gentzkow & Shapiro (2004) show that different education systems have very different effects on individual views towards Americans, where education with little Western influence creates strong negative views towards Americans. Similarly, beliefs on political views, social attitudes, political violence, and cooperation are also influenced by different teaching methods (Friedman et al., 2011; Algan et al., 2013). Separating *non-family* from *family* children to mitigate selection bias hinges on the concept that parents are likely to pass on their behaviors to their children, so one could relate my findings to the literature on transmission of characteristics from parents to their children (Bisin & Verdier, 2001, 2010; Cunha et al., 2010). On this, studies have found strong evidence on parents transmitting their willingness to take risks, prosociality, and patience to their children (Doepke & Zilibotti, 2008; Dohmen et al., 2011; Kosse et al., 2019; Deckers et al., 2019). Overall, my paper corroborates findings from studies that explore the role of education, family, and social environment in shaping preferences and how it affects behaviors.

I have organized the paper in the following way. Section 2 discusses the relationship between religious teachings and economic behaviors. Section 3 describes the field setting and section 4 describes the experimental design. I discuss the main results, possible mechanisms, and heterogeneity in section 5 and selection issues in section 6. Section 7 concludes.

2 Religion and economic behaviors

Among the many motivations behind giving to charity, gambling less, and fostering trust, truthfulness, and cooperation, one important motivation is religion (Norenzayan & Shariff, 2008; Guiso et al., 2003; Noussair et al., 2013; Asadullah, 2016). This is because religious instructions in almost all major religions teach people to be other-regarding and honest, where behavior and actions of one must not negatively affect others.[6] Therefore, the main purpose of this section is to understand how these five economically important behaviors are related to religious education. To do so, I discuss the existing literature in social sciences as well as Islamic teachings associated with these behaviors.

Charitable giving and cooperation. All major religions teach their adherents to be philanthropic, helpful, and sharing, which is usually done to please the deity and guaranteeing protection for oneself from tragedy (although it is often encouraged to do it unconditionally). Regarding this, Eckel & Grossman (2004) highlights some important quotes from religious scrip-

et al., 2018).

[6]Here risk-taking or gambling also fits well because all major religions such as Buddhism, Christianity, Hinduism, Islam, and Judaism consider gambling to be a sinful activity as winning by one (that is completely based on chances) involves loss of wealth of others. Also, gambling fosters greed and establishes faith in chances, rather than the God itself.

tures that encourage philanthropy, whereas Norenzayan & Shariff (2008) discusses sociological, psychological, and economic mechanisms behind religious prosociality. Association between religion and prosociality (charitable giving and cooperation) have been widely studied in social sciences, where the majority of studies have found it to be positive (McGranahan, 2000; Shariff & Norenzayan, 2007; Anderson & Mellor, 2009; Henrich et al., 2010; Bekkers & Wiepking, 2011); while few others have found no significant relationship between the two (Tan, 2006; Ahmed & Salas, 2011; Benjamin et al., 2016).[7] Although there seems to be conflicting evidence, in general, a higher level of religiosity has been found to encourage altruistic and cooperative behavior.[8]

On in-group bias in charitable giving, Preston & Ritter (2013) and the references therein report that religious people display significantly more prosocial attitudes (both in terms of helping others and giving to the charity) towards the in-group than the out-group members. Eckel & Grossman (2004) finds that, even though there is no overall difference in giving between groups, religious givers show more generosity towards church-based institutions than towards secular charities. They also find that non-religious individuals have a higher tendency to give to secular charities, for example to an HIV/AIDS-based charity.

Trust and dishonesty. Religion teaches its followers to establish trust in religious figures, authority, and deities (Hoffmann, 2013). Thus, developing complete faith in God and establishing faith and goodwill between people (e.g., friends, neighbors, relatives, etc.) originates from its core teachings. Moreover, the notion of betraying or lacking trustworthiness is also considered a heinous sin and is taught to be avoided by all means. Studies on trust, trustworthiness, and religion show that religious people show more trust towards others (Asadullah, 2016; Fehr et al., 2003) as well as towards people who are also religious, exhibiting bias towards the in-group (Fershtman et al., 2005; Tan & Vogel, 2008; Norenzayan & Shariff, 2008), and such behavior is reciprocated when it came to exhibiting trustworthiness (Tan & Vogel, 2008; Norenzayan & Shariff, 2008). Along with 'faith', the term 'trust' in religious scriptures is also synonymous with another profoundly important concept: 'truth' or 'honesty'. On this, Mazar et al. (2008) shows that people cheat less often when they are reminded about God, which is also consistent with other studies, such as Brown & Choong (2003) and Storch & Storch (2001). Likewise, cheating behavior diminishes when God is viewed as a more punishing and less loving figure (Shariff & Norenzayan, 2011). In contrast, Bruggeman & Hart (1996) found no such association.

Cheating, lying, or deceit is forbidden in almost all religions, where one of the many reasons behind such teaching is due to its consequence of misleading others into unlawful and unjust paths. Hence, truthfulness and trust coincide with each other where being truthful establishes trust among people.

Risk aversion. The relationship between risk-taking and religion has originated through gambling, wherein gambling is considered a sinful activity. Religious instructions advise people to determine the morality of gambling through its motivations and outcomes. The motivation

[7] Survey data from the Independent Sector (2002) also indicate that religious individuals are more generous than non-religious individuals.

[8] On Islamic teachings on charity, one of its five pillars is *zakat* or wealth tax, which is a religious obligation to donate 2.5% of a person's total wealth every year. Moreover, there is *sadaqah* or voluntary charity, which can be both monetary and non-monetary, where non-monetary *sadaqah* concerns sharing and helping others that involve sharing wise advice, helping one another in every way of life, etc. Thus, religion teaches prosociality as a virtue and instructs individuals to treat others the way one would like to be treated.

comes from believing in chances that establish faith in chance instead of God. Similarly, the outcome is determined through chances where the winner puts no effort to win, whereas the loser grieves for lost wealth, which goes against the very core of many religions. On risk attitudes and religion, existing studies show that more religious people tend to be more risk-averse (Dohmen et al., 2011; Pope et al., 2018; Noussair et al., 2013; Hilary & Hui, 2009; Liu, 2010; Osoba, 2003), and this attitude is consistent with religion induced gambling norms (Hoffmann, 2000; Kumar et al., 2011; Benjamin et al., 2016). Benjamin et al. (2016) tests and confirms the conjecture that Catholics prefer more gambling than Protestants, whereas Kumar et al. (2011) shows Protestants make safer financial investments than Catholics. Since Islam also considers gambling (or *maisir*) a sinful activity, children from religious institutions are expected to be taught to comply with this anti-gambling *shari'a* law, which should develop their aversion towards risk.

3 School settings

To identify the effect of long-term exposure to religious schooling on economic behavior, I ran a battery of controlled experiments in *residential schools for orphans* in Bangladesh. In this section, I define this natural setting in detail.

3.1 Religious and secular orphan schools

Residential orphan schools in Bangladesh (orphan schools hereinafter) are institutions for orphaned children aged 6 or above. These children do not have their biological parents or family, either because the family permanently abandoned them or because families are missing, cannot be traced, or dead. Orphans live in school dormitories that are shared with other orphans. Along with dormitories, these schools also have large classrooms, mostly without tables and chairs. Thus, children sit on the floor during their classes. If orphan schools are over capacity, children use their classrooms as dormitories. Furthermore, schoolyards are also occasionally used as classrooms, where children sit on the floor in the open. Therefore, orphans both live and attend school within their school premises.

Orphan schools are of two major categories: either religious or secular. Religious orphan schools or *Qawmi Madrasa* orphanages are Islamic seminaries that strictly follow the theological curriculum and a devoted religious way of life. These schools are independent of state regulation and are managed by a private education board (called Befaqul Madarisil Arabia Bangladesh). Hence, they are supported by private donations. *Qawmi Madrasas* were established in the early twentieth century in the country with a theological curriculum that follows scriptural Islam very closely and focuses mostly on a person's spiritual development through teaching religious scriptures, jurisprudence, theological literature, and Islamic sciences.[9] Thus, children in religious orphan schools are taught about religious way of life, which involves reciting *Surahs* (or chapters) from the *Quran* daily, following *Hadiths* as guidance, performing *Salah* (or prayer) five times a day, fasting in the month of *Ramadan*, wearing Islamic clothing (*thawb* and *taqiyah* for boys and *hijab* and *niqab* for girls), discourage reading newspapers and magazines, and

[9]See also Delavande & Zafar (2015), Asadullah & Chaudhury (2009), and Bano (2008) for more details on Madrasas.

watching television (primarily on the ground of obscenity), drawing or production of images of living things, and so on (Bhuiyan, 2010). While young children are encouraged to perform daily prayers and fast during *Ramadan*, such rules become obligatory and strict when children become adolescents.

Until the primary, children in *Qawmi Madrasa* orphan schools follow both secular subjects, such as mathematics, Bangla, English, Arabic, Urdu, etc., and religious-based curriculum that focuses on the *Quran* and Islamic texts closely (Blanchard, 2007; Bano, 2008). After primary education, children begin their training to be an Islamic scholar called the *Hafiz*. The training, known as *Hifzul Quran*, requires children to memorize the *Quran* and attend classes on *Alimyath* (Bhuiyan, 2010).[10] In fact, this curriculum is followed by all 15,000 registered *Qawmi Madrasas* in the country, that are both residential (for orphans and non-orphans) and non-residential, that educate over 4 million students (Bano, 2008). Due to dated pedagogical techniques and static curricula, graduates from *Qawmi Madrasas* find it difficult to enroll into general schools for higher secondary education or colleges, and, hence, commonly work in the religious sector as prayer leaders, Islamic scholar, teachers in *Qawmi Madrasas*, and so on (Blanchard, 2007). However, few colleges now operate under the *Qawmi* education board in Bangladesh.

In contrast, secular orphan schools follow curricula that are identical to the secular national curricula followed by (secular) public and most private schools in Bangladesh. Children study mathematics, Bangla, English, Bangladesh studies, natural environment, etc., subjects at the primary level (i.e., until grade 5). Vocational subjects are then introduced after grade 5. From grades 6-10, children study between twelve to fourteen subjects, such as mathematics, Bangla, English, science, social science, arts and culture, etc. (Education Board Bangladesh, 2017). Therefore, subjects taught in secular orphan schools educate children more on non-religious reasoning and thinking. Moreover, secular orphan schools have relatively relaxed rules in terms of practicing Islam. For instance, there are no strict regulations on memorizing or following scriptures, praying, fasting, or watching television in secular orphan schools; however, these are strongly encouraged. In fact, secular schools hold group prayers five times a day, have dedicated teachers that teach the *Quran* to students and recites *Hadith* daily, etc. However, participation is not obligatory, rather strongly encouraged. Students in secular orphan schools are also required to follow various strict rules and regulations of the school – limited TV (often once a week), selected TV channels (only Bangla channels), limited play time, good behavior, etc. – and noncompliance often leads to detention.

To summarize, religious schooling involves studying scriptures (after primary) and practicing Islam closely, where various rules are obligatory for adolescents. In contrast, secular schools do not have strict regulations on practising Islam.

3.2 School admission

Regarding orphan school admission, there are no official criteria followed by orphan schools. Hence, I obtain the following information by merely interviewing school authorities and orphanage directors.

[10]The *Hifzul Quran* training generally begins early in the morning and ends in the evening every day. During this period, children devote most of their time memorizing the *Quran*, attending classes on *Alimyath*, and Arabic grammar and literature. See Bhuiyan (2010) for more information on the *Hafizul Quran* training.

Children at orphan schools in my sample, who are usually aged 6 or above, come from three sources only: family, orphanages for infants (or baby homes), and streets. Many parents permanently abandoned their children as they (or widowed mothers) were unable to provide for their children. Children were also admitted by their relatives or distant family who were responsible for their care immediately after their parents' demise but later did not continue the care. Therefore, I call these children *family* children, because they were either with their biological parents or their relatives before they were permanently abandoned to orphan schools.[11]

On the other hand, many children grew up in infant orphanages where children were provided with care, pre-school education, recreational activities, etc. until they were 5 or 6 years old and could join an orphan school. Biological parents of most of these children had died. However, few children were permanently abandoned to infant orphanages by their biological parents when they were either infants or very young.[12] These infant orphanages are mostly NGOs or privately funded and are generally secular.[13] Then, at the age of 5 or 6, children from orphanages are either transferred to orphan schools or, on rare occasions, schools take in children from infant orphanages. Furthermore, children are also taken up from the streets (but rarely), such as orphaned children who beg for money and food at railway stations. In such cases, children had spent their pre-schooling years in a non-family environment prior to being admitted to an orphan school, so I call them *non-family* children in this paper.

To have more general insights into how orphans are placed into one school or another, I interviewed headmasters of all participating orphan schools and directors of three infant orphanages located in the same district. I present some important excerpts from my conversation with orphanage directors and school headmasters in Appendix B.1. In a nutshell, children are admitted to orphan schools based on space availability rather than individual characteristics or academic merit. Admission based on characteristics, such as children's etiquette, IQ, or agility, are also unlikely, as orphanages do not transfer orphans to schools individually, rather all school-aged children in each dormitory are often transferred to the same orphan school together (if capacity permits). Besides, these children often share the same dormitory in orphan schools following admission. Importantly, the decision (by infant orphanages) to admit orphans to religious versus secular schools depends entirely on orphan schools' capacity to accommodate children.[14]

[11] Orphan school authorities do not have information on whether *family* children were enrolled in other schools prior to joining the orphan school.

[12] Due to lack of record-keeping in orphan schools, I do not know the age when children were admitted to infant orphanages or where they lived prior to joining infant homes.

[13] Note that many Christian missionaries operate baby homes and orphan schools in Bangladesh and are, thus, religious (in such organizations, children from religious baby homes go to mission schools). To my knowledge, *Qawmi Madrasas* do not operate any baby homes in Bangladesh. Besides, there are very few government-funded baby homes in Bangladesh. Following the time spent in government-funded infant orphanages, children are transferred to public orphan schools, which are called *shishu poribar* (Bangladesh Ministry of Social Welfare, 2015; UNICEF, 2009). Schools that participated in this study are neither Christian missionaries nor public orphan schools.

[14] Other possibilities of 'vertical' transmissions of preferences might persist, such as carers in infant orphanages might pass on their beliefs and preferences to children. However, carers do not get to select or choose orphan schools for children. Such decisions are always made by the orphanage directors.

4 The experiment

I measure five economically important behavior using five standard decision-making games that children played inside their school classrooms. I measure altruism of individuals using a donation game (Eckel & Grossman, 1996), risk aversion using a simple one-shot investment game (Gneezy & Potters, 1997), and dishonesty using Hanna & Wang (2017). Then to measure cooperation and trust, I use a simultaneous prisoner's dilemma game as played in Goette et al. (2006) and a trust game (Berg et al., 1995) respectively.

4.1 Experiment procedure

I invited seven orphan schools (4 religious and 3 secular) from three subdistricts of the Rajshahi district in Bangladesh to take part in this study, which took place in September 2017. In total, 3 religious and 3 secular orphan schools participated – one religious and one secular from each subdistrict.[15] Before experiments, I advertised a call for participation for children aged 8 or above from the six participating orphan schools. At the end of a school day, teachers informed their students (aged 8 or above) about the experiment and rewards involved, and the option to sign-up for the experiment. In total, 210 students participated in this study ($N_{total} = 210$), among which 146 students were *non-family* ($N_{nf} = 146$). All students who signed-up participated in the experiment.[16]

I ran parallel sessions (i.e., sessions in pairs) in all schools where all students were able to finish at the same time. Firstly, it eliminated the possibility of contamination. Then, it also allowed me to pair students from one session (e.g., classroom A) to students from another (e.g., classroom B) in *Trust* and *Prisoner's Dilemma* games. Prior to each session, students were asked to form a queue outside the classrooms and were randomly sent to either classroom A or B by one of the assistants. There was an enrolment desk at the entrance where students were given an ID and were seated according to their unique ID number. All students had cardboard boxes in front so that neither the other participants nor the experimenter could observe their choices. Once everyone was seated, the experimenter read out a general instruction as an introduction and then read out instructions for the games, one at a time. Along with instructions, the experimenter also read out some examples and frequently asked questions and their answers to make sure everyone understood the instructions correctly.[17] Following examples, the experimenter demonstrated how one should make decisions using the pen, paper, and envelope provided. After clarifying any questions they might have, the experimenter left the room, and then participants played their games.[18] Since orphan schools were of different sizes (in terms of the number of students), sessions were also of varying sizes. The smallest parallel

[15] One religious orphan school did not participate because they were relocating from rented to their newly owned school premises located in the same district.

[16] In my sample, all orphanages are non-government-owned and boys-only schools. Girls-only orphan schools are very small in number and are difficult to convince to participate in experiments. The relatively small number can be explained by the fact that girls are often married young in Bangladesh.

[17] Although I had subjects from diverse age groups, I could not give different instructions to different age groups due to data comparability issues. Instead, I followed Sutter & Kocher (2007) and provided many simple examples for each game to make sure everyone understands the instructions accurately.

[18] Note that the absence of an experimenter in the room allowed them to feel comfortable to cheat in this private task (Hanna & Wang, 2017). However, the experimenter waited right outside the classroom door and did not hear any children talking. The classroom door was kept open.

sessions had 10 participants in each whereas the largest parallel sessions had 21 participants in each. Each session happened on an off day, as determined by the school authority, and lasted for around 90 minutes. In total, I conducted 14 sessions (7 pairs). In the end, students were paid in cash and were instructed to go to prearranged pop-up shops to spend their winning money. To economize on space, I explain why cash incentives followed by an opportunity to spend money immediately at a pop-up shop were offered in Appendix B.2.

4.2 Experimental games

Subjects made decisions in a series of games that were both strategic and individual decision problems. The design for each game is laid out below.

Donation game. To measure altruism, subjects were asked to play a simple donation game (Eckel & Grossman, 1996). Participants were endowed with 50 Taka (or 0.60 USD approx.) and then were asked how much of this money would they like to donate to an orphan school that was either religious or secular.[19] I implicitly revealed the religious affiliation of the recipient school by using religious and secular sounding names. Hence, there were two treatments in this game, where subjects were either paired with a religious or a secular recipient, which makes this a 2×2 design.

Dishonesty game. Dishonesty was measured using a dishonesty game (Hanna & Wang, 2017), where subjects were asked to roll a six-sided die 10 times, in private, where payoff would equal to the corresponding number of the die in Taka. For example, if the die comes up with 1, 2, 3, 4, 5, and 6 then payoff would be 1, 2, 3, 4, 5, and 6 Taka respectively. Thus, the minimum possible payment from this task is 10 Taka (for all 1's) and the maximum is 60 Taka (for all 6's). After rolling each die, subjects recorded the number of each roll, which they did privately (with cardboard boxes in front) and in the absence of an experimenter in the room that allowed them to feel comfortable to cheat.

Investment (risk) game. To capture risk aversion, I used a simple investment game (Gneezy & Potters, 1997) where subjects were given an endowment of 50 Taka to decide whether to invest it into a risky lottery that had a 50 percent probability of winning (determined by a coin toss). If heads came up, the lottery yielded 3 times the amount invested (high expected return); however, if tails came up, the subject lost any amount invested. Although it provides limited knowledge about risk aversion, measuring it in such a simple manner has been useful in comparing levels of risk aversion across distinct groups (Gneezy et al., 2016).

Prisoner's dilemma (cooperation) game. I used a simultaneous prisoner's dilemma game to capture cooperation between peers (as played in Goette et al. (2006)). Players from one session (i.e., a classroom) were paired with players from another, where each player was endowed with 50 Taka. Players then simultaneously decided whether to keep the entire endowment to themselves or pass it all to the player they were paired with, where the transferred amount was always doubled. Therefore, if players decided to keep the money to themselves then it was considered a defection, whereas passing the entire endowment meant cooperation. None of the subjects knew with whom they were paired and, hence, played this game anonymously. Subjects were told about the decision of their paired players at the end, in private.

[19] 1 USD = 80 Taka at the time of the experiment.

Trust game. Using the trust game (Berg et al., 1995), I was able to capture the individual level of trust and trustworthiness that students had towards their peers. Sessions were randomly assigned to the role of 'trustor' or 'trustee', where all participants in the 'trustor' session played the role of trustors and all participants in the 'trustee' session played the role of trustees. These trustors and trustees were paired within schools and not between schools or school types, and, hence, captures trust and trustworthiness of students towards their peers only (i.e. students from the same school).[20] In this game, both trustors and trustees received an equal endowment of 50 Taka. In the first stage, trustors were asked to choose how much money they would like to send to their paired trustee (any amount between zero and fifty). I then tripled each amount sent. That is, if a trustor sent 10 Taka then the trustee received the tripled amount, which is $10 \times 3 = 30$ Taka. Then in the second stage, each trustee decided how much of the tripled amount would they like to return to the trustor. In this game, the amount sent by the trustor roughly measures their level of trust and the amount returned by the trustee roughly measures their level of trustworthiness.[21] Therefore, the final payoff of the trustor is the endowment minus any amount sent to the trustee, plus any amount received back from the trustee. Whereas the final payoff of the trustee is the endowment plus any tripled amount received from the trustor minus any amount returned. Subjects never knew with whom they were paired, so transfers between pairs were anonymous.

Payment. Subjects were told that they would be paid according to only one game, which would be determined by a lottery at the end, to minimize wealth effects. Also, the order of games was randomized. To economize on space, I explain the procedure of how games were conducted in Appendix B.3. Experimental instructions are provided in Appendix C.

5 Results

In this section, I investigate whether religious schooling affects behavior. To mitigate possible selections, I focus my analysis on *non-family* children in this section ($N_{nf} = 146$). Then in section 6, I use the entire children sample to document the magnitude and direction of selection bias.

5.1 Main results

Individual Characteristics. Table 1, Panel A, provides individual characteristics of *all* children. The average age of participants (*family* and *non-family* combined) is around 12 years with religious schools having relatively older pupils than secular schools (12.2 vs 11.7) and this difference is marginally significant using a two-sided Mann-Whitney U test (MW-test hereinafter: $p = 0.100$). Moreover, children in secular schools were older while joining schools relative to children in religious schools (MW-test: $p = 0.019$). In terms of years of schooling (i.e., the difference between current age and the age when joined school), participants have spent an average of 4.7 years, with children from religious schools spending more time. However, I do

[20] It was not possible to pair trustors from one school with trustees from another due to problems with the payment. I clarify this issue in Appendix B under "Incentives".

[21] However, before concluding about trust and trustworthiness of children, such transfers need to take individual risk aversion and altruism that is not conditional on the behavior of other participants into account (Cox, 2004). I control for these in my regressions.

not find any anthropometric (weight and height) differences across religious and secular school children.[22] I compare anthropometric measures to show that children have similar physical and growth characteristics and schools do not differ in terms of provisions of nutrition and other facilities to children that might affect their growth and development.

When I compare children's characteristics across religious and secular schools using only the *non-family* children sample, as shown in Panel B in Table 1, I find no significant differences in terms of age, years of schooling, weight, and height across the two school types (MW-test: all $p > 0.10$). However, the difference in terms of school joining age remains significant at 5% significance level. Thus, having similar characteristics of children roughly indicates that they might be fairly similar, except for the type of education they receive. Nevertheless, I control for age, years of schooling, and anthropometry in the regressions to ensure my results are robust.[23]

Altruism. Panel A in Table 2 and Graph A in Figure 1 illustrate the difference in charitable giving between religious and secular children. Religious children donated 39 percent of their endowment, while secular children donated 26 percent of their endowment and this difference is statistically significant (MW-test: $p = 0.002$). When I further examine whether children show any in-group bias in donation, statistical tests show that children from religious schools donated statistically the same amount to religious and secular recipients (MW-test: $p = 0.340$). Children from religious schools also donated more to secular recipients relative to what secular children donated and this difference is statistically significant at the 5% level ($p = 0.025$). Similarly, secular children also seem to have donated relatively more to religious recipients than to secular recipients; however, this difference fails to reach marginal significance (MW-test: $p = 0.104$), suggesting no in-group bias in charitable giving among secular children.

To warrant that my result is robust to the inclusion of control variables, I regress the proportion of endowment donated on school type while also controlling for age, quadratic of age, years of schooling, weight, and height using a simple OLS regression. I also include subdistrict fixed effects. Therefore, I estimate the following OLS regression:

$$Y_{ijk} = \alpha + \beta Religious\ Schooling_{jk} + \Gamma^{'} X_{ijk} + s_k + \epsilon_{ijk} \tag{1}$$

where Y is the outcome variable (in this case, the proportion of endowment donated) of child i from orphan school j in subdistrict k. *Religious Schooling* is an indicator for religious orphan schools; and X is a vector of controls such as age, quadratic of age, years of education, weight, and height of children.[24] s is the subdistrict fixed effects – thus, the comparisons are between children from religious and secular schools in the same subdistrict. In all specifications, I cluster standard errors by experimental sessions.[25] As the number of sessions is small (14), I also compute p-values using the wild-t cluster bootstrap method (with 1,000 replications) (Cameron et al., 2008, CGM). I present the CGM p-values in the main regression table (Table

[22] I also compare anthropometrics adjusted for age and find that children in religious school weigh about 1.6 kilograms less than children in secular schools, which is only marginally significant at 10% level.

[23] Note that the university research ethics committee did not allow me to collect additional individual information from children due to the vulnerability of this population.

[24] Recall that years of education is simply the current age minus the age when children joined the orphan school. Thus, to avoid multicollinearity, I do not have the school joining age as a control variable because it is correlated with the years of education variable.

[25] Clustering standard errors at the school level (with 6 clusters) does not change my results.

3).[26]

My first result shows that children from religious schools donated 13 percentage points more from their endowment than children from secular schools ($p = 0.005$). This is shown in the first column of Table 3. To show how the main coefficient of interest and R^2 change with covariates, I also incrementally add controls to this specification. I find that the coefficient on *Religious Schooling* is relatively stable with R^2 increasing, suggesting unobservables are unlikely to capture much variations (Oster, 2019). These results are reported in Table A.1 in Appendix A. In Column 2, I test for in-group biases, and to do that I add an indicator variable for the religious recipient (i.e., a religious orphanage, who was the recipient in the donation task) and then interact it with *Religious Schooling* in the main specification. Firstly, I check for any in-group bias among religious school children. This is the difference in donations received by religious and secular recipients from religious school children. Regression results show that the difference is not statistically significant at conventional levels (F-test $p = 0.967$).[27] Moreover, children from religious schools also donated significantly more to secular recipients than what children from secular schools donated ($p = 0.021$). However, children from secular schools do not exhibit any in- or out-group bias while making donations. Overall, I find that neither religious nor secular school children have any bias in terms of charitable giving toward their in-groups. I also present this result with controls being added incrementally in Table A.2 in Appendix A.

Dishonesty. I compare the distribution of reports by children coming from the two educational backgrounds, which allows me to check whether one group is more dishonest than the other. The summary of the dice task is shown in Panel B of Table 2 and Graph B in Figure 1, both in terms of points recorded by children and the frequency of recording the highest two numbers (i.e., 5 and 6). I find significant differences in both. Specifically, children from secular schools record higher numbers in the dice task than children from religious schools (MW-test: $p = 0.009$ (points in dice task) and $p = 0.019$ (in recording 5s and 6s)). A two-sample Kolmogorov-Smirnov test also confirms the difference in the two distributions ($p = 0.008$ and $p = 0.016$ respectively). Using OLS, I regress recorded dice points on school type (*Religious Schooling*), which is presented in Column 3 in Table 3. I find that religious school children recorded 4.2 points less on average than secular children, which is significant at the 5% level ($p = 0.011$). This result does not change when I also control for session size. I also present this result with controls being added incrementally in Table A.3 in Appendix A. To check the robustness of this result, I also regress the 'frequency of 5s and 6s' on the school type and find

[26] As we have several regressions with a very similar set of regressors, wherein some dependent variables from some regressions are controls in other regressions (cooperation, trust, and trustworthiness), it is possible for the error terms to be correlated. Therefore, to allow for correlation between the error terms across the regressions, I also run a seemingly unrelated regression analysis to check if my initial results hold. The results presented below are robust to using a seemingly unrelated regression framework.

[27] To simplify Column 2, the coefficient on *Religious Schooling* gives me the difference in donations made by religious and secular school children when the recipient is a secular orphanage; similarly, the coefficient on *Religious Recipient* gives me the difference in donations received by religious and secular recipients from secular school children (i.e., in-group bias among secular school children); the interaction term gives me the difference-in-differences, where the first difference is donations received by religious and secular recipients from religious school children, and the second difference is that from secular school children. To obtain the first difference (which gives me the in-group bias among religious school children), I added *Religious Recipient* and *Religious Schooling* × *Religious Recipient*. An F-test confirms the difference to be statistically insignificant at conventional levels.

that children from religious schools were roughly 11 percentage points less likely to record 5s and 6s in the dice task relative to children from secular schools ($p = 0.032$). This result is also robust to using a probit model. Moreover, following Hanna & Wang (2017), comparing religious and secular school children's behavior to the theoretical distribution shows that children from both school types cheated in the dice task. These distributions are presented in Figure A.1 in Appendix A.

Cooperation and risk aversion. Using a simultaneous *Prisoner's Dilemma* game, I find that children from both religious and secular schools display the same level of cooperation (CS-test: $p = 0.563$), which is presented in Panel D of Table 2 and Graph D in Figure 1. I also find no significant difference in risk aversion between children from both school types (MW-test: $p = 0.275$; Panel C in Table 2 and Graph C in Figure 1). When I regress both on school type with usual controls while also controlling for risk aversion in the cooperation regression specification (column 5), I find no significant effects (Column 4 for Risk Aversion and Column 5 for Cooperation in Table 3). Although these are OLS estimates, running probit and logit regressions for cooperation also do not change this result. For simplicity, I only present the OLS estimates. I also present these results with controls being added incrementally in Tables A.4 (for risk aversion) and A.5 (for cooperation) in Appendix A.

Trust and trustworthiness. Through the *Trust* game, I measure both trust and trustworthiness among children. Panel E in Table 2 and Graphs E and F in Figure 1 shows the raw transfers made by trustors and trustees. Comparing mean transfers show that neither trust nor trustworthiness differs across children from the two school types (MW-test: $p = 0.230$ and $p = 0.668$ respectively). I, then, regress trust on school type (Table 3; Column 6;). Along with the usual controls, I also control for risk aversion and altruism of individuals, as aversion towards risk and the level of altruism (as measured in the *Donation Game*) might affect the way someone makes initial transfers in the *Trust* game (Cox, 2004). I find that trust among religious school children does not statistically differ from trust among secular school children ($p = 0.982$). Similarly, when I regress back transfers (i.e., trustworthiness) on school type with the usual set of controls (excluding risk aversion, as risk is not involved in back transfers while keeping altruism as a control), I find that religious school children also do not differ in terms of trustworthiness from secular school children ($p = 0.245$). The latter is presented in Column 7 of Table 3. I also report these results with controls being added incrementally in Tables A.6 (for trust) and A.7 (for trustworthiness) in Appendix A.

5.2 Multiple hypothesis testing corrections

Since I estimate linear regressions for multiple outcomes, it is important that I correct p-values for each outcome I have tested. Otherwise, having many hypotheses testing with a considerable amount of insignificant outcomes might raise the concern that the significant effects that I have reported are due to chances (i.e., due to Type-I error). Therefore, to address this concern, I re-do the regressions reported in Table 3 using the Westfall-Young adjustment that uses bootstrap resampling (with 5,000 replications) to account for correlations across different outcomes (Westfall & Young, 1993). I find that coefficients on *Religious Schooling* in columns 1 (Donation) and 3 (Dishonesty) remain statistically significant (the family-wise $p = 0.011$ and

$p = 0.029$ for specifications 1 and 3 respectively). However, insignificant effects from the other specifications remain statistically insignificant (the family-wise $p > 0.10$ for risk aversion, cooperation, trust, and trustworthiness). Although conservative, I also compute the Bonferroni adjusted p-values, which further strengthens the result obtained in specifications 1 and 3 (Bonferroni adjusted $p = 0.029$ for donation and $p = 0.056$ for dishonesty). This implies that results reported in this section are unlikely to be the consequences of multiple hypotheses testing.

5.3 Heterogeneity and potential mechanisms

I explore two potential mechanisms–directly associated with Islamic education–that might be driving the behavioral differences reported in section 5.1: (i) whether children have hit puberty; and, (ii) whether children have begun their *Hifzul Quran* training following primary education completion.

Islamic rules, such as *Ramadan* fasting and praying five times daily, become obligatory for children that enter puberty. Thus, children in religious schools are obligated to observe such religious practices when they are adolescents. However, these practices are not binding for adolescents in secular schools. Similarly, after completing primary education (i.e., grade 5), children in religious schools do not start secondary education (i.e., grade 6 and beyond). Instead, they begin training to be *Hafiz*, which is short for *Hafiz-e-Quran* and the training is called *Hifzul Quran* (Bano, 2008; Bhuiyan, 2010). To become a *Hafiz*, children are required to memorize the *Quran* and attend classes in *Alimyath* – the academic route to become an Islamic scholar (Bhuiyan, 2010). Thus, the rigorous *Hifzul Quran* training in religious schools can make children more competent in religious reasoning and thinking.

To examine these two sources of heterogeneity, I regress measured behaviors on the school dummy, a dummy for either hitting puberty (i.e., age above 12 years) or completing primary education, an interaction between the two, and the usual set of controls. These results are reported in Table 4. Specifically, panels A to F in Table 4 present results for the six experimentally measured behavior, where, under each panel, I present whether behavior varies by children's adolescence status and primary education completion. In columns 1 and 2 of Table 4, I divide the sample into two categories: one that belongs to the panel title category (column 1) and another that does not belong to the panel title category (column 2). Then, in column 3, I present the estimated difference between the two coefficients (i.e., column 1 minus column 2). Thus, column 3 reports the coefficient on the interaction term. I also present the summary of behavior by school type and age group in Figure A.2 and by school type and primary education completion in Figure A.3, both in Appendix A. Both figures suggest that adolescence and primary education completion play important roles in shaping economic behaviors.

In Table 4, I find that the average behavioral differences captured in altruism and dishonesty across religious and secular school children, as reported in columns 1 and 3 in Table 3, seem to be driven by those who are adolescents and have completed primary education. In particular, children from religious schools, who are 13 or above, have donated significantly more (column 1, panel A1) and cheated less (column 1, panel B1) than children from secular schools in the same age bracket. In contrast, children under 13, from both school types, are statistically the same in terms of altruism and dishonesty (column 2, panels A1 and B1 respectively). The difference

between these differences is statistically significant at 1% (for altruism) and 5% (for dishonesty) levels (column 3, panels A1 and B1 respectively). Interestingly, adolescents in religious schools also cooperate more than adolescents in secular schools (column 1, panel D1), with no difference across religious and secular children under 13 (column 2, panel D1), and the difference between these two is significant at 1% level (column 3, panel D1). However, I find no difference in risk aversion, trust, and trustworthiness by children's adolescent status.

Turning to primary education completion, I find that children in religious schools, who are training to be *Hafiz*, have marginally donated more (column 1, panel A2) and cheated significantly less (column 1, panel B2) than their counterparts from secular schools. However, such differences cannot be observed among children who are still in primary education (column 2, panels A2 and B2). While the difference between children above and below primary education in dishonesty is statistically significant at 1% level (column 3, panel B2), that in altruism fails to reach significance at conventional levels (column 3, panel A2). Finally, I do not observe any differences in risk aversion, cooperation, trust, and trustworthiness by children's primary education completion.

I also explore heterogeneity by two other characteristics: (i) age when children joined the orphan school, and (ii) their mid-term exam scores. In Table A.8 in Appendix A, I find that children who joined later in religious schools are more altruistic and less dishonest than children who joined later in secular schools (row R). On the other hand, children who enrolled at the age of six in religious schools are more risk-averse than those in secular schools (row R+RE). However, there is no heterogeneity in other behaviors. Likewise, behavior does not differ between children who enrolled at the age of six and children who enrolled after six within both secular (row E) and religious schools (row E+RE). Thus, one additional year of exposure to religious or secular schooling did not have any statistically significant impact on economic behavior of children. Similarly, in terms of mid-term exam performance (Table A.9 in Appendix A), I only find that bottom-performing children in religious schools trust less than bottom-performing children in secular schools (row R). Otherwise, I do not find any heterogeneity in any other behavior based on children's mid-term test scores. Although this analysis is exploratory, the results corroborate the claims made by orphanages and school authorities on having no merit-based admission rules.

In all, it appears that exposure to obligatory religious rules, rituals, and training are the main sources of difference in behavior that I find in this study.

6 Selection and other biases

6.1 Does this school setting mitigate selection?

It is crucial for the integrity of this study that this setting mitigates selection bias. For instance, if the inclusion of *family* children in the analysis does not affect my results then that would mean studying *non-family* children is as uninformative as studying the entire sample. To address this concern, I compare behaviors between *family* and *non-family* children to test if they differ. Table 5 presents the raw test differences. I find some evidence that *family* children are more altruistic, less dishonest, and are more trusting towards their peers than *non-*

family children, with all differences being statistically significant using a MW-test (under the 'Pooled' columns). When I split my data to separate religious and secular school children, I find that these differences can only be observed among children from religious schools (under the 'Religious' columns), whereas in secular schools *family* and *non-family* children do not statistically differ (all $p > 0.10$, under the 'Secular' columns). These raw comparisons provide some support to the notion that *family* children in religious schools coming from a (religious) family background must have already been taught to be more altruistic, honest, and trusting before being sent to orphanages. Thus, there is some evidence for positive selection of children into religious schools.

To ensure that these results are robust, I regress behaviors on school type, an indicator for *family* children (equals 1 if *family* and 0 otherwise), and their interactions while also controlling for age, quadratic of age, years of schooling, weight, height, and subdistrict fixed effects. This result is presented in Table 6. The coefficient on $Family\,Children$ gives me the effect of *family* relative to *non-family* children who study in secular schools. I find that this is marginally significant (at 10% level) and positive only for trust, suggesting *family* children are slightly more trusting than *non-family* children in secular schools. Similarly, adding 'Family Children' with the interaction term gives me the difference between *family* and *non-family* children in religious schools. I find this difference to be positive and statistically significant but only in the case of altruism and trust (F-test $p = 0.016$ in column 1 and $p = 0.010$ in column 5). That is, *family* children are more altruistic and trusting than *non-family* children in religious schools. Although I find raw differences in dishonesty, regression results show that *family* and *non-family* children do not statistically differ in terms of cheating in a private task, i.e., at recording both dice points ($p = 0.151$) and 5s and 6s ($p = 0.113$). However, for the rest, I do not find any significant differences. Therefore, the inclusion of *family* children from religious schools in the analysis would have overstated the impact of religious schooling on altruism by almost 10 percentage points and trust by almost 26 percentage points. Thus, among *family* children, there is some evidence of positive selection of children into religious schools.

6.2 Are younger children different?

One notable limitation of this study is that I do not have information on who admitted children to infant orphanages, at what age were they admitted, or what criteria infant orphanages followed while admitting children to orphan schools. Therefore, a *non-family* orphan child could be sent to a certain school based on his level of agility, academic merit, or even based on his (infant orphanage) carers' preferences or characteristics. If this is true then it is likely that religious carers at infant orphanages would bring up their children under a religious environment and are likely to send them to religious orphan schools, which would then overstate the effect captured in this study. In other words, under this scenario, *non-family* children who are sent to religious schools are going to be more prosocial, risk-averse, and honest during their early years of schooling relative to *non-family* children from secular schools in the same age bracket. Therefore, in the presence of such selections, I would observe a sharp difference in behaviors among children who are young and in their early years of schooling. However, if I find no such differences among younger children then that would suggest that selection might not be the key

driver of differences that I have captured.

Following arguments laid out in Delavande & Zafar (2015), I look at the difference in behavior across religious and secular school children who are 12 years or younger. These results are available in column 2 in Table 4. In panels A1 through F1, all but one difference are statistically insignificant at conventional levels. The only significant difference observed is in risk-taking, which is marginal and significant at 10% level (column 2, panel C1). Moreover, among children who are still in the primary education level (i.e., grade 5 or below), none of the differences in behavior between religious and secular school children are statistically significant at conventional levels (column 2, panels A2-F2). This suggests that children are possibly similar (in terms of behavior) during their early school years and, hence, might not have been positively selected into religious schools. It is possible that children did not develop a strong attachment with the caregivers at infant orphanages and (Bowlby, 1969; Ricciuti, 1974; Ainsworth, 1979; Farran & Ramey, 1977), thus, children's preferences were not correlated with that of their caregivers when they were admitted to orphan schools.

To show that selection could prevail at an early age, I compare behavior of young *non-family* to young *family* children (12 or younger). These results are reported in Table A.10 in Appendix A. I find that young *family* children differ in terms of altruism, honesty, and trust than young *non-family* children within religious schools, implying that selection can prevail even among the young. Therefore, insignificant differences between religious and secular young children that are *non-family* suggest that *non-family* children must have been similar in terms of behavior when they joined orphan schools.

6.3 Peer effects

We now turn to another source of bias: peer effects. In subsection 6.1, I show that, among *family* children, there is some evidence of positive selection into religious schools. Thus, it is possible that *non-family* children 'learn' from their close peers in schools that are *family* children. In that case, estimated β from equation 1 would be biased as the effect of religious schooling confounds selection effects (through *family* peers). To assess this, I exploit with whom children share their dormitories in orphan schools.[28] From section 3, we know that schools always assign children coming from the same infant orphanage (who often shared dormitories at infant orphanages and are possibly friends) to the same dormitory in orphan schools. In contrast, *family* children always get assigned to dormitories with available spaces. Thus, dormitory assignment is not entirely random but it is also unlikely to be based on children's characteristics. Using the information with whom children share their dormitories, I compute the proportion of *family* children in each dormitory (or *family* peers) in each school.[29] To be consistent with previous analyses, I divide the dormitories into two groups (into a dummy variable) based on the proportion of *family* peers: dormitories with *family* peers above the median proportion (*high exposure*) and that below the median proportion (*low exposure*). I, then, augment regression

[28] Given the restrictions imposed by the institutional review board on collecting sensitive and personal information from this vulnerable population, I do not have information on individual children's friendship networks within schools. Therefore, I cannot directly assess the effect of having best friends that are *family* children on behavior.

[29] 'Proportion' equals the number of family children in a dormitory divided by the total number of children in the dormitory. Thus, a higher number corresponds to having more family children as dormitory mates.

model 1 by adding this indicator variable and the interaction of this indicator variable with the school type dummy as independent variables. Estimates are presented in Table 7.

I find that the coefficients on the interaction term are statistically insignificant throughout. That is, the effect of religious schooling does not vary by exposure to dormitory peers that are *family* children. Also, the interaction term does not reach statistical significance at conventional levels when the 'proportion' variable is continuous instead of categorical (see Table A.11 in Appendix A). A reasonable explanation is that, since *non-family* children share dormitories with their (*non-family*) peers from infant orphanages, the friendship (and, thus, interactions) between *non-family* dormitory peers may be stronger relative to the friendship between *non-family* and *family* peers within dormitories. Thus, being exposed to different numbers of *family* peers in dormitories have no impact on *non-family* children's behavior. As an additional robustness check, I also compute the average behavior of *family* peers within dormitories and then check whether it correlates with the behavior of *non-family* peers.[30] If correlations are positive and statistically significant then *family* peers might have some influence on the behavior of *non-family* peers in the same dormitory. These correlation coefficients are reported in Table A.12 in Appendix A, where the only statistical significance can be observed in column 1, in terms of altruism. Then, when I reproduce Table 3 with the average behavior of family children peers as an additional control, I find that the main results become weak but remain statistically significant (see Table A.13 in Appendix A). However, due to collinearity between the school type variable and the average behavior of *family* peers variable, standard errors appear to be inflated (for instance, by twofold in column 1; Table A.13 in Appendix A), which is possibly influencing the t-statistics and producing weaker results.

6.4 Attrition

Finally, I briefly discuss why attrition is unlikely to bias my main results. There are three sources of 'potential' attrition in play here. First, bias due to expulsion of unruly children from schools. If religious orphan schools often expel or transfer children for not behaving according to their religious norms, then the remaining children in religious schools would be highly religious or altruistic for instance. However, in both religious and secular orphan schools, expelling orphans from schools or transferring them to other schools for unruly behavior is not common. On the expulsion of orphans, a headmaster from a religious school stated (translated from Bengali):

> *"We do not expel (orphan) children because they have no family left and have nowhere to go. Orphan schools, in general, are not very resourceful, so we cannot transfer our 'bad' children to other schools. Like us (our school), they (other schools) also have a very limited capacity... From the beginning, we know that these children are our responsibility and we are their only family. We cannot abandon them, can we?"*

Importantly, this view is shared by all schools in my sample. Thus, bias ensuing from such attrition is very unlikely in this study.

The second source of attrition bias is non-participation. Among the invited orphan schools

[30] That is, I compute six averages. For example, regarding donation, I compute the average donation made by *family* peers in each dormitory.

for the experiment (also discussed in subsection 4.1), only one orphan school could not participate as the school relocation (from rented to newly owned school premises) conflicted with the experiment dates. Moreover, according to school authorities, almost all eligible children (aged 8 or above) signed up and later participated in the experiment.[31] Thus, bias due to non-participation is also implausible.

The final source of bias is through attriting or withdrawing from the study during or after the experiment. In this study, none of the participants or schools withdrew or asked to withhold their data, discarding the possibility of bias via this source.

7 Conclusion

In this study, I exploit a natural school setting in Bangladesh to investigate how long-term exposure to religious schooling affects economic behavior. In schools for orphans, children are taught different educational curriculum and grow up in different social environments that happen to vary in terms of religiosity. Also, learning from family and relatives stops after children enrol in orphan schools. Thus, orphan children are not as imbued with the vertically-transmitted cultural characteristics that occur between family/parent and child. Under this setting, I measure orphan children's level of altruism, dishonesty, risk aversion, cooperation, trust, and trustworthiness using a battery of controlled experiments. Then, using the information on where children grew up during their early-childhood, I mitigate selection issues by restricting the analysis to children who spent most of their early-childhood in *non-family* environments, e.g., orphanages for infants. Eventually, I reach three main conclusions. Firstly, children receiving religious education are more altruistic and honest relative to children receiving secular education. Secondly, religious schooling does not have any effect on risk aversion, cooperation, trust, and trustworthiness of children. Finally, behavioral differences are entirely driven by children that are in their adolescence and have completed primary education.

In contrast to much recent work on the effects of religiosity, my study does not investigate the effect of short-term interventions, such as going to the *Hajj* pilgrimage, the length of the *Ramadan* fasting hours, or the month of *Ramadan* itself (Clingingsmith et al., 2009; Oosterbeek & van der Klaauw, 2013; Campante & Yanagizawa-Drott, 2015), but at the effect of long-term exposure to religious teachings over several years. Another important feature of this paper is that I directly aim to measure behavior of children that has important implications for clarifying the mechanisms through which religious education may affect longer-term outcomes. Moreover, in modern societies, one potential downside of religiosity is that it may create cleavages between the believers and non-believers, so by measuring the in-group bias in charitable giving, I can test this directly for both religious and secular school children. Finally, I test the effect of religious schooling among a vulnerable population that is of direct interest, i.e., orphans who are brought up with different degrees of religiosity, and in real-world institutions, the likes of which exist in many parts of the world. I also restricted my experiment to male orphans as recruiting children from female orphanages for experiments can be very difficult. It of course carries a cost in

[31] Only one orphan school (secular) reported that few children did not willingly sign up. Since I did not measure behavior of non-participating children, I cannot check whether unwillingness to participate was influenced by certain individual characteristics. However, the main results hold even if I drop this entire school from the analysis.

terms of external validity in being able to generalize to non-orphans and females; however, this population and this setting are both of intrinsic interest. Besides, this educational setting allows me to show that mitigating selection bias is possible in a natural environment when natural experiments are unavailable, and conducting large-scale randomized experiments is difficult.

Due to limitations, I cannot entirely rule out the possibility of selection driving the main differences captured. However, I believe that results from this study provide helpful insights into the formation of preferences during childhood and adolescence and how it is affected by religious schooling. This is crucial for education policy-makers as taking important features from the religious curriculum and introducing it to the secular curriculum might improve many long-term outcomes. Another notable limitation of this study is that the information on the enrolment of children in schools is not sufficiently detailed. Moreover, information on admission of children to infant orphanages, socioeconomic backgrounds of parents and carers, and survey measures of religiosity of individual children and their closest peers would have made this investigation more clear and precise. Thus, future research should explore such possibilities to corroborate findings from this important literature.

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Main Tables & Figures

Table 1: Children Characteristics

	Panel A: All Children					
Individual Characteristics	**Pooled** (*Std. Dev.*)	**Religious** (*Std. Dev.*)	**Secular** (*Std. Dev.*)	**MW-test** ***p*-values**	**T-Test/CS-Test** ***p*-values**	**N**
Age	11.95 (*2.52*)	12.20 (*2.42*)	11.67 (*2.58*)	0.100	0.127	210
Age When Joined School	7.00 (*1.39*)	6.89 (*1.36*)	7.13 (*1.41*)	0.019	0.214	210
Years of Schooling	4.94 (*2.70*)	5.31 (*2.78*)	4.54 (*2.55*)	0.048	0.038	210
% Non-Family	0.70 (*0.46*)	0.65 (*0.48*)	0.75 (*0.44*)	0.101	0.100	210
Weight (in kg)	36.63 (*9.14*)	36.62 (*8.80*)	36.64 (*9.55*)	0.919	0.989	210
Height (in cm)	140.61 (*11.25*)	140.87 (*11.07*)	140.33 (*11.50*)	0.647	0.729	210
N	210	110	100	-	-	-

	Panel B: *Non-Family* Children					
Individual Characteristics	**Pooled** (*Std. Dev.*)	**Religious** (*Std. Dev.*)	**Secular** (*Std. Dev.*)	**MW-test** ***p*-values**	**T-Test/CS-Test** ***p*-values**	**N**
Age	11.93 (*2.45*)	12.08 (*2.42*)	11.79 (*2.49*)	0.412	0.465	146
Age When Joined School	6.83 (*1.08*)	6.75 (*1.30*)	6.91 (*0.82*)	0.024	0.376	146
Years of Schooling	5.01 (*2.71*)	5.33 (*2.84*)	4.88 (*2.58*)	0.368	0.310	146
Weight (in kg)	36.51 (*9.36*)	36.02 (*9.00*)	36.97 (*9.73*)	0.477	0.545	146
Height (in cm)	140.51 (*11.49*)	140.47 (*11.63*)	140.56 (*11.43*)	0.930	0.962	146
N	146	71	75		-	-

Note: Age is the age of participants in years; Age When Joined School is the age when they joined their current school; Schooling is the difference between their age and their age when they joined their current school; % Non-Family is the percentage *non-family* children; Weight and Height are measured in kilograms and centimeters respectively. N is the sample size; MW-test is the two-sided Mann-Whitney U test; T-test is the two-sample t-test with unequal variances; CS-test is the Pearson's Chi-Squared test (only performed on % Non-Family).

Table 2: Summary of Experimental Measures of Non-Family Children Sample

	Pooled (*Std. Dev.*)	**Religious [N]** (*Std. Dev.*)	**Secular [N]** (*Std. Dev.*)	**MW-test** ***p*-values**	**T-Test/CS-Test** ***p*-values**	**N**
Panel A: Altruism						
% Donations	0.33	0.39 [71]	0.26 [75]	0.002	0.006	146
	(*0.30*)	(*0.32*)	(*0.27*)			
% To Religious Orph.	0.36	0.41 [35]	0.31 [36]	0.018	0.146	71
	(*0.30*)	(*0.29*)	(*0.30*)			
% To Secular Orph.	0.30	0.39 [36]	0.22 [39]	0.025	0.017	75
	(*0.28*)	(*0.35*)	(*0.24*)			
Panel B: Dishonesty						
Points in Dice Task	40.91	38.92 [71]	42.80 [75]	0.009	0.002	146
	(*7.78*)	(*5.95*)	(*8.81*)			
% of 5s & 6s	0.48	0.43 [71]	0.53 [75]	0.019	0.004	146
	(*0.23*)	(*0.16*)	(*0.27*)			
Panel C: Risk Aversion						
% Invested	0.39	0.41 [71]	0.37 [75]	0.275	0.473	146
	(*0.30*)	(*0.30*)	(*0.31*)			
Panel D: Cooperation						
% Cooperated	0.21	0.23 [71]	0.19 [75]	0.565	0.563	146
	(*0.41*)	(*0.42*)	(*0.39*)			
Panel E: Trust and Trustworthiness						
% Trust	0.19	0.22 [36]	0.16 [36]	0.526	0.230	72
	(*0.24*)	(*0.28*)	(*0.17*)			
% Trustworthiness	0.23	0.20 [35]	0.25 [39]	0.708	0.668	74
	(*0.49*)	(*0.46*)	(*0.52*)			

Note: % Donations: the numbers indicate the amount donated divided by the endowment (50 Taka). % To Religious (Secular) Orph. is when the recipient of the donation is a religious (secular) orphanage. Points in a Dice Task is the average points recorded in the Cheating Game. % of 5s & 6s: the numbers indicate the frequency of throws with 5s and 6s reported in the dice task divided by the total number of throws (10 throws). % Invested: the numbers indicate the amount invested divided by the endowment (50 Taka). % Cooperated: the proportion of participants opted to send their endowment. % Trust: the numbers indicate the amount sent to the trustee divided by the endowment (50 Taka). % Trustworthiness: the numbers indicate the amount returned to the trustor divided by the amount received from the trustor. MW-test is the two sided Mann-Whitney U test; T-test is the two-sample t-test with unequal variances; CS-test is the Pearson's Chi-Squared test. Reported p-value in the T-test/CS-test column for % Cooperated uses a CS-test; all other tests in that column use a T-test.

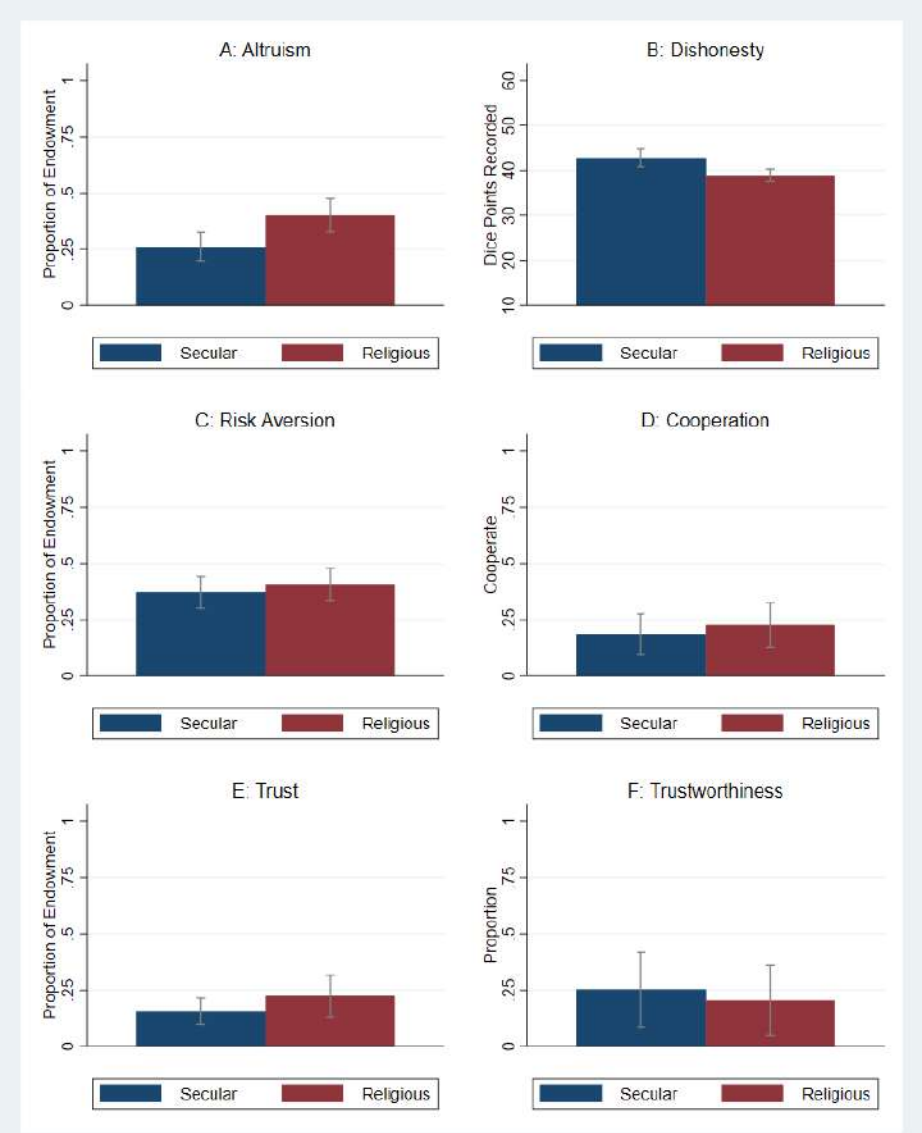
Table 3: Regression Analysis of the Effect of Religious Schooling

VARIABLES	**(1) Donation I**	**(2) Donation II**	**(3) Dishonesty**	**(4) Risk**	**(5) Cooperation**	**(6) Trust**	**(7) Trustworthy**
Religious Schooling	0.130***	0.146**	-4.167**	0.050	0.012	0.001	-0.120
	(0.038)	(0.056)	(1.409)	(0.039)	(0.054)	(0.056)	(0.093)
Religious Recipient	-	0.031	-	-	-	-	-
		(0.071)					
Religious Schooling×Religious Recipient	-	-0.033	-	-	-	-	-
		(0.078)					
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Subdistrict FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CGM p-value	0.005	-	0.007	0.244	0.833	0.969	0.375
Observations	146	146	146	146	146	72	74
R-squared	0.161	0.162	0.086	0.102	0.060	0.236	0.128

Robust standard errors clustered by sessions are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: OLS regression estimates for donation, dishonesty, risk aversion, cooperation, trust, and trustworthiness are reported. Columns 1 and 2 show the relationship between religious schooling and donations made to orphanages controlling for age, a quadratic in age, years of schooling, weight, height, and subdistricts where schools are situated. The dependant variable here is the percentage of donations made from a given endowment. The dependent variable in Column 3 is Dishonesty, which is the total dice points recorded in the *Cheating Game* (from 10 throws). The dependent variable in column 4 is Risk, which is the proportion of endowment invested in a risky lottery. The dependent variable in Column 5 is Cooperation, which equals 1 if the individual decided to cooperate and 0 otherwise. The dependent variable in Column 6 is Trust, which is the proportion of endowment sent to the trustee. The dependent variable in Column 7 is Trustworthiness, which is the proportion of received amounts that were sent back to the trustor. CGM p-value on Religious Schooling is the p-value calculated following Cameron et al. (2008) by clustering at the session level using the wild-cluster bootstrap-t method (with 1,000 replications).

Figure 1: Summary of Experimental Measures (*non-family* children only)



Note: Each bar is presented with a 95 percent confidence interval.

Table 4: Heterogeneity analysis

	A: Donation		
	Belongs to panel title category?		
VARIABLES	Yes (1)	No (2)	Difference (β_3) (3)
Panel A1: Hit Puberty			
Religious Schooling	0.344***	0.011	0.338***
	(0.081)	(0.051)	(0.105)
Panel A2: Completed Primary Education			
Religious Schooling	0.257*	0.074	0.172
	(0.118)	(0.063)	(0.138)
	B: Dishonesty		
Panel B1: Hit Puberty			
Religious Schooling	-10.470***	0.129	-10.717**
	(2.680)	(1.665)	(4.086)
Panel B2: Completed Primary Education			
Religious Schooling	-11.528***	0.352	-12.377***
	(2.775)	(1.512)	(3.530)
	C: Risk Aversion		
Panel C1: Hit Puberty			
Religious Schooling	-0.001	0.093*	-0.106
	(0.083)	(0.050)	(0.116)
Panel C2: Completed Primary Education			
Religious Schooling	-0.041	0.087	-0.138
	(0.087)	(0.074)	(0.131)
	D: Cooperation		
Panel D1: Hit Puberty			
Religious Schooling	0.204***	-0.067	0.296***
	(0.061)	(0.056)	(0.088)
Panel D2: Completed Primary Education			
Religious Schooling	0.159	-0.089	0.224
	(0.113)	(0.084)	(0.156)
	E: Trust		
Panel E1: Hit Puberty			
Religious Schooling	0.101	-0.002	0.119
	(0.277)	(0.031)	(0.129)
Panel E2: Completed Primary Education			
Religious Schooling	-0.014	-0.032	0.132
	(0.142)	(0.070)	(0.091)
	F: Trustworthiness		
Panel F1: Hit Puberty			
Religious Schooling	-0.158**	-0.051	-0.150
	(0.054)	(0.167)	(0.152)
Panel F2: Completed Primary Education			
Religious Schooling	-0.369	0.102	-0.521
	(0.185)	(0.213)	(0.313)
Controls	Yes	Yes	Yes
Subdistrict FE	Yes	Yes	Yes

Robust standard errors clustered by sessions are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: OLS regression estimates reported. Panel title 'Hit Puberty' is a dummy variable that equals 1 if students are 13 years or older (column 1) and 0 if below (column 2). Panel title 'Completed Primary Education' is a dummy variable that equals 1 if students are beyond primary education, i.e., after grade 5, (column 1) and 0 if not (column 2). Column 3 is the difference between columns 1 and 2 (i.e., difference-in-differences by interacting the panel title variable with *Religious Schooling*). Controls include age and quadratic of age.

Table 5: Family vs. Non-Family Children

	Pooled			Religious			Secular		
	NF [N] (*Std. Dev.*)	**F [N]** (*Std. Dev.*)	**MW-Test** ***p*-values**	**NF [N]** (*Std. Dev.*)	**F [N]** (*Std. Dev.*)	**MW-Test** ***p*-values**	**NF [N]** (*Std. Dev.*)	**F [N]** (*Std. Dev.*)	**MW-Test** ***p*-values**
Panel A: Donation Game									
% Donations	0.33 [146] (*0.30*)	0.40 [64] (*0.25*)	0.010	0.39 [71] (*0.32*)	0.50 [39] (*0.20*)	0.009	0.26 [75] (*0.27*)	0.25 [25] (*0.23*)	0.939
% Donations To Religious Orph.	0.36 [71] (*0.30*)	0.39 [34] (*0.25*)	0.228	0.41 [35] (*0.29*)	0.50 [20] (*0.20*)	0.047	0.31 [36] (*0.30*)	0.24 [14] (*0.25*)	0.427
% Donations To Non-Religious Orph.	0.30 [75] (*0.31*)	0.41 [30] (*0.24*)	0.018	0.39 [36] (*0.35*)	0.50 [19] (*0.20*)	0.068	0.22 [39] (*0.24*)	0.25 [11] (*0.22*)	0.497
Panel B: Cheating Game									
Points in Dice Task	40.91 [146] (*7.78*)	38.47 [64] (*8.00*)	0.027	38.92 [71] (*5.95*)	37.38 [39] (*5.67*)	0.145	42.80 [75] (*8.81*)	40.16 [25] (*10.59*)	0.241
% of 5s & 6s	0.48 [146] (*0.23*)	0.42 [64] (*0.23*)	0.028	0.43 [71] (*0.16*)	0.38 [39] (*0.15*)	0.098	0.53 [75] (*0.27*)	0.49 [25] (*0.31*)	0.376
Panel C: Investment Game									
% Invested	0.39 [146] (*0.30*)	0.43 [64] (*0.32*)	0.454	0.41 [71] (*0.30*)	0.44 [39] (*0.30*)	0.520	0.37 [75] (*0.31*)	0.40 [25] (*0.34*)	0.981
Panel D: Prisoner's Dilemma Game									
% Cooperated	0.21 [146] (*0.41*)	0.28 [64] (*0.45*)	0.230	0.23 [71] (*0.45*)	0.26 [39] (*0.44*)	0.715	0.19 [75] (*0.39*)	0.32 [25] (*0.48*)	0.166
Panel E: Trust Game									
% Trust	0.19 [72] (*0.24*)	0.39 [33] (*0.30*)	0.001	0.22 [36] (*0.28*)	0.51 [19] (*0.28*)	0.000	0.16 [36] (*0.17*)	0.23 [14] (*0.27*)	0.449
% Trustworthiness	0.23 [74] (*0.49*)	0.16 [31] (*0.23*)	0.936	0.24 [35] (*0.58*)	0.15 [20] (*0.23*)	0.728	0.25 [39] (*0.52*)	0.20 [11] (*0.24*)	0.552

Note: NF is for *Non-Family* and F is for *Family* children. Sample size of each bin is given in brackets, [N]. % Donations: the numbers indicate the amount donated divided by the endowment (50 Taka). % Donations To Religious (Non-Religious) Orph. is when the recipient of the donation is a religious (non-religious) orphanage. % Invested: the numbers indicate the amount invested divided by the endowment (50 Taka). Points in a Dice Task is the average points recorded in the Cheating Game. % of 5s & 6s: the numbers indicate the frequency of throws with 5s and 6s reported in the dice task divided by the total number of throws (10 throws). % Trust: the numbers indicate the amount sent to the trustee divided by the endowment (50 Taka). % Trustworthiness: the numbers indicate the amount returned to the trustor divided by the amount received from the trustor. Two sided Mann-Whitney U test p-values have been reported.

Table 6: Does the Inclusion of the Family Children Sample Overstate the Impact?

VARIABLES	(1) Donation	(2) Dishonesty	(3) Risk Aversion	(4) Cooperation	(5) Trust	(6) Trustworthy
(R) Religious Schooling	0.140***	-4.258**	0.048	0.016	0.015	-0.094
	(0.036)	(1.449)	(0.045)	(0.052)	(0.045)	(0.113)
(F) Family Children	-0.009	-2.796	-0.011	0.155*	0.097**	0.002
	(0.045)	(1.785)	(0.043)	(0.086)	(0.039)	(0.129)
(RF) Religious Schooling×Family Children	0.107*	0.979	0.018	-0.114	0.163*	-0.075
	(0.057)	(2.074)	(0.080)	(0.127)	(0.080)	(0.154)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Subdistrict FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	210	210	210	210	105	105
R-squared	0.176	0.122	0.096	0.071	0.349	0.088
F-test *p*-values are given below						
F+RF	0.015	0.175	0.921	0.687	0.007	0.389

Robust standard errors clustered by sessions are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: OLS regression estimates for donation, dishonesty, risk aversion, cooperation, trust, and trustworthiness are reported. Family Children is a dummy that equals 1 if children are *family* children (i.e., spent pre-school years with family) and 0 if children are *non-family* children. All specifications include the same controls as in Table 3. R, F, and RF are abbreviations of variables; F+RF row provides F-test results of testing coefficients of F+RF= 0.

Table 7: Effect on Behavior, by Dormitory Peers that are Family Children

VARIABLES	(1) Donation	(2) Dishonesty	(3) Risk Aversion	(4) Cooperation	(5) Trust	(6) Trustworthy
(R) Religious Schooling	0.191***	-5.076**	0.034	0.019	0.065	0.108
	(0.054)	(2.135)	(0.067)	(0.062)	(0.094)	(0.178)
(P) Dormitory Peers	-0.021	-3.048**	-0.047	-0.093	0.059	-0.149
	(0.095)	(1.227)	(0.081)	(0.116)	(0.093)	(0.207)
(RP) Religious Schooling×Dormitory Peers	-0.082	2.929	0.049	0.039	-0.128	-0.266
	(0.107)	(2.550)	(0.095)	(0.094)	(0.127)	(0.210)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Subdistrict FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	146	146	146	146	72	74
R-squared	0.172	0.111	0.119	0.116	0.282	0.190
F-test *p*-values are given below						
R+RP	0.160	0.221	0.267	0.520	0.435	0.251
P+RP	0.253	0.962	0.981	0.462	0.337	0.067

Robust standard errors clustered by sessions are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: OLS regression estimates reported. Dormitory Peers is a dummy that equals 1 if the proportion of family children in one's dormitory is above the median value and 0 if below the median (the median proportion of family children in dormitories is 25%). All specifications include the same controls as in Table 3. R, P, and RP are abbreviations of variables. R+RP and P+RP rows provide F-test results of testing R+RP= 0 and P+RP= 0 respectively.

Behavioral Consequences of Religious Schooling

Online Appendix

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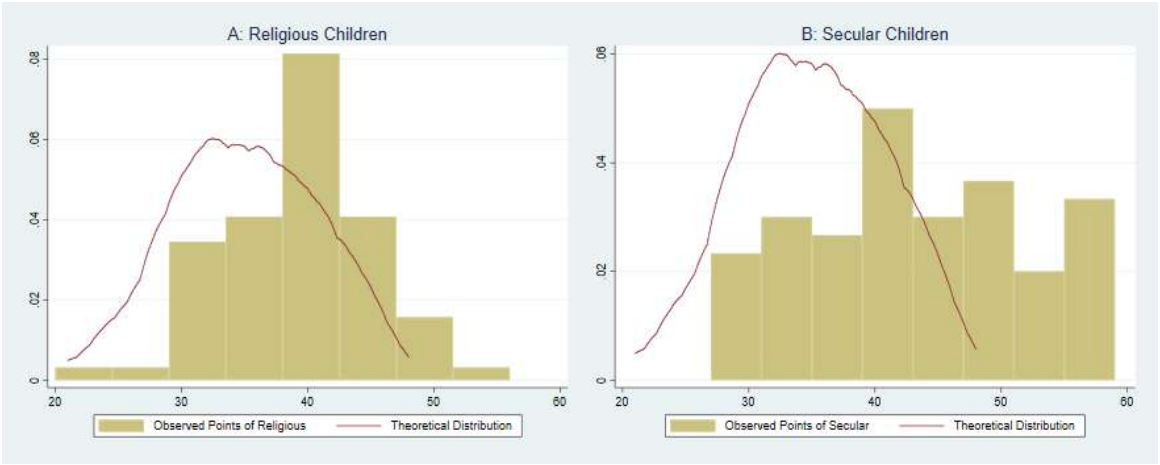
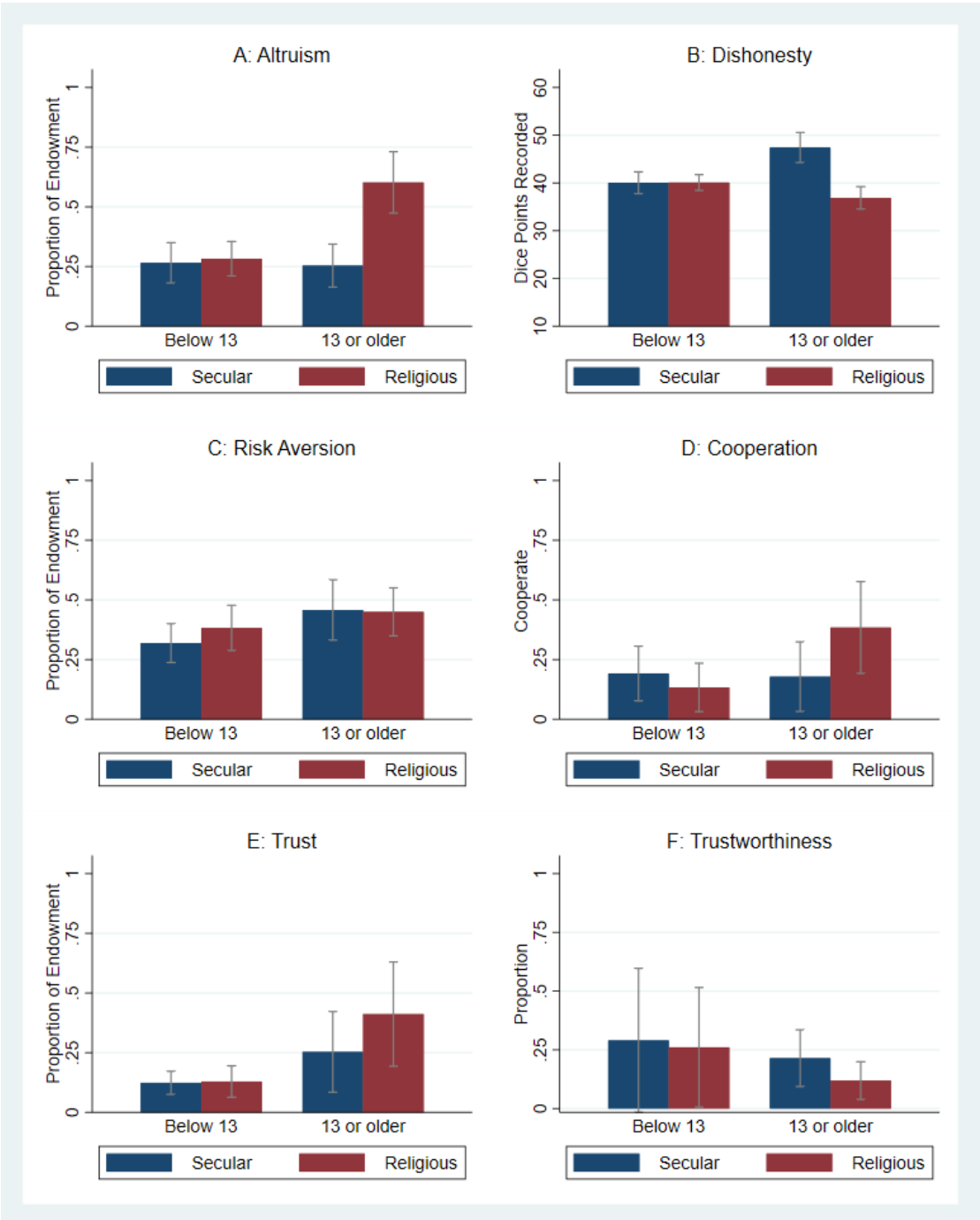
A Appendix: Supplementary figures and tables

Figure A.1: Points recorded in the dice task



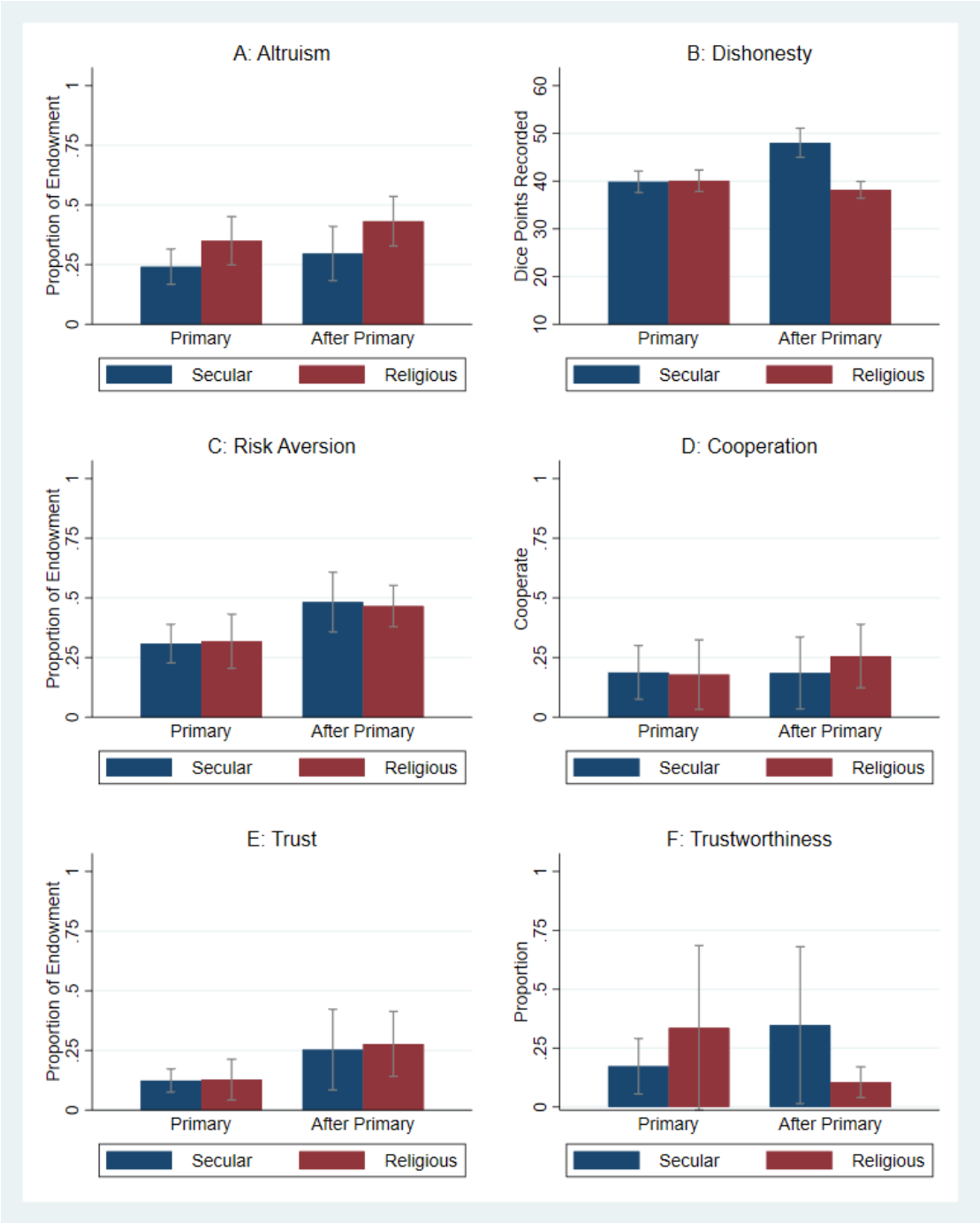
Note: The probability density function of the theoretical distribution (lines) and the frequency of points recorded (bars) across school types are shown in this figure.

Figure A.2: Behavior, by school type and age groups



Note: Each bar is presented with a 95 percent confidence interval.

Figure A.3: Behavior, by school type and primary education completion



Note: Each bar is presented with a 95 percent confidence interval. 'Primary' corresponds to children in primary education (grade 5 or below); 'After Primary' corresponds to children who completed primary education.

Table A.1: Effect of Religious Schooling on Altruism

VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Religious Schooling	0.139**	0.147***	0.132***	0.133***	0.134***	0.130***	0.130***
	(0.048)	(0.036)	(0.037)	(0.039)	(0.039)	(0.037)	(0.038)
Age	-	-	0.036**	0.016	0.031	0.036	0.037
			(0.014)	(0.110)	(0.120)	(0.122)	(0.123)
Age^2	-	-	-	0.001	0.001	0.001	0.001
				(0.005)	(0.005)	(0.005)	(0.004)
Years of Schooling	-	-	-	-	-0.013	-0.014	-0.014
					(0.025)	(0.025)	(0.025)
Weight (in kilograms)	-	-	-	-	-	-0.002	-0.002
						(0.006)	(0.007)
Height (in cm)	-	-	-	-	-	-	-0.000
							(0.003)
Subdistrict FE	No	Yes	Yes	Yes	Yes	Yes	Yes
CGM p-value	0.024	0.001	0.003	0.003	0.003	0.003	0.005
Observations	146	146	146	146	146	146	146
R-squared	0.053	0.082	0.159	0.159	0.161	0.162	0.162

Robust standard errors clustered by sessions are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: OLS regression estimates are reported. The dependent variable is the proportion of endowment donated. Control variables are added incrementally. CGM p-value for Religious Schooling is the p-value calculated following Cameron et al. (2008) by clustering at the session level using the wild-cluster bootstrap-t method (with 1,000 replications).

Table A.2: Effect of Religious Schooling on Altruism: In-group Bias

VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Religious Schooling	0.171**	0.175***	0.148**	0.150**	0.150**	0.146**	0.146**
	(0.063)	(0.053)	(0.059)	(0.060)	(0.059)	(0.055)	(0.056)
Religious Recipient	0.084*	0.080	0.033	0.033	0.031	0.031	0.031
	(0.042)	(0.049)	(0.070)	(0.070)	(0.071)	(0.071)	(0.071)
Religious Schooling×Religious Recipient	-0.067	-0.059	-0.033	-0.035	-0.034	-0.033	-0.033
	(0.092)	(0.067)	(0.081)	(0.080)	(0.081)	(0.078)	(0.078)
Age	-	-	0.035**	0.014	0.028	0.033	0.033
			(0.015)	(0.109)	(0.120)	(0.121)	(0.123)
Age^2	-	-	-	0.001	0.001	0.001	0.001
				(0.005)	(0.005)	(0.004)	(0.004)
Years of Schooling	-	-	-	-	-0.013	-0.013	-0.013
					(0.025)	(0.025)	(0.025)
Weight (in kilograms)	-	-	-	-	-	-0.002	-0.002
						(0.006)	(0.007)
Height (in cm)	-	-	-	-	-	-	-0.000
							(0.003)
Subdistrict FE	No	Yes	Yes	Yes	Yes	Yes	Yes
CGM p-value (Religious Schooling)	0.046	0.049	0.064	0.064	0.0.060	0.047	0.050
CGM p-value (Religious Recipient)	0.109	0.247	0.712	0.706	0.736	0.734	0.740
CGM p-value (Interaction)	0.531	0.472	0.730	0.703	0.723	0.727	0.728
Observations	146	146	146	146	146	146	146
R-squared	0.063	0.092	0.161	0.161	0.162	0.164	0.164

Robust standard errors clustered by sessions are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: OLS regression estimates are reported. The dependent variable is the proportion of endowment donated. Control variables are added incrementally. Religious Recipient is an indicator for a religious recipient (i.e., a religious orphanage) in the charitable giving game. CGM p-value is the p-value calculated following Cameron et al. (2008) by clustering at the session level using the wild-cluster bootstrap-t method (with 1,000 replications).

Table A.3: Effect of Religious Schooling on Dishonesty

VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Religious Schooling	-3.885**	-3.895**	-4.019**	-3.955**	-3.922**	-4.053**	-4.167**
	(1.388)	(1.322)	(1.442)	(1.440)	(1.418)	(1.361)	(1.409)
Age	-	-	0.304	-3.026	-2.570	-2.397	-2.117
			(0.646)	(2.867)	(2.700)	(2.598)	(2.524)
Age^2	-	-	-	0.134	0.133	0.135	0.141
				(0.108)	(0.104)	(0.102)	(0.101)
Years of Schooling	-	-	-	-	-0.411	-0.416	-0.418
					(0.460)	(0.454)	(0.479)
Weight (in kilograms)	-	-	-	-	-	-0.069	-0.051
						(0.110)	(0.112)
Height (in cm)	-	-	-	-	-	-	-0.124
							(0.126)
Subdistrict FE	No	Yes	Yes	Yes	Yes	Yes	Yes
CGM p-value	0.012	0.010	0.017	0.016	0.014	0.007	0.007
Observations	146	146	146	146	146	146	146
R-squared	0.063	0.063	0.071	0.083	0.086	0.088	0.098

Robust standard errors clustered by sessions are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: OLS regression estimates are reported. The dependent variable is the total points recorded in the cheating game (from 10 throws). Control variables are added incrementally. CGM p-value for Religious Schooling is the p-value calculated following Cameron et al. (2008) by clustering at the session level using the wild-cluster bootstrap-t method (with 1,000 replications).

Table A.4: Effect of Religious Schooling on Risk Aversion

VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Religious Schooling	0.036	0.048	0.045	0.047	0.050	0.055	0.050
	(0.068)	(0.044)	(0.043)	(0.043)	(0.044)	(0.041)	(0.039)
Age	-	-	0.009	-0.119	-0.080	-0.086	-0.074
			(0.014)	(0.121)	(0.131)	(0.135)	(0.127)
Age^2	-	-	-	0.005	0.005	0.005	0.005
				(0.005)	(0.005)	(0.005)	(0.005)
Years of Schooling	-	-	-	-	-0.035**	-0.035**	-0.035**
					(0.015)	(0.016)	(0.016)
Weight (in kilograms)	-	-	-	-	-	0.003	0.003
						(0.004)	(0.004)
Height (in cm)	-	-	-	-	-	-	-0.005
							(0.003)
Subdistrict FE	No	Yes	Yes	Yes	Yes	Yes	Yes
CGM p-value	0.614	0.337	0.364	0.335	0.318	0.220	0.244
Observations	146	146	146	146	146	146	146
R-squared	0.004	0.073	0.078	0.089	0.102	0.104	0.117

Robust standard errors clustered by sessions are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: OLS regression estimates are reported. The dependent variable is proportion of endowment invested in a risky lottery. Control variables are added incrementally. CGM p-value for Religious Schooling is the p-value calculated following Cameron et al. (2008) by clustering at the session level using the wild-cluster bootstrap-t method (with 1,000 replications).

Table A.5: Effect of Religious Schooling on Cooperation

VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Religious Schooling	0.039	0.038	0.026	0.031	0.030	0.027	0.013	0.012
	(0.051)	(0.050)	(0.047)	(0.050)	(0.049)	(0.059)	(0.055)	(0.054)
Age	-	-	0.027	-0.218	-0.229	-0.225	-0.191	-0.189
			(0.017)	(0.140)	(0.136)	(0.145)	(0.169)	(0.170)
Age^2	-	-	-	0.010	0.010	0.010	0.011	0.011
				(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Years of Schooling	-	-	-	-	0.010	0.010	0.010	0.011
					(0.030)	(0.030)	(0.031)	(0.032)
Weight (in kilograms)	-	-	-	-	-	-0.002	0.001	0.000
						(0.006)	(0.006)	(0.006)
Height (in cm)	-	-	-	-	-	-	-0.015***	-0.015***
							(0.005)	(0.005)
Risk Aversion	-	-	-	-	-	-	-	0.025
								(0.071)
Subdistrict FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CGM p-value	0.500	0.566	0.645	0.610	0.613	0.687	0.813	0.833
Observations	146	146	146	146	146	146	146	146
R-squared	0.002	0.009	0.034	0.057	0.058	0.058	0.111	0.111

Robust standard errors clustered by sessions are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: OLS regression estimates are reported. The dependent variable is a dummy that equals 1 if a participant opted to send his entire endowment in a Prisoner's Dilemma game and 0 otherwise. Control variables are added incrementally. CGM p-value for Religious Schooling is the p-value calculated following Cameron et al. (2008) by clustering at the session level using the wild-cluster bootstrap-t method (with 1,000 replications).

Table A.6: Effect of Religious Schooling on Trust

VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**
Religious Schooling	0.067	0.082	0.057	0.056	0.048	0.037	0.029	0.027	0.001
	(0.070)	(0.045)	(0.048)	(0.047)	(0.044)	(0.040)	(0.037)	(0.037)	(0.056)
Age	-	-	0.034**	0.107	0.126	0.132	0.159	0.161	0.184
			(0.011)	(0.132)	(0.113)	(0.123)	(0.128)	(0.127)	(0.154)
Age^2	-	-	-	-0.003	-0.002	-0.002	-0.003	-0.003	-0.004
				(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
Years of Schooling	-	-	-	-	-0.031*	-0.032	-0.030	-0.030	-0.029
					(0.015)	(0.018)	(0.016)	(0.016)	(0.018)
Weight (in kilograms	-	-	-	-	-	-0.003	-0.003	-0.003	-0.003
						(0.007)	(0.006)	(0.006)	(0.006)
Height (in cm)	-	-	-	-	-	-	-0.006	-0.006	-0.007
							(0.006)	(0.006)	(0.007)
Risk Aversion	-	-	-	-	-	-	-	0.039	0.030
								(0.049)	(0.053)
Altruism	-	-	-	-	-	-	-	-	0.141
									(0.187)
Subdistrict FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CGM p-value	0.531	0.156	0.500	0.500	0.531	0.422	0.406	0.422	0.969
Observations	72	72	72	72	72	72	72	72	72
R-squared	0.021	0.084	0.194	0.200	0.217	0.221	0.241	0.243	0.267

Robust standard errors clustered by sessions are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: OLS regression estimates are reported. The dependent variable is the proportion of endowment sent by trustors in a Trust game. Control variables are added incrementally. CGM p-value for Religious Schooling is the p-value calculated following Cameron et al. (2008) by clustering at the session level using the wild-cluster bootstrap-t method (with 1,000 replications).

Table A.7: Effect of Religious Schooling on Trustworthiness

VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Religious Schooling	-0.049	-0.049	-0.046	-0.046	-0.083	-0.086	-0.085	-0.120
	(0.126)	(0.112)	(0.099)	(0.104)	(0.099)	(0.095)	(0.090)	(0.093)
Age	-	-	-0.030	-0.057	-0.192	-0.172	-0.176	-0.263
			(0.028)	(0.285)	(0.279)	(0.253)	(0.246)	(0.244)
Age^2	-	-	-	0.001	0.003	0.003	0.003	0.005
				(0.010)	(0.010)	(0.010)	(0.011)	(0.010)
Years of Schooling	-	-	-	-	0.090	0.091	0.092	0.102
					(0.087)	(0.087)	(0.088)	(0.085)
Weight (in kilograms)	-	-	-	-	-	-0.008	-0.008	-0.009
						(0.009)	(0.009)	(0.011)
Height (in cm)	-	-	-	-	-	-	0.003	0.005
							(0.008)	(0.008)
Altruism	-	-	-	-	-	-	-	0.400
								(0.311)
Subdistrict FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CGM p-value	0.719	0.813	0.781	0.797	0.563	0.531	0.516	0.375
Observations	74	74	74	74	74	74	74	74
R-squared	0.003	0.029	0.049	0.049	0.077	0.083	0.085	0.138

Robust standard errors clustered by sessions are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: OLS regression estimates are reported. The dependent variable is the proportion of amount received that was returned back to trustors in a Trust game. Control variables are added incrementally. CGM p-value for Religious Schooling is the p-value calculated following Cameron et al. (2008) by clustering at the session level using the wild-cluster bootstrap-t method (with 1,000 replications).

Table A.8: Effect on Behavior, by School Enrolment Age

VARIABLES	**(1) Donation**	**(2) Dishonesty**	**(3) Risk Aversion**	**(4) Cooperation**	**(5) Trust**	**(6) Trustworthy**
(R) Religious Schooling	0.168**	-4.952**	0.015	-0.015	0.024	-0.107
	(0.071)	(2.115)	(0.061)	(0.092)	(0.052)	(0.150)
(E) Enrolled When Age is Six	-0.039	-4.121	-0.068	-0.081	0.076	0.277
	(0.103)	(3.231)	(0.069)	(0.116)	(0.039)	(0.424)
(RE) Religious Schooling×Enrolled When Age is Six	-0.059	3.178	0.096	0.086	-0.087	-0.115
	(0.120)	(3.050)	(0.073)	(0.144)	(0.056)	(0.479)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Subdistrict FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	146	146	146	146	72	74
R-squared	0.169	0.117	0.122	0.115	0.276	0.157
F-test p-values are given below						
R+RE	0.214	0.305	0.011	0.459	0.423	0.559
E+RE	0.330	0.508	0.668	0.974	0.902	0.574

Robust standard errors clustered by sessions are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: OLS regression estimates for donation, dishonesty, risk aversion, cooperation, trust, and trustworthiness are reported. Enrolled When Age is Six is a dummy that equals 1 if children enrolled in the school at age of 5 or 6 and 0 if enrolled after 6. All specifications include the same controls as in Table 3. R, E, and RE are abbreviations of variables; R+RE and E+RE rows provide F-test results of testing R+RE= 0 and E+RE= 0 respectively.

Table A.9: Heterogeneity: Top Performer

VARIABLES	(1) Donation	(2) Dishonesty	(3) Risk Aversion	(4) Cooperation	(5) Trust	(6) Trustworthy
(R) Religious Schooling	0.102	0.169	0.104	-0.125	-0.089**	-0.270
	(0.064)	(2.129)	(0.076)	(0.085)	(0.030)	(0.292)
(T) Top Performer	0.093	0.654	-0.035	-0.062	-0.104*	0.027
	(0.098)	(1.518)	(0.086)	(0.073)	(0.044)	(0.221)
(RT) Religious Schooling×Top Performer	-0.081	-0.197	-0.012	0.099	0.234	0.443
	(0.133)	(2.802)	(0.109)	(0.133)	(0.125)	(0.504)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Subdistrict FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	93	93	93	93	50	43
R-squared	0.177	0.136	0.159	0.180	0.209	0.281
F-test *p*-values are given below						
R+RT	0.807	0.988	0.190	0.810	0.307	0.564
T+RT	0.883	0.845	0.465	0.738	0.348	0.205

Robust standard errors clustered by sessions are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: OLS regression estimates for donation, dishonesty, risk aversion, cooperation, trust, and trustworthiness are reported; Top Performer is a dummy that equals 1 if children scored above the median in their grade (within their school) in the mid-term exam and 0 if below the median. All specifications include the same controls as in Table 3. R, T, and RT are abbreviations of variables; R+RT and T+RT rows provide F-test results of testing R+RT= 0 and T+RT= 0 respectively.

Table A.10: Does Inclusion of the Family Children Sample Overstate the Impact? Only Young Children

VARIABLES	(1) Donation	(2) Dishonesty	(3) Risk Aversion	(4) Cooperation	(5) Trust	(6) Trustworthy
(R) Religious Schooling	0.024	0.076	0.091*	-0.106*	-0.006	-0.064
	(0.047)	(1.822)	(0.051)	(0.056)	(0.034)	(0.130)
(F) Family Children	-0.014	-3.540	0.064	0.220	0.076**	-0.032
	(0.047)	(2.229)	(0.060)	(0.154)	(0.023)	(0.264)
(RF) Religious Schooling×Family Children	0.254***	-0.076	0.003	-0.146	0.359**	-0.157
	(0.069)	(2.599)	(0.095)	(0.177)	(0.123)	(0.216)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Subdistrict FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	130	130	130	130	70	60
R-squared	0.220	0.134	0.153	0.107	0.419	0.149
F-test *p*-values are given below						
F+RF	0.000	0.021	0.351	0.414	0.010	0.335

Robust standard errors clustered by sessions are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: OLS regression estimates for donation, dishonesty, risk aversion, cooperation, trust, and trustworthiness are reported. Family Children is a dummy that equals 1 if children are *family* children (i.e., spent pre-school years with family) and 0 if children are *non-family* children. Young Children sample consists of children that are 12 or younger. All specifications include the same controls as in Table 3. R, F, and RF are abbreviations of variables; F+RF row provides F-test results of testing F+RF= 0.

Table A.11: Effect on Behavior, by Dormitory Peers that are Family Children (Robustness Check)

VARIABLES	(1) Donation	(2) Dishonesty	(3) Risk Aversion	(4) Cooperation	(5) Trust	(6) Trustworthy
Religious Schooling	0.290***	-2.079	0.047	0.042	0.130	0.027
	(0.083)	(3.153)	(0.101)	(0.100)	(0.153)	(0.200)
Dormitory Peers (C)	0.083	-3.611	-0.205	-0.365	-0.113	-0.970
	(0.347)	(5.439)	(0.342)	(0.438)	(0.288)	(0.567)
Religious Schooling×Dormitory Peers (C)	-0.523	-5.217	0.078	0.032	-0.334	-0.116
	(0.342)	(11.492)	(0.381)	(0.394)	(0.411)	(0.466)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Subdistrict FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	146	146	146	146	72	74
R-squared	0.176	0.104	0.120	0.118	0.285	0.172

Robust standard errors clustered by sessions are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Same as Table 7 but with Dormitory Peers (C) as a continuous variable. All specifications include the same controls as in Table 3.

Table A.12: Correlation between Behavior of Participant and the Average Behavior of Dormitory Peers that are Family Children

VARIABLES	(1) Donation	(2) Dishonesty	(3) Risk Aversion	(4) Cooperation	(5) Trust	(6) Trustworthy
Average Donation by Family Peers	0.228***	-	-	-	-	-
	(0.059)					
Average Dishonesty by Family Peers	-	0.076	-	-	-	-
		(0.095)				
Average Risk Aversion by Family Peers	-	-	0.193	-	-	-
			(0.171)			
Average Cooperation by Family Peers	-	-	-	0.065	-	-
				(0.089)		
Average Trust by Family Peers	-	-	-	-	-0.001	-
					(0.101)	
Average Trustworthiness by Family Peers	-	-	-	-	-	-0.337
						(0.226)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Subdistrict FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	146	146	146	146	72	74
R-squared	0.148	0.043	0.117	0.113	0.267	0.140

Robust standard errors clustered by sessions are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Average behavior by family peers are average donation, dishonesty, risk aversion, cooperation, trust, or trustworthiness of dormitory peers that are family children.

Table A.13: Effect on Behavior with Average Behavior of Dormitory Peers that are Family Children as Controls

VARIABLES	(1) Donation	(2) Dishonesty	(3) Risk Aversion	(4) Cooperation	(5) Trust	(6) Trustworthy
Religious Schooling	0.119*	-4.808**	0.030	0.012	0.004	-0.101
	(0.066)	(1.639)	(0.055)	(0.053)	(0.058)	(0.103)
Average Donation by Family Peer	0.029	-	-	-	-	-
	(0.121)					
Average Dishonesty by Family Peer	-	0.119	-	-	-	-
		(0.085)				
Average Risk Aversion by Family Peer	-	-	0.134	-	-	-
			(0.225)			
Average Cooperation by Family Peer	-	-	-	0.065	-	-
				(0.089)		
Average Trust by Family Peer	-	-	-	-	-0.005	-
					(0.116)	
Average Trustworthiness by Family Peer	-	-	-	-	-	-0.293
						(0.220)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Subdistrict FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	146	146	146	146	72	74
R-squared	0.163	0.129	0.119	0.113	0.267	0.149

Robust standard errors clustered by sessions are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Same table as Table 3 but with the average behavior of dormitory peers that are family children as an additional control. For instance, Average Donation by Family Peer is the average donation made by dormitory peers that are family children.

B Appendix: Details on the experimental design

B.1 Details on orphan school admission

I interviewed headmasters from all six orphan schools and directors of three infant orphanages located within the same district where this study takes place. I present only the most detailed responses here as these responses also summarize all conversations thoroughly. On how orphan children are placed into schools, a director of an infant orphanage said:

> *"Every year, we have more (school-aged) orphans than what a single orphan school admits. So we try to transfer our (orphan) children to several orphan schools, sometimes to 3-4 orphan schools in a year. Even after that, some (orphan) children do not get transferred due to place shortages in orphan schools. These children remain with us until they are transferred to orphan schools. ... Sometimes we are very lucky, as some parents reach out to us to adopt children. But they always adopt young infants or children that are below 2 years. School-aged children never get adopted."*

The director also added:

> *"We do not have any specific rules (neither merit tests nor lottery) about transferring children to orphan schools. However, we try to transfer children from the same dormitory to the same school because we want them to have some old friends in their new school after moving. Also, we always transfer siblings (if any) to the same orphan school."*

On how orphanages decide whether an orphan child should be placed into a religious or a secular orphan school, a director said:

> *"We do not make any lists of children that should be transferred to religious or secular orphan school based on how 'good' or 'bad' they are. Actually, we do not have any resources or the luxury to make such choices. We always 'work' with a few orphan schools (both religious and secular) as we know each other well. As I said, we try to transfer children that sleep in the same dormitory to the same school. So if we have some six-year-olds in dormitory X and some in dormitory Y, and they need to be transferred to two different orphan schools as one school cannot accommodate them, then dormitory X children are transferred to one school and dormitory Y children get transferred to another school. ... Sometimes such orphan schools are of both types (i.e., one is religious and another is secular), or sometimes both are religious or secular, as we cannot tell until we speak to the school authorities. If they (schools) have the capacity and say 'yes', then we make arrangements for the transfer."*

According to orphan school headmasters, admission of orphans to schools does not involve merit-based admission tests. Because the main purpose of such schools is to give shelter and education to orphaned children, admitting based on interviews or tests is rather unconventional. When the headmaster of a religious school was asked about the process of admission, the headmaster replied:

"Our school is not financially stable. So, our capacity is very limited. We want to take in and give shelter to as many orphans as possible, but we cannot always do it. When mothers come crying to our door to take their children, we often cannot turn them back. If we are over capacity then we ask them to go and talk to a different orphan school."

On admission, a headmaster of a secular orphan school said:

"We are on good terms with some infant orphanages. Sometimes they help us (with securing donations) when we need them. When they have some school-aged orphans, they reach us and discuss the issue. If our school can accommodate some children then we inform them. But eventually, we often end up with more children than our capacity. As a result, many children sleep on the floor or share beds with others."

When asked about dormitory assignment of *non-family* and *family* children, another headmaster replied:

"Children from infant orphanages used to share their dormitories. After admission to our school, we try to keep it that way because we do not want to separate them from their (infant orphanage) friends... However, children that were abandoned by their parents get assigned to dormitories with available spaces."

B.2 Incentives

I used both pecuniary and non-pecuniary incentives in this study. As a reward for participation, I offered food in a lunch box that was worth 100 Taka each. However, for the games, I used monetary incentives. Since students are orphans, there is a possibility that in the post-experiment period elders who bully youngsters might snatch one's winning money. This fear of losing money might affect decision making in the actual experiment (e.g., giving too much in the donation game to avoid giving to bullies later or taking more unnecessary risks and so on), which would bias my results. However, if non-pecuniary incentives are used, such as sweets, cookies, chocolates or toys, then there is a risk of under/over incentivizing. Also, older students might not be induced properly by non-pecuniary incentives. Therefore, I only use monetary incentives for the games. However, immediately after completing a session, students were able to buy their preferred goods (e.g., sweets, chocolates, toys, ice-cream, notebooks, pencils, etc.) in exchange for money from mobile pop-up shops (locally known as *feriwala*), whom I asked to visit the school on the day of the experiment in advance. Therefore, students had to be paid on the same day as the experiment.[1]

[1]Since payments had to be made immediately, it was not possible to pair students across schools in strategic games. This is because, if they were paired with individuals from another school then payment would delay, which then possessed the risk of being bullied during the waiting period. Therefore, knowing this, participants were paid immediately and were always paired with their school peers in strategic games. This is the reason why an important behavior, time preference, which measures impatience, was not elicited in this study.

B.3 Games Procedure

For all but the *Investment* game, each subject received a pen, a piece of paper, and an envelope (with their unique ID on it). After everyone finished their task, an assistant collected all envelopes and passed them to another assistant seating at the enrolment desk, where he recorded the data and payoff from that particular game.

Charitable Giving: Subjects already knew, from instructions, whether the recipient orphan school was religious or secular. After instructions and frequently asked questions, participants were asked to write down the amount they wanted to donate to the orphanage (any amount between 0 and 50) on the blank piece of paper and then stuff it into the envelope provided.

Cheating: Along with the usual stationery, all subjects received a six-sided dice. The piece of paper had a list of numbers from 1-10, with blank boxes below each number. Subjects had to record the numbers in these boxes after throwing their die. After completing this task, they were asked to stuff the piece of paper into the given envelope and wait for the assistant to collect it.

Investment: Subject were asked to go to the enrolment desk, one by one, and state their investment choices. The experimenter at the desk flipped the coin and the outcome was always confirmed by the subject. After finishing this task, subjects returned to their designated seats.

Prisoner's Dilemma: Students were asked to put a cross ($\times$) on the paper if they did not wish to transfer their entire endowment to the other player (i.e. defection). However, if they wanted to transfer then they had to put a tick ($\surd$) on the paper (i.e. cooperation). Once they finished making their decisions, they were asked to stuff it into the envelope and wait for an assistant to collect it.

Trust: Subjects in classroom A always played the role of trustors and that in B always played the role of trustees (A and B were randomly assigned to classrooms). In classroom A, all trustors were asked to write down any number between 0 and 50 that they would like to send to their paired trustees. Once they finished writing, they were asked to stuff the paper into the envelope provided and wait for an assistant to come. The assistant then collected all envelopes and gave them to another assistant seating at the enrolment desk. The assistant at the enrolment desk recorded all 'trusted' transfers first and then copied the tripled amount in a separate piece of paper, stuffed in the matched trustee's envelope and then sent them to trustees in classroom B. After receiving their envelopes, trustees wrote the amount they wanted to transfer back in the same piece of paper, and then stuffed this paper back into the envelope and wait for an assistant to collect it. The assistant then collected all envelopes and gave them to the assistant seating at the enrolment desk. After recording the transferred-back amount, the assistant again wrote that amount in a separate piece of paper and sent back to the trustors. This way the game preserves complete anonymity, where subjects were not able to know their paired individuals' ID or handwriting.

After the final game, subjects were debriefed about the experiment and then received their rewards (food box and cash). The pop-up shop was waiting outside the school gate where participants were able to buy their preferred goods under adult supervision.[2]

[2]The research ethics committee at the University of Southampton instructed me not to administer any exit surveys as it involves collecting sensitive data from vulnerable children.

C Appendix: Experimental instructions

C.1 General Instruction

Welcome to our study. Today you are going to play 5 short games which will take around 90 minutes. For attending today's meeting, you will receive a food box that has fruits and savoury snacks. Also by playing games, you have the chance to earn money. All money you earn will be yours to keep. We will pay you at the end in private after all games end. No one will know how much money you earned, unless you choose to tell them. Using the money you earn, you can then buy chocolates, candies, cookies, ice-cream, stationeries, etc., from a pop-up shop that will be waiting for you outside.

One of the rules of today's study is that you cannot talk to each other. If you have any questions, then please raise your hand and ask. But please remember, if you do not want to stay or if you do not feel well then you can leave at any time. If you leave then no one will be upset or mad with you. If you want to leave then please raise your hand at any time. We will go to you to help you out. If you leave before completing all 5 games, then you will only receive the food box.

Today you will be playing 5 simple games. We call them decision making games because in these games you will make simple decisions. Please remember that what you earn from this study depends on what decisions you make in these games. Also you do not need any experience or practice to do well. These games cannot say anything about how well you can perform compared to your peers. So if someone earns more/less than you then that does not mean they performed better/worse than you. You will play most of these games while sitting in your desks. After you complete all 5 games, we will choose <u>one</u> game from a lottery and pay you according to that game. So, in order to get paid, you will have to complete all 5 games. If you leave before completing all 5 games then unfortunately you will only receive the food box. But please remember that the food box has 100 Taka worth of delicious food in it so you should not feel bad about not earning money if you leave.

Now we will explain the rules of the first game. Before we do that, do you have any questions? [*pause*] Are you happy with the rules that I just told you? [*pause*] If you are not happy or do not wish to take part then you can raise your hand at any time to say so.

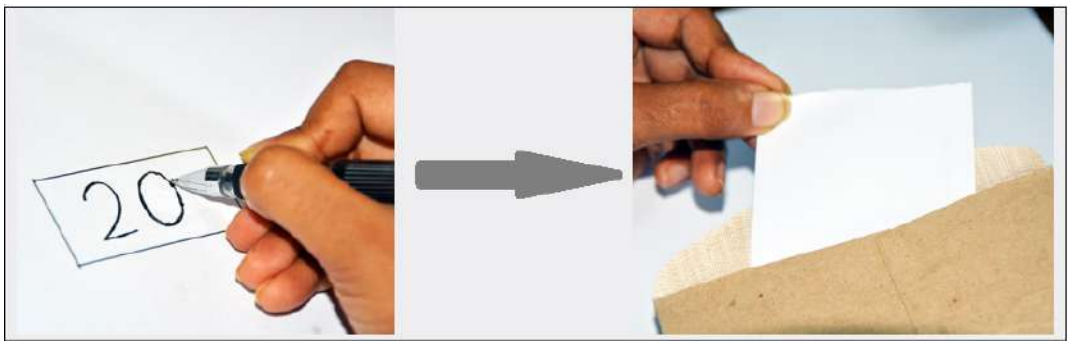
C.2 Game 1 (Donation)

For participating in this game, we will give you 50 Taka. Now you can decide how much of this 50 Taka that you just earned you wish to keep for yourself and how much you wish to donate to an orphan school called: Al Aziz Orphan Madrasa or Sunshine Orphan School [*delete as appropriate*]. If you do not give anything to the orphan school then you will earn 50 Taka from this game. However, if you decide to donate some money then any money you give will go to the orphan school, and the remaining amount will be yours. Therefore, the more you give, the less you will have but the more money the orphan school will receive. I will now give you some examples. Please listen very carefully:

- If you donate no money or *zero* then the orphan school will receive nothing from you but

you will have 50 Taka for yourself.

- If you donate all money or 50 Taka then the orphan school will receive 50 Taka from you but you will have no money for yourself.
- If you donate 10 Taka then the orphan school will receive 10 Taka from you but you will have 40 Taka for yourself.
- If you donate 25 Taka then the orphan school will receive 25 Taka from you but you will have 25 Taka for yourself.
- If you donate 45 Taka then the orphan school will receive 45 Taka from you but you will have 5 Taka for yourself.



To make your donations, you will have to write any number between 0 and 50 on the blank piece of paper that we have given you. After you are happy with the number, please fold the paper and put it inside the envelope provided. Any number you write in the paper will correspond to the amount of money you wish to give to the orphan school. For example, if you write 20 then it will mean you want to give 20 Taka to the orphan school and so on. See the picture. If you write any amount that is more than 50, then the maximum amount of 50 Taka will be donated. Please do not tell/show me or anyone how much you are donating. Once you finish, please keep the envelop on your table and wait for us to collect it.

Do you have any questions? [*pause*] Now, please write in the paper how much you wish to give to the orphan school Al Aziz Orphan Madrasa or Sunshine Orphan School [*delete as appropriate*] from your 50 Taka.

C.3 Game 2 (Investment)

For participating in this game, we will give you 50 Taka. Now you can decide how much of this 50 Taka that you just earned you wish to invest in a risky lottery. This can be any amount between 0 and 50 Taka. The rest of the money will be for yours to keep. So if you do not want to play this game then you will have 50 Taka for yourself. However if you want to play then you have the chance to either multiply this amount or lose it. So this game is risky. Please listen to the rules carefully.

Risky lottery: In this lottery there is an equal chance you will fail or succeed, which will be determined by a coin toss. After you choose how much you wish to invest in this lottery, we will do a coin toss to determine whether you win or lose. If the coin comes up heads, you win 3 times the amount you chose to invest in the lottery. If the coin comes up tails, you lose the

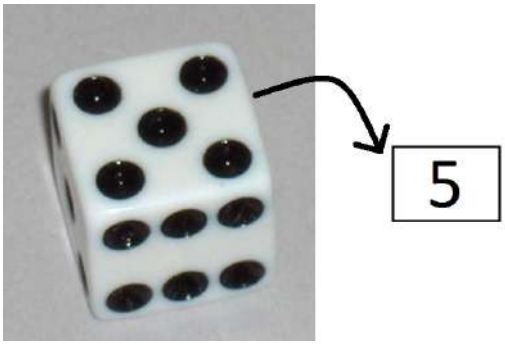
amount invested. I will now give you some examples. Please listen carefully:

- If you choose not to play, then you will get 50 Taka for sure. That means, we will not do any coin flips for you.
- If you choose to invest all of 50 Taka, then if the coin comes up heads, you win 150 Taka, and if it comes up tails, you win nothing and end up with 0.
- If you choose to invest 20 Taka, then if the coin comes up heads, you win 60 Taka from this lottery which will then be added to the remaining 30 Taka which you did not invest. But if it comes up tails, you only end up with 30 Taka which you did not invest.
- If you choose to invest 5 Taka, then if the coin comes up heads, you win 15 Taka from this lottery which will then be added to the remaining 45 Taka which you did not invest. But if it comes up tails, you only end up with 45 Taka which you did not invest.

Do you have any questions? [*pause*] Now, please be seated. We will ask you to go to the desk outside, one by one, to make your choice on how much you want to invest in the lottery. Please be patient and wait for your turn.

C.4 Game 3 (Rolling a Dice)

In this game you can earn money based on the outcome of rolling a dice. So this game is also based on luck. We will ask you to throw a dice and report the number outcome on top of the dice after it lands. For example, the outcome is *five* if the top of the die looks like the picture below:



You will have to roll the dice 10 times and have to write the outcome each time in the table provided. [*Show the table*] You will receive 1 Taka if the outcome is 1; 2 Taka if the outcome is 2 and so on. So if you are lucky and get all 6's in all 10 rolls, then you will receive 60 Taka from this game. However, if you are unlucky and get all 1's in all 10 rolls, the you will receive 10 Taka from this game.

Do you have any questions? [*pause*] Now, please roll your dice in private and make sure others do not observe your game. After writing down all 10 outcomes from all 10 throws, please fold the paper and put it into the envelope provided (just like the first game). Once you finish, please keep the envelope on your table and wait for us to collect it.

C.5 Game 4 (Cooperation)

In this game you will play with another person from another room, but you do not know whom, and the other person does not know that he plays with you. You will not know until the end of the experiment how the other person played in this game. For participating in this game, we will give you 50 Taka. Now you can either decide to send all of it to the person you are playing with or keep all to yourself. If you send all of it to the other person then this amount will be doubled and the other person will receive 100 Taka. In the same manner, the other person is also going to decide whether to send all of his money to you or keep to himself. If he transfers all his money to you then you will receive the doubled amount, that is 100 Taka. Therefore, if you both decide to keep the money to yourselves then both of you will have 50 Taka. However, if you both decide to transfer all to one another, then both of you will end up with 100 Taka each. I will now give you some examples. Please listen carefully:

- If both of you transfer all your money, then all money will be doubled. In that case you will receive 100 Taka from the other person and the person you are playing with will receive 100 Taka from you.
- If none of you transfer your money, then you will be able to keep the 50 Taka to yourself and the other person will also be able to keep his 50 Taka to himself.
- If you transfer all your money but the other person decides not to transfer, then you will end up with no money because the other person did not transfer you any money. But the other person will receive 100 Taka from you, that you transferred, and 50 Taka that he did not send you. In total he will end up with 150 Taka and you will end up with nothing.
- If you do not transfer your money but the other person decides to transfer, then you will end up with your own 50 Taka and the doubled amount of 100 Taka from the other person, in total 150 Taka. However, in that case, since the other person transfers all his money to you and you do not transfer anything, the other person ends up with nothing.

Do you have any questions? [*pause*] If you have decide to transfer all your money to the other person, then please put a tick ($\surd$) on the paper provided. If you have decide not to transfer any money to the other person, then please put a cross ($\times$) on the paper provided. Once you complete the task, please fold the paper and put it inside the envelope provided. Once you finish, please keep the envelope on your table and wait for us to collect it. If this game is chosen as the payoff game, then you will be informed at the end about the other player's decision when we are paying you.

C.6 Game 5 (Trust)

C.6.1 Instruction for Trustors

In this game you will play with another person from the other classroom, but you do not know whom, and the other person does not know that he plays with you. We will give 50 Taka to you and also 50 Taka to the other participant who is playing with you. Your decision in this game is to decide how much money you wish to send to the person you are playing with. All money that you send will be tripled ($\times 3$) by us before it reaches the other person. Then

the other person will decide how much of that tripled money to send back to you. After the other person's decision, this game will be over. So your earning from this game will be any money that you decide to keep to yourself plus any amount the other person sends back to you. Earning of the other person will be 50 Taka that we gave him plus the tripled amount that you send to him minus any amount he decides to return you back. I will now give you some examples. Please listen carefully:

- Imagine you transfer 20 Taka to the person you are playing with. Then that person will receive the tripled amount, 60 Taka, from you. Now, you have 30 Taka and the other person has 60 Taka plus 50 Taka, or 110 Taka. Imagine the other person sends you nothing back. Then your outcome will be 30 Taka and the other person's outcome will be 110 Taka.
- Imagine you transfer 20 Taka to the person you are playing with. Then that person will receive the tripled amount, 60 Taka, from you. Now, you have 30 Taka and the other person has 60 Taka plus 50 Taka, or 110 Taka. Imagine the other person sends you 20 Taka back. Then your outcome will be 50 Taka and the other person's outcome will be 90 Taka.
- Imagine you transfer 0 Taka to the person you are playing with. Then that person will receive nothing from you and the game will end there. But he will still have 50 Taka that we gave him at the start. In that case, your outcome will be 50 Taka and the other person's outcome will be 50 Taka.
- Imagine you transfer 50 Taka to the person you are playing with. Then that person will receive the tripled amount, 150 Taka, from you. Now, you have 0 Taka and the other person has 150 Taka plus 50 Taka, or 200 Taka. Imagine the other person sends you nothing back. Then your outcome will be 0 Taka and the other person's outcome will be 200 Taka.

Do you have any questions? [*pause*] To send money to the other person, you will have to write the amount between 0 and 50 on the blank piece of paper provided. After you are happy with the number, please fold the paper and put it inside the envelope provided. Once you finish, please keep the envelop on your table and wait for us to collect it. Then I will give this envelope to the helper seating at the enrollment desk. He will then replace your transfer with a tripled amount before sending it to the person you are playing with. Since your paper will be replaced, the other person will not be able to guess you from your handwriting. After the other person makes his decision, the envelope will once again come here through the enrollment desk, so you will not be able to guess the person you are playing with from his handwriting.

Do you have any questions? [*pause*] Now, please write in the paper how much you wish to send to the other person.

C.6.2 Instruction for Trustees

In this game you will play with another person from another room, but you do not know whom, and the other person does not know that he plays with you. We will give 50 Taka to you and also 50 Taka to the person you are playing with. That person will then decide how

much of that money to send to you. All money he sends will be tripled by us before it reaches you. Then your decision will be how much money you wish to send back to the person you are playing with. This game will end after you make this decision. So your earning from this game will be 50 Taka that we have given you plus the tripled money you receive from the other person minus any amount you send back to that person. Earning of the other person will be any money that he keeps to himself plus any amount you send him back. I will now give you some examples. Please listen carefully:

- Imagine the other person transfers 20 Taka to you. Then you will receive the tripled amount, 60 Taka from him. Now, you have 60 Taka plus 50 Taka, or 110 Taka, and the other person has 30 Taka. If you do not send anything back then your outcome will be 110 Taka and the other person's outcome will be 30 Taka.
- Imagine the other person transfers 20 Taka to you. Then you will receive the tripled amount, 60 Taka from him. Now, you have 60 Taka plus 50 Taka, or 110 Taka, and the other person has 30 Taka. If you send 20 Taka back to that person, then your outcome will be 90 Taka and the other person's outcome will be 50 Taka.
- Imagine the other person transfers nothing to you. Then you will receive nothing from him but will still have 50 Taka that we have given you at the start. The other person will have 50 Taka.
- Imagine the other person transfers all of his 50 Taka to you. Then you will receive the tripled amount, 150 Taka from him. Now, you have 150 Taka plus 50 Taka, or 200 Taka, and the other person has nothing. If you send nothing back to that person, then your outcome will be 200 Taka and the other person's outcome will be 0 Taka.

Do you have any questions? [*pause*] We will now give you your envelopes which contains the tripled amount sent from the person you are playing with. Please open the envelope without showing to anyone and check how much you have received. For example, if it says 50, then it means you have received 50 Taka from the person you are playing with. To send back any amount to that person, you will have to write that amount on that same piece of paper, in the blank box. If you do not wish to send back anything, write zero. Also, you cannot send back more than what you have received. After you are happy with the amount, please fold the paper and put it back inside the same envelope. Once you finish, please keep the envelop on your table and wait for us to collect it. Then I will give this envelope to the helper seating at the enrollment desk. He will then replace your back transfer paper with another paper, so that the other person cannot guess who you are from the handwriting. The other person's transfer have also come to you through the enrollment desk, so that you cannot guess who the other person is from the handwriting.

Do you have any questions? [*pause*] Now, please write in the paper how much you wish to send back to the other person.

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