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Essays in Behavioral Development Economics

by

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Abstract

This thesis consists of three essays on *behavioral development economics* that explores how different cultures, institutions, and social environments shape economic behavior and preferences of people.

In many countries, ethnic minorities have a persistent disadvantageous socioeconomic position. In the first essay, co-authored with Michael Vlassopoulos, we investigate whether aversion to competing against members of the ethnically dominant group could be a contributing factor to this predicament. We conducted a lab-in-the-field experiment in rural Bangladesh recruiting males from the ethnic majority (Bengali) and an underprivileged ethnic minority group (Santal) that is severely discriminated against. We randomly assign participants into groups with different ethnic composition and elicit a measure of their competitiveness. We find that when compelled to compete, there are no ethnic differences in performance and that both ethnic groups perform better in ethnically-mixed groups than in homogeneous groups. We also find that the ethnic composition of the group of competitors is an important determinant of competitive entry and its effect varies by ethnic group. Members of the ethnic minority group are less likely to compete in groups where they are a numerical minority than when all competitors are co-ethnic, whereas the reverse is true for members of the ethnic majority group. This difference is not explained by heterogeneity in performance, risk preferences, beliefs about relative ability or various socioeconomic characteristics of individuals or of their opponents. We conclude that differences in unmeasured markers of ethnicity, such as social power and status, must underpin these differences in preferences for interethnic competition.

In the second essay, to understand how long-term exposure to religious education is shaping economic behavior and preferences of students, I exploit a natural field setting in Bangladesh where I overlay a battery of controlled experiments to identify the causal effect of religious education on behavior and preferences of students, namely altruism, dishonesty, trust, cooperation, and risk aversion. I compare behavior in schools for orphans that differ in terms of intensity of religious education and lifestyle, restricts transmission of beliefs and preferences from parents to children, makes social learning by students limited after school enrolment and addresses issues concerning endogenous school choice. I find that (i) students receiving religious education are more altruistic and honest relative to students receiving secular education; (ii) these effects are driven by students who have spent 6 or more years in schooling; and (iii) religious education

has no effect on risk preference, trust, trustworthiness, and cooperation of students. A series of robustness tests confirm that results are not driven by selection and that the relationship is causal.

In the third essay, co-authored with Michael Vlassopoulos, we carry out an experiment in Bangladesh to capture ethnic discrimination in the agricultural market. We organize a competition among rice farmers followed by a series of rice evaluation programs. To determine the winner, we recruit rice buyers from various marketplaces to assess rice quality and determine their willingness to pay for each rice sample collected from participants. To measure discrimination, we randomly assign ethnic majority and minority sounding names on each rice sample prior to evaluation to implicitly reveal the farmer's ethnic identity to the buyer. We find that buyers are willing to pay 2% less to ethnic minority farmers than what they are willing to pay to ethnic majority farmers, albeit not discriminating in terms of the quality of rice produced. We interpret this finding as being consistent with the *taste-based* model of discrimination. We also find that discrimination reported is driven exclusively by buyers from the villages whereas city buyers do not discriminate ethnic minority farmers. Further analyses suggest that lack of interethnic interaction might be an underlying source of prejudice. Finally, we show that ethnic minority farmers would benefit from selling rice to city buyers only, an initiative that would help avoid discrimination while also generate a week's additional income every year.

Declaration of Authorship

I, Abu Bakker Siddique , declare that the thesis entitled *Essays in Behavioral Development Economics* and the work presented in the thesis are both my own, and have been generated by me as the result of my own original research. I confirm that:

- this work was done wholly or mainly while in candidature for a research degree at this University;
- where any part of this thesis has previously been submitted for a degree or any other qualification at this University or any other institution, this has been clearly stated;
- where I have consulted the published work of others, this is always clearly attributed;
- where I have quoted from the work of others, the source is always given. With the exception of such quotations, this thesis is entirely my own work;
- I have acknowledged all main sources of help;
- where the thesis is based on work done by myself jointly with others, I have made clear exactly what was done by others and what I have contributed myself;
- none of this work has been published before submission.

Signed:.....

Date:.....

Co-Authorship Statement

Chapter 2 (*Competitive Preferences and Ethnicity: Experimental Evidence from Bangladesh*) and Chapter 4 (*Discrimination in the Agricultural Market: The Case of Bangladeshi Rice Farmers*) were co-written with Professor Michael Vlassopoulos (University of Southampton). My contribution to the production of these research works is outlined below:

- Identification of research question – Shared responsibility with co-author.
- Experimental design – Shared responsibility with co-author.
- Fieldwork and data collection – Shared responsibility with co-author.
- Data analyses – Shared responsibility with co-author.
- Manuscript preparation – Shared responsibility with co-author.

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To Ammu, Abbu & Dadi

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Chapter 1

Introduction

Recent research in economics have expanded our understanding of how psychological, social, and cultural factors influence behavior and decision making, and how such influences can have a significant impact on numerous development outcomes and help achieve important development goals (see the World Bank (2015b)'s *World Development Report - Mind, Society, and Behavior* for a range of successful interventions by changing human behavior). For instance, La Ferrara (2016) argues how television and radio can provide new information to people to update their existing beliefs and change their preferences, and how that can have impacts on family planning, gender norms, education, and so on – components that are directly associated with economic development. Similarly, Habyarimana & Jack (2015) shows how providing evocative messages to passengers can reduce traffic accidents in Kenya. While governments mostly target drivers to reduce road accidents, this paper highlights the importance of nudging passengers whose behavior subsequently affect drivers' reckless driving. Also, to understand the mechanism behind engaging in corruption in public services, Hanna & Wang (2017) measures corruption in a laboratory experiment and find that dishonest people self-select into public services and engage in corrupt activities once there. Understanding this behavior is important to better inform policymakers about the importance of screening out corrupt applicants during recruitment to ensure an effective bureaucracy. Therefore, what existing research in *behavioral development economics* highlight is that knowing whom to target and how they make decisions would lead to designing effective interventions that would help households and individuals to make effective economic choices.[1] Recent literature also suggests that insights from behavioral economics can be effective in fighting a crucial economic problem such as poverty (Mullainathan, 2005; Banerjee & Duflo, 2011). On poverty, Haushofer & Fehr (2014) finds that the psychological effects of poverty can affect economic choices of the very poor which might make it difficult for them to escape it. Overall the literature suggests that targeting psychological and behavioral variables for policies would be the most effective in leading individuals to make beneficial choices and alleviating poverty.

[1] For a review on *behavioral development economics* literature, see Demeritt & Hoff (2018) and Kremer, Rao & Schilbach (2019).

Therefore, to extend our understanding on behavioral aspects in the development context, I investigate issues like why some social groups continue to fare badly in various socioeconomic outcomes and how much role does different cultures, institutions, and social environments play in shaping economic behavior and preferences of individuals in three separate essays in this thesis. Specifically, I, first, explore why many ethnic minorities around the world are in persistent disadvantageous positions; secondly, I try to understand whether long-term exposure to scriptures, religious rules, rituals, and upbringing affect behaviors and preferences of children; and finally, I investigate whether ethnic minorities face discrimination in the agricultural market – a sector that plays a crucial role in early economic development and fighting poverty. Exploring such behaviors would certainly give us a better understanding of where the problems lie and how to formulate effective social, educational and agricultural policies. I explore these issues in Bangladesh where I collect data using lab-in-the-field and field experiments. An overview of the economic problems and findings of the three chapters of this thesis are laid out below.

The first essay, entitled *Competitive Preferences and Ethnicity: Experimental Evidence from Bangladesh*, co-authored with Michael Vlassopoulos, explores why many ethnic minorities around the world are in persistent disadvantageous positions. One possible reason could be that many ethnic majorities and minorities in various parts of the world have conflicting relationships, which might indoctrinate the two groups from a very young age to form opposing stereotypes regarding one another and, eventually, shape their preferences differently. One important preference that has gained significant attention in the behavioral economics literature lately is of one's preference for competition. We know that competition is ubiquitous. There are environments that are more competitive than the other and such environments require constant competition for survival: animals in forests compete for food and territory, shopkeepers compete for customers in a busy village market, lawyers compete for clients, employees compete for promotions, and so on. So one's willingness to self-select into competitive environment could explain important life decisions and outcomes such as occupational and academic track choices, investment and sales decisions, and other important labor market decisions and outcomes (Buser, Niederle & Oosterbeek, 2014; Zhang, 2013; Berge, Bjorvatn, Pires & Tungodden, 2015). Recent literature on competitive preference also suggests that how being nurtured in distinct environments forms one's preference towards competition differently (Gneezy, Leonard & List, 2009; Leibbrandt, Gneezy & List, 2013). Such evidence on endogenous formation of preferences argue that variations in competitiveness emerge from observational, social, and cultural learning (Bandura, 1977; Tomasello, Kruger & Ratner, 1993; Bowles, 1998); hence, exposure to different environments and pressures are a crucial driver of differences in competitive attitudes.

To test whether such socialization has any effect on minorities' preference to compete against majorities, we ran a lab-in-the-field experiment in multi-ethnic villages in Bangladesh with a severely discriminated ethnic minority group and the dominant ethnic majority group. In our experiment, we randomly assign participants into groups of

six with different ethnic compositions. That is, participants are either all six co-ethnic group members (i.e. homogeneous treatment), a group with only four co-ethnic group members (i.e. majority treatment) or a group with only two co-ethnic group members (i.e. minority treatment). They perform a simple manual task in three stages. In the first stage, they are compensated according to a piece-rate scheme for their task performance; in the second stage, they are compensated according to a winner-take-all compensation scheme for their task performance; and in the final stage, they are asked to choose whether they wish to be paid according to a piece-rate or a tournament compensation scheme for their task performance. The choice made in the third stage measures their willingness to compete. We find that when members of the ethnic minority are a numerical minority in a group, they have an aversion towards competing against the ethnic majority, whereas when members of the ethnic majority are a numerical minority in a group, they show an inclination towards competing against the ethnic minority. Our interpretation for this result is that when ethnic minority participants are in a numerical minority position in a group, the stereotype of them being ethnically inferior kicks in, which as a result discourages them to engage in competition with the ethnic majority group members. Likewise, for the ethnic majority in the same situation, the stereotype of them being ethnically superior kicks in that encourages them to compete more with ethnic minority group members. In a world where most of the ethnic minorities are numerical minorities in the society and are often subject to exploitation by the ethnic majorities, it is very likely that such environments also discourage ethnic minorities from self-selecting into competitive occupations, study tracks, or business choices, which might be affecting their socioeconomic status.

In my next essay, entitled *Behavioral Consequences of Religious Education: A Lab-in-the-Field Experiment in Bangladesh*, I explore whether long-term exposure to scriptures, religious rules, rituals, and upbringing affect behaviors and preferences of children. With the growing number of religious schools around the world, it is important to understand the mechanisms through which religious education might affect longer-term outcomes. To investigate this, I ran a battery of controlled experiments in Bangladeshi religious and secular schools for orphans. I chose this field setting because it restricts transmission of preferences from parents to children, limits social learning of students, and addresses problems associated with selection bias. Establishing the causal effect of religion has always been difficult (Altonji, Elder & Taber, 2005), especially when it involves identifying the causal effects of religious schooling. This is because parents educate their children about many religious values prior to sending them to schools and as school enrolment is parents' choice, hence endogenous, simply comparing behavior across religious and secular schools would lead to biased outcomes. Therefore, exploring orphan children who grew up in orphanages that vary in terms of religiosity, I am able to explore behaviors and preferences of children who grew up in infant orphanages prior to joining their schools. Hence, this setting restricts transmission of values and behaviors from parents to children during pre-school years (Bisin & Verdier, 2000; Dohmen, Falk,

Huffman & Sunde, 2011), and also do not allow parents to partake in school selection for their children.

Through various well-established games, I measure children's level of altruism, trust, dishonesty, cooperation, and risk preference. These five behaviors and preferences are directly associated with religious teachings, where religions teach their adherents to be more altruistic, honest, cooperative, trusting, trustworthy, and risk-averse. My results show that students in religious schools are more altruistic and honest relative to students in secular schools, and such differences are driven by students who have spent 6 or more years in schooling. Through a series of robustness checks, I show that religious education only affects altruism and dishonesty among children. However, I did not find any differences in terms of trust, cooperation, and risk aversion. While many existing studies looked at the effects of short-term interventions, such as going to the *Hajj* or fasting during *Ramadan*, I look at the long-term exposure of being in a religious environment. Since the number of religious schools is growing around the world, mostly in developing countries, exploring behaviors of children who are are growing up under such environments might explain how they would make important economic decisions as adults, such as making investment decisions, engage in corruption, and so on.

The third essay titled *Discrimination in the Agricultural Market: The Case of Bangladeshi Rice Farmers*, co-authored with Michael Vlassopoulos, explores discrimination against ethnic minority rice farmers by the ethnic majority rice buyers in the agricultural market in Bangladesh. The agricultural sector plays a crucial role during the early stages of economic development, subsequent structural transformation, and poverty alleviation (Johnston & Mellor, 1961; Irz, Lin, Thirtle & Wiggins, 2001; McArthur & McCord, 2017). Moreover, there are also evidence of agricultural technologies reducing poverty across many developing countries by adopting improved crop varieties, such as in Mexico (Becerril & Abdulai, 2010), Uganda (Kassie, Shiferaw & Muricho, 2011), Madagascar (Minten & Barrett, 2008), and many others. Even though the agricultural sector plays a crucial role in poverty reduction, farmers around the world are still some of the poorest. While they have limited access to market information, opportunities, and technologies, farmers from ethnic minority groups, such as indigenous communities who contributes greatly to the ecosystem through their traditional farming methods, are also subject to social, political, and economic exclusions, which limits their access to a plethora of opportunities and agricultural resources. Therefore, documenting discrimination against ethnic minority farmers and identifying its underlying mechanisms would provide empirical evidence to policymakers about whether discrimination exists in the agricultural market and, if so then, how it could be tackled.

For this study, we took the case of the agricultural market in Bangladesh to document ethnic discrimination and its underlying mechanisms. To capture discrimination, we conduct a field experiment with rice farmers and buyers. We organize a competition among rice farmers where the farmer who cultivated the "best" rice in the previous rice season is offered a large cash prize. To determine the winner of the competition, we recruited ethnic majority rice buyers (people who buy rice for business) from various

locations to assess the physical quality of rice and state how much they are willing to pay for that rice. In particular, we recruited buyers from multi-ethnic villages, mono-ethnic villages (where all residents are the ethnic majority), and the city. To measure discrimination, we adopt the methodology presented in Bertrand & Mullainathan (2004) by randomly assigned ethnic majority and minority sounding names on each rice sample to be assessed by rice buyers. We find that ethnic majority rice buyers discriminate ethnic minority farmers by willing to pay them less but do not discriminate in terms of rice quality assessment. This behavior is consistent with the *taste-based* model of discrimination (Becker, 1957). We also find that discrimination is profound in the villages only but not in the city. While exploring the underlying sources for establishing a strong preference against ethnic minority farmers, we find, and then argue, that socialization and the lack of interethnic interaction might have formed the distaste that we capture. Furthermore, as only village buyers discriminate, we also show that it is optimal for ethnic minority farmers to sell their products to buyers in the city.

In the final chapter, I provide a brief summary of the results and conclude with some policy implications.

Chapter 2

Competitive Preferences and Ethnicity: Experimental Evidence from Bangladesh

2.1 Introduction

A number of recent studies have found ample evidence of gender differences in competitive preferences (Croson & Gneezy, 2009; Niederle & Vesterlund, 2011; Niederle, 2017).[1] Subsequent studies have shown that measures of competitiveness elicited experimentally can explain economic outcomes outside the laboratory such as career and educational choices, earnings and investment decisions (Buser et al., 2014; Zhang, 2013; Reuben, Sapienza & Zingales, 2015; Berge et al., 2015; Flory, Leibbrandt & List, 2014).[2] Like women, ethnic minorities in many countries and contexts (e.g. African Americans in the US, Roma in Europe, indigenous people in various parts of the world) also fare badly in the labor market and are lagging behind the respective ethnically dominant group in various socioeconomic indicators. Extending the insight about the explanatory power of competitive preferences for gender differences, one could conjecture that competitive preferences might help us understand the persistent disadvantageous position of ethnic minorities. That is, if members of ethnic minority groups are reluctant to compete against members of the ethnically dominant group, they might be refraining

[1] Findings from this growing literature suggest that women and men react to competition differently, with women exhibiting distaste for competition and performing less well in competitive environments (Gneezy, Niederle & Rustichini, 2003; Niederle & Vesterlund, 2007), while a handful of studies have further provided evidence that both women and men dislike competing against men in particular (Datta Gupta, Poulsen & Villeval, 2013; Niederle, Segal & Vesterlund, 2013; Geraldes, 2016; Burow, Beblo, Beninger & Schröder, 2017).

[2] Zhang (2013) and Buser et al. (2014) find that students who are more inclined towards competition are more likely to take competitive high school entry exams and opt to choose prestigious study tracks. Also, exhibiting competitive tendencies in the lab has been shown to be associated with higher (expected and actual) earnings and working in high-paying industries (Reuben et al., 2015; Reuben, Wiswall & Zafar, 2017), as well as investing more in businesses in the field (Berge et al., 2015). Finally, Flory et al. (2014) provide field experimental evidence that women are less likely to apply for jobs in which compensation is based on relative performance.

from taking actions, such as, investing in education, applying for positions of authority or accessing scarce resources that would help them elevate their socioeconomic status. A good starting point to address these issues is to examine empirically whether differences in preferences for interethnic competition exist between the majority (dominant) and minority (subordinate) ethnic groups, which is the main aim of this paper.

We conducted a lab-in-the-field experiment in rural Bangladesh, drawing participants from the ethnic majority (Bengali) and a minority group (Santal). Bangladesh is a suitable context for our purposes, as it is home to many ethnic minority/indigenous communities that do severely poorly in several socioeconomic outcomes and are subject to exploitation and discrimination by members of the ethnic majority group.[3] In this environment, it is very likely that the two ethnic groups are indoctrinated from a young age to form opposing stereotypes regarding non-co-ethnic members: Santals grow up to believe that they are inferior to the majority ethnic group, while the latter are nurtured to look-down-upon the minority group.[4] Consequently, this socialization process could shape the norms surrounding interethnic interactions, including attitudes towards engaging in competition with each other. Indeed, recent studies have shown that culture, socialization, the local environment, social and economic institutions and even local work experiences play significant role in the shaping of competitive preferences (Gneezy et al., 2009; Booth & Nolen, 2012; Cárdenas, Dreber, Von Essen & Ranehill, 2012; Andersen, Ertac, Gneezy, List & Maximiano, 2013; Leibbrandt et al., 2013; Zhang, 2018; Booth, Fan, Meng & Zhang, 2018). We, thus, expect that given the ethnic background of our setting, the two ethnic groups will have developed tastes for interethnic competition that mirror the clear hierarchical relationship and the imbalance of power that exists between them.

In the experiment, we randomly assign participants into groups of six, and we obtain a measure of their competitive preferences following the design of Niederle & Vesterlund (2007). In particular, we first ask our participants to perform a simple manual task (separating lentils from rice) under a piece-rate and then a competitive compensation scheme (winner-take-all tournament). In a third stage, we ask them to select their preferred compensation scheme, which reveals their preferences with respect to competition. Our experimental design involves three treatment groups: a *homogeneous* group where subjects are all co-ethnics, a group where ethnic minority people are a numerical *minority* (2 to 4), and a 'reversed' group where ethnic minority people are a numerical *majority* (4 to 2). Ethnicity in our context is easily identifiable by physical markers and hence is unambiguous. Thus, even though ethnic composition was never discussed in the course of the experiment, our subjects could easily identify the ethnicity of their group members and hence the ethnic composition of their group. We expect

[3] See, for example, AIPP (2007) and Roy (2012) for unfair treatments of ethnic minorities in Bangladesh. We describe these in detail in Section 2.2.

[4] Tribal minorities are seen as 'inferior races' by the ethnic majority (Hardiman, 1987; Bal, 2007), which is believed to be a product of multi-generational socialization process (Barndt, 2007). On children internalizing socialized lies regarding superiority and inferiority, Joseph R. Barndt (2007, p. 125) says, "...If I am consciously and unconsciously taught from the moment of my birth that I am inferior (superior) and a member of an inferior (superior) race, I will believe and act according to this message."

that while there should not be an overall difference in competitive inclination towards co-ethnics across the two ethnic groups, in ethnically diverse groups, we would see ethnic differences in preferences for engaging in competition, with the minorities showing increased aversion to competing the more the members of the majority group in the group of competitors.

We find that in the compulsory tournament stage, there are no ethnic differences in performance and that both ethnic groups perform better in ethnically-mixed treatment groups than in *homogeneous* treatment groups. This suggests that participants are more willing to internalize the negative externality their effort imposes on a group of co-ethnics under a relative performance incentive scheme and is consistent with previous lab and field evidence of an in-group bias in people's social preferences (Bandiera, Barankay & Rasul, 2005; Chen & Li, 2009). We also find that ethnic composition of group of competitors is an important determinant of self-selection into the competitive scheme is stage 3. Despite the fact that overall competitiveness is similar across ethnic groups, group composition affects tournament entry decisions by members of the two ethnic groups differently. When compared to choices made in *homogeneous* treatment groups, members of the ethnic minority show a distaste for competition in groups where they are a *minority*, whereas ethnic majority members demonstrate a preference for competition in groups where they are a *minority*. To be more precise, ethnic minorities are 25 percentage points less likely to compete in groups where they are a *minority* (decrease of 60 percent) than when all competitors are co-ethnic, whereas ethnic majorities are 22 percentage points more likely to compete in groups where they are a *minority* (increase of 80 percent) than when all competitors are co-ethnic.

We show that these patterns are not explained by heterogeneity in task proficiency, risk preferences, beliefs about relative ability on the task, or a host of demographic characteristics. Hence, our study points to participants from the two ethnic groups having a genuinely different attitude toward entering competitive environments in which the pool of potential competitors is multiethnic. Given the degree of familiarity among the participants and the substantial differences in socioeconomic status across the ethnic groups, we also explore whether the ethnic differences in preferences for interethnic competition remain even when we take into account the income and land ownership of competitors. This allows us to explore to what extent the differences that we find are mediated through ethnic differences in income or land ownership. We find that differences in material well-being are not driving the results that we find. Instead, it might be differences in unmeasured dimensions, such as social power and status, marked by ethnic identity, that are important for understanding the different attitudes toward interethnic competition.

Beyond the literature on competitive preferences mentioned above, our study connects to the literature on social identity, the formation of stereotypes and their impact on behavior that has a long history in psychology and sociology (Tajfel, 2010; Greenwald & Banaji, 1995; Steele & Aronson, 1995; Shih, Pittinsky & Ambady, 1999). In economics, the seminal paper by Akerlof & Kranton (2000) introduces a theoretical framework that

connects social identity based on social differences, e.g. race, class, ethnicity, etc. with economic behavior and outcomes. More recently, experimental studies have shown that making ethnic or racial identity salient affects risk and time preferences (Benjamin, Choi & Strickland, 2010), and induced group identity affects social preferences (Chen & Li, 2009). Furthermore, a few more recent studies have shown experimentally that social identity can affect the performance of a deprived group or the treatment that they receive from out-group members. In particular, two related studies of caste in India find that publicly revealing the social identity of the lower-caste diminishes their performance in a cognitive task (Hoff & Pandey, 2006, 2014), while Afridi, Li & Ren (2015) find similar effects for rural workers in China. Finally, Fershtman & Gneezy (2001) find that behavior in experimental games (trust, dictator, and ultimatum games) conducted with opponents from different ethnic groups in Israel to be consistent with ethnic stereotypes. Our results extend this line of research by showing that ethnic group identity (majority or minority) matters for one's willingness to engage in interethnic competition.

This paper is organized as follows. Section 2.2 provides background on the ethnic minority group studied. Section 3.4.2 describes the design of our study and the hypotheses to be tested. In Section 4.4, we present our results. Section 3.7 concludes.

2.2 Background on Ethnic Groups in Bangladesh

In Bangladesh, besides the ethnic majority group (Bengali) there are around 45 different tribal/ethnic communities that constitute about 2 million of the country's total population of 150 million, including many of the country's extreme poor (IMF, 2013; People's Republic of Bangladesh, 2016).[5] These groups are culturally, racially, ethnically and linguistically distinct from the majority Bengali population, and are the most persecuted of all minorities.[6] They have restricted access to basic social services such as health, food and nutrition, education, employment, justice and politics (AIPP, 2010; Roy, 2012; IMF, 2013), and are subject to extortion by the ethnic majority land grabbers (Roy, 2012). They also receive unfair prices for their products (AIPP, 2010) and have been at the receiving end of crimes for generations (Roy, 2012; D'Costa, 2014). Illegal dispossession of ethnic people from their lands is widespread (in both Bangladesh and India) where the dispossessed receive nominal to no compensation in some instances, even though the State Acquisition and Tenancy Act 1950 (Section 97, 1950) strictly prohibits alienation of such lands. Figure 2.1 illustrates the geographic concentration of the ethnic population in Bangladesh.

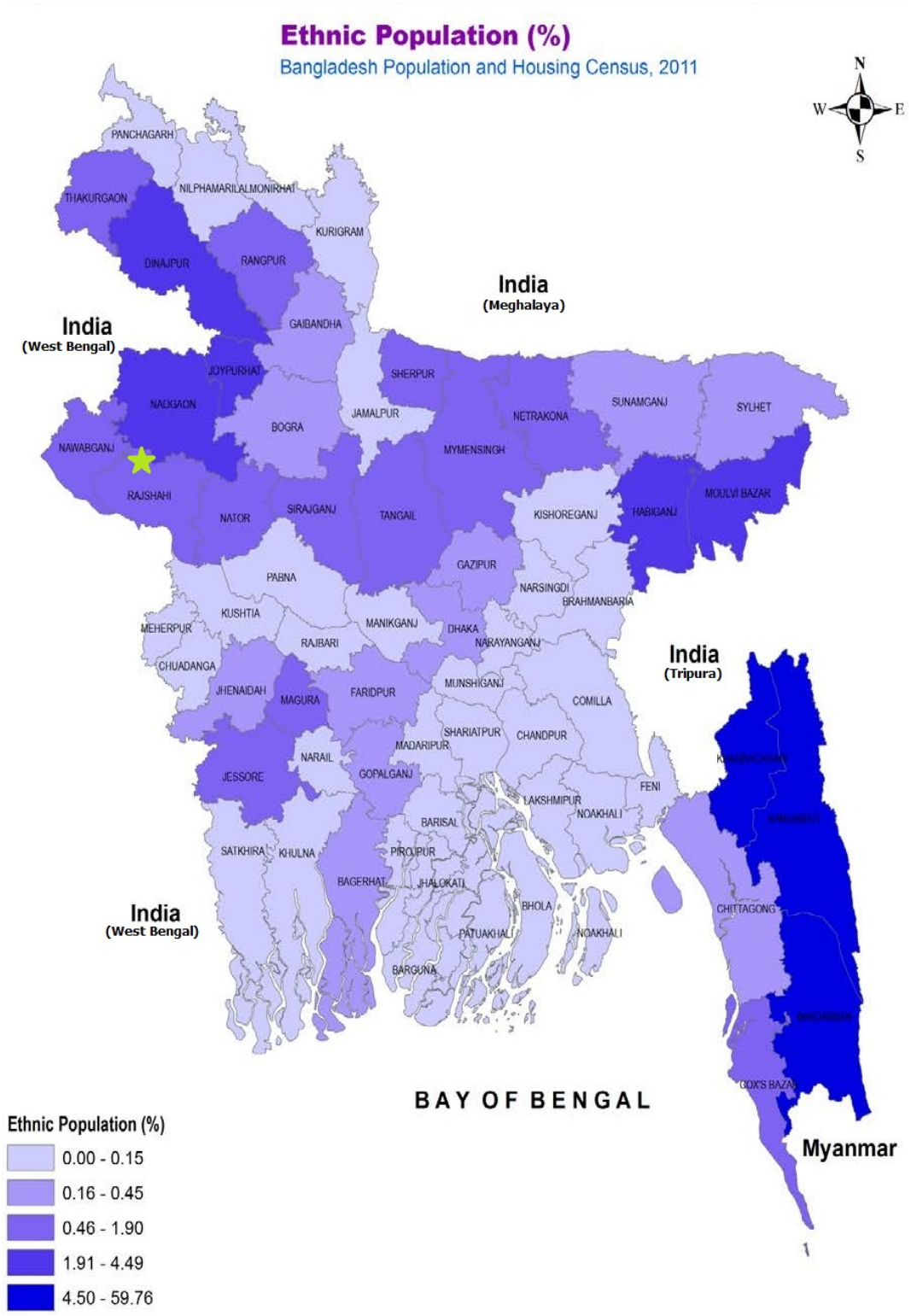
Ethnic minority participants in our study are entirely comprised of people who identified themselves as Santals - the second most populous ethnic community in Bangladesh.[7]

[5] The Bangladeshi government uses the terms tribal, indigenous, and ethnic population interchangeably.

[6] Tribal people in India face similar treatments by the ethnic majority population (Kijima, 2006; Shah, 2007; Bhengra, Bijoy & Luithui, 1999).

[7] Three of the most populous ethnic communities in Bangladesh are the Chakma, the Marma and the Santal; the former two are usually known as 'hill-people' (live in high altitudes) and the latter is known as 'plains-dwelling' (live in the plains).

FIGURE 2.1: Map of Bangladesh



Note: The shading corresponds to the proportion of ethnic population relative to the total population in each Bangladeshi district. The deeper the shading (i.e. blue color), the more ethnic population resides in those districts. **Source:** Population Monograph of Bangladesh (2015, p. 39).

Santals predominantly reside in Rajshahi, Dinajpur, Chapai Nawabganj and Borgra districts in the north-western region of Bangladesh, although the largest portion of Santals

lives in Jharkhand of India (Ali, 1998; Cavallaro & Rahman, 2009; Ahmed, 2010). Like the ethnic majority people in Bangladesh, Santals also follow patriarchy (Upadhyay & Pandey, 1993). They have their own tribal religion (worship a Supreme Deity called *Thakur*) (Risley, 1891), although many also follow Hinduism or Christianity; the ethnic majority Bengali, however, follow Islam (86.6 percent) (People's Republic of Bangladesh, 2016). The majority of Santals are involved in farming, working for landlords as day laborers as most of them are landless (Ali, 1998). They speak Santali language within their community, though they learn to speak Bangla to communicate with the ethnic majority population. One of the first and oldest accounts of ethnic origins and physical characteristics of Santals by Herbert H. Risley (1891, p. 225) describes them as "pure Dravidians", having very dark and "...almost charcoal like" complexion, "large mouth" and "thick lips".[8] They are among the poorest ethnic groups and are severely disadvantaged in terms of employment, land ownership and education (Cavallaro & Rahman, 2009; Samad, 2006). In schools, Santal children face discrimination and physical abuse from their teachers and classmates, e.g. Bengali classmates avoid sitting beside their Santal peers in classrooms, which results in dropouts from schools at a very young age (Samad, 2006; Sarker & Davey, 2009). Their lack of literacy is considered one of the major reasons for easy forgery and illegal dispossession by the ethnic majority population (Sarker & Davey, 2009). In this regard, Cavallaro & Rahman (2009, p. 204) stated:

"... in Bangladesh the Santals face discrimination from the majority community, and the Bangla speaking population and the government has done little to help the Santals protect themselves from the continuous land grabbing and dispossessions. Indeed there is a feeling among the minority people of Bangladesh that they are continuously being overlooked in favor of the majority group in all facets of life. These include employment opportunities and education. These issues have led to a deep sense of social insecurity."

2.3 The Experiment

2.3.1 Recruitment and Procedures

We conducted a lab-in-the-field experiment (Gneezy & Imas, 2016) in the summer of 2016 in six different multi-ethnic villages in the Rajshahi district of rural Bangladesh. We recruited our participants from the two distinct ethnic groups that populate these villages: the ethnic majority Bengali and the ethnic minority Santal. In total, 252 male adults of equal proportion from the two ethnic groups – 126 Santals and 126 Bengalis – participated in our experiment. Our subjects came from fourteen different multi-ethnic villages; although multi-ethnic, segregation within villages is commonplace, as Santal/Bengali houses cluster around their co-ethnics (each cluster is known as a

[8] These features are different than that of the ethnic majority, which makes their ethnicity easily identifiable. See Risley (1891, p. 224-235) for a more detailed explanation of physical characteristics and ethnic origins of Santals. Also, see Orans (1965) and Ali (1998) for more details on customs and lifestyle of Santals.

para) (Ali, 1998).[9] In these villages, 19 percent of the population (and households) are Santals whereas the rest are the ethnic majority Bengali.[10] Also, exogamy or interethnic marriages are not prevalent. People tend to work collectively with their co-ethnics, i.e. generally with family, extended family or relatives, but not with their non-co-ethnic fellow villagers. Since all villagers are considerably poor and do not posses the capacity to hire others for work, members of one ethnic group do not employ members of the other; hence, personal and professional attachments are trivial among members of the two ethnic groups.

Participants were recruited through in-person advertisements: experiment helpers of both ethnicities visited random marketplaces, houses, and crop fields, and advertised our experiment by reading out an experiment advert. Through the advert, people were informed about the pecuniary incentives involved, the conditions for participation and the lab location for registration. Initial registration was carried out in seven different lab locations where we also set up our laboratories. During registration, participants were only asked to provide their full name, age, and ethnicity. Prospective subjects also had the option to choose their preferred lab location and time for a session from seven different lab location options. All people chose their initial registration location as their preferred location for their experiment session. This was expected because all seven of our lab locations are quite far from one another, which was intentionally done to minimize contamination. Furthermore, people were also expected to know each other because our villages are small, so some degree of social networks within and across ethnic groups were expected.

After the initial registrations were complete, potential subjects were given a piece of paper that contained their name, age, and ethnicity, which they were asked to bring to the laboratory. Registration was done on a first-come-first basis, so people who were registered were all invited to the experiment and any 'extra' arrivals at the registration desk were asked to go to our next registration location on another day to register for a session. In total, 296 people were initially registered (four extra for each session). In the experimental sessions, participants were also enrolled in the lab on a first-come-first basis. Four over-recruited individuals for each session were asked to leave with a show-up fee. All people who initially registered arrived at the lab on time. There were 11 sessions in total.

The location of each lab was a central place in the village, e.g. either primary schools, churches or public office spaces. Upon arrival, participants were asked to form a queue outside the lab, on a first-come-first basis. Five minutes prior to the experiment's start time, participants were asked to enter the lab and report to the enrolment table, one by one. At enrolment, they were asked to hand in their initial registration paper

[9]The reason we had participants from fourteen different villages is that people from smaller villages go to larger villages to work every day. Our laboratories were set up in larger villages so that we could attract people from villages other than the villages where we set up our labs. Therefore, our participants did not travel to our laboratory to attend a session, rather they participated right after they finished their work (which is in the afternoon when farmers finish their work and have had their lunch).

[10]We obtain these figures from the Household Survey Report 2012 assembled by *Ashrai*, an NGO.

as well as state their full name and ethnicity, and then based on that information, they were asked to pick their ID numbers from a bowl. In order to ensure that we had the desired number of Santals and Bengalis in each group, we prepared two bowls with ID numbers; one for Santals, in which IDs were matched with seats that were only for Santal participants, and another for Bengali participants. After randomly picking their ID numbers, they were taken to their respective seats by our assistants.

2.3.2 Experimental Design

The experiment follows the standard experimental protocol of eliciting competitive preferences developed by Niederle & Vesterlund (2007). We introduce two main changes: (i) we implemented a different task that is more appropriate for the participant pool in hand, and (ii) we manipulated the ethnic composition of the groups to which participants were randomly assigned to, in order to test for whether the ethnic affiliation of competitors matters for willingness to compete.

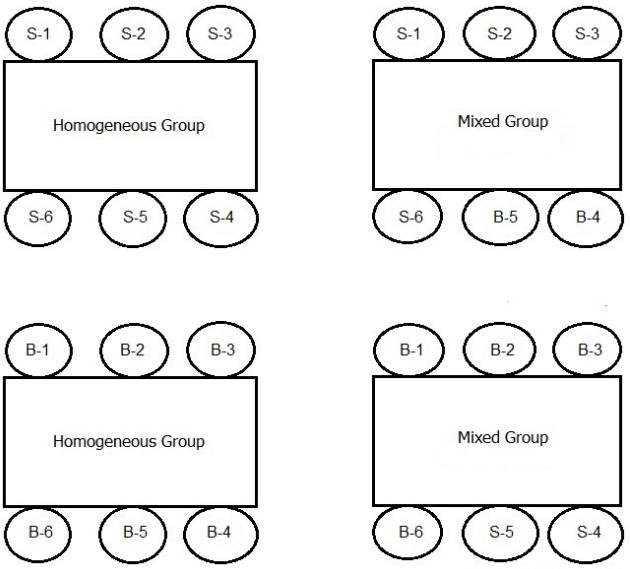
The task used in this experiment was separating red lentils from white rice grains. Specifically, each person received a bowl with a mixture of rice and lentils, and another empty bowl to place the separated lentils.[11] We used red colored bowls for separated lentils so that color of lentils matches with the bowl and make it difficult for our subjects to count/guess the other person's score. Each separated lentil won participants points, while each rice grain picked along with lentils made them lose points. Hence, performance is measured by the number of lentils minus any rice grains placed in the red bowl. This task was selected because it was very simple to explain and implement. Since most of the participants were uneducated and from the unskilled labor force, with the majority of them working as farmers, a task involving everyday grains and pulses was comprehensible to our average participant. In addition, this task was ethnicity neutral (as was found in a pilot and further established in the first stage of the experiment), so no differences in ability were expected across ethnicity. Furthermore, our subjects were all men coming from patriarchal societies where women are mostly considered homemakers and are involved in cooking, so men were expected to know very little about cooking and hence sorting rice and lentils. So, this task was completely new to our subjects, as also confirmed in the exit survey. See the Experimental Instructions and Surveys at the end for a detailed explanation of the task and experimental instructions.

Each session lasted about ninety minutes and was attended in equal proportions by members of the two ethnic groups. Therefore, by design, our sample is balanced across ethnicity in all sessions and the experiment overall.

Participants were assigned to groups of six and each session had four groups in total. Thus, twenty-four participants in total participated in each session, of which half

[11] The mix was always one-fifth lentils and four-fifth rice in terms of volume. Since lentils are smaller and lighter than rice grains, this ratio gave us a near fifty-fifty ratio of numbers of rice and lentils in each bowl.

FIGURE 2.2: Group Composition in Each Session



Note: S is for Santal and B is for Bengali; Numbers 1-6 correspond to the last digit of a subject's ID.

were Santals and half Bengalis.[12] In the lab, there were four big rectangular tables with six chairs around, so each group was assigned to a table. By varying the ethnic composition of a group, our design involves three group treatments: *homogeneous*, *majority*, and *minority*. A *homogeneous* treatment was composed of participants from a single ethnicity, i.e. either they had six Santals or six Bengalis in the group. Mixed treatments comprised participants of both ethnic identities. They either had two Santals and four Bengalis (i.e. a group where Santals are a numerical *minority*) or four Santals and two Bengalis (i.e. a group where Santals are a numerical *majority*). Moreover, in mixed treatments, a minority member of that group was always seated next to or in front of their ethnic peer. For example, a Bengali in a *minority* treatment was always seated next to or in front of another Bengali.[13] Figure 2.2 depicts the arrangement of participants and groups in a session.

To sum up, in each session, there were two *homogeneous* treatment groups, one with all Santals and another with all Bengalis; and two mixed treatment groups, one with Santals as a numerical *minority* and another with Santals as a numerical *majority*.[14] These ethnic compositions were never discussed with or revealed to participants, and the ethnicity of participants was never made salient before or during the experiment. However, participants could see each other and hence could identify the ethnicity of their group members. Later, in the exit survey, we asked our subjects about ethnic identities and compositions of their groups to ensure correct identification.

[12]The only exception was one session where there were twelve participants in mixed treatments: a Santal-majority (where Santals are a numerical *majority*) and a Santal-minority (where Santals are a numerical *minority*).

[13]Only exception was in group 1 in the first session where the numerical minority members were not seated adjacently. This occurred due to a swap of two ID numbers in the ID bowls by mistake.

[14]Only in one session there were two mixed groups.

After having seated, participants were asked to remain silent and then the instructions were read aloud. All instructions were translated from English into the local common language, Bangla, and were also back-translated to evaluate the equivalence of meaning between both instructions.[15] After having read out the instructions, participants were asked to raise hands if they had any questions or doubts. To ensure participants had understood all compensation schemes perfectly, at the end of each instruction, a range of frequently asked questions (FAQs) and their answers were also read out to participants. For example, a FAQ in stage 1 instruction was "How much will you earn per lentil?" with the answer being "5 Takas". Please see the Experimental Instructions and Surveys at the end for all instructions and FAQs. They were advised to listen to them very carefully and were told to raise hands if they had any questions or doubts. Then, after answering any questions, the experimenter asked the participants to rub their hands twice and then gave the signal to start the task. After 60 seconds, participants had to stop performing the task and immediately put their hands up (the same as when someone is called to surrender).[16] Participants were informed at the start of the experiment that they would perform the task in three stages and one of these stages would be randomly chosen for payment. Additionally, participants received detailed instruction on each stage only prior to performing the task in that stage and were never given feedback on absolute or relative performance between stages. At the end, participants were told how well they had done in each stage, but they were not informed about their relative performance. The incentive structure of each stage is laid out below.

- *Stage 1: Piece-rate* Participants performed the task for 60 seconds and received a piece rate of 5 Takas for each separated lentil.[17]

- *Stage 2: Tournament* Participants performed the task for 60 seconds. Only the group member with the highest lentil count would receive payment, while others in the group would receive no payment. The winner received 30 Takas for each lentil separated by himself. In the case of ties, winning amounts were divided equally among winners.

- *Stage 3: Choice* Before performing the task, participants chose which of the two compensation schemes would be applied to their performance in this stage. If a participant were to choose piece-rate, then he would receive 5 Takas for each separated lentil. However, if a participant chose tournament, then he would receive

[15] All Santal participants were fluent in Bangla, so only Bangla instructions were used. We confirmed their fluency and their understanding of Bangla during the initial registration.

[16] The gesture of rubbing hands before performing the task ensured that no one was cheating by hiding lentils in their hands. Likewise, putting hands up after completing the task ensured all subjects took equal time. Also putting hands up diverted their attention towards performing the action and look at others who have done it, and away from checking other group members' scores right after completing the task, which gave our assistants enough time to move the bowls away from their sights.

[17] The Bangladeshi currency is called Taka (pl. Takas). USD 1 was equal to 80 Takas during the time of the experiment.

30 Takas for each lentil separated by himself only if his stage 3 score exceeded the stage 2 scores of his five group members.

Note that performance of those who opted to compete in stage 3 was evaluated against those who had already competed under the competitive compensation scheme in stage 2. Therefore, beliefs regarding choices of others in stage 3 would not affect someone's decision to enter into competition. Moreover, choice of entering into competition would not affect payments of other participants, which ruled out the possibility of imposing negative externalities on others by winning in stage 3 (Niederle & Vesterlund, 2007). In addition, it also ensured that consequences, such as fear of being punished by other group members outside the lab would not affect choice.

It should also be mentioned that all assistants were male Bengalis working as anthropology researchers (who were well respected among villagers) from a local public university in collaboration with a local NGO, which was also well respected and trusted for providing micro-loans, eradicating poverty and fighting for human rights for the needy. We can thus be confident that behavior of Santals would not be affected by fear of being discriminated by experimenters in terms of payment.

2.3.3 Confidence and Risk Preferences

The decision to enter into competition in stage 3 could be affected by individuals' beliefs about their relative performance in their group. So, to control for this we elicited these beliefs for performance in stage 2, the compulsory tournament stage, paying participants for correct reports.[18] Another important factor that might affect one's willingness to compete in stage 3 is attitudes towards risk. We elicited risk attitudes through a standard risk game (Gneezy & Potters, 1997; Gneezy et al., 2009), immediately after completing stage 3, i.e. after completing all three real effort tasks.[19]

[18]Immediately after stage 2, we randomly asked our participants to go to the registration desk, one by one, where we showed them a picture with six heads. Heads were arranged vertically where the topmost head represented the 'best' (or rank 1) and that at the bottom represented the 'worst' (or rank 6) performer in stage 2. We asked them about how they rank themselves based on their stage 2 performance. For example, if they thought they separated the highest (lowest) number of lentils then they should point to the head at the top (bottom). Participants only had to point to a head with their finger, and then return to their respective desks. They received 50 Takas if their guesses were correct and no money if incorrect. See the Experimental Instructions and Surveys at the end for the heads' arrangements. We decided to conduct the guessing game immediately after the compulsory tournament in order to obtain more accurate beliefs about their stage 2 performance.

[19]In this one-shot independent game, we asked our subjects to bet a proportion [0, 100] of their endowment of 100 units, or 20 Takas, into a lottery. The bet had a fifty-fifty chance of winning which was determined by a coin toss. Subjects received six times the amount invested if there was a head but lost the bet money if there was a tail. If the proportion of the bet was less than 100 percent, then subjects received the remaining un-bet amount, irrespective of the coin-toss outcome. Following Gneezy et al. (2009), stakes in the risk game overlap the stakes in the competitiveness game, wherein the initial endowment is equivalent to the payment for separating four lentils under the piece-rate scheme. Likewise, the maximum payoff is equivalent to the payment for separating the same amount of lentils under the tournament scheme. After the instruction was read aloud, subjects were asked to raise hands if they had any queries. Then, after clearing any confusions, the experimenter asked the subjects to go to the registration desk, one by one, in random order, where they were asked to state their risk choices. The coin toss was performed immediately after a bet was placed and the outcome of the toss was always

2.3.4 Exit Survey and Payment

After completing the risk game, participants were asked to go to an assistant to complete an exit survey. For each subject, we obtained data on their socioeconomic background and some other individual level data, namely marital status, the level of intercultural competence, land possessions, handedness and so on. In addition to money earned from the games, each subject received a show-up fee of 100 Takas. For 90 minutes of their time, our average subject earned about 1.5 times more (320 Takas) than their average daily income (207 Takas) and six times more than the daily national minimum wage (Minimum Wage Board Bangladesh, 2015). At the end of the experiment, participants were paid in cash, individually and in private.

2.3.5 Hypotheses

We formulate three hypotheses. The first hypothesis concerns performance in the first stage of the experiment. Since our study is concerned with ethnic differences in preferences for competing on a task, it is important that the chosen task is not associated with an ethnic stereotype attached to a specific group. Indeed, we selected a simple manual task that was expected to be novel and neutral to participants of both ethnicities and hence we do not expect (and pretests confirmed this) to see any ethnic differences in performance in the first stage of the experiment.

Hypothesis 1: There is no difference in performance across ethnic groups in the piece-rate stage.

Our second hypothesis concerns performance in the tournament stage. Here, we expect participants to perform differently in homogeneous and mixed treatments. This is because under a tournament scheme an individual's effort negatively affect others, so subjects are more likely to internalize the negative externality their effort imposes on a group of co-ethnics as opposed to that of non-co-ethnics (Bandiera et al., 2005).

Hypothesis 2: In the tournament stage, performance would be lower in homogeneous treatment than in mixed treatments.

Our last hypothesis, concerns behavior in the third stage. Given the power structure that connects the two ethnic groups, we expect the ethnic minority Santals to respond differently to the ethnic composition of potential competitors than the ethnic majority Bengalis.

confirmed by the subject. See the Experimental Instructions and Surveys at the end for the Risk Game instructions. Registration desks were located outside the lab room (though on few occasions it was inside when the room was large enough), so other subjects could not see or hear any risky choices and outcomes that were made at the desk. Hence, choices for risky bets were individual decisions that did not affect decisions or payoffs of others, which is analogous to the mechanism of making choices in stage 3.

Hypothesis 3: (i) Santals would be less willing to compete in mixed treatments; (ii) Bengalis would be more willing to compete in mixed treatments.

This hypothesis is consistent with the evidence from the literature on gender differences in competitive preferences, which has found that the gender of a competitor significantly affects one's inclination towards competition (Booth & Nolen, 2012; Datta Gupta et al., 2013; Geraldes, 2016; Burow et al., 2017).

2.4 Results

2.4.1 Participant Summary Statistics

Table 2.1 presents summary statistics of the participants' characteristics that we collected through the exit survey by ethnic group. The average age of our participants is around 36 years, the average education attainment is in the range of 5-6 years, and around 80 percent of the participants were married at least once. None of these characteristics is significantly different across the two ethnic groups. However, the average daily income and land possessions of Bengalis are significantly higher than those of Santals. It means that, as expected, Santals are rather poorer and from a lower social class, as land holdings are good indicators of one's social status in a village (Rao, 2001). Another good indicator of socioeconomic status is one's family background. Santal parents are significantly less educated and their fathers' earn less than Bengali fathers, while the opposite is true of their mothers.[20] Note, however, that these comparisons rely on information on parental income reported by only about half of participants.

With regards to the occupation of participants, around 60 percent of Bengalis and 80 percent of Santals engage in farming; this difference is statistically significant according to a Pearson's Chi-Squared test (CS-test hereinafter) ($p < 0.01$). The rest are either students, owners of small businesses or working in other non-farming areas such as weaving baskets, making bamboo furniture and so on. In terms of income, farmers earn significantly less than non-farmers according to a two sided Mann-Whitney U test (MW-test hereinafter) ($p < 0.01$).[21]

Almost all participants were able to correctly identify the ethnicity of members in their group and hence the ethnic composition of their group,[22] while around 50 percent of our subjects knew all five names of their group members and around 80 percent knew at least four of them. There is no significant difference in these measures across ethnic groups (MW-test $p = 0.57$ and $p = 0.70$ respectively). We also asked participants some questions to assess their knowledge of the other ethnic group's culture. We call

[20] This difference may be explained by the fact that Bengali women (who are Muslims) are mostly homemakers and hence might engage in paid-work less than Santal women.

[21] It should be noted that, since students have no income and are better educated than individuals who are working, income and education have a negative relationship in our sample. In addition, the majority of farmers work as day laborers for a fixed daily wage, which is independent of educational attainment.

[22] Only three participants could not identify the ethnicity of at least one of their group members. Excluding these three participants from our analysis does not affect the main results of the paper.

TABLE 2.1: Participant Characteristics and Beliefs

Individual Characteristics & Beliefs	**Bengali Mean** (*Std. Dev.*)	**Santal Mean** (*Std. Dev.*)	**MW-test** ***p*-values**	**T-test** ***p*-values**	**N**
Age	34.59	37.04	0.106	0.167	252
	(*13.95*)	(*14.15*)			
Education	5.58	5.37	0.626	0.711	252
	(*4.57*)	(*4.58*)			
Profession	0.58	0.77	0.001	0.001	252
	(*0.50*)	(*0.42*)			
Hourly Income	38.81	29.99	0.059	0.007	250
	(*33.41*)	(*12.94*)			
Land Possession	17.89	12.07	0.049	0.209	252
	(*39.65*)	(*33.40*)			
Marriage	0.79	0.83	0.337	0.338	252
	(*0.41*)	(*0.37*)			
Mother's Education	2.43	0.56	0.000	0.000	211
	(*2.96*)	(*1.75*)			
Father's Education	2.51	1.54	0.021	0.033	201
	(*3.33*)	(*3.02*)			
Mother's Income	583.3	1,551	0.000	0.002	138
	(*1,571*)	(*1,908*)			
Father's Income	6,578	4,434	0.000	0.000	111
	(*3,692*)	(*2,205*)			
Mother's Profession	0.17	0.53	0.000	0.000	211
	(*0.38*)	(*0.50*)			
Father's Profession	0.89	0.93	0.320	0.315	188
	(*0.31*)	(*0.25*)			
Parents' Income	7,162	6,330	0.386	0.243	104
	(*3,830*)	(*3,358*)			
Parents' Education	5.01	2.12	0.000	0.000	200
	(*5.54*)	(*4.39*)			
IC Competence	0.48	0.88	0.000	0.000	252
	(*0.27*)	(*0.21*)			
Know Other Participants' Name	4.31	4.34	0.695	0.824	240
	(*0.86*)	(*0.87*)			
Met Group Members Before	0.93	0.86	0.067	0.067	252
	(*0.26*)	(*0.35*)			
Identify Ethnicity of Other Group Members	0.98	0.99	0.562	0.563	252
	(*0.13*)	(*0.09*)			

Note: All educations and age are in years; all professions are dummy variables where 1 equals farming and 0 otherwise; apart from Hourly Income, all other incomes are monthly (in Bangladeshi Taka); Land Possession is the amount of land owned in 'katha', where 1 katha = 720 square feet; Marriage is a dummy variable where 1 equals married (at least once) and 0 otherwise; Parents' Education and Income are accumulated education and incomes of mothers and fathers; IC Competence is the level of inter-cultural competence among Santals and Bengalis, where 0 means no knowledge about the other ethnic group, 0.25 means some knowledge, 0.5 means good knowledge, 0.75 means very good knowledge and 1 means excellent knowledge; Know Other Participant's Name shows the number of group members' names a participant knew, where 5 implies knowing everyone's name and 0 means not knowing anyone's name; Met Group Members Before is a dummy variable that shows the proportion of individuals who have met/known their group members from before; Identifying Ethnicity of Other Group Members is a variable that shows the proportion of individuals who could identify all their group members' ethnic identities correctly; MW-test is a two sided Mann-Whitney U test and T-test is a two-sample t-test with unequal variances.

this the level of intercultural competence (Fantini, 2010).[23] There is a significant difference in intercultural competence across ethnic groups (MW-test $p < 0.01$), with the Santals displaying significantly better knowledge about their non-co-ethnic peers. All

[23]Our questions are a very simplified version of Fantini's intercultural competence assessment questions, focusing only on the 'awareness dimension' of individuals. During the exit survey, we asked four simple questions regarding the opposite culture and produced a score from 0 to 1 for each participant, by assigning 0.25 to each correct answer.

background characteristics, and in particular the ones that differ across ethnicity that we have underlined here, will be directly controlled for in our regression analysis.

We next turn to experimentally elicited characteristics. In terms of beliefs regarding relative performance in the tournament stage, we compute the perceived probability of winning the tournament in stage 2 following Sutter, Glätzle-Rützler, Balafoutas & Czermak (2016). We construct a dummy variable which equals 1 if a subject has reported a perceived rank of either 1 or 2 for his own performance in stage 2 and 0 otherwise. One's perceived probability of winning in the compulsory tournament can be a proxy for the probability of winning in the discretionary tournament in stage 3, assuming no expected increase in ability from stage 2 to 3. There are no significant differences across group compositions and ethnic groups in this measure (CS-test $p > 0.10$ for all). See Table A1 in Appendix A for the summary and test results. Summary of guessed ranks is available in Table A2 in Appendix A.

Finally, Table 2.2 summarizes elicited risk attitudes by ethnic group.[24] The two ethnic groups demonstrate almost identical attitude towards risk (CS-test $p = 0.543$).[25] We also control for these elicited attributes in our regression analysis.

2.4.2 Performance in Stages 1 and 2

Summary statistics of performance in stages 1 and 2 and tests of the equality of means and distributions across ethnic groups and treatments are presented in Table 2.3. Overall, when comparing performance across ethnic groups, there are no differences in either stage. The same is true when we make interethnic comparisons of performance for each treatment separately (Panel A of Table 2.3), with the exception of the *majority* treatment in stage 1, in which Bengali subjects perform better than Santal subjects, though the difference is marginally significant (MW-test: $p = 0.054$). Thus, we confirm that there are no significant differences in ability to perform the task across the two ethnic groups.

We next examine whether there are across treatment differences in performance in stage 1; see Graph A in Figure 2.3 for a visualization of these differences for each ethnic group separately. What emerges is a similar pattern for both ethnic groups: performance is higher in the mixed (*majority* and *minority*) treatments than the *homogeneous* treatment, though the differences are statistically significant only for Bengalis (tests are

[24] We elicited risk attitudes through a standard risk game (Gneezy & Potters, 1997), immediately after completing stage 3, i.e. after completing all three real effort tasks.

[25] Since the Risk Game was conducted as a separate 'bonus' game which took place immediately after the main game ended, participants knew they were not part of their group any longer prior to making risky investment decisions. Also, participants made their risky decisions individually and away from their group table, so group composition should not have affected their behavior. However, test results suggest otherwise. Between ethnic groups tests show that Santals, who were in a *homogeneous* treatment, invested significantly more than Bengalis (CS-test $p = 0.011$); investments in other groups, however, were not significantly different between ethnicity (CS-test $p > 0.10$ for all other groups). Likewise, within ethnic groups tests show that Santals from the *homogeneous* treatment invested significantly more than their co-ethnics in *majority* and *minority* treatment groups. Although among Bengali subjects, all investments were equal (CS-test $p > 0.10$ for all).

TABLE 2.2: Summary of Risky Investment

Panel A: Compares Risky Investment Between Ethnic Groups

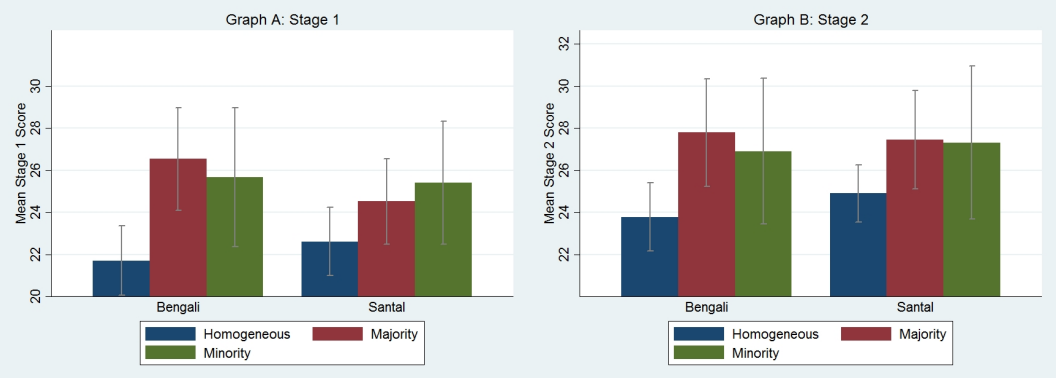
	Bengali (*Std. Dev.*)	N_B	**Santal** (*Std. Dev.*)	N_S	**MW-test** ***p*-values**	**CS-test** ***p*-values**
Proportion Invested	**0.85** (***0.23***)	**126**	**0.88** (***0.22***)	**126**	**0.219**	**0.543**
Homogeneous	0.81 (*0.26*)	60	0.93 (*0.20*)	60	0.002	0.011
Majority	0.91 (*0.20*)	44	0.83 (*0.24*)	44	0.095	0.402
Minority	0.85 (*0.23*)	22	0.85 (*0.24*)	22	0.823	0.597
Mixed	0.89 (*0.21*)	66	0.84 (*0.24*)	66	0.231	0.631

Panel B: Compares Risky Investment Within Ethnic Groups

	Group Treatment		**Group Treatment**	**MW-test** ***p*-values**	**CS-test** ***p*-values**
Bengali	Homogeneous	*vs*	Majority	0.052	0.136
	Homogeneous	*vs*	Minority	0.749	0.420
	Majority	*vs*	Minority	0.180	0.253
	Homogeneous	*vs*	Mixed	0.114	0.280
Santal	Homogeneous	*vs*	Majority	0.006	0.025
	Homogeneous	*vs*	Minority	0.047	0.073
	Majority	*vs*	Minority	0.729	0.879
	Homogeneous	*vs*	Mixed	0.005	0.022

Note: 'Proportion Invested' is the proportion of endowment invested in the risky lottery; Mixed group combines risky investments of both *Minority* and *Majority* group treatments; N_B is the sample of Bengali and N_S is the sample of Santal.

FIGURE 2.3: Performance: Comparing Performances Within Ethnic Groups



Note: Each bar represents the mean score of participants in Stages 1 and 2, where the whiskers indicate a 95 percent confidence interval.

reported in Panel B of Table 2.3). The fact that in mixed groups performance is higher when compensation is not competitive is surprising.

Regarding stage 2 performance, we find a similar pattern as in stage 1 (see Figure

2.3; Graph B). For both ethnic groups, performance in *majority* and *minority* treatments is higher compared to the *homogeneous* treatment. Furthermore, the difference in performance between *homogeneous* and mixed treatments is statistically significant for both ethnic groups (though only marginal for Santals), when we pool scores from *majority* and *minority* treatments together (MW-test: $p = 0.071$ for Santals and $p < 0.01$ for Bengalis).[26] This finding is expected in this stage with a competitive incentive structure in place, as participants may be more likely to internalize the negative externality imposed on their co-ethnic as opposed to members of the other ethnicity.

Comparing performance across stages 1 and 2, we see a significant performance improvement from stage 1 to stage 2 for both ethnic groups (Wilcoxon signed-rank test (SR-test): $p < 0.01$ for both). Among Bengalis, this improvement is entirely driven by members of the *homogeneous* group, however, this is not the case among Santals. See Table A3 in Appendix A for the summary and test results. The increase in performance moving from piece-rate to competitive incentive is consistent with existing studies on performance and competitiveness, which show that participants, on average, perform significantly better in tournaments as compared to piece-rate schemes (Gneezy et al., 2003; Niederle & Vesterlund, 2007; Buser et al., 2014).

In summary, we find no significant differences, overall or by treatment, in performance across the two ethnic groups, which is consistent with the first hypothesis. At the same time, we do see that for both groups performance is higher in ethnically-mixed as compared to *homogeneous* treatments, which provides support for the second hypothesis.

2.4.3 Competitive Choice

Overall, we do not find any differences in competitive choices across ethnic groups in stage 3. As shown in Table 2.4, an equal fraction of Santals and Bengalis (37 percent) opted to compete in the choice stage (CS-test: $p = 0.896$). However, when we look across treatments, we see some marked differences between the two ethnic groups. In particular, although marginal, in *homogeneous* treatment more Santals opted to compete (CS-test: $p = 0.087$), whereas in *minority* treatment more Bengalis opted to compete (CS-test: $p = 0.026$). Considering all treatment groups, there is a completely reverse pattern in competitive choices made by the two ethnic groups, which is evident in Figure 2.4. We see that Santals choose to compete the most in *homogeneous* treatment and the least in *minority* treatment, whereas this relationship is reversed for Bengalis, who

[26] There is no statistically significant difference in performance across the *majority* and the *minority* treatments (MW-test: $p > 0.10$ for both ethnic groups).

TABLE 2.3: Summary of Performance in Stages 1 and 2

Panel A: Compares Performance Between Ethnic Groups

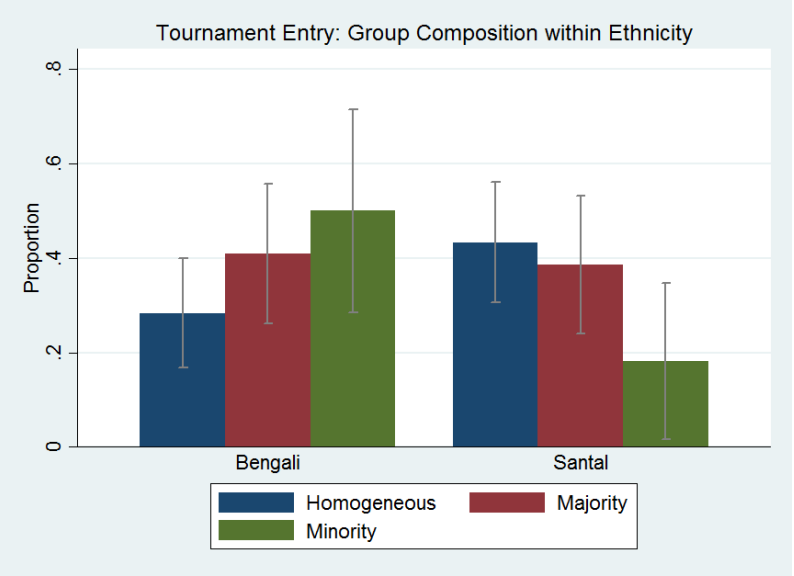
	Mean Score of Bengali & Santal (*Std. Dev.*)	**Mean Score of Bengali** (*Std. Dev.*)	**Mean Score of Santal** (*Std. Dev.*)	**MW-test *p*-values**	**T-test *p*-values**		**Mean Score of Bengali & Santal** (*Std. Dev.*)	**Mean Score of Bengali** (*Std. Dev.*)	**Mean Score of Santal** (*Std. Dev.*)	**MW-test *p*-values**	**T-test *p*-values**	**N**
Stage 1	**23.93** (***7.19***)	**24.10** (***7.67***)	**23.77** (***6.69***)	**0.566**	**0.720**	**Stage 2**	**25.97** (***7.36***)	**25.73** (***7.71***)	**26.21** (**7**)	**0.904**	**0.602**	**252**
Homogeneous	22.17 (*6.43*)	21.72 (*6.51*)	22.62 (*6.37*)	0.242	0.446	Homogeneous	24.34 (*5.88*)	23.78 (*6.35*)	24.90 (*5.38*)	0.383	0.301	120
Majority	25.53 (*7.60*)	26.55 (*8.22*)	24.52 (*6.87*)	0.054	0.214	Majority	27.63 (*8.20*)	27.80 (*8.61*)	27.45 (*7.86*)	0.871	0.847	88
Minority	25.55 (*7.32*)	25.68 (*7.85*)	25.41 (*6.93*)	0.707	0.903	Minority	27.11 (*8.34*)	26.91 (*8.23*)	27.32 (*8.64*)	0.526	0.873	44
Mixed	25.54 (*7.48*)	26.26 (*8.05*)	24.82 (*6.85*)	0.057	0.271	Mixed	27.45 (*8.22*)	27.5 (*8.44*)	27.41 (*8.07*)	0.662	0.950	132

Panel B: Compares Performance Within Ethnic Groups

	Group Treatment		**Group Treatment**	**MW-test *p*-values**	**T-test *p*-values**		**Group Treatment**		**Group Treatment**	**MW-test *p*-values**	**T-test *p*-values**
Stage 1						**Stage 2**					
Bengali	Homogeneous	*vs*	Majority	0.000	0.002	**Bengali**	Homogeneous	*vs*	Majority	0.006	0.011
	Homogeneous	*vs*	Minority	0.005	0.042		Homogeneous	*vs*	Minority	0.024	0.117
	Majority	*vs*	Minority	0.605	0.680		Majority	*vs*	Minority	0.989	0.687
	Homogeneous	*vs*	Mixed	0.000	0.001		Homogeneous	*vs*	Mixed	0.002	0.006
Santal	Homogeneous	*vs*	Majority	0.363	0.153	**Santal**	Homogeneous	*vs*	Majority	0.051	0.067
	Homogeneous	*vs*	Minority	0.163	0.108		Homogeneous	*vs*	Minority	0.444	0.230
	Majority	*vs*	Minority	0.629	0.626		Majority	*vs*	Minority	0.658	0.951
	Homogeneous	*vs*	Mixed	0.185	0.064		Homogeneous	*vs*	Mixed	0.071	0.041

Note: Mixed group combines scores of both Minority and Majority group treatments; N is the total sample size, wherein ethnic groups are in equal proportions in each group composition; MW-test is the two sided Mann-Whitney U test; T-test is the unpaired t-test with unequal variances; test p-values are comparing performance differences between ethnic groups in Panel A and within ethnic groups in Panel B.

FIGURE 2.4: Competitive Choice: Comparing Choices Within Ethnic Groups



Note: Each bar represents the proportion of participants who opted to compete in stage 3, where the whiskers indicate a 95 percent confidence interval. *Homogeneous* is a group composition where all individuals are co-ethnics; *Majority* is a group composition where either Santals or Bengalis are numerical majorities; *Minority* is a group composition where either Santals or Bengalis are numerical minorities.

opted to compete the most in *minority* treatment and the least in *homogeneous* treatment.[27] While half of Bengalis chose to compete in a *minority* treatment, only 18 percent of Santals opted to compete in that same treatment, and for both ethnic groups, these differences are statistically significant (though only marginal for Bengalis) when compared against choices made in *homogeneous* treatment (CS-test: $p = 0.067$ for Bengalis and $p = 0.036$ for Santals), which is consistent with our final hypothesis.

This implies that as groups get more populated by Santals, Bengalis show higher tendency to compete; on the other hand, as the number of Bengalis increases in a group, Santals are less likely to compete. It is noteworthy that for the Bengalis the pattern of entry across treatment we see in Figure 2.4 is consistent with performance in stage 2, namely, there is more entry in the mixed treatment that performed better in stage 2 than the *homogeneous* treatment. For the Santals, however, it is not, as we see more entry in the *homogeneous* treatment that performed the worst in stage 2.

To probe further what drives the patterns underscored above, it is instructive to examine whether there are differences between ethnic group within treatment or within ethnic group between treatment in the optimality of the decisions made in this stage, both for entrants and non-entrants. Following Buser et al. (2014), for each subject we compute the probability of winning the tournament conditional on stage 2 performance, by randomly drawing 1,000 different comparison groups from a participant's own group composition type, i.e. homogeneous, Santal-majority, and Santal-minority. Using the winning probabilities, we then compute the expected payoff of each participant in a

[27] Tests of treatment differences for each ethnic group are presented in Panel B of Table 2.4.

TABLE 2.4: Summary of Competitive Choice

Panel A: Compares Competitive Choice Between Ethnic Groups

	Proportion of Bengali (*Std. Dev.*)	N_B	**Proportion of Santal** (*Std. Dev.*)	N_S	**MW-test** ***p*-values**	**CS-test** ***p*-values**
Compete	**0.37** (***0.48***)	**126**	**0.37** (***0.49***)	**126**	**0.896**	**0.896**
Homogeneous	0.28 (*0.45*)	60	0.43 (*0.50*)	60	0.088	0.087
Majority	0.41 (*0.50*)	44	0.39 (*0.49*)	44	0.829	0.828
Minority	0.50 (*0.51*)	22	0.18 (*0.39*)	22	0.028	0.026
Mixed	0.44 (*0.50*)	66	0.32 (*0.47*)	66	0.153	0.151

Panel B: Compares Competitive Choice Within Ethnic Groups

	Group Treatment		**Group Treatment**	**MW-test** ***p*-values**	**CS-test** ***p*-values**
Bengali	Homogeneous	*vs*	Majority	0.182	0.180
	Homogeneous	*vs*	Minority	0.069	0.067
	Majority	*vs*	Minority	0.486	0.483
	Homogeneous	*vs*	Mixed	0.070	0.069
Santal	Homogeneous	*vs*	Majority	0.633	0.631
	Homogeneous	*vs*	Minority	0.037	0.036
	Majority	*vs*	Minority	0.095	0.093
	Homogeneous	*vs*	Mixed	0.184	0.182

Note: 'Compete' is a dummy variable and is equal to 1 if the individual opted to compete and 0 otherwise; Mixed group pools both Minority and Majority groups together; N_B is the Bengali sample; N_S is the Santal sample; test p-values are comparing choices horizontally.

tournament, conditional on their stage 2 performance, to assess the optimality of their tournament entry decisions.[28] Summary statistics and test results are presented in Table 2.5.

We first examine those who under-entered the tournament.[29] In Panel A, we see that there are no significant differences in under-entry between ethnic groups in any group treatments (CS-test: $p > 0.10$ for all group treatments). Likewise, within ethnic group tests in Panel B also suggest that the patterns of competitive entry underlined in

[28] Entering competition is optimal when the expected payoff under the tournament is higher than the expected payoff under the piece-rate given the stage 2 performance. As entering competition and winning results in six times the piece-rate payment (x) for our subjects, choosing competition is optimal for a risk neutral subject if $6x \times Pr(Win) > x$, or $6 \times Pr(Win) > 1$.

[29] Following Andersen et al. (2013), we say that under-entry occurs when it is optimal for subjects to enter competition but they do not enter. Similarly, over-entry occurs when it is not optimal for subjects to enter competition but they enter anyway. Finally, correct entry occurs when it is the optimal decision for subjects to enter/not enter the competition.

the previous subsection cannot be explained by across treatment differences in under-entry. Now turning our attention to those who over-entered the tournament, it turns out that Bengalis over-entered significantly more than Santals in the *minority* treatment (Panel A; CS-test: $p = 0.009$). Moreover, within ethnicity across treatment tests suggest that Santals over-entered significantly more in *homogeneous* and *majority* treatments than in the *minority* treatment (Panel B; CS-test: $p = 0.029$ for both *homogeneous* vs. *minority* and *majority* vs. *minority* treatments). For Bengalis, we do not find any significant differences in over-entry across treatments.

Overall, this optimal-entry analysis indicates that for Santals the difference in competitive choice between the *homogeneous* and the *minority* treatment, seen in Table 2.4, is driven by inefficient over-entry in the *homogeneous* treatment. Furthermore, in the *minority* treatment, the ethnic difference in stage 3 choices is driven by inefficient over-entry of the Bengalis.

2.4.4 Regression Analysis of Compensation Choice in Stage 3

The foregoing analysis provides some insight on how group compositions influence inclinations towards competition. In this subsection, we further scrutinize the choice made in stage 3 in a regression framework that allows us to control for other factors that might affect a subject's tournament entry decision such as previous performance, risk preference, perceived probability of winning a tournament and various socioeconomic characteristics (age, income, education and so on). Firstly, to examine whether the two ethnic groups differ in terms of the across treatments differences in willingness to compete, we estimate the following equation:

$$Pr(compete_i) = \Phi(\alpha + \beta_1 Santal_i + \beta_2 Minority_i + \beta_3 Majority_i + \beta_4 Santal_i \times Minority_i + \beta_5 Santal_i \times Majority_i + S'\gamma + \lambda p_i + \sigma r_i + X'\theta) \tag{2.1}$$

The dependent variable *Compete* is equal to 1 if a participant chooses to compete in stage 3 and 0 otherwise. *Santal* is an indicator variable for the ethnic minority. $Minority$ and $Majority$ are indicator variables for *minority* and *majority* treatments, respectively. S is a vector of previous performances. p and r are perceived probability of winning and risk respectively, which are described in section 3.4.2 and summaries are given in Table A1 in Appendix A and Table 2.2. X is a vector of all other controls, which include age, hourly income, education, land possession, profession, the level of intercultural competence, having met other participants and the village of the participant. $\Phi(\cdot)$ is the cumulative distribution function of the standard normal distribution. Standard errors are clustered at the group level (42 groups in total).[30] Columns 1-6 in Table 2.6 contain

[30] Clustering standard errors at the sessions (11 sessions) and at the village of participants (14 villages) level yields very similar results.

TABLE 2.5: Optimality of Choice in Stage 3

Panel A: Compares Optimality of Choice Between Ethnic Groups

	Pooled		Homogeneous		Majority		Minority	
Ethnicity	**Over(%)**	**Under(%)**	**Over(%)**	**Under(%)**	**Over(%)**	**Under(%)**	**Over(%)**	**Under(%)**
Santal	23.02	17.46	26.67	16.67	27.27	20.45	4.55	13.64
	[29]	[22]	[16]	[10]	[12]	[09]	[01]	[03]
Bengali	24.60	14.29	20.00	18.33	25.00	13.64	36.36	4.55
	[31]	[18]	[12]	[11]	[11]	[06]	[08]	[01]
Pooled	23.81	15.87	23.33	17.50	26.14	17.05	20.45	9.09
	[60]	[40]	[28]	[21]	[23]	[15]	[09]	[04]
Santal vs Bengali								
CS-test (p-values)	0.767	0.490	0.388	0.810	0.808	0.395	0.009	0.294
MW-test (p-values)	0.768	0.491	0.340	0.811	0.809	0.398	0.010	0.230

Panel B: Compares Optimality of Choice Within Ethnic Groups

	Group Treatment		**Group Treatment**	**MW-test p-values (Over vs. Over)**	**MW-test p-values (Under vs. Under)**	**CS-test p-values (Over vs. Over)**	**CS-test p-values (Under vs. Under)**
Santal	Homogeneous	*vs*	Majority	0.945	0.623	0.945	0.621
	Homogeneous	*vs*	Minority	0.030	0.741	0.029	0.739
	Majority	*vs*	Minority	0.030	0.502	0.029	0.498
Bengali	Homogeneous	*vs*	Majority	0.546	0.524	0.544	0.522
	Homogeneous	*vs*	Minority	0.129	0.120	0.126	0.118
	Majority	*vs*	Minority	0.340	0.262	0.336	0.258

Note: Here 'Over' ('Under') corresponds to over (under) entry by subjects. It is over entry when it is not optimal for subjects to enter competition but they enter anyway. Similarly, it is under entry when it is optimal for subjects to enter competition but they do not enter. Observations under each entry type is given in brackets.

TABLE 2.6: Treatment Effects on Willingness to Compete: Ethnic Group Differences

VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Santal	0.146	0.127	0.131	0.089	-0.008	0.005
	(0.099)	(0.107)	(0.107)	(0.104)	(0.117)	(0.109)
Minority	0.212	0.150	0.163	0.149	0.084	0.038
	(0.141)	(0.144)	(0.143)	(0.140)	(0.149)	(0.162)
Majority	0.127	0.047	0.066	0.026	0.006	-0.022
	(0.116)	(0.127)	(0.127)	(0.122)	(0.121)	(0.142)
Santal×Minority	-0.343***	-0.338***	-0.337***	-0.318***	-0.308***	-0.309***
	(0.081)	(0.079)	(0.079)	(0.088)	(0.089)	(0.071)
Santal×Majority	-0.157	-0.125	-0.132	-0.060	-0.088	-0.145
	(0.140)	(0.150)	(0.149)	(0.158)	(0.152)	(0.124)
Stage 1 Score	-	0.003	0.002	0.002	0.003	0.002
		(0.007)	(0.007)	(0.007)	(0.006)	(0.006)
Stage 2 Score	-	0.014**	0.013*	0.012*	0.012*	0.009
		(0.006)	(0.007)	(0.007)	(0.007)	(0.007)
Perceived Probability of Winning	-	-	0.075	0.053	0.064	0.049
			(0.063)	(0.064)	(0.067)	(0.063)
Risk	-	-	-	0.004***	0.004***	0.003**
				(0.001)	(0.001)	(0.001)
IC Competence	-	-	-	-	0.286*	0.290*
					(0.148)	(0.149)
Hourly Income	-	-	-	-	0.003**	0.003**
					(0.001)	(0.001)
Profession	-	-	-	-	-0.004	0.010
					(0.070)	(0.069)
Land Possession	-	-	-	-	-0.001	-0.001
					(0.001)	(0.001)
Other Controls	No	No	No	No	No	Yes
Village Fixed Effects	No	No	No	No	No	Yes
Observations	252	252	252	252	250	250
Pseudo R^2	0.026	0.073	0.077	0.103	0.128	0.213

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

Note: Probit regressions with average marginal effects reported; dependent variable is "compete", which equals 1 when the individual opted to compete and 0 otherwise; two observations were dropped in Column 5 and 6 because a Santal and a Bengali in a *homogeneous* treatment did not provide their income level; 'Other Controls' include age, education and having met other group members before. There are 42 groups in total. Table 2.1 describes all control variables.

estimated average marginal effects from a probit regression. All results are robust to using logit and linear probability models.

Column 1 only includes the main effects of interest, and we incrementally add controls as we proceed from Column 2-6 to see whether each subsequently affects our main results. The rightmost column has the full set of controls as laid out in our probit regression equation 2.1. What we see is that without any controls, the $Santal \times Minority$ interaction is statistically significant and negative, suggesting that the Santals are significantly less likely to enter competition in the *minority* treatment relative to the *homogeneous* treatment than the Bengalis (a difference-in-difference of almost 34 percentage points), while Santals in the *minority* treatment are 20 percentage points less likely to enter competition than the Bengalis in the same treatment. As we add to the specification previous performances, perceived probability of winning a tournament and attitudes

towards risk in Columns 2-4, respectively, we observe a small reduction in the size of the marginal effect which remains though large and negative.[31] When we further add background characteristics such as hourly income, profession, land possession and knowledge regarding other culture as controls (Column 5) as well as other controls (Column 6), our main marginal effects of interest remain robustly negative and sizeable.[32] With a full set of controls, in Column (6), the difference in competitive inclination between Santals in a *minority* treatment and a *homogeneous* treatment is 30.9 percentage points less than the difference in competitive inclination between Bengali in a *minority* treatment and a *homogeneous* treatment. Note that this difference-in-difference is fully accounted by the across ethnic group difference in the *minority* treatment as the across ethnic group difference in the *homogeneous* treatment has been reduced almost to zero. Hence, with regard to ethnic differences, our regression results are consistent with our third hypothesis.

Next, to obtain a clear picture of the size of the group composition effects, we estimate another probit regression model for each ethnic group separately. Table 2.7 shows our estimated results: Columns S1-S3 for Santals and Columns B1-B3 for Bengalis. As expected, we observe heterogeneous effects of group composition across the two ethnic groups. Results for the Santal subsample, presented in Columns S1-S3, suggest that Santals in both *minority* and *majority* treatments are less likely to enter competition than if they are in the *homogeneous* treatment. The size of the estimated differences increase in size and statistical significance as we add controls in Columns S2 and S3, such that, with a full set of controls, we find that Santals in the *minority* treatment are 41.3 percentage points less likely to compete than Santals in the *homogeneous* treatment. Likewise, Santals in the *majority* treatment are 29.9 percentage points less likely to compete than Santals in *homogeneous* treatment. Of the rest of the controls, intercultural knowledge seems to be the only one to have substantial explanatory power over competitive entry. Note that the coefficients in Table 2.6 differ from that in Table 2.7 because, in split sample models, all coefficients are allowed to differ between the two groups (i.e. Santal and Bengali subsamples). However, in interaction models, only the coefficients of the interactions are allowed to differ. Therefore, the two approaches are not equivalent, hence effects differ.

Results for the Bengali subsample, presented in Columns B1-B3, imply a reverse relationship as depicted in Figure 2.4. Bengalis are more likely to compete in *minority* and *majority* treatments than their co-ethnics in the *homogeneous* treatment; however,

[31] Our results are robust to using guessed rank (see Table A2 in Appendix A for summary statistics) or the difference between actual and guessed rank (a measure of overconfidence used in Niederle & Vesterlund (2007) and other subsequent studies) as a measure of beliefs about relative ability instead of the perceived probability of winning. Also, controlling for the average score of adjacent peers (those seated next to, in front of and in the adjacent corner of a subject a well as all peers in the group) in stage 2 does not affect the results.

[32] When we also add stage 3 performance as a control, with a full set of controls, it has no significant effect on choosing to compete ($p = 0.895$) and leaves the average marginal effects of *minority* treatments and all other effects unchanged. This suggests, the anticipation of performing better/worse did not affect tournament entry decisions. Also, instead of village dummies, using percentage of Santal population (or household) for each village of participants yields identical results.

TABLE 2.7: Within Ethnic Group Treatment Effects on Willingness to Compete

	Santal			**Bengali**		
VARIABLES	**(S1)**	**(S2)**	**(S3)**	**(B1)**	**(B2)**	**(B3)**
Santal-Majority	-0.047	-0.159	-0.299***	-	-	-
	(0.142)	(0.124)	(0.105)			
Santal-Minority	-0.252**	-0.339***	-0.413***	-	-	-
	(0.112)	(0.086)	(0.093)			
Bengali-Majority	-	-	-	0.126	-0.011	0.006
				(0.131)	(0.125)	(0.180)
Bengali-Minority	-	-	-	0.217	0.119	0.108
				(0.158)	(0.173)	(0.223)
Stage 1 Score	-	-0.004	-0.003	-	0.010	0.011
		(0.013)	(0.015)		(0.009)	(0.010)
Stage 2 Score	-	0.016**	0.012	-	0.004	0.002
		(0.008)	(0.009)		(0.011)	(0.012)
Perceived Probability of Winning	-	0.105	0.121	-	-0.008	-0.005
		(0.099)	(0.080)		(0.092)	(0.096)
Risk	-	0.002	0.002	-	0.006***	0.006***
		(0.002)	(0.001)		(0.002)	(0.002)
IC Competence	-	0.632***	0.647***	-	0.075	0.081
		(0.235)	(0.156)		(0.192)	(0.184)
Hourly Income	-	-	0.001	-	-	0.002**
			(0.004)			(0.001)
Profession	-	-	-0.005	-	-	0.034
			(0.044)			(0.068)
Land Possession	-	-	-0.001	-	-	-0.001
			(0.001)			(0.001)
Other Controls	No	No	Yes	No	No	Yes
Village Fixed Effects	No	No	Yes	No	No	Yes
Observations	126	126	125	126	126	123
Pseudo R^2	0.029	0.139	0.314	0.023	0.141	0.190

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

Note: Probit regressions with average marginal effects reported; dependent variable is "compete", which equals 1 when the individual opted to compete and 0 otherwise; the omitted group for Santals (specifications S1-S3) is the Santal *homogeneous*, and that of Bengalis (specifications B1-B3) is the Bengali *homogeneous*; two observations were dropped in Columns S3 and B3 because a Santal and a Bengali in a *homogeneous* group refused to provide their income level. Also two villages were dropped in the Bengali data due to having single observations in each village; 'Other Controls' include age, education and having met other group members before. There are 42 groups in total. Table 2.1 describes all control variables.

the difference is not statistically significant, and it reduces in size as we add controls in Columns B2-B3. Further, it seems, for Bengalis, income and risk preferences explain almost all their tournament entry decisions. Other factors, including overconfidence and intercultural knowledge factors, fail to explain tournament entry decisions by the ethnic majority members.

2.4.5 Understanding the Mechanism

As highlighted in Section 2.2, the two ethnic groups that we study differ markedly in terms of their socio-economic situation. This is also true in our subject pool: Bengali participants are wealthier (in terms of land ownership) and have higher earnings than their Santal counterparts (see Table 2.1). In this subsection, we examine to what extent the ethnic differences in preferences for interethnic competition that we find are mediated through ethnic differences in income or social status. To this end, we exploit the fact that we have measures of participants' income and land ownership, which we believe, given the degree of familiarity among the participants, can also be estimated by one's opponents in the group. We do not, however, have individual measures of social status and power, though, it is quite plausible that income/wealth and social status are correlated in this context. Therefore, if controlling for material resources, such as income and land ownership, of the group of competitors eliminates the main treatment effects that we find above, then this would provide support for the notion that preferences for interethnic competition amongst our subjects are driven mainly by ethnic differences in material well-being (and social status to the extent that they are correlated). However, if there remains a significant treatment effect, then this would indicate that it is mainly differences in unmeasured dimensions, e.g. social power and status, marked by ethnic identity, that are important for understanding the different attitudes toward interethnic competition.

Table 2.8 contains regression estimates of specification 2.1, in which in addition to the full set of controls used in column 6 of Table 2.6 we are also controlling for the average daily income and/or land possession of one's group of competitors. We also present estimates where we use the number of competitors in the group who are among the top 25% earners/wealthiest, as an alternative measure of the opponents' wealth.[33] We see that while opponents' measures of income and wealth are negatively associated with competitive entry, the pattern of within treatment ethnic differences in competitive entry are similar and statistically indistinguishable from the ones reported above in Table 2.6, that is, Santals in the *minority* treatment are around 30 percentage points less likely to enter competition than Bengalis in the same treatment.[34] We also estimated within ethnic group treatment differences adding measures of the opponents' wealth as controls, separately for the two ethnic groups (see Table A4 in Appendix A); results are again similar to those reported above in Table 2.7.

This analysis suggests that since observed differences in wealth/income of opponents do not explain differences in preferences for interethnic competition, it must be ethnic differences in dimensions unobserved to the experimenters (but evident to participants), such as social power and status, that underpin these patterns.

[33] Top 25% earners are those that earn more than 200 Takas per day or own more than 5 *kathas* of land (those who are in the top quartile only).

[34] We perform a number of unreported additional separate robustness checks: we control for the maximum income and wealth of competitors, the number of top 10% earners/wealthiest competitors in a group and the number of top earners/wealthiest opponents nonlinearly. We find that our main results remain robust throughout.

TABLE 2.8: Treatment Effects on Willingness to Compete: Ethnic Group Differences with Material Well-Beings of Competitors as Controls

	Averages of Competitors			**Number of Top 25%**		
VARIABLES	**(Income)**	**(Land)**	**(Both)**	**(Income)**	**(Land)**	**(Both)**
Santal	-0.021	0.002	-0.021	-0.013	-0.014	-0.023
	(0.105)	(0.104)	(0.103)	(0.109)	(0.104)	(0.105)
Minority	0.035	0.012	0.023	0.009	-0.009	-0.021
	(0.156)	(0.166)	(0.163)	(0.158)	(0.162)	(0.159)
Majority	-0.008	-0.033	-0.014	-0.043	-0.038	-0.050
	(0.141)	(0.145)	(0.145)	(0.143)	(0.143)	(0.144)
Santal×Minority	-0.280***	-0.304***	-0.279***	-0.290***	-0.290***	-0.278***
	(0.084)	(0.074)	(0.085)	(0.082)	(0.083)	(0.089)
Santal×Majority	-0.103	-0.148	-0.107	-0.116	-0.148	-0.127
	(0.131)	(0.124)	(0.131)	(0.138)	(0.123)	(0.135)
Average Income of Competitors	-0.001**	-	-0.001**	-	-	-
	(0.000)		(0.000)			
Average Land of Competitors	-	-0.002	-0.001	-	-	-
		(0.002)	(0.002)			
No. of Top 25% Income	-	-	-	-0.025	-	-0.017
				(0.027)		(0.028)
No. of Top 25% Land	-	-	-	-	-0.033	-0.027
					(0.031)	(0.034)
Other Controls	Yes	Yes	Yes	Yes	Yes	Yes
Village Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	250	250	250	250	250	250
Pseudo R^2	0.225	0.215	0.226	0.216	0.217	0.218

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

Note: Probit regressions with average marginal effects reported; dependent variable is "compete", which equals 1 when the individual opted to compete and 0 otherwise; 'Average Income (Land) of Competitors' is the average daily income (land possession) of all five group competitors; 'No. of Top 25% Income (Land)' is the number of competitors in a group whose income (land possession) is in the top 25% among all participants; 'Other Controls' include stages 1 and 2 scores, the perceived probability of winning, risk aversion, intercultural competence, income, profession, land possession, age, education, and having met other group members before. There are 42 groups in total. Table 2.1 describes all 'Other Controls'.

2.5 Conclusion

This paper extends the literature on competitive preferences by exploring whether there are differences in competitive choices across members of the dominant and of a vulnerable ethnic minority group in rural Bangladesh. By varying the ethnic composition of the group of competitors, we find that members of the ethnic minority group are more averse to competing in mixed-ethnicity groups than in groups of co-ethnics. We find the opposite pattern for members of the ethnic majority group, that is, they are more keen to compete in ethnically mixed groups than in *homogeneous* ones.

How important quantitatively are the ethnic differences in competitiveness that we find? After accounting for the influence of past performance, beliefs about relative performance, risk attitudes and socioeconomic characteristics, our regression analysis indicates that Santals in the *minority* treatment are 31 percentage points less likely to enter competition than the Bengalis in the same treatment, while Santals in the *minority*

treatment are 41 percentage points less likely to enter competition than Santals in the *homogeneous* treatment. For comparison, the estimated gender gap is typically found to be about 30 percentage points (Niederle & Vesterlund, 2007), so the quantitative effect found here would appear to be of the same or greater magnitude than the gender gap.

The aversion to interethnic competition by the ethnic minority group may have its roots in the self-perception of the value of the ethnic identity of the particular group and the associated stereotypes that it evokes. Since, historically, one ethnic group has oppressed the other and the two have rarely coexisted peacefully, we might expect that the two ethnic groups have developed different identities and stereotypes. Assigning ethnic minority members into a group dominated by ethnic majority members might invoke the stereotype of being ethnically inferior, which might discourage them from choosing to compete. Likewise, assigning ethnic majority members into an ethnic minority dominated domain might invoke the stereotype of being ethnically superior, which might encourage them to compete more often than when being in a group of co-ethnics.

Although more evidence of the differences in preferences for interethnic competition is needed, a few pointers for policy stem from the findings we have. While policies to improve workplace diversity would be a good starting point, policy makers could also target improving competitiveness of members of minority groups through education and awareness. Educating minority members regarding self-worth, self-esteem and self-importance, and increasing public awareness to break negative stereotypes associated with minority members might improve their competitive attitude towards the dominant group. Similarly, fostering interethnic interaction might also improve competitive attitudes of ethnic minorities. These possibilities remain interesting avenues for future research.

Appendix A: Supplementary Tables

TABLE A1: Perceived Probability of Winning

Panel A: Compares Perceived Probability of Winning Between Ethnic Groups

	Proportion of Bengali (*Std. Dev.*)	**Proportion of Santal** (*Std. Dev.*)	**MW-test** ***p*-values**	**CS-test** ***p*-values**	**N**
Perceived Probability of Winning	**0.60**	**0.58**	**0.798**	**0.798**	**252**
	(*0.49*)	(*0.50*)			
Homogeneous	0.65	0.63	0.850	0.849	120
	(*0.48*)	(*0.49*)			
Majority	0.52	0.57	0.670	0.669	88
	(*0.51*)	(*0.50*)			
Minority	0.59	0.45	0.371	0.365	44
	(*0.50*)	(*0.51*)			
Mixed	0.55	0.53	0.862	0.861	132
	(*0.50*)	(*0.50*)			

Panel B: Compares Perceived Probability of Winning Within Ethnic Groups

	Group Treatment		**Group Treatment**	**MW-test** ***p*-values**	**CS-test** ***p*-values**
Bengali	Homogeneous	*vs*	Majority	0.193	0.191
	Homogeneous	*vs*	Minority	0.625	0.623
	Majority	*vs*	Minority	0.603	0.600
	Homogeneous	*vs*	Mixed	0.234	0.232
Santal	Homogeneous	*vs*	Majority	0.504	0.502
	Homogeneous	*vs*	Minority	0.148	0.145
	Majority	*vs*	Minority	0.387	0.383
	Homogeneous	*vs*	Mixed	0.244	0.242

Note: Mixed group combines perceived probability of winning of both Minority and Majority group treatments; N is the total sample size, wherein ethnic groups are in equal proportions in each group composition; MW-test is the two sided Mann-Whitney U test; CS-test is the Pearson's Chi-squared test; Test p-values are comparing choices horizontally.

TABLE A2: Summary of Guessed Ranks

Experiment Summary	Mean Guessed Rank of Bengali (*Std. Dev.*)	Mean Guessed Rank of Santal (*Std. Dev.*)	MW-test *p*-values	CS-test *p*-values	N
Guessed Rank	**2.51 (*1.38*)**	**2.55 (*1.41*)**	**0.867**	**0.834**	**252**
Homogeneous	2.43 (*1.32*)	2.38 (*1.43*)	0.656	0.806	120
Majority	2.66 (*1.48*)	2.52 (*1.41*)	0.687	0.993	88
Minority	2.41 (*1.40*)	3.05 (*1.33*)	0.093	0.012	44
Mixed	2.58 (*1.45*)	2.70 (*1.39*)	0.544	0.315	132

Note: 'Guessed Rank' is the relative guessed rank based on stage 2 performance, where 1 is the best and 6 is the worst; Mixed group combines ranks of both Minority and Majority group treatments; MW-test is the two sided Mann-Whitney U test; CS-test is the Pearson's Chi-squared test; Test *p*-values are comparing differences horizontally.

TABLE A3: Performance Improvement from Stage 1 to Stage 2

Groups	Mean Score in Stage 1	Mean Score in Stage 2	SR-test *p*-values	T-test *p*-values	N
Pooled	**23.93 (*7.19*)**	**25.97 (*7.36*)**	**0.000**	**0.000**	**252**
Bengali	**24.10 (*7.67*)**	**25.73 (*7.71*)**	**0.000**	**0.000**	**126**
Homogeneous	21.72 (*6.51*)	23.78 (*6.35*)	0.000	0.000	60
Majority	26.55 (*8.22*)	27.80 (*8.61*)	0.144	0.121	44
Minority	25.68 (*7.85*)	26.91 (*8.23*)	0.274	0.178	22
Mixed	26.26 (*8.05*)	27.5 (*8.44*)	0.058	0.042	66
Santal	**23.77 (*6.69*)**	**26.21 (*7*)**	**0.000**	**0.000**	**126**
Homogeneous	22.62 (*6.37*)	24.90 (*5.38*)	0.000	0.000	60
Majority	24.52 (*6.87*)	27.45 (*7.86*)	0.011	0.005	44
Minority	25.41 (*6.93*)	27.32 (*8.64*)	0.039	0.112	22
Mixed	24.82 (*6.85*)	27.41 (*8.07*)	0.002	0.001	66

Note: 'Pooled' includes scores of both Bengali and Santal sample; Mixed group combines scores of both Minority and Majority group treatments; SR-test is the Wilcoxon signed-rank test; T-test is the paired T-test with equal variances.

TABLE A4: Within Ethnic Group Treatment Effects on Willingness to Compete with Material Well-Being of Competitors as Controls

	Panel A: Santal					
	Averages of Competitors			**Number of Top 25%**		
VARIABLES	**(Income)**	**(Land)**	**(Both)**	**(Income)**	**(Land)**	**(Both)**
Santal-Majority	-0.255**	-0.305***	-0.268**	-0.291**	-0.319***	-0.320***
	(0.124)	(0.114)	(0.125)	(0.117)	(0.120)	(0.121)
Santal-Minority	-0.375***	-0.406***	-0.373***	-0.404***	-0.404***	-0.404***
	(0.111)	(0.101)	(0.113)	(0.103)	(0.109)	(0.108)
Average Income of Competitors	-0.001***	-	-0.001**	-	-	-
	(0.000)		(0.000)			
Average Land of Competitors	-	-0.007***	-0.005**	-	-	-
		(0.003)	(0.002)			
No. of Top 25% Income	-	-	-	-0.041	-	0.001
				(0.057)		(0.060)
No. of Top 25% Land	-	-	-	-	-0.077	-0.077
					(0.049)	(0.054)
Other Controls	Yes	Yes	Yes	Yes	Yes	Yes
Village Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	125	125	125	125	125	125
Pseudo R^2	0.344	0.344	0.360	0.318	0.331	0.331
	Panel B: Bengali					
	Averages of Competitors			**Number of Top 25%**		
VARIABLES	**(Income)**	**(Land)**	**(Both)**	**(Income)**	**(Land)**	**(Both)**
Bengali-Majority	0.018	0.050	0.092	-0.005	-0.001	-0.009
	(0.168)	(0.177)	(0.185)	(0.176)	(0.174)	(0.179)
Bengali-Minority	0.113	0.194	0.233	0.091	0.090	0.078
	(0.190)	(0.208)	(0.214)	(0.193)	(0.212)	(0.208)
Average Income of Competitors	-0.000	-	-0.001	-	-	-
	(0.001)		(0.001)			
Average Land of Competitors	-	0.004	0.005	-	-	-
		(0.003)	(0.003)			
No. of Top 25% Income	-	-	-	-0.014	-	-0.012
				(0.034)		(0.037)
No. of Top 25% Land	-	-	-	-	-0.011	-0.008
					(0.044)	(0.049)
Other Controls	Yes	Yes	Yes	Yes	Yes	Yes
Village Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	123	123	123	123	123	123
Pseudo R^2	0.191	0.199	0.206	0.191	0.190	0.191

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

Note: Probit regressions with average marginal effects reported; dependent variable is "compete", which equals 1 when the individual opted to compete and 0 otherwise; 'Average Income (Land) of Competitors' is the average daily income (land possession) of all five group competitors; 'No. of Top 25% Income (Land)' is the number of competitors in a group whose income (land possession) is in the top 25% among all participants; 'Other Controls' include stages 1 and 2 scores, the perceived probability of winning, risk aversion, intercultural competence, income, profession, land possession, age, education, and having met other group members before; two villages were dropped in the Bengali data due to having single observations in each village; Table 2.1 describes all 'Other Controls'; There are 42 groups in total.

Chapter 3

Behavioral Consequences of Religious Education: A Lab-in-the-Field Experiment in Bangladesh

3.1 Introduction

All religions teach their adherents to be selfless, forgiving, trusting, and honest, which makes it an important institution that shapes economic behavior and preferences and affects decision making and outcomes.[1] With the growing number of religious schools around the world (Noor, Sikand & Bruinessen, 2008; Riaz, 2008; Robert, 2009; Merry & Driessen, 2012; Johnson, Zurlo, Hickman & Crossing, 2017), it is, therefore, important to understand how religious education is affecting economic behaviors and preferences of children who are enrolled in such schools. While behaviors and outcomes that are affected by religious affiliation have been tested previously using different methods, establishing the causal effect of religion, religiosity, or religious schooling has always been a difficult task (Altonji et al., 2005; Hungerman, 2014; Iyer, 2016).[2] This is because religious participation is rarely random, which makes identification difficult. One could circumvent this issue by either designing a large-scale field experiment (see Bryan,

[1] Initially, Max Weber studied how Protestant work ethics drove economic progress in Northern Europe (Weber, 1930). Later, Becker & Woessmann (2009) empirically tests Weber's hypothesis and finds that Protestantism led to higher acquisition of literacy which as a result led to economic prosperity amongst Protestants, discarding the fact that religious work ethics regarding hard work and thrift were the primary driver of economic progress (although the authors acknowledge the fact that acquisition of education could have been influenced by such ethics in the first place). Other studies have shown how religion/religiosity influences economic outcomes, such as contributions to public goods (Benjamin, Choi & Fisher, 2016); educational outcomes (Oosterbeek & van der Klaauw, 2013); discrimination (Chuah, Gächter, Hoffmann & Tan, 2016); economic growth and happiness (Campante & Yanagizawa-Drott, 2015); trust and ethics (Guiso, Sapienza & Zingales, 2003); views toward out-group members (Clingingsmith, Khwaja & Kremer, 2009); and, depression (Fruehwirth, Iyer & Zhang, 2018).

[2] See Iannaccone (1998), McCleary & Barro (2006), Hoffmann (2013), Tan (2014) and Iyer (2016) for surveys on religion from an economic perspective.

Choi & Karlan (2018) for instance) or by exploiting a natural experiment, where children are randomly assigned to different institutions which happens to vary in religiosity. While conducting the former would be costly, the latter is rare, particularly in developing countries. Hence, having such limitations makes it somewhat challenging to explore how theological education, school environment, and moral teachings are shaping behaviors and preferences of children that are already enrolled in schools.

The main objective of this paper is to establish the causal effect of religious education on economic behaviors and preferences, namely altruism, dishonesty, trust, cooperation, and risk aversion, by exploiting a natural field setting in a developing country, Bangladesh, that allows me to reduce selection bias. I experimentally measure behaviors and preferences of orphan children from religious (i.e. Islamic) and secular orphan schools only, as children in such institutions are living, receiving education, and growing up within their school premises that are segregated from everyday social activities. Hence, children go through a somewhat restricted social learning process (Bandura, 1977) that relies largely on learning from educational curriculum, teachers, peers (who are also orphans), and school environment and lifestyle, with no learnings from family, relatives, and neighborhoods following being orphaned. Moreover, the difference between religious and secular schools is more than just nominal - students in religious schools get much more 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, wearing religious outfits, and so on. However, students in secular schools are not bound by such restrictions.[3]

Another equally important feature of this field setting is that it allows me to reduce selection bias. A frequent problem encountered while exploring the effects of nurture is selection, as it might explain some or entire behavioral differences reported instead of the environment itself. For instance, religious parents are more likely to admit their children to religious schools and teach them religious values prior to being admitted than secular parents. So children in religious schools from a religious family background would already be more religious to start out with, which would overstate the impact of religious education (positive selection). However, this field setting mitigates this endogeneity concern. Even though the children are orphans, admission into orphan schools could still be non-random as many children are abandoned and admitted by their parents and relatives. I call them *family* children. However, along with *family* children, orphan schools also have children who did not have a family prior to joining schools and, hence, they were brought up under a non-family environment during their pre-school years. Therefore, by combining this unique admission feature along with the evidence from a large body of literature on intergenerational transmission of behaviors, beliefs, and preferences from parents to children (Bisin & Verdier, 2001; Dohmen et al., 2011), I assume that children who grew up in a non-family environment prior to starting

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

school would have minimal transmission of behaviors and preferences from their parents or from other family members.[4] Therefore, by exploiting schools' admission data, I am able to separate children who were admitted by their parents or relatives (suggesting they had spent most of their pre-schooling years with their family) from children who were admitted by third parties such as orphanages for infants and streets (suggesting they had spent most of their pre-schooling years in a non-family environment). For simplicity, I call the latter *non-family* children. Therefore, when I include in my analysis only children who were admitted to orphan schools by non-family-members (*non-family* children), factors such as individual and family preferences or characteristics, parental pressure, endorsed norms within neighborhoods, and so on, are uncorrelated or, at least, weakly correlated with school participation. To my knowledge, this is the first study to exploit this method in order to reduce selection bias in a natural setting. Later, using a series of robustness checks, I show that the relationship documented in this paper is causal.

To measure behaviors and preferences, I ran a battery of controlled experiments on orphan children to capture their level of altruism, dishonesty, risk preference, 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 Northwestern part of Bangladesh that either follows an Islamic or a secular educational curriculum and lifestyle. 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 (Fischbacher & Föllmi-Heusi, 2013), risk aversion (Gneezy & Potters, 1997), cooperation (Goette, Huffman & Meier, 2006), and trust (Berg, Dickhaut & McCabe, 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 schools differ in terms of intensity of religious education and lifestyle, I expect students to behave in accordance with the type of education they receive.

My first result shows that (*non-family*) children from religious schools donate significantly more to the charity relative to children from secular schools and this result is in accordance with Islamic teachings on donation and also corroborates results documented in Shariff & Norenzayan (2007) and McGranahan (2000). Moreover, on in-group bias in charitable giving, I find no evidence for religious (secular) students donating more to religious (secular) orphanages, which is inconsistent with Preston & Ritter (2013)

[4]Children develop such characteristics by learning from their parents (Eisenberg & Mussen, 1989) which could be at the age of 7-8 (Alan, Baydar, Boneva, Crossley & Ertac, 2017), which is around their school joining age. Please also note that I cannot discard the fact that children could still be influenced by their non-family care-givers at infant orphanages; however, I assume that transmission of behaviors and preferences from care-givers should not be as strong as that from parents. This is because time and home education received by children from parents is more concentrated (as parents devote all their time to 2-3 children only) relative to that from care-givers, which is likely to be more spread out (as each care-giver is responsible for many children at a baby-home). If transmission of preferences by care-givers and parents are equal then I should get null differences. Therefore, this paper also tests this conjecture.

that uses a religious priming experiment to test this conjecture. In accordance with the belief that religion teaches people to be honest and truthful, which is also documented in Mazar, Amir & Ariely (2008) and Shariff & Norenzayan (2011), 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 education having any statistically significant effect on risk preference, trust, trustworthiness, and cooperation amongst children. I also find that behavioral differences that I captured are driven by children who have spent relatively more years in schooling, suggesting the source of difference in behaviors and preferences between children from religious and secular schools is the duration of exposure to distinct educational regimes as learning from the curriculum and social environment becomes more intensive over time.

While exploring behaviors of orphans and then truncating the sample seems to be a good strategy in reducing selection, concerns regarding other types of selection among the *non-family* sample might still persist, such as selection based on academic merit, agility, and other individual characteristics. Therefore, to attenuate endogeneity concerns, I carry out some statistical tests to probe the robustness of my results. First, I test whether young individuals are different in terms of behaviors and preferences as any difference would be evidence for selection (I consider measures of behaviors and preferences of young individuals a proxy for that of pre-school individuals since I lack such information). Comparing behaviors of young children (below the age 11) across school types show that their behaviors do not differ during their early years, suggesting children must have been comparatively similar during their admission to schools (see Delavande & Zafar (2015) for a similar discussion). Then, by exploring behaviors of both *family* and *non-family* children, I find that children in religious schools with a family background are indeed more charitable, honest, and trusting relative to that with non-family background, suggesting this field setting is indeed successful at reducing selection. Therefore, such tests strengthens my argument that any behavioral differences captured in this study must be due to the exogenous variation in school environment induced by different educational curriculum and the intensity of religiosity followed by orphan schools, and are unlikely to be selection driven. Among *family* children, I also find some evidence for positive selection of children into religious schools.

While this paper provides support to the existing literature on religion and its impact on decision making using a more robust identification strategy, it also relates to studies on the influence of education and 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, Kremer, Miguel & Thornton, 2011; Algan, Cahuc & Shleifer, 2013). Moreover, this study also relates to studies on how habitat can endogenously form behaviors and preferences (Bowles, 1998) such as cooperation

(Gneezy, Leibbrandt & List, 2016), competitiveness (Leibbrandt et al., 2013; Siddique & Vlassopoulos, 2017), altruism, risk, and time preferences (Voors, Nillesen, Verwimp, Bulte, Lensink & Van Soest, 2012), and antisocial behavior (Prediger, Vollan & Herrmann, 2014) to name a few. This paper also fits into the literature on intergenerational transmission of beliefs and preferences. Since the procedure of removing *family* children to reduce selection bias hinges on the idea that parents are likely to pass on their preferences to their children, one could relate my findings to the literature on transmission of beliefs and preferences from parents and teachers to young children (Bisin & Verdier, 2001, 2010; Alan, Ertac & Mumcu, 2018), wherein parental environments and investments shape children's noncognitive skills (Cunha, Heckman & Schennach, 2010). Parents also "exert a direct socialization effort to influence their children's process of preference formation" (Bisin & Verdier, 2000, p. 967) namely by choosing appropriate neighbourhoods, schools, and determining attendance to religious institutions. On this, studies have found strong evidence on parents transmitting their willingness to take risks, trust, and patience to their children (Dohmen et al., 2011; Doepke & Zilibotti, 2008). Overall, my paper corroborates findings from studies that explore the role of education, family, and social environment in shaping economic behaviors and preferences.

In contrast to much recent work on the effects of religiosity, I am not looking at the effect of short-term interventions, e.g. going on the *Hajj* or the length of the Ramadan fasting hours or the Ramadan itself (Clingingsmith et al., 2009; Oosterbeek & van der Klaauw, 2013; Campante & Yanagizawa-Drott, 2015), but at the effect of long-term exposure over several years. Another important feature of this paper is that I directly aim to measure behavior and preferences of children which has important implications for clarifying the mechanisms through which religiosity 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 am able to test this directly for both religious and secular school children. Finally, I test the effect of religious education among a vulnerable population that is directly of 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. This of course carries a cost in terms of external validity in being able to generalize to non-orphans; however, this population and this setting are both of intrinsic interest. Besides, this field setting allows me to show that reducing selection bias is possible in a natural environment when natural experiments are unavailable and conducting large-scale field experiments are costly and, perhaps, unethical.

I have organized the paper in the following way. Section 3.2 discusses the association between religious teachings and economic behavior and connects it to the existing literature. Section 3.3 describes the field setting while section 4.3 describes the experiment and lays out some research hypotheses. Then after discussing the main results in section 4.4, I investigate whether my results are selection driven and whether the field setting is able to minimize selection after all in section 3.6. Section 3.7 concludes with some policy implications.

3.2 Religious Education and Economic Behavior

Why prosociality, trust, dishonesty, and risk aversion? These behaviors explain a plethora of economic decisions and outcomes. Risk aversion not only explains how an individual might make investment decisions in the market, but also indicates how much an individual is open to new ideas, trust new technologies, take health-related risks such as ignoring vaccines or birth controls, participate in unethical activities such as in bribery or corruption, engage in contracts, acquire insurance, investing in education, savings, and so on (Eeckhoudt & Schlesinger, 2008; Søreide, 2009; Pope, Price & Lillard, 2018; Bui, Crainich & Eeckhoudt, 2005; Anderson & Mellor, 2008; Allen & Lueck, 1995; Eeckhoudt & Hammitt, 2001; Eeckhoudt, Meyer & Ormiston, 1997). On the other hand, prosociality explains an individual's attitude towards helping and sharing, protection and defence, and cooperation with others, which affects her/his concerns towards giving to charitable organizations, donate blood or organs to people in need, vote, management of commons, environmental issues, public good provision, and many more (Andreoni, 2006; Meier, 2006; Shabman & Stephenson, 1994; Van Dijk, 2015; Ostrom, 2000; Nolan & Schultz, 2015; Unger, 1991). Furthermore, trust, trustworthiness, and honesty are crucial components for societal well-being and economic interactions, where relationships in trading and investments, school performance, production of public goods, economic growth, etc., are all affected by trust and honesty (Bolton, Katok & Ockenfels, 2004; Gneezy, 2005; Mazar & Ariely, 2006; Bunn, Caudill & Gropper, 1992; Beugelsdijk, 2005; Dearmon & Grier, 2009; Gurun, Stoffman & Yonker, 2017). These are crucial economic elements that directly and indirectly affect a country's economic development. Therefore, understanding how religious education is shaping attitudes, concerns, and personality traits of individuals during their developmental stages would also provide a reasonable notion on how such individuals would make economic decisions as adults in the real world.

Among the numerous motivations behind giving to charity, gambling less, and fostering trust, truthfulness, and cooperation, one recurring motivation is religion (Norenzayan & Shariff, 2008; Guiso et al., 2003; Noussair, Trautmann, Van de Kuilen & Vellekoop, 2013; Asadullah, 2016). This is because religious instructions in almost all major religions teach agents to be other-regarding and honest, where behavior and actions of one must not negatively affect others.[5] Therefore, the main purpose of this section is to understand how these five economically important behavior and preferences are related to religious teachings. To do so, I discuss both existing literatures from economics and psychology as well as Islamic instructions associated with these behaviors. Later, I construct some hypotheses based on the laid-out discussions in Section 4.3.

[5]Here risk-taking or gambling also fits well because all major religions such as Buddhism, Christianity, Hinduism, Islam, and Judaism considers 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.

3.2.1 Charitable Giving and Cooperation

All major religions teach individuals 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 scriptures that encourage philanthropy, whereas Norenzayan & Shariff (2008) discusses sociological, psychological, and economic mechanisms behind religious prosociality (i.e. both altruism and cooperation). Association between these two 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, Ensminger, McElreath, Barr, Barrett, Bolyanatz, Cardenas, Gurven, Gwako, Henrich & others, 2010; Bekkers & Wiepking, 2011); while few others have found no significant relationship between the two (Tan, 2006; Ahmed & Salas, 2011; Benjamin et al., 2016).[6] Although there seems to have some contradictions in the literature, in general, a higher level of religiosity has been found to encourage altruistic and cooperative behavior. On Islamic education on prosociality, one of its five pillars is *zakat* or wealth tax which is a religious obligation. Moreover, there is *sadaqah* or voluntary charity, which can be both monetary and non-monetary, where non-monetary *sadaqah* concerns helping and sharing with others that involve sharing wise advice, helping each other in every way of life (e.g. at work and home), and so on. Hence, religion teaches prosociality as a virtue and instructs individuals to treat others the way one would like to be treated.

On in-group favoritism in charitable giving, Preston & Ritter (2013) and the references therein report that religious people display significantly more prosocial attitude (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. In the same study, they also find that non-religious individuals have higher tendency to give to secular charities, for example to an HIV/AIDS based charity, than religious individuals.

3.2.2 Trust and Dishonesty

What relates trust with religion is its teaching to its followers on establishing their trust in religious figures, authority and deities (Hoffmann, 2013), where having complete faith in the God and establishing faith and goodwill between people (e.g. friends, neighbors, relatives, etc.) stems 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, Fischbacher, Von Rosenbladt, Schupp & Wagner, 2003) as well as towards people who are also religious, exhibiting an

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

in-group favoritism (Fershtman, Gneezy & Verboven, 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 the 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). However, Bruggeman & Hart (1996) found no such relationship. 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.

3.2.3 Risk Aversion

The relationship between risk attitude and religion has originated through gambling, wherein gambling is considered a sinful activity (Hoffmann, 2000). Religious instructions advise people to determine the morality of gambling through its motivations and outcomes. The motivation comes from believing in chances which establishes faith in chance instead of the God. Similarly, the outcome is determined through chances where the winner puts no effort in order 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, Falk, Huffman, Sunde, Schupp & Wagner, 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, Page & Spalt, 2011; Benjamin et al., 2016). Benjamin et al. (2016) tests the conjecture that Catholics prefer gambling, 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 would develop their aversion towards risk.

3.3 Field Setting

Orphan schools in Bangladesh are of two major categories: either religious or secular. Religious orphan schools or Madrasa orphanages are Islamic seminaries that strictly follow the theological curriculum and a devoted religious lifestyle. In contrast, secular orphan schools follow an education curriculum that is identical to regular public schools in Bangladesh. Moreover, secular schools also do not follow a strict religious lifestyle. Therefore, children devote a significant amount of time to studying scriptures, rituals and lead a committed spiritual lifestyle in religious schools whereas children in secular schools do not follow such rules closely. Thus, children are exposed to two different social

and educational environment that varies in terms of intensity of religious education and lifestyle followed by schools. This field setting also has two important characteristics that are crucial in determining how a child's behavior is shaped and whether selection bias could be reduced. First, children receive education and live within their school premises that are segregated from everyday social activities. Therefore, children's learning and socialization are rather restrained which results in a somewhat restricted social learning process that relies largely on learning from educational curriculum, teachers, peers (who are also orphans), and the religious rules (i.e. lifestyle) followed by the schools. In contrast, learning from parents, siblings, relatives, and neighborhoods are negligible following being orphaned. Second, children in orphan schools are either abandoned by parents (i.e. *family* children) or they are admitted by a third party (i.e. *non-family* children). Therefore, this information on who admits a child reveals about where he had spent his pre-schooling years. Then focusing only on those who were admitted by a third party allows me to reduce selection based on individual and family preferences or characteristics as, for such children, individual or parent's choice in school selection do not partake. I discuss these more in detail in the following subsections.

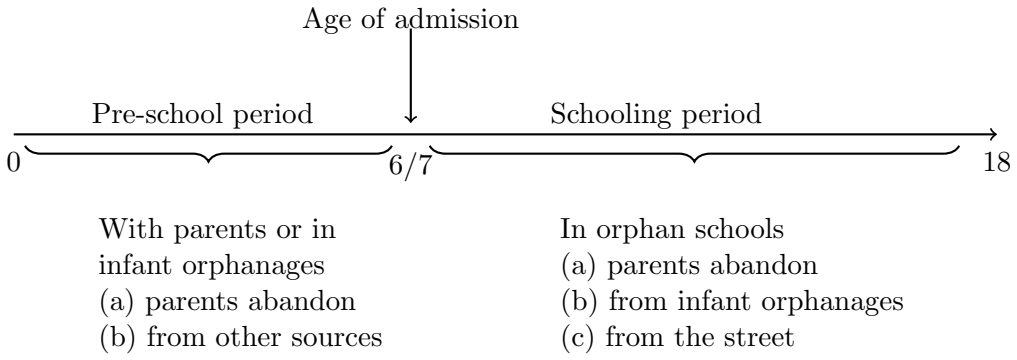
3.3.1 Religious Schools in Bangladesh

There are two categories of Madrasas in Bangladesh: *Aliya* and *Qawmi*. *Aliya* Madrasas were first set up in India under the British rule in the eighteenth century with the primary purpose of training clerks for colonial offices to interpret Muslim laws.[7] Later it was relocated in Bangladesh and started operating under the state-sponsored education board, and have since provided government approved degrees to students (Bhuiyan, 2010). *Qawmi* Madrasas, however, are independent of state regulation and is managed by a private education board (called Befaqul Madarisil Arabia Bangladesh). It was 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.[8] Therefore, *Aliya* Madrasa involves teaching of both secular and theological curriculum, whereas *Qawmi* Madrasa focuses only on theological curriculum after the fifth grade. In contrast, secular orphan schools follow government-approved secular curriculum, such as sciences, arts, commerce, etc, which educates students more on non-religious reasoning and thinking. Besides, *Madarasas* also teach students about religious lifestyle, which involves reciting *Surahs* (or chapters) from the Quran, performing *Salah* (or prayer) five times a day, fasting in Ramadan, wearing religious outfit - *thawb* (robe) and *taqiyah* (cap) for boys, discourage reading newspapers and magazines,

[7] It teaches *Dars-e-Nizami*, which is the traditional *Madrasa* curriculum in the Indian Subcontinent.

[8] See also Delavande & Zafar (2015), Asadullah & Chaudhury (2009), and Bano (2008) for more details on Madrasas. According to national education statistics, there are currently more than 9,300 *Aliya* Madrasas under operation, teaching more than 2.4 million students (Bangladesh Ministry of Education, 2015). However, the exact number of *Qawmi* Madrasa is contradictory, with few sources claiming the number to be similar to that of *Aliya* Madrasas, whereas others claim it to be a many as 64,000 (Bhuiyan, 2010).

Table 3.1: Orphan Children Timeline



and watching television (primarily on the ground of obscenity), drawing or production of images of living things and so on (Bhuiyan, 2010; Asadullah, 2016). While religious schools follow these principles very closely, secular schools have relatively relaxed and modified rules, especially in terms of strict religious practices. Therefore, children are exposed to two distinct educational and social environment that differ in terms of intensity of religious education and lifestyle. All religious schools in this study are *Qawmi* Madrasas.

3.3.2 Orphan Schools and Admission

The most significant benefit of studying behaviors in this natural field setting is that it allows me to differentiate *family* children from children who were raised in non-family environments (e.g. baby homes, streets, etc.) prior to joining a school and are, therefore, *"non-family"*. Since there are no written rules and regulations on how children should be admitted to an orphan school in Bangladesh, I obtain the following information by merely interviewing orphanage heads, teachers, and school authorities.

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 (see Table 3.3.2 for the timeline). Many parents *abandon* their children as they (or widowed mothers) are unable to provide for their children (UNICEF, 2009). Children are also admitted by their relatives who were responsible for their care immediately after their parents' deaths but are now unwilling to continue doing it. On the other hand, many children grow up in baby homes or orphanages for infants where children are provided with care, pre-school education, recreational activities, etc., until they reach a certain age (UNICEF, 2009). Such homes are mostly NGO funded and are generally secular.[9] Then, at the age of 6 or 7, children from baby homes are sent to various orphan schools or schools get to admit students from such homes. Furthermore, children are also taken up from the streets, such as orphaned children who beg for money/food at railway stations

[9]However, there are many Christian missionaries that operate baby homes and orphan schools and are hence religious (in such organizations, students from religious baby homes go onto studying at mission schools). To my knowledge, Madrasas do not operate any baby homes in Bangladesh. Following time spent in baby homes, children from government-funded baby homes are transferred to public orphan schools, which are called *shishu poribar* (Bangladesh Ministry of Social Welfare, 2015; UNICEF, 2009)

and children who are lost and do not have any family. In such cases, children had spent their pre-schooling years in a non-family environment prior to being admitted to a school and, hence, are *non-family* children.

It is highly likely that schools or baby homes follow certain criteria to send a child to a particular school, e.g. based on academic merit, agility, and so on. Also, carers in baby homes might also pass on their behaviors and preferences to children. Likewise, parents might abandon their children at baby homes when they were infants. Therefore, in all these cases, selection bias would persist and reducing such biases in this natural setting using the limited available data is outside the scope of this study.

3.3.3 Admission Data

I obtained admission data on participants from each school. School authorities provided data that includes a student's current age, their age when they were admitted to the school, the person who admitted a student, the grade they are currently studying in, and test scores from their most recent midterm exam (which takes place in May/June every year) that covers three general subjects (Bangla, English, and Mathematics) and either Arabic (in religious schools) or General Knowledge (in secular schools). Since religious schools in my sample only teach and take exams on general subjects until grade 5, students from grade 6 (or *mutawassitah*) onwards do not have such exams. Religious schools test such students informally and have provided an average score for these tests, but this data is likely to be very noisy. Also, exams are not standardized across school types so I cannot use the test score data in my analysis. The data on the person who admitted a student includes whether a student was admitted by (i) both/one of the parents or a relative, (ii) taken/sent from other orphanages for infants, or (iii) admitted children from the railway stations or streets.[10] Therefore, I call children from source (i) as *family* and that from (ii) and (iii) as *non-family*. The admission data do not include specific information on the admission criteria used by baby homes or orphan schools. It also does not include information on who sent a child to a baby home in the first place. Therefore, due to data limitations, I cannot trace back *non-family* children to check at what age they got admitted to a baby home and by whom. Such detailed data should reduce selection further.

3.3.4 Assumptions for Reducing Selection

Drawing inspiration from a large body of literature on intergenerational transmission of behaviors, beliefs, and preferences from parents to children (Bisin & Verdier, 2001; Dohmen et al., 2011), I assume that children who grew up in a non-family environment prior to starting school would have minimal transmission of behaviors and preferences from their parents or from other family members. Children develop such characteristics by learning from their parents (Eisenberg & Mussen, 1989) which could be at the age of

[10] I also ask children, individually, about whether they were living with their family or not before joining the school. Their answers match with the information provided by the schools.

7-8 (Alan et al., 2017), which is around their school joining age. Therefore, by exploiting schools' admission data, I am able to separate children who were admitted by their parents or relatives (suggesting they had spent most of their pre-schooling years with their family) from children who were admitted by third parties such as orphanages for infants and streets (suggesting they had spent most of their pre-schooling years in a non-family environment). Therefore, when I only explore behavior of *non-family* children, factors such as individual and family preferences or characteristics, parental pressure, endorsed norms within neighborhoods, and so on, are uncorrelated or, at least, weakly correlated with school participation.

However, I cannot entirely discard the fact that children could still be influenced by their non-family care-givers at infant orphanages; however, I could assume that transmission of behaviors and preferences from care-givers should not be as strong as that from parents. This is because time and home education received by children from parents is more concentrated (as parents usually devote all their time to a few children only) relative to that from care-givers, which is likely to be more spread out (as each care-giver is usually responsible for care of many children at a baby-home) (NICHD Early Child Care Research Network, 2002; Shpancer, 2002). If transmission of preferences by care-givers and parents are equal then I should get null differences between *family* and *non-family* children.

3.4 The Experiment

To test the effect of religious education, I measure five economically important behavior and preferences using five decision making games that children played inside their school classrooms. I measure altruism of individuals using a donation game (Eckel & Grossman, 1996), risk preference using a simple one-shot investment game (Gneezy & Potters, 1997), dishonesty using Fischbacher & Föllmi-Heusi (2013)'s modified cheating game as laid out in Hanna & Wang (2017). Then to measure cooperation and trust, I use a simultaneous prisoner's dilemma game (Goette et al., 2006) and a trust game (Berg et al., 1995) respectively.

3.4.1 Experiment Procedure

I invited orphan schools from the Northwestern part of Bangladesh to take part in this study, which took place in September 2017. Then a few days prior to an experiment, I advertised a call for participation to all students from all schools who had accepted my invitation to take part in this study. At the end of a school day, in each class, teachers informed their students (aged 8 or above) about the experiment and rewards involved and gave them the option to sign-up if they wanted to participate. In total, three religious and three secular orphan schools took part and a total of 210 students

participated in this study ($N_{total} = 210$) among which 146 students were *non-family* children ($N_{nf} = 146$). All students who signed up participated in the experiment.[11]

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 a 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 of them so that neither other participants nor experimenters could observe their decision making.[12] Once everyone was seated, the experimenter readout 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.[13] Following examples, the experimenter demonstrated how one should make decisions using the pen, paper, and envelope provided. After clarifying any questions they might have, participants were asked to play their games. Since orphan schools were of different sizes (in terms of the number of students), sessions were also of varying sizes. The smallest parallel 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 were offered in Appendix B.

3.4.2 Experimental Games

In this study, subjects made decisions in a series of games that were both strategic and individual decision problems. Design for each game is laid out below.

Charitable Giving: 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.[14] 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. Moreover,

[11] Religious and secular orphan schools in this study are governed by Muslims with 100 percent students following Islam. In my sample, all orphanages are non-government owned and boys-only schools.

[12] None of the school classrooms had desks and chairs, so all students were seated on the floor.

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

[14] 1 USD = 80 Taka at the time of the experiment.

recipients were randomly assigned to each session (in this case, each classroom) and not to each individual.

Cheating: Dishonesty was measured using a cheating game (Fischbacher & Föllmi-Heusi, 2013; 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 experimenter in the room that allowed them to feel comfortable to cheat. Therefore, cheating in this experiment would mean reporting a different (higher) number than the one rolled. Please note that I do not compare their distribution of reports with the theoretical distribution within each school type. Instead, I simply compare religious children's distribution of reports with secular children's distribution of reports to check who cheats more in the task.

Investment: 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 preferences, 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: 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 with and, hence, played this game anonymously. Subjects were told about the decision of their paired players at the end, in private.

Trust: Using the trust game (Berg et al., 1995), I was able to capture 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).[15] In this

[15]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 in "Incentives" subsection.

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 back 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. However, prior to reaching a conclusion 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). 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 back. Subjects never knew with whom they were paired with, so transfers between pairs were anonymous.

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.[16] Also, the order of games were randomized to control for order effects.[17] However, *Trust* game was always played at the end.[18] To economize on space, I explain the procedure of how games were conducted in Appendix B (Details on the Experimental Design).

3.4.3 Hypotheses

To comprehend how religious education may affect one's behavior, I construct the following hypotheses based on the arguments laid out in section 3.2 that I am going to test using the design presented above. Since my main strategy is to truncate the sample in order to remove children who have very likely adapted behaviors and preferences from their parents (Bisin & Verdier, 2001), I assume that taking such measures would reduce selection bias and would give me results that are likely to be as good as if admission was completely random. Therefore, all hypotheses applies to the *non-family* children sample only ($N_{nf} = 146$).

Hypothesis 1: Overall donations made by children from religious schools would be higher than overall donations made by children from secular schools.

[16] The research ethics committee at the University of Southampton did not allow me to pay according to the lottery outcome. They were concerned about the inequality in earnings among children and, hence, instructed me to pay a uniform amount of 100 taka to all participants for the randomly chosen game.

[17] Controlling for the order does not change my results and have no significant effect at conventional levels.

[18] Trust game is a sequential game (whereas others are not), where both trustors and trustees observe each other's behavior. So, even though participants are told that only one of the games would be selected for payment, playing the trust game before the last could still affect decisions in subsequent games.

Hypothesis 2: Children would demonstrate an in-group bias while donating. That is, children from religious (secular) schools would donate more to religious (secular) orphanages than to secular (religious) orphanages.

Hypothesis 3: Children from religious schools would show more honesty (i.e. cheat less) than children from secular schools.

Hypothesis 4: Children from religious schools would be more risk averse than children from secular schools.

Hypothesis 5: Children from religious schools would be more cooperative than children from secular schools.

Hypothesis 6: Children from religious schools would trust more and would be more trustworthy than children from secular schools.

Since I test for multiple hypotheses, I use the Westfall-Young adjustment in section 3.6.1. to correct p-values for each outcome that I test.

3.5 Results

Here I analyze whether religious education affects economic behavior using a battery of experiments conducted on orphan children in Bangladesh. Since orphan schools are composed of both *family* and *non-family* children, I focus my analysis primarily on students who were not abandoned to orphanages by their parents or family members ($N_{nf} = 146$: 71 in religious and 75 in secular schools). Later in section 3.6, in order to document the size and direction of bias (if any), I compare behavior between *family* and *non-family* children.

3.5.1 Individual Characteristics

Table 4.1 provides individual characteristics of *all* participants. 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: $p = 0.100$). In terms of years of schooling (i.e. years spent in current school), participants have spent an average of 4.7 years. When I compare religious vs secular children using only the *non-family* sample as shown in Panel B in Table 4.1, I find no significant differences in terms of years of schooling across the two school types (MW-test: $p = 0.368$). Likewise, age of *non-family* children between school types is also not statistically different (MW-test: $p = 0.412$). Since my natural setting does not allow for parental socioeconomic factors to affect behavior, having similar age and years of schooling of children indicates that they are quite similar, with the exception of the type of education

TABLE 3.2: Participant Characteristics

	Panel A: All Sample					
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
N	210	110	100	-	-	-

	Panel B: Non-Family Sample					
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
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 of students that were *non-family*; 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-Abandoned).

they receive. Nevertheless, I control for both age and years of schooling in the regressions to ensure my results are robust. In all regression specifications, I cluster standard errors at the session level because clustering at the class/grade level is not feasible. This is because in religious schools, after the fifth grade, children starts *Hifzul Quran*, i.e. starts memorizing the Quran, to be a *Hafiz* or priest and that does not have any grades.

3.5.2 Charitable Giving

Panel A in Table 3.3 illustrate the difference in charitable giving between religious and secular children. Religious children donated 36 percent of their endowment, while secular children donated 26 percent of their endowment and this difference is statistically significant (MW-test: $p = 0.011$). When I further examine whether students show any in-group favoritism in donation by randomizing the recipient (i.e. an orphanage) as being either religious or non-religious, statistical tests show that only religious children donated significantly more to religious recipients than to secular recipients (MW-test: $p = 0.031$). Although secular students also seem to have donated relatively more to religious recipients than to secular recipients, this difference fails to reach marginal

TABLE 3.3: Summary of Experimental Measures of Non-Family Children

	Pooled (*Std. Dev.*)	**Religious [N]** (*Std. Dev.*)	**Secular [N]** (*Std. Dev.*)	**MW-test** ***p*-values**	**T-Test/CS-Test** ***p*-values**	**N**
Panel A: Donation Game						
% Donations	0.31	0.36 [71]	0.26 [75]	0.011	0.037	146
	(*0.29*)	(*0.30*)	(*0.27*)			
% Donations To Religious Orph.	0.36	0.41 [35]	0.31 [36]	0.018	0.146	71
	(*0.30*)	(*0.29*)	(*0.30*)			
% Donations To Secular Orph.	0.26	0.32 [36]	0.22 [39]	0.131	0.144	75
	(*0.28*)	(*0.31*)	(*0.24*)			
Panel B: Cheating Game						
Points in Dice Task	41.37	39.86 [71]	42.80 [75]	0.051	0.019	146
	(*7.65*)	(*5.90*)	(*8.81*)			
% of 5s & 6s	0.49	0.45 [71]	0.53 [75]	0.073	0.022	146
	(*0.22*)	(*0.16*)	(*0.27*)			
Panel C: Investment Game						
% Invested	0.39	0.41 [71]	0.37 [75]	0.275	0.473	146
	(*0.30*)	(*0.30*)	(*0.31*)			
Panel D: Prisoner's Dilemma Game						
% Cooperated	0.21	0.23 [71]	0.19 [75]	0.565	0.563	146
	(*0.41*)	(*0.42*)	(*0.39*)			
Panel E: Trust Game						
% 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). % Donations To Religious (Secular) Orph. is when the recipient of the donation is a religious (secular) 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. 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.

significance (MW-test: $p = 0.104$), suggesting no in-group bias while donating among secular students.

To warrant that my result is robust to the inclusion of control variables, in Table 3.4, I regress proportions of endowment donated on school type while also controlling for age, quadratic of age, years of schooling, and school dummies using a simple OLS regression.[19] I find that children from religious schools donated around 14 percentage points more from their endowment than children from secular schools. Hence, *Hypothesis 1* holds. This is shown in the first column of Table 3.4. Also, religious recipients received more donation than secular recipients. In Column 2, I test for in-group biases. Firstly, I check in-group bias among religious students, i.e. the difference between donations received by religious and by secular orphanages from religious school children. Regression results show that the difference is statistically insignificant.[20] Likewise, children from secular

[19]Figure B1 in Appendix B shows that age and behavior have a non-linear relationship. Also, adding age linearly as a control does not change my main result.

[20]To simplify Column 2, the variable 'Religious Education' gives me the difference in donations made by religious and by secular school children when the recipient is a secular orphanage; similarly, the variable 'Religious Recipient' gives me the difference in donations received by religious and by secular recipients from secular school children (this difference gives me the in-group bias among secular school

TABLE 3.4: Regression Analysis of the Effect of Religious Education

VARIABLES	(1) Donation I	(2) Donation II	(3) Dishonesty	(4) Risk	(5) Cooperation	(6) Trust	(7) Trustworthy
Religious Education	0.142***	0.129***	-2.216**	-0.060	0.057	0.036	0.139
	(0.039)	(0.040)	(0.765)	(0.064)	(0.098)	(0.032)	(0.134)
Religious Recipient	0.069*	0.054	-	-	-	-	-
	(0.032)	(0.055)					
Religious Education×Religious Recipient	-	0.030	-	-	-	-	-
		(0.049)					
Age	0.004	0.007	-1.957	-0.086	-0.235	0.121	-0.306
	(0.118)	(0.119)	(3.116)	(0.133)	(0.144)	(0.127)	(0.277)
Age^2	0.001	0.001	0.113	0.005	0.010	-0.002	0.007
	(0.005)	(0.005)	(0.119)	(0.005)	(0.006)	(0.005)	(0.009)
Years of Schooling	-0.017	-0.017	-0.305	-0.035*	0.012	-0.031	0.109
	(0.025)	(0.025)	(0.419)	(0.016)	(0.031)	(0.017)	(0.088)
Risk	-	-	-	-	0.053	0.024	-
					(0.081)	(0.068)	
Donation	-	-	-	-	-	0.032	0.410
						(0.104)	(0.368)
Constant	0.084	0.070	50.367**	0.813	1.445	-0.853	2.141
	(0.712)	(0.716)	(20.034)	(0.823)	(0.857)	(0.783)	(1.832)
School Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	146	146	146	146	146	72	74
R-squared	0.168	0.169	0.084	0.107	0.066	0.266	0.179

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. Column 1 and 2 show the relationship between religious education and donations made to orphanages controlling for age, a quadratic in age and years of schooling of children, and school fixed effects. 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 average dice points recorded in the *Cheating Game*. 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 amount received that were sent back to the Trustor. Standard errors clustered at the session level are in parentheses.

schools also do not exhibit in-group bias while making donations. Instead, I find that children from religious schools donated 13 percentage points more from their endowment to secular recipients relative to children from secular schools. Therefore, I do not find any robust evidence to support my *Hypothesis 2*.

3.5.3 Dishonesty

The main purpose of this exercise is to test whether children receiving religious education cheats more or less than that receiving secular education. Therefore, merely comparing the distribution of reports by children coming from the two backgrounds would give me a good idea about whether one is more dishonest relative to the other. Level of cheating in the dice task is shown in Panel B of Table 3.3 both in terms of points recorded by children and the frequency of recording the highest two numbers (5 and 6). I find marginal differences in both, where children from secular schools record higher numbers than children from religious schools (MW-test: $p = 0.051$ (points) and $p = 0.073$ (in recording 5s and 6s)). Using OLS regression, I regress dice points recorded on school type while also controlling for age, quadratic of age, years of schooling, and school dummies which is presented in Column 3 in Table 3.4. I find that religious

children); the interaction term gives me the difference-in-difference, where the first difference is donations received by religious and by secular recipients from religious school children, whereas the second difference is the same but from secular school children. To obtain the first difference (which gives me the in-group bias among religious school children), I simply added 'Religious Recipient' and 'Religious Education×Religious Recipient'.

students recorded 2.2 fewer points on average than secular students which is significant at the 5% level.[21] This result confirms my *Hypothesis 3*.

3.5.4 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 3.3. I also find no significant difference in risk aversion between children from both school types (MW-test: $p = 0.275$; Panel C in Table 3.3). When I regress both on school type while also controlling for age, quadratic of age, years of schooling, risk aversion (only in specification 5), and school dummies, I find no significant effects (Column 4 for Risk Aversion and Column 5 for Cooperation in Table 3.4). Although these are OLS estimates, running a probit/logit regression for cooperation (Column 5) also does not change my results. For simplicity, I only present the OLS estimates. Therefore, *Hypotheses 4* and *5* do not hold.

3.5.5 Trust

Through the *Trust* game, I measure both trust and trustworthiness among children. Panel E in Table 3.3 shows the raw transfers made by trustors and trustees. Comparing mean transfers show that neither trust nor trustworthiness differs across children from different educational background (MW-test: $p = 0.230$ and $p = 0.668$ respectively). Using OLS regression, I regress trust on school type (Table 3.4; 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. None of the controls significantly predicts trust at the conventional levels. Similarly, when I regress back transfers (i.e. trustworthiness) on religious education with the usual control variables (excluding risk aversion, as risk is not involved in back transfers), I find no significant effect (presented in Table 3.4; Column 7). Overall, I find no strong evidence to support my *Hypothesis 6*.

3.5.6 Years of Education and Economic Behavior

Does spending relatively more time in school and learning intensively from curriculum affects behavior and preferences? To explore this I take advantage of information on years of schooling, which is the difference between a student's current age and the age when they joined their school. I divide my sample into two parts: those who received at most 5 years of education (i.e. completed primary) and those who received 6 or more years of education in their current schools. Mean comparisons of behavior

[21] To check the robustness of my results, I also regress the 'frequency of 5s and 6s' on the aforementioned regressors and find that children from religious schools were roughly 6 percentage points less likely to record 5s and 6s in the dice task relative to children from secular schools. Using a probit model also does not change the result.

show that children who have received 6 or more years of education in religious schools are more altruistic, cooperative, and trusting relative to children who have received 5 or less years of education in religious schools (MW-test p-values=0.016, 0.014 and 0.014 respectively). For children in secular schools, more education leads to recording more dice points and highest two numbers in a private task, suggesting more dishonesty among such children (MW-test p-values<0.01 for both). Furthermore, trust among children who have received more education is also marginally higher than those who have had less education (MW-test p-values=0.09). However, in terms of risk preference and trustworthiness, I do not observe any difference between more and less educated children. These test results are shown in Table B1 in Appendix B.

Using an OLS regression, I regress behaviors on school type and a dummy variable 'More' (equals 1 if a child has obtained 6 or more years of education and 0 otherwise) and their interaction while also controlling for age, quadratic of age, risk aversion (in specifications 4 and 5 only), donation (in specifications 5 and 6 only) and school dummies. Table B2 in Appendix B contains the regression results. I find that children from religious schools who have had 6 or more years of education are more charitable, honest, risk averse, and cooperative relative to children from secular schools who have had the same level of schooling (joint test of 'Religious Education' and the interaction: F-test p-values are 0.02, 0.02, 0.01, and 0.05 respectively). However, for trust and trustworthiness there are no such differences (F-test p-values are 0.49 and 0.57 respectively). Although, children who have had less than 6 years of schooling are marginally more trustworthy in religious schools relative to that in secular schools. Therefore, most of the behavioral differences seem to be driven by children who have had 6 or more years of schooling, suggesting the source of difference in behaviors and preferences between children from religious and secular schools is the duration of exposure to distinct educational regimes as learning from the curriculum and social environment becomes more intensive over time. I show this relationship in Figure B2 in Appendix B.

3.6 Robustness Tests

3.6.1 Multiple Hypothesis Testing

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 hypothesis 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. are due to Type I error). Therefore, to address this concern, I re-do the regressions reported in Table 3.4 using the Westfall-Young adjustment that uses bootstrap resampling (with 10,000 replications) to account for correlations across different outcomes (Westfall & Young, 1993). I find that the coefficient 'Religious Education' in specifications 1 and 3 remain statistically significant with small increases in p-values (p-value<0.01 for specification 1 and p-value=0.038 for specification 3). Likewise, insignificant effects from the remaining

specifications stay unchanged. Furthermore, I also compute Bonferroni adjusted p-values, which further strengthens the result obtained in specification 1 (p-value=0.02) but it fails to reach significance in specification 3 (p-value=0.19). This finding indicates that religious education has some effect on economic behavior, as results reported in this study are not simply consequences of multiple hypotheses testing.[22]

3.6.2 Are Behavioral Differences Driven by Selection?

Admission data of children in this study have many limitations. For instance, I do not have any information on who admitted a child to a baby home, at what age were they admitted or what criteria baby homes or orphan schools followed while admitting a child to a school and so on. Therefore, an orphan child could be sent to a certain school based on their level of agility, academic merit, or even based on (baby home) carers' preferences or characteristics. If this is true then it is likely that religious carers/authorities at baby homes would bring up their children under a religious environment and are likely to send them to religious schools, which would overstate the impact of religious education in my study. In other words, under this scenario, children who are sent to religious schools are going to be more prosocial, trusting, risk averse, and honest during their early years of schooling relative to children from secular schools. Therefore, in the presence of such selections, I would observe a sharp difference in behaviors among children who are very young and in their early years of schooling, suggesting the fact that behavioral differences captured are due to selection (Delavande & Zafar, 2015). However, if I find no such differences among younger children then that would suggest that selection is not a key driver for the results I have reported. I carry out this simple robustness analysis by, first, making raw comparisons of means for young children (below the age 11) and then using regression analysis I check whether younger children are causing any behavioral differences. I choose 11 as the cut-off age following Piaget (1952)'s theory on cognitive stages of child development, which states that the major developmental changes among children occur when they are below the age of 11. Likewise, Fehr, Bernhard & Rockenbach (2008) finds that other-regarding preferences start developing at the age of 7-8, which is why exploring behaviors of children who are above 7 and below 11 would suffice.

Using 11 as the cut-off age to distinguish younger and older children, where children that are less than 11 years old are considered young, I compare mean differences in behavior between old and young children within school types (Table B3 in Appendix B) and then again compare mean differences in behavior within old and young children between school types (Table B4 in Appendix B). While making the 'young vs. young' comparison across school types, all but one MW-test p-values confirm that children were similar in their early years of schooling.[23] Later, when I regress all six behaviors

[22] Also, my null findings are not a consequence of small sample size as I carried out power analyses both prior to data collection and then after. I computed sample size both under individual and cluster randomisation following McConnell & Vera-Hernández (2015) and found that a sample of 146 is sufficient.

[23] I find that only in terms of risk aversion the difference reaches statistical significance.

TABLE 3.5: Are Younger Children Different?

VARIABLES	(1) Donation	(2) Dice Points	(3) Risk Aversion	(4) Cooperation	(5) Trust	(6) Trustworthy
Religious Education	0.105*	-5.080**	-0.219***	0.053	0.056	0.074
	(0.053)	(1.964)	(0.052)	(0.067)	(0.046)	(0.144)
Young	-0.109	-1.579	-0.114	-0.034	-0.032	-0.023
	(0.094)	(2.976)	(0.086)	(0.129)	(0.066)	(0.111)
Religious Education×Young	0.032	7.164	0.407***	-0.083	-0.059	0.575
	(0.096)	(4.054)	(0.135)	(0.105)	(0.120)	(0.503)
Observations	146	146	146	146	72	74
R-squared	0.148	0.112	0.168	0.046	0.210	0.184

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. Young is a dummy variable which equals 1 if a subject is less than (<) 11 years old, and 0 otherwise (i.e. Old). The dependant variable in Column 1 is the proportion of endowment donated. The dependent variable in Column 2 is Dice Points, which is the average points recorded in the Cheating Game. The dependent variable in Column 3 is Risk Aversion which is the proportion of endowment invested in a risky lottery. The dependent variable in Column 4 is Cooperation, which equals 1 if the individual decided to cooperate and 0 otherwise. The dependent variable in Column 5 is Trust, which is the proportion of endowment sent to the trustee. The dependent variable in Column 6 is Trustworthiness, which is the proportion of amount received that were sent back to the Trustor. Standard errors clustered at the session level are in parentheses; All regressions include the same controls as in Table 3.4.

on school type and a dummy variable indicating whether a child is 'Young' (equals 1 or 0 otherwise) and their interaction while also controlling for years of schooling and school dummies, I find that the addition of coefficients for 'Religious Education' and the interaction term (which gives me the difference in behavior of young children between religious and secular schools) in all specifications are statistically insignificant (Wald-test p-values$>$ 0.10 for all). This suggests that behavioral differences might not haven been driven by children who are younger than 11 years, weakening the notion that children might be different during their early years and, hence, might have been positively selected into religious schools. I report these results in Table 3.5. However, it should be noted that the sample size in the 'young vs. young' comparison is only 45 (19 in religious and 26 in secular schools), so I cannot entirely rule out the possibility that there was selection into school types at a younger age.

3.6.3 Does this Field Setting Minimize Selection?

It is very crucial for the integrity of this study that this field setting is successful at reducing selection. If the inclusion of *family* children in the analysis has no effect on my results then that would mean studying only *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 vary. Table B5 in Appendix B shows the differences. I find some raw evidence that *family* children are more altruistic, less dishonest, and are more trusting towards their peers than *non-family* children, with all differences being significant at the 1% level using a MW-test (under the 'Pooled' column). When I spilt my data to separate religious and secular school children, I find that such differences can only be observed within religious schools whereas in secular schools *family* and *non-family* children are not so different. This provides support to

TABLE 3.6: Does Inclusion of the Family Children Sample Overstate the Impact?

VARIABLES	(1) Donation	(2) Dishonesty	(3) Risk Aversion	(4) Cooperation	(5) Trust	(6) Trustworthy
Religious Education	0.134***	-3.479**	-0.124**	0.017	0.055*	0.072
	(0.033)	(1.383)	(0.046)	(0.061)	(0.024)	(0.078)
Family	-0.030	-2.800	-0.011	0.151	0.107*	-0.036
	(0.048)	(1.805)	(0.047)	(0.089)	(0.046)	(0.124)
Religious Education×Family	0.192***	-0.152	0.020	-0.111	0.152	-0.016
	(0.062)	(2.304)	(0.075)	(0.128)	(0.098)	(0.145)
Observations	210	210	210	210	105	105
R-squared	0.197	0.111	0.090	0.055	0.309	0.143

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. The dependant variable in Column 1 is the proportion of endowment donated. The dependent variable in Column 2 is Dishonesty, which is the average points recorded in the *Cheating Game*. The dependent variable in Column 3 is Risk Aversion which is the proportion of endowment invested in a risky lottery. The dependent variable in Column 4 is Cooperation, which equals 1 if the individual decided to cooperate and 0 otherwise. The dependent variable in Column 5 is Trust, which is the proportion of endowment sent to the trustee. The dependent variable in Column 6 is Trustworthiness, which is the proportion of amount received that were sent back to the Trustor. Family is a dummy that equals 1 if a child was abandoned by their parents and 0 otherwise. Standard errors clustered at the session level are in parentheses; All regressions include the same controls as in Table 3.4.

the notion that children in religious schools coming from a religious family background must have already been taught to be more charitable, honest, and trusting prior to being abandoned. Hence, there is some evidence for positive selection of children into religious schools.

To ensure that my results are robust to the inclusion of control variables, I regress behaviors on schooling type and a dummy variable for *family* children (equals 1 if *family* and 0 otherwise) and their interactions while also controlling for age, quadratic of age, years of schooling and school dummies. This result is presented in Table 3.6. The coefficient of 'Family' gives me the effect of *family* relative to *non-family* children who study in secular schools. I find that this is marginally significant and positive only for trust, suggesting *family* children are slightly more trusting than *non-family* children in secular schools, although this is inconsistent with what I found during the raw comparison of means. Similarly, adding 'Family' with the interaction term gives me the difference in effect between *family* and *non-family* children who study in religious schools. I find the coefficient to be positive and statistically significant (F-test p-value=0.01), suggesting *family* children trusts more than *non-family* children in religious schools. Similarly, for donation and dice points I also find this difference among religious school children to be statistically significant (F-test p-value<0.01 for donation and p-value=0.09 for dice points). For the rest, I do not find any significant differences. Therefore, my regression results also suggest that inclusion of *family* children in the analysis would have overstated the impact of religious education on altruism by almost 19 percentage points, dishonesty by almost 3 dice points, and trust by almost 26 percentage points.

I, then, combine my tests from subsection 3.6.2 with subsection 3.6.3 to do a subsample analysis in order to check whether there are any differences in behavior and preferences between *family* and *non-family* children who are young (< 11 years). Tables B6 in Appendix B reports the regression results respectively. Among young children who

attend secular schools, I find that *family* children are significantly more altruistic and risk-averse than *non-family* children, whereas for the rest I do not find any statistically significant effects.[24] Similarly, among young children who attend religious schools, I find that *family* children are significantly more altruistic, honest, and trusting towards others relative to *non-family* children (F-test p-values=0.003, 0.007, and 0.099 respectively).[25] Moreover, within the *family* children sample, I find that young children in religious schools are more altruistic, honest, and trusting toward others than young children in secular schools (F-test p-values=0.010, 0.041, and 0.040 respectively), which could be due to positive selection into religious schools by religious parents. Since children who are *non-family* and young are not different (as concluded in subsection 3.6.2) whereas *family* children seem to be different since a very young age, this provides further support to my argument that behavioral differences reported in this study might not be driven by selection after all.

3.6.4 Neighborhood Effects

Although orphan schools are very much segregated from everyday social activities (a key characteristic of my field setting), it is still likely that children might occasionally interact with outsiders (e.g. at mosques). For instance, if a region, e.g. a subdistrict, is very religious then children might also be learning from their neighborhood given they occasionally interact with the outside world. If this is true then not controlling for it in the regression might bias our findings. To attenuate such concerns, I obtained the data on the number of religious institutions (i.e. mosques, temples, churches, etc.) available in subdistricts where schools are located from the Bangladesh Bureau of Statistics (Bangladesh Bureau of Statistics, 2011).[26] I construct a variable that gives the number of religious institutions per person in a subdistrict so that a higher value would correspond to more institutions per person in the subdistrict and hence would be a good proxy for the level of religiosity in a neighborhood. I control for this along with other controls from Table 3.4. My main effects remain unchanged with no significant effect from this new control, suggesting religiosity of a neighborhood has no effect on the formation of behavior and preferences of children in this study. In a similar fashion, I also construct the proportion of Muslim population in a neighborhood to test if living in Muslim concentrated regions have any effect on behaviors and preferences of children. Again I find no statistically significant effects, suggesting children do not learn from their neighborhood as all learning happens within the school territory. I present these regression results in Tables B7 and B8 in Appendix B.

[24] Coefficients of 'Family' gives me the difference between *family* and *non-family* children in secular schools.

[25] Adding the coefficients of 'Family' and the interaction term gives me the difference between *family* and *non-family* children in religious schools.

[26] 2011 is the latest available data.

3.7 Conclusion

In this study, I exploit a natural setting in Bangladesh to show that religious education affects economic behavior. In this field setting, children are taught different educational curriculum and grow up in different social environments that differ in terms of intensity of religiosity. Moreover, information on who admitted a child to a certain school allows me to explore whether that child had spent his pre-schooling years with his family (*family*) or whether he grew up in a non-family environment (*non-family*). By taking advantage of this information, I reduce some effects of selection into schools that are only based on individual and family preferences or characteristics. Using a battery of experiments, I measure children's level of altruism, dishonesty, risk aversion, cooperation, trust, and trustworthiness. Eventually, I reach to three main conclusions. Firstly, children receiving religious education are more altruistic and honest towards others. Secondly, religious education does not have any impact on risk preference, cooperation, trust, and trustworthiness. Finally, children who have spent relatively more years in schooling drives all the differences suggesting the fact that spending more time in learning from distinct curriculum and social environment must have shaped their behavior differently. Through a series of robustness checks, I also show that behavioral differences reported in this study are not driven by selection. Moreover, I also report that the inclusion of *family* children in the study would have overstated the impact of religious education.

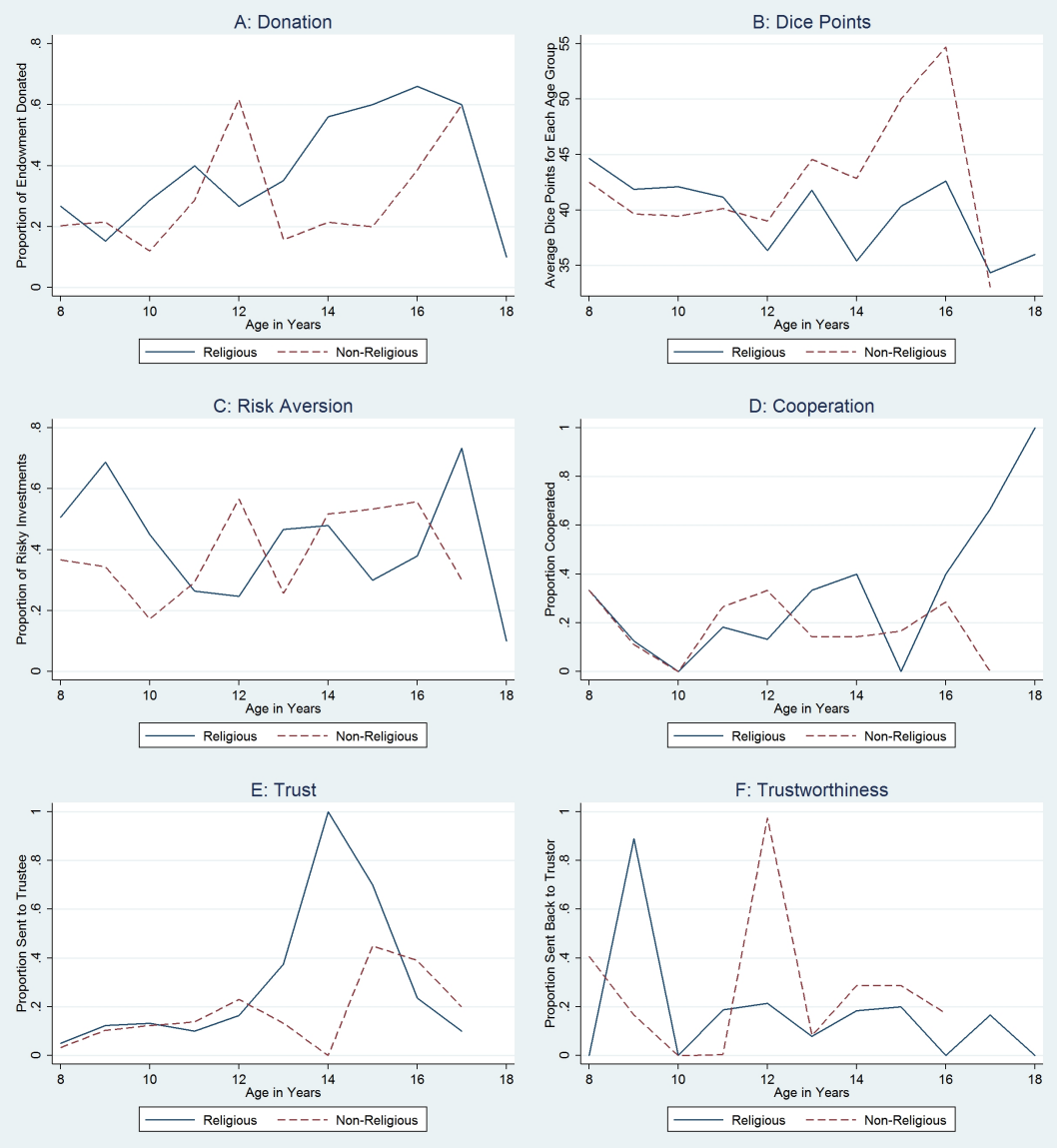
One implication of my findings is that religious education has important features that can shape how people make important economic decisions. This is crucial for education policy-makers as taking such important features from the religious curriculum and introducing it to the secular curriculum would improve economic decision making and outcomes – a crucial element for economic development. Also, with a growing number of religious schools in South Asia as well as in other parts of the world, how children are being educated and in what social environment they grow up is also important as it affects how such individuals would make economic decisions as adults. Therefore, investing in curriculum design, teachers training, subsidizing schools to improve social environment, and so on should be another important focus for policy-makers.

While some of my findings are in accordance with existing studies, in other cases I fail to replicate existing evidence. Furthermore, one might question the external validity of this study as I only study a specific type of school and then sampled a small number of schools, so these findings might not be generalized among all religious and secular schools around the globe as several other crucial factors might vary across schools, societies, and regions. However, the main purpose of this research was to show that reducing selection bias is possible in a natural environment when natural experiments are unavailable and conducting large-scale field experiments are costly. A major limitation of this paper is the limited nature of admission data obtained from schools. However, this paper uses a more robust identification strategy than many other existing studies that look at the effect of long-term exposure of religiosity over several years. A good way to

corroborate this evidence would be by either exploring similar questions or studying other consequences of schooling by exploiting a much detailed admission data. Such future replications and explorations would better inform education policy-makers who are willing to redesign education curriculum.

Appendix B: Supplementary Tables and Figures

FIGURE B1: Change in Behavior Over Age



Note: Each graph shows the average outcome for each age group.

TABLE B1: More vs Less Years of Schooling: Within Education Type Comparisons

	Religious			**Secular**		
	More [N] (*Std. Dev.*)	**Less [N]** (*Std. Dev.*)	**MW-Test** ***p*-values**	**More [N]** (*Std. Dev.*)	**Less [N]** (*Std. Dev.*)	**MW-Test** ***p*-values**
Panel A: Donation Game						
% Donations	0.43 [34] (*0.31*)	0.30 [37] (*0.29*)	0.016	0.27 [31] (*0.27*)	0.26 [44] (*0.28*)	0.722
% Donations To Religious Orph.	0.46 [18] (*0.30*)	0.36 [17] (*0.28*)	0.256	0.31 [19] (*0.28*)	0.30 [17] (*0.34*)	0.538
% Donations To Non-Religious Orph.	0.40 [16] (*0.32*)	0.25 [20] (*0.30*)	0.014	0.21 [12] (*0.25*)	0.23 [27] (*0.24*)	0.676
Panel B: Investment Game						
% Invested	0.36 [34] (*0.23*)	0.45 [37] (*0.35*)	0.551	0.43 [31] (*0.33*)	0.33 [44] (*0.29*)	0.128
Panel C: Cheating Game						
Points in Dice Task	38.88 [34] (*6.18*)	40.76 [37] (*5.55*)	0.154	46.68 [31] (*8.31*)	40.07 [44] (*8.17*)	0.002
% of 5s & 6s	0.43 [34] (*0.16*)	0.47 [37] (*0.16*)	0.324	65 [31] (*0.25*)	0.45 [44] (*0.24*)	0.001
Panel D: Prisoner's Dilemma Game						
% Cooperated	0.35 [34] (*0.49*)	0.11 [37] (*0.31*)	0.014	0.19 [31] (*0.40*)	0.18 [44] (*0.39*)	0.900
Panel E: Trust Game						
% Trust	0.35 [13] (*0.33*)	0.15 [23] (*0.23*)	0.014	0.24 [11] (*0.23*)	0.12 [25] (*0.13*)	0.089
% Trustworthiness	0.17 [21] (*0.19*)	0.26 [14] (*0.71*)	0.246	0.35 [20] (*0.67*)	0.15 [19] (*0.26*)	0.385

Note: More is a dummy variable which equals 1 if a subject has obtained 6 or more years of schooling, and 0 otherwise (i.e. Less). % 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; N is the sample size. Two sided Mann-Whitney U test p-values have been reported.

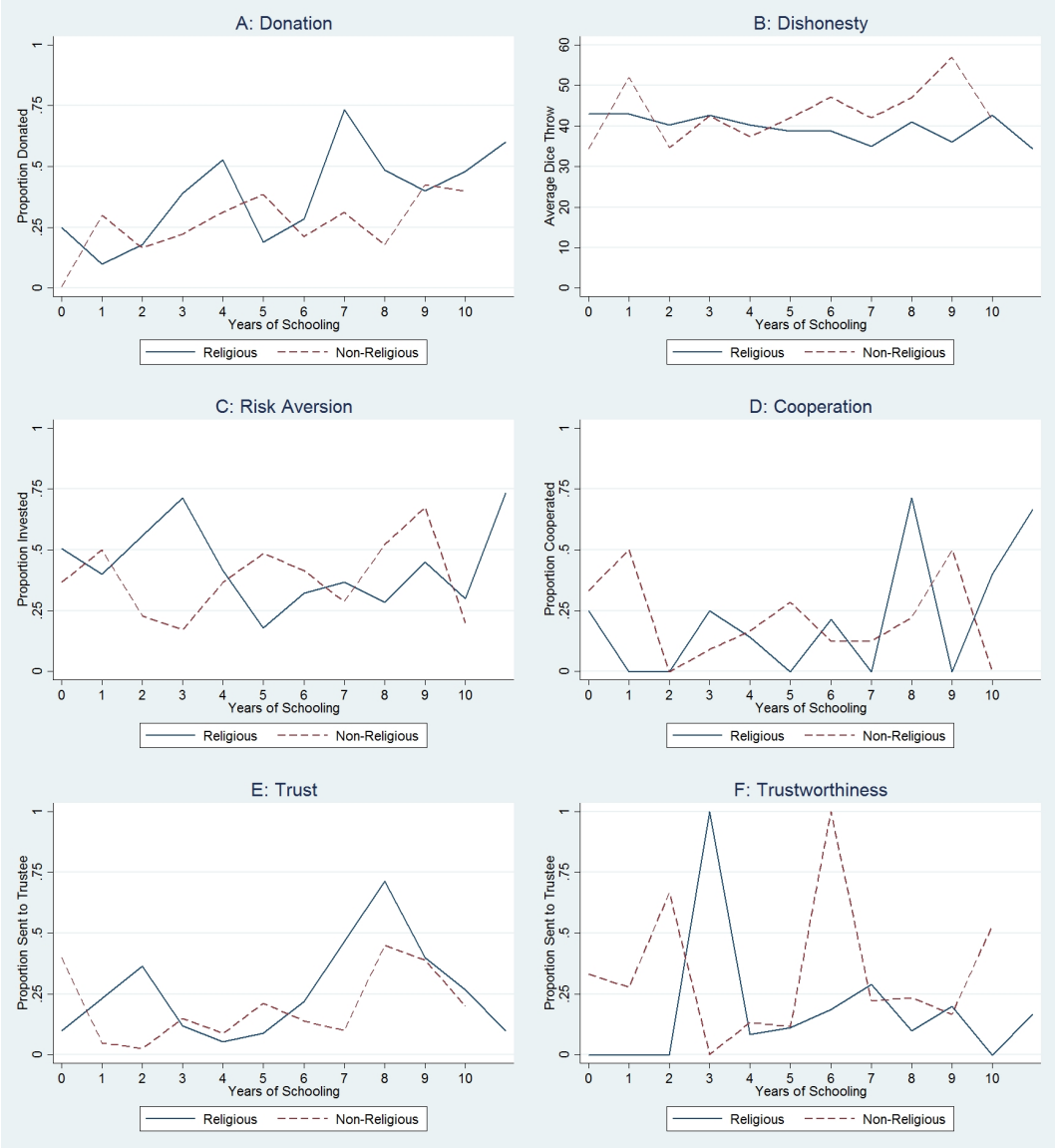
TABLE B2: Regression Analysis: More vs Less Years of Schooling

VARIABLES	(1) Donation	(2) Dice Points	(3) Risk Aversion	(4) Cooperation	(5) Trust	(6) Trustworthy
Religious Education	0.117***	0.394	0.008	-0.039	-0.001	0.421*
	(0.036)	(0.722)	(0.098)	(0.115)	(0.049)	(0.200)
More	-0.192	6.911*	0.081	-0.071	0.035	0.697
	(0.111)	(3.269)	(0.110)	(0.134)	(0.068)	(0.465)
Religious Education×More	0.111	-8.640**	-0.222*	0.248*	0.076	-0.590
	(0.109)	(3.653)	(0.111)	(0.117)	(0.128)	(0.367)
Age	0.024	-3.719	-0.140	-0.205	0.104	-0.341
	(0.128)	(2.408)	(0.132)	(0.142)	(0.159)	(0.299)
Age^2	0.001	0.151	0.006	0.009	-0.003	0.010
	(0.005)	(0.096)	(0.005)	(0.006)	(0.006)	(0.011)
Risk Aversion	-	-	-	0.089	0.039	-
				(0.082)	(0.059)	
Donation	-	-	-	-	0.018	0.400
					(0.118)	(0.384)
Constant	-0.083	62.521***	1.152	1.276	-0.658	2.439
	(0.791)	(15.255)	(0.828)	(0.837)	(0.944)	(1.913)
School Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	146	146	146	146	72	74
R-squared	0.177	0.158	0.126	0.088	0.264	0.248

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. The dependant variable in Column 1 is the proportion of endowment donated. The dependent variable in Column 2 is Dice Points, which is the average points recorded in the Cheating Game. The dependent variable in Column 3 is Risk Aversion which is the proportion of endowment invested in a risky lottery. The dependent variable in Column 4 is Cooperation, which equals 1 if the individual decided to cooperate and 0 otherwise. The dependent variable in Column 5 is Trust, which is the proportion of endowment sent to the trustee. The dependent variable in Column 6 is Trustworthiness, which is the proportion of amount received that were sent back to the Trustor. Standard errors clustered at the session level are in parentheses.

FIGURE B2: Change in Behavior Over Years of Schooling



Note: Each graph shows the average outcome for each years of schooling.

TABLE B3: Old vs Young Children: Within Education Type Comparisons

	Religious			**Secular**		
	Old [N] (*Std. Dev.*)	**Young [N]** (*Std. Dev.*)	**MW-Test *p*-values**	**Old [N]** (*Std. Dev.*)	**Young [N]** (*Std. Dev.*)	**MW-Test *p*-values**
Panel A: Donation Game						
% Donations	0.41 [52] (*0.32*)	0.23 [19] (*0.19*)	0.033	0.31 [49] (*0.31*)	0.17 [26] (*0.16*)	0.082
% Donations To Religious Orph.	0.44 [27] (*0.32*)	0.30 [8] (*0.11*)	0.415	0.35 [28] (*0.33*)	0.14 [08] (*0.08*)	0.034
% Donations To Non-Religious Orph.	0.38 [25] (*0.33*)	0.17 [11] (*0.22*)	0.022	0.25 [21] (*0.28*)	0.19 [18] (*0.18*)	0.688
Panel B: Investment Game						
% Invested	0.35 [52] (*0.25*)	0.56 [19] (*0.38*)	0.071	0.42 [49] (*0.32*)	0.28 [26] (*0.26*)	0.032
Panel C: Cheating Game						
Points in Dice Task	38.92 [52] (*5.85*)	42.42 [19] (*5.37*)	0.030	44.16 [49] (*8.26*)	40.23 [26] (*9.38*)	0.055
% of 5s & 6s	0.43 [52] (*0.16*)	0.51 [19] (*0.16*)	0.004	0.56 [49] (*0.26*)	0.48 [26] (*0.27*)	0.237
Panel D: Prisoner's Dilemma Game						
% Cooperated	0.27 [52] (*0.45*)	0.11 [19] (*0.32*)	0.146	0.22 [49] (*0.42*)	0.12 [26] (*0.33*)	0.252
Panel E: Trust Game						
% Trust	0.28 [24] (*0.31*)	0.12 [12] (*0.19*)	0.011	0.19 [22] (*0.18*)	0.10 [14] (*0.14*)	0.042
% Trustworthiness	0.16 [28] (*0.19*)	0.38 [7] (*1.00*)	0.162	0.30 [27] (*0.60*)	0.16 [12] (*0.29*)	0.282

Note: Young is a dummy variable which equals 1 if a subject is less than 11 years old, and 0 otherwise (i.e. Old). % 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; N is the sample size. Two sided Mann-Whitney U test p-values have been reported.

TABLE B4: Difference in Behavior among Young and Old Children Across Religious and Secular Schools

	Young vs. Young *p*-value	**Old vs. Old *p*-value**
% Donations	0.294	0.088
% Invested	0.001	0.284
Points in Dice Task	0.190	0.000
% Cooperated	0.916	0.393
% Trusted	0.849	0.601
% Trustworthiness	0.722	0.323

Note: All means are given in Table B3; Two sided Mann-Whitney U test p-values have been reported; each p-value tests the difference in behavior between religious and secular school children within the age category; Old is someone who is 11 years or older; Young is someone who is less than 11 years old.

TABLE B5: Family vs. Non-Family Children

	Pooled			Religious			Non-Religious		
	NF (*Std. Dev.*)	**F** (*Std. Dev.*)	**MW-Test** ***p*-values**	**NF** (*Std. Dev.*)	**F** (*Std. Dev.*)	**MW-Test** ***p*-values**	**NF** (*Std. Dev.*)	**F** (*Std. Dev.*)	**MW-Test** ***p*-values**
Panel A: Donation Game									
% Donations	0.31	0.40	0.002	0.36	0.50	0.001	0.26	0.25	0.939
	(*0.29*)	(*0.25*)		(*0.30*)	(*0.20*)		(*0.27*)	(*0.23*)	
% Donations To Religious Orph.	0.36	0.39	0.228	0.41	0.50	0.05	0.31	0.24	0.427
	(*0.30*)	(*0.25*)		(*0.29*)	(*0.20*)		(*0.30*)	(*0.25*)	
% Donations To Non-Religious Orph.	0.27	0.41	0.003	0.32	0.50	0.003	0.22	0.25	0.497
	(*0.28*)	(*0.24*)		(*0.31*)	(*0.20*)		(*0.24*)	(*0.22*)	
Panel B: Investment Game									
% Invested	0.39	0.43	0.454	0.41	0.44	0.520	0.37	0.40	0.981
	(*0.30*)	(*0.32*)		(*0.30*)	(*0.30*)		(*0.31*)	(*0.34*)	
Panel C: Cheating Game									
Points in Dice Task	41.37	38.47	0.010	39.86	37.38	0.031	42.80	40.16	0.241
	(*7.65*)	(*8.00*)		(*5.90*)	(*5.67*)		(*8.81*)	(*10.59*)	
% of 5s & 6s	0.49	0.42	0.012	0.45	0.38	0.025	0.53	0.49	0.376
	(*0.22*)	(*0.23*)		(*0.16*)	(*0.15*)		(*0.27*)	(*0.31*)	
Panel D: Prisoner's Dilemma Game									
% Cooperated	0.21	0.28	0.230	0.23	0.26	0.715	0.19	0.32	0.166
	(*0.41*)	(*0.45*)		(*0.45*)	(*0.44*)		(*0.39*)	(*0.48*)	
Panel E: Trust Game									
% Trust	0.19	0.39	0.001	0.22	0.51	0.000	0.16	0.23	0.449
	(*0.24*)	(*0.30*)		(*0.28*)	(*0.28*)		(*0.17*)	(*0.27*)	
% Trustworthiness	0.23	0.16	0.936	0.24	0.15	0.728	0.25	0.20	0.552
	(*0.49*)	(*0.23*)		(*0.58*)	(*0.23*)		(*0.52*)	(*0.24*)	

Note: F is for *Family* and NF is for *Non-Family* student sample. % 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 B6: Are Younger Children Different across the Family and the Non-Family Children Sample?

VARIABLES	(1) Donation	(2) Dishonesty	(3) Risk Aversion	(4) Cooperation	(5) Trust	(6) Trustworthy
Religious Education	0.027	-4.877***	0.136	0.096	0.102	0.742***
	(0.052)	(1.519)	(0.219)	(0.136)	(0.109)	(0.162)
Family	0.117*	-3.619	0.149**	0.280	0.078	0.186
	(0.064)	(2.535)	(0.056)	(0.230)	(0.100)	(0.333)
Religious Education×Family	0.191*	-1.883	-0.167	-0.150	0.376	-0.435
	(0.107)	(2.810)	(0.114)	(0.283)	(0.262)	(0.608)
Age	0.141	-16.681	1.717	-0.783	0.737	2.063
	(1.053)	(32.607)	(1.904)	(2.232)	(1.641)	(3.750)
Age^2	-0.007	0.798	-0.100	0.039	-0.037	-0.125
	(0.059)	(1.776)	(0.105)	(0.119)	(0.089)	(0.213)
Years of Schooling	-0.023	-0.177	0.006	-0.064	-0.063	0.060
	(0.020)	(1.316)	(0.047)	(0.064)	(0.039)	(0.086)
Risk	-	-	-	-0.019	0.104	-
				(0.149)	(0.086)	
Donation	-	-	-	-	0.181	0.009
					(0.275)	(0.267)
Constant	-0.420	131.767	-6.913	4.164	-3.504	-8.281
	(4.616)	(147.404)	(8.570)	(10.391)	(7.471)	(16.460)
School Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	69	69	69	69	38	31
R-squared	0.397	0.264	0.203	0.179	0.474	0.326

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; Young is someone is who is below the age 11. The dependant variable in Column 1 is the proportion of endowment donated. The dependent variable in Column 2 is Dishonesty, which is the average points recorded in the *Cheating Game*. The dependent variable in Column 3 is Risk Aversion which is the proportion of endowment invested in a risky lottery. The dependent variable in Column 4 is Cooperation, which equals 1 if the individual decided to cooperate and 0 otherwise. The dependent variable in Column 5 is Trust, which is the proportion of endowment sent to the trustee. The dependent variable in Column 6 is Trustworthiness, which is the proportion of amount received that were sent back to the Trustor. Family is a dummy that equals 1 if a child was abandoned by their parents and 0 otherwise. Standard errors clustered at the session level are in parentheses.

TABLE B7: Regression Analysis of the Effect of Religious Education: Religiosity of a Neighborhood as a Control

VARIABLES	(1) Donation I	(2) Donation II	(3) Dishonesty	(4) Risk	(5) Cooperation	(6) Trust	(7) Trustworthy
Religious Education	0.146***	0.132***	-2.326**	-0.061	0.068	0.033	0.103
	(0.039)	(0.041)	(0.785)	(0.061)	(0.094)	(0.034)	(0.131)
Religious Recipient	0.069*	0.054	-	-	-	-	-
	(0.032)	(0.055)					
Religious Education×Religious Recipient	-	0.030	-	-	-	-	-
		(0.049)					
Religious Institutions	-8.192	-7.930	269.144	2.442	-26.321	9.346	86.584**
	(18.657)	(18.093)	(643.956)	(26.600)	(22.382)	(7.538)	(24.637)
Constant	0.101	0.086	49.825**	0.808	1.498	-0.872	1.966
	(0.733)	(0.737)	(20.200)	(0.806)	(0.885)	(0.788)	(1.791)
School Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	146	146	146	146	146	72	74
R-squared	0.168	0.169	0.084	0.107	0.066	0.266	0.179

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. 'Religious Institutions' is the number of religious institution per person that is present in a region; Column 1 and 2 show the relationship between religious education and donations made to orphanages controlling for age, a quadratic in age and years of schooling of children, and school fixed effects. 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 average dice points recorded in the *Cheating Game*. 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 amount received that were sent back to the Trustor. Standard errors clustered at the session level are in parentheses.

TABLE B8: Regression Analysis of the Effect of Religious Education: Muslim Population in a Neighborhood as a Control

VARIABLES	(1) Donation I	(2) Donation II	(3) Dishonesty	(4) Risk	(5) Cooperation	(6) Trust	(7) Trustworthy
Religious Education	0.158***	0.144**	-2.715*	-0.064	0.106	0.019	-0.022
	(0.047)	(0.053)	(1.357)	(0.067)	(0.086)	(0.043)	(0.127)
Religious Recipient	0.069*	0.054	-	-	-	-	-
	(0.032)	(0.055)					
Religious Education×Religious Recipient	-	0.030	-	-	-	-	-
		(0.049)					
Muslims	-3.087	-2.989	101.429	0.920	-9.919	3.522	32.630**
	(7.031)	(6.819)	(242.681)	(10.024)	(8.435)	(2.841)	(9.285)
Constant	1.564	1.503	1.738	0.372	6.201	-2.542	-13.504***
	(3.803)	(3.713)	(119.958)	(4.592)	(4.613)	(1.810)	(3.116)
School Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	146	146	146	146	146	72	74
R-squared	0.168	0.169	0.084	0.107	0.066	0.266	0.179

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. 'Muslims' is the proportion of Muslim population in a region; Column 1 and 2 show the relationship between religious education and donations made to orphanages controlling for age, a quadratic in age and years of schooling of children, and school fixed effects. 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 average dice points recorded in the *Cheating Game*. 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 amount received that were sent back to the Trustor. Standard errors clustered at the session level are in parentheses.

Details on the Experimental Design

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 (*feriwalas*), 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.[27]

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.[28] Subjects had to fill in those 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

[27]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 explored in this study.

[28]Please see the instructions for chapter 3.

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.[29]

[29]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.

Chapter 4

Discrimination in the Agricultural Market: The Case of Bangladeshi Rice Farmers

4.1 Introduction

The agricultural sector is crucial for creating employment, food production, fighting poverty, and fostering economic development around the world (World Bank, 2015a). Countries during the early stages of development predominantly rely on the agricultural sector in terms of income and labor force participation, which serves as a major player in its subsequent structural transformations and poverty reduction (Johnston & Mellor, 1961; Schultz, 1964; Christiaensen, Demery & Kuhl, 2011; Irz et al., 2001; Herrendorf, Rogerson & Valentinyi, 2014; McArthur & McCord, 2017). Moreover, agricultural technologies, such as adopting improved crop varieties, have been successful at reducing poverty across many developing countries (Minten & Barrett, 2008; Becerril & Abdulai, 2010; Kassie et al., 2011). Despite being a key sector in promoting economic growth and development and reducing poverty, agricultural farmers continue being some of the poorest in the world (IFAD, 2014, 2016). Farmers in developing countries still face numerous challenges to make a living, such as accessing markets, information, technologies, etc. (Chambers & Ghildyal, 1985; Altieri, 2002), whilst farmers from disadvantaged social groups are also subject to social, political, and economic exclusions, and still continue to face unfair treatment that limits their access to various opportunities and resources (Curtis, 2009; AIPP, 2010; IFAD, 2016). Agriculture has proved to be an effective tool to improve well-beings of individuals and escape poverty, yet we do not know enough about why ethnic minorities, such as indigenous groups, continue being one of the poorest and the most vulnerable social groups around the world (IFAD, 2016; United Nations, 2018). One explanation is that they are severely discriminated against in the agricultural sector.[1] Literature in social science suggest that discrimination, exploitation, and extortion

[1]Ethnic and racial discrimination is a deep-rooted phenomenon in our society and has been widely studied in economics and social sciences to determine its nature and consequences. For reviews, see

of ethnic minority farmers by the dominant majority population exists (AIPP, 2010); however, social researchers have largely relied on survey measures of unfair treatment of ethnic minority farmers. This might be problematic as other unobservable characteristics, such as farming skills or rice quality, might also be correlated with ethnicity. Therefore, the two major questions that still remain unanswered are that whether there is ethnic discrimination in the agricultural market when factors other than the ethnic identity of farmers remain constant and, if so, why.

In this paper, we investigate discrimination towards ethnic minority farmers and explore its underlying mechanisms through a field experiment that we carried out in Bangladesh. Bangladesh is a suitable field for this study due to its dependence on the agricultural sector, where 41 percent of its total labor force depends on agriculture for livelihood (Bangladesh Bureau of Statistics, 2017). Furthermore, Bangladesh is also home to 45 different indigenous communities who are mostly swidden agriculturists and sedentary rice farmers, and, according to survey results, are also severely discriminated in everyday life by the ethnic majority, Bengali, population (AIPP, 2010; Roy, 2012; IMF, 2013; Chakma & Maitrot, 2016). To measure discrimination, we organized a competition among rural rice farmers where the winner was determined based on the quality of rice produced and the potential price at which one could sell the rice to buyers. Following rice collection from participants, we recruited ethnic majority rice buyers (i.e. people who buy rice from farmers to sell it in the market) to evaluate the physical quality of rice and state how much they would be willing to pay for a kilogram of that rice. These two assessment outcomes determined the winner of the rice competition. To test for discrimination, on each rice sample given to buyers for assessment, instead of revealing the actual name of participants, we randomly assigned ethnic majority and minority sounding names on each sample to implicitly reveal the farmer's ethnic identity to buyers. This way, we break any systematic relationship that the ethnicity of farmers might have with the quality of rice they produce, and, thus, finding any association between ethnicity and assessment outcomes would be documenting discrimination.

In recent studies on discrimination, manipulation of perceived group identity (e.g. race, ethnicity, etc.) through names have been successful in documenting discrimination in the field setting. For instance, Bertrand & Mullainathan (2004) randomly assigns African-American and White-sounding names to resumes to capture discrimination in the labor market. Likewise, among many other subsequent studies, Zussman (2013) uses manipulation of names in email inquiries to capture ethnic discrimination in the used car market; Giulietti, Tonin & Vlassopoulos (2017) use distinctly Black and White sounding names in emails to document racial discrimination in public service delivery; Ahmed & Hammarstedt (2008), Ewens, Tomlin & Wang (2014) and Edelman, Luca & Svirsky (2017) capture discrimination in long-term and short-term housing rental markets using a similar methodology; Carlsson & Rooth (2007), Booth, Leigh & Varganova (2012) and

Altonji & Blank (1999), Riach & Rich (2002), Pager (2007), Guryan & Charles (2013), Bertrand & Duflo (2017), and Neumark (2018).

Kaas & Manger (2012) also manipulate names on resumes to capture ethnic discrimination in labor markets across different countries; likewise, Siddique (2011) captures caste-based discrimination using the same methodology. Other correspondence studies that randomize other characteristics through different experimental designs, such as caste (Hanna & Linden, 2012), gender (Lahey, 2008), criminal records (Pager, 2003), immigrant status (Oreopoulos, 2011), sexual orientation (Ahmed & Hammarstedt, 2009), attending for-profit institutions for degree (Deming, Yuchtman, Abulafi, Goldin & Katz, 2016), long unemployment history (Ghayad, 2013), and so on, have also been successful at capturing discrimination based on such characteristics. Therefore, what field correspondence studies do is that it keep characteristics other than race, ethnicity, caste, etc. of all individuals statistically the same throughout, so that any observed differences in treatment between the two varying characteristics (e.g. different ethnicities) would be evidence of discrimination. Also, it captures discrimination in the actual market by measuring the behavior of agents making real decisions.

In field experiments, discrimination is either measured using auditors (single-blinded experiments, as in Fix & Turner (1998) and Gneezy, List & Price (2012)) or by correspondence tests (double-blinded experiments, as in Bertrand & Mullainathan (2004) and Banerjee, Bertrand, Datta & Mullainathan (2009)). Since auditors in single-blinded studies are aware of the study purpose, they are likely to behave in a biased way to fit their audit agencies' goals and, thus, it might lead to superfluous findings (Heckman & Siegelman, 1993; Heckman, 1998). In comparison, double-blinded studies do not possess such threats to internal validity. In an actual marketplace, List (2004) takes advantage of both audit studies and double-blindness to capture discrimination against non-Whites, women, and elderly agents. In another field study that deviates from the conventional audit and correspondence studies, Hanna & Linden (2012) uses an exam competition in India to determine discrimination in grading against low-caste children. It measures behavior of actual teachers making real decisions that have factual consequences. Our study also possesses such advantages. Firstly, in the actual market, buyers evaluate and set reservation prices to rice prior to buying it from farmers; so, in our experiment, they also do the same during the rice evaluation program. Secondly, their decisions have factual consequences since the farmer with the highest assessment score receives a monetary reward. Thirdly, buyers are unaware of being part of an experiment and, hence, behave in the same way as they would behave in the real market. Finally, research assistants were not informed about the true nature of this study and, hence, worked for a rice evaluation program. This ensures that they do not imply the true purpose of this study to buyers neither during advertisement nor during the experiment, which makes it double-blinded.

In our study, randomizing names were sufficient to signal buyers about ethnic identities of farmers because, in Bangladesh, ethnic minorities either have tribe or clan patronyms – surnames that are named after their tribes or septs (Risley, 1891), whereas ethnic majority Bengalis are mostly Muslims with names that are very different from

that of ethnic minorities.[2] Findings from our experiment show that ethnic majority rice buyers are willing to pay significantly less for rice that had ethnic minority sounding names attached. Specifically, buyers are willing to pay 2% less than what they are willing to pay to ethnic majority farmers. However, in terms of quality assessment, we do not find any evidence of discrimination against ethnic minority farmers. Our main finding is in accordance with findings from correspondence studies on job recruitments, such as Carlsson & Rooth (2007), Kaas & Manger (2012), Booth et al. (2012), etc., in which ethnic minorities are discriminated by the recruiters. Our result is also consistent with studies that explore ethnic discrimination using laboratory experiments (Fershtman & Gneezy, 2001; Fershtman et al., 2005). These studies use dictator, ultimatum, trust, etc., games to determine its existence and use their combinations to deduce its nature (Lane, 2016). We also find that discrimination reported overall is entirely driven by buyers from both multi-ethnic and mono-ethnic villages (where buyers and village residents are co-ethnics), whereas city buyers do not discriminate ethnic minority farmers. This finding is in line with findings on (racial) discrimination as reported in Loureiro, Carneiro & Sachsida (2004) and Giulietti et al. (2017), where Blacks are discriminated more in rural than in urban areas.

Furthermore, the buyers' behavior towards ethnic minority farmers is consistent with the *taste-based* model of discrimination (Becker, 1957). Buyers were found to not discriminate against ethnic minority farmers in terms of the quality of rice they produce, suggesting that their judgments are not driven by stereotypes associated with skills and ability of ethnic minority farmers in terms of rice cultivation. Instead, buyers are willing to pay less to ethnic minority farmers that are not driven by holding wrong beliefs about farming skills, rather they might have a preference for not paying higher to ethnic minority farmers. Since our data show that buyers discriminate ethnic minority farmers only in terms of willingness to pay and not in terms of the quality of rice they produce, this certainly isolates *statistical* discrimination from the lot. Moreover, we do a second test to determine the robustness of our initial finding. We investigate whether the order in which rice samples were evaluated have any relationship with the assessment outcomes. We find that assessment outcomes to be constant regardless of the order, suggesting that there is no correlation between the two and, hence, discrimination is likely to be *taste-based*. If discrimination was *statistical*, we would have observed some pattern. For instance, discriminating at the early stages of assessment would mean buyers use the ethnic identity of farmers as a signal to where the quality of a particular rice sample will end up in the yet unknown distribution, whereas discrimination at the end would mean that boredom or fatigue might be inducing them to reduce the amount of time spent evaluating by quickly predicting rice quality based on farmer's ethnic identity. Although, there still remain concerns about *statistical* discrimination being present because other beliefs, such as ethnic minority farmers have weaker bargaining abilities and they tend to accept lower prices or the belief that they have relatively less

[2]For simplicity, we have only used Santal and Bengali sounding names on our rice samples to measure discrimination.

information about market prices of rice are not entirely eliminated. In these cases, the observed discrimination in terms of willingness to pay could be partly explained by the *statistical* model of discrimination.

Having established the existence of ethnic discrimination in the agricultural market, we turn our attention to further investigating reasons behind the discrimination we observe. Firstly, we find that the frequency of interacting with ethnic minority farmers matter for discrimination. For instance, ethnic majority buyers who interact very less with ethnic minority farmers discriminate minority farmers more than those who have more frequent interactions. This result is consistent with the theory and empirical evidence that show how the frequency of intergroup interactions might affect prejudice between majority and minority groups (Allport, 1954; Pettigrew & Tropp, 2006; Rao, 2018; Paluck, Green & Green, 2018). Besides, this result is further supported by our main results where we find discrimination to be more profound in mono-ethnic villages than in multi-ethnic villages, suggesting that having less social interaction have a negative impact on discriminatory attitudes towards out-group members. Interestingly, we do not observe any discrimination in the city, where intergroup interactions are also minimal as in mono-ethnic villages; however, according to Becker (1957), it is very likely that competitive market pressures in the city might have driven out discrimination against ethnic minority farmers. This leads to our final finding, which suggests that ethnic minority farmers would benefit significantly if they sell rice only to the city buyers, as it would help minorities avoid discrimination while also generate a week's additional income every year.

We organize the rest of the paper as follows. After briefly explaining the field background in Section 4.2, we explain our experimental design in Section 4.3. We summarize our results in Section 4.4 and then discuss it further in Section 4.5. Finally, we conclude in Section 4.6 with some policy implications.

4.2 Rice Farmers and Buyers in Bangladesh

Bangladesh is dependent on the agricultural sector, where 41 percent of its total labor force depends on it for livelihood and 75 percent of cultivated crops are rice (Bangladesh Bureau of Statistics, 2017). It is also home to 45 different ethnic minorities who also primarily depend on agriculture for livelihood. These ethnic minorities are different in terms of race and culture, speak a different language, and follow customs and religion that are distinct from that among the ethnic majority (Bengali) population (Roy, 2012). In the Northwestern region (i.e. in Rajshahi and Rangpur divisions), rice farmers living there are mostly plain-land sedentary farmers. Besides, this region is home to the second largest ethnic minority community, Santal, who are also primarily agriculturalists. Both ethnic majority and minority farmers from this region that are living in remote villages use traditional farming methods for land preparation, sowing seeds, harvesting, drying, storing, and husking prior to selling it to buyers (Bäckman, Islam & Sumelius, 2011; Shelley, Takahashi-Nosaka, Kano-Nakata, Haque & Inukai,

2016). For instance, plowing is either done by the farmer or by the help of bulls and buffaloes, sowing and harvesting is carried out by hand using tools like sickles and knives, and husking to remove husk from the paddy grain to produce edible rice grains is also done at home using traditional methods (Zaman, Mishima, Hisano & Gergely, 2001). Therefore, small farmers from remote villages usually go through such labor-intensive tasks mostly due to unavailability of machinery or to avoid additional costs. Therefore, in this region, the skills and ability of farmers are directly reflected on the rice they cultivate and, hence, organizing a rice competition among such farmers is plausible. Moreover, ethnic minorities in Bangladesh are severely discriminated against in terms of access to healthcare, education, employment, etc. over generations (Roy, 2012; D'Costa, 2014).[3] Therefore, these villages in the Northwestern region serve as a suitable field for this study.

The majority of ethnic minorities, who are known as *Adivasis* in South Asia, live in remote villages, which are not easily accessible and, hence, remain outside the range of basic services. Another major drawback of living in remote areas is having less access to information about the market, which, as a result, might lead to difficulty in getting fair prices for products to be sold. The rice buyers, who are mostly Bengali, have storehouses in marketplaces that are commonly known as *arots*. Farmers could either sell rice by directly visiting an *arot* or buyers, usually in villages, visit farmer households to buy rice from them directly. These buyers in the villages are locally known as *foriyas*. Rice buyers who are owners of *arots* usually operate as the middlemen between farmers and grocery shops – where rice is sold for final consumption.

It is widely known that buyers usually take advantage of ethnic minority farmers by buying their rice at a lower price than what they generally offer to ethnic majority farmers (AIPP, 2010). However, this could be due to various reasons: (i) minority farmers travel from home to the local market and back, and are sometimes forced to sell rice at lower prices; (ii) buyers usually travel to households to buy rice, so they ask for a lower price to compensate for the cost that had incurred; (iii) ethnic minority farmers have inferior bargaining abilities; (iv) differential treatment is based on buyer's profit motives; (v) buyers either use observable characteristics, such as ethnicity, to make inference about farmers' rice cultivating skills or they take advantage of the lack of market information, such as latest market prices, available to ethnic minority farmers (*statistical* discrimination); (vi) they prefer to pay minority farmers less in general (*taste-based* discrimination). Therefore, in our study, under a controlled field setting, we are able to discard reasons (i), (ii), (iii), and (iv), and test whether discrimination against ethnic minority farmers exist and, if so, of what nature (*statistical* or *taste-based*). However, buyers' beliefs about inferior bargaining abilities of ethnic minority farmers are not entirely eliminated in this design. Therefore, presence of such beliefs while discriminating would retain the possibility of statistical discrimination.

[3]We broadly discuss economic and social conditions of ethnic minorities in Bangladesh in Section 2.2 of Chapter 2 in this thesis.

4.3 The Experiment

We organized a rice competition followed by a series of rice evaluation programs in the Northwestern part of Bangladesh in April 2018 with the support of the NGO *Ashrai* that works on ethnic minority issues in Bangladesh.[4] The ethnic majority, Bengali, rice buyers participated in these events to assess rice quality and state how much they are willing to pay (for one kilogram of rice) for 30 different rice samples that were collected from 30 different farmer households. To ensure that rice quality is not correlated with the actual ethnicity of farmers, we randomly assigned ethnic majority and minority sounding names to each rice sample to implicitly reveal the farmer's ethnic identity to the buyer. This way, if we find any correlation between the assigned ethnicity and the assessment outcomes, then that would be capturing discrimination. We have organized our study design into six categories: rice competition and sample collection, randomizing ethnic identities, locations for rice evaluation, the evaluation program, experimental procedure and, finally, research hypotheses.

4.3.1 Rice Competition and Sample Collection

We organized a rice competition in multi-ethnic villages with the help of our NGO, where the farmer who produced the "best" rice won a 2,000 Taka (or USD 25) cash prize. The average daily income of farmers in the Northwestern part is around 225 Taka. So, the prize money was about 30 percent of their monthly income. In total, 30 farmers (15 ethnic majorities and 15 ethnic minorities) from 30 different households took part in the competition.[5] To determine the "best" rice produced, actual rice buyers from both villages and the city assessed rice quality and then also declared how must they are willing to pay for one kilogram of rice. Both rice quality and willingness to pay were given equal weight to determine the winner. For the competition, we collected rice samples by randomly visiting farmer households. After entering each farmer household, we asked if the male head of the household is a farmer, asked their ethnicity, and then asked to speak to the head (if the door was attended by someone else). Then we invited him to take part in the rice competition and mentioned the cash prize involved. We also informed him about the assessment process, which would be carried out by rice buyers from different (and not their own) villages and the city, but never mentioned the ethnicity of assessors. If someone was willing to participate then he had to submit 500 grams of his most recently produced rice. This way we went to 15 Bengali households and 15 Santal households to collect 30 different rice samples.[6] In total, we went to three different multi-ethnic villages to collect rice samples for the competition. The outcome of the competition was later announced by visiting all 30 households separately

[4] *Ashrai:* `<http://ashrai.org.bd/>`

[5] Competitions involving farmers are not uncommon in Bangladesh. *Channel i*, Bangladesh's first digital TV channel, organize competitions with farmers twice every year. Such competitions (where farmers compete with other farmers in different games) are widely televised and known around the country. However, such competitions are only organized during the *Eid* festivals.

[6] Rice were of 9 different varieties. Please see Table C1 in Appendix C for the list of names and their market prices. See the Experimental Instructions and Surveys at the end for the advert.

and one's absolute quality score and price was only revealed to the winner. No relative feedback about the achieved score and price was given to any participants and the winner's identity was never revealed to non-winners.

4.3.2 Randomizing Farmers' Names

For each assessor or rice buyer, we made 30 different small rice samples that were collected from 30 different farmers. We then attached these packets on a large hardboard and assigned ID numbers to each sample. We call these hardboards, rice boards. We used transparent packets so that buyers could easily examine the rice. In this case, the quality of rice samples are likely to be uneven, so if the ethnicity of a farmer is labeled to his original rice sample then it would be difficult to disentangle discrimination from quality. It would also be difficult to identify what quality score and willingness to pay a buyer would have attached to the sample had another farmer, with different ethnicity, produced the same rice. Hence, to solve this issue, we randomized ethnic majority and minority sounding names of farmers on each rice sample that are observed by buyers so that ethnicity is uncorrelated with rice quality. Specifically, next to each rice ID on a rice board, we randomly attached either a Bengali (ethnic majority) or a Santal (ethnic minority) sounding name. In Bangladesh, ethnic minorities have either tribe or clan patronyms, which are surnames that are named after their tribes or septs. For example, Santals have 12 clans or septs (Risley, 1891), so a male Santal's name could be Horen Tudu (if from the *Tudu* clan), Horen Hasda (if from the *Hasdak* clan), Horen Kisku (if from the *Kisku* clan), and so on. Similarly, ethnic majority Bengalis are mostly Muslims with names either starting "Muhammad" or ending "Rahman", "Ahmed" or "Islam". Therefore, for simplicity, we have only used Santal and Bengali sounding names on our rice samples to measure discrimination. We told buyers that the name attached on each rice sample was that of the farmer who produced that particular rice and was a participant in the rice competition. This way, if we find a correlation between the ethnicity of farmers and assessment outcomes, then that would be capturing discrimination. We provide the list of names in Table C2 in Appendix C and a picture of the rice board in Figure C1 in Appendix C. Please note that we did not use the actual names of farmers. Instead, we created some widely common Bengali and Santal sounding names. For Santal sounding names, we sought help from Risley (1891) and Ali (1998).

4.3.3 Locations for Rice Evaluation

As a field, the Northwestern part of Bangladesh has several advantages. To begin with, Rajshahi Division is home to the Santal and the Oraon ethnic minorities, who are two of the largest ethnic minority communities in Bangladesh and are mostly agriculturists (Ali, 1998; Ahmed, 2010). Hence, this makes their ethnic minority sounding names widely known and, thus, are easily identifiable by ethnic majority rice buyers from the same region. Our buyers all come from the Northwestern part. To invite

buyers for rice evaluation sessions, we randomly selected marketplaces from the main city, multi-ethnic villages, and mono-ethnic villages. In this case, the city is the region within the Rajshahi City Corporation, multi-ethnic villages are villages where residents are both ethnic minorities and the majority, and mono-ethnic villages are villages where residents are only the ethnic majority. We selected such locations for two reasons: firstly, it increases the external validity of our study and, secondly, it allows us to explore any heterogeneity driven by the level of intergroup interaction.[7] We assume, and later confirm through our survey, that the level of interethnic interaction would be the highest in multi-ethnic villages, as different ethnic groups live together in such locations; while the level of interethnic interaction would be minimal in mono-ethnic villages and the city. To invite buyers, we randomly selected villages from the list provided by the NGO and then randomly went to marketplaces to invite ethnic majority rice buyers for the rice evaluation program. In total, we went to nine marketplaces in nine different villages (five multi-ethnic and four mono-ethnic) and six marketplaces in the main city.[8]

4.3.4 The Rice Evaluation Program

120 ethnic majority rice buyers were invited as independent assessors to evaluate the physical quality of a set of rice samples and then state their willingness to pay for one kilogram of each of the rice samples (e.g. 30 rice samples in total). According to International Food Policy Research Institute (IFPRI), physical quality of rice is evaluated based on its physical appearance that depends on its shape, color, chalkiness, proportion of dead rice in a batch, and so on, and is different from chemical quality (Ayeduvor, 2018). To make such assessments, we invited buyers who were all males, as having female buyers in Bangladeshi villages is very uncommon. Also, only ethnic majority, Bengali, rice buyers took part in the rice evaluation program because it is very uncommon for ethnic minorities to be rice buyers in the Northwestern region. Through advertisements, buyers were asked to visit a central location (usually a primary school or a resting place within marketplaces) at a given time to take part in the rice evaluation program in exchange for a participation fee (200 Taka or USD 2.50) and a chance to earn more by evaluating 30 different rice samples (5 Taka for evaluating each rice sample).[9] Buyers were informed that the evaluation program is part of a competition and their assessment would determine the winner, who would win a 2,000 Taka cash prize. This was important because it ensured that buyers believed that their assessments has a real impact on the well-being of farmers, the same way their day-to-day assessments affect farmers' earnings when they buy rice in the actual market. They were also informed that

[7] Mono-ethnic villages with only ethnic minority residents would have further increased our external validity and made the study more interesting, but such villages were not available according to the NGO we worked with.

[8] The share of buyers from the three location types differ because the NGO we collaborated with had limited influence in mono-ethnic villages and the city.

[9] 1 USD = 80 Taka at the time of the fieldwork.

both quality score and willingness to pay would be given equal weight while determining the winner.[10]

Buyers rated the rice quality on a scale from 0 to 10, with 10 being the highest quality, and then stated how much they are willing to pay for one kilogram of that particular rice (which could be any amount). Since each buyer evaluated 30 rice samples in total, with a sample size of 120 buyers, we had 3,600 observations in total. In addition, with both quality scores and prices, we had two measures of discrimination. We also obtained blind assessments from three rice buyers (one from each location type: city, multi-ethnic village, and mono-ethnic village), which allows us to check for the internal validity of the experiment (i.e. successful randomization) and also control for the "actual" quality of rice in the regression analysis.

4.3.5 Experimental Procedure

Buyers were sent to tables on arrival and were given unique ID cards. They were informed that their identity would be kept anonymous, and, hence, they should always use their ID numbers on each evaluation sheet. During the evaluation, a rice board with 30 attached rice samples (in transparent plastic bags) were given to the buyer. Each rice sample on the rice board had an ID and a randomly assigned farmer name. A separate paper (an evaluation sheet), with blank columns to write down rice IDs, assigned farmers' names, quality scores, and willingness to pay, were also given to each buyer. Under each category, buyers had to write ID numbers of each rice sample, the full name of the farmer, the quality score, and their willingness to pay (always in this order). An example of the evaluation sheet is provided in Table C3 in Appendix C. This had two advantages: firstly, we knew in which order buyers assessed rice samples; and secondly, writing down farmer's name ensured that buyers had read the full name. After completion, buyers were asked to fill out a short survey before getting paid in cash. Each session ran for around 60 minutes.

4.3.6 Hypotheses

The primary goal in this study is to check whether discrimination against ethnic minority farmers exist. Existing correspondence studies on ethnic discrimination suggest that members of the ethnic minority group are often discriminated during recruitment in the labor market (Carlsson & Rooth, 2007; Booth et al., 2012; Kaas & Manger, 2012), which is also consistent with findings from the laboratory (Fershtman & Gneezy, 2001). Moreover, other audit studies with testers also suggest the same (Jowell & Prescott-Clarke, 1970; Riach & Rich, 2002). Similarly, discrimination against minority groups are also profound in various product markets (List, 2004; Zussman, 2013). Based on the existing evidence, we formulate the following hypothesis.

[10]Most of the rice buyers from the city participated in their shop, individually, and did not go to a central place.

Hypothesis 1: Buyers discriminate ethnic minority farmers both in terms of quality assessment and willingness to pay.

To explore whether the frequency of interethnic interaction induce any heterogeneity in terms of discrimination, we recruited buyers from three types of locations: multi-ethnic villages (where different ethnic groups live together, hence buyers are likely to have higher interaction with ethnic minority farmers), mono-ethnic villages (where only the ethnic majority reside, hence buyers are likely to have very low or no interaction with ethnic minority farmers), and the city (most of the residents are ethnic majorities as minorities primarily live in remote villages, hence buyers are likely to have very low or no interaction with ethnic minority farmers). Research in social psychology, and very recently in economics, suggest that increasing intergroup contact can reduce discrimination towards out-group members (Allport, 1954; Brown, Brown, Jackson, Sellers & Manuel, 2003; Rao, 2018). This is because, higher contacts between different groups promote positive and tolerant attitudes towards out-group members that can curb discrimination. Based on the theory and empirical evidence, we formulate our second hypothesis.

Hypothesis 2: (i) Buyers from multi-ethnic villages discriminate ethnic minority farmers the least; (ii) Buyers from mono-ethnic villages and the city discriminate ethnic minority farmers the most.

We test these two hypotheses based on the experimental design laid out in this section. The following section discusses our main results.

4.4 Results

Since we randomize farmers' ethnicity on the rice boards while other farmers' characteristics remain unknown to the buyer, we decided not to collect any demographics of farmers for this study. Instead, through a brief survey that was administered at the end of the evaluation program, we collected a range of individual information from buyers on their demographics, business experiences, shop locations, level of intercultural competence, and so on. This information later allows us to check whether discrimination (if any) varies by any of their characteristics.

4.4.1 Descriptive Statistics

We present descriptive statistics of rice buyers in Table 4.1. The first column (All) provides the summary of characteristics of the total sample and then in the next three columns (i.e. A, B, and C) we disaggregate the sample by locations: multi-ethnic villages, mono-ethnic villages, and the city respectively. In the last three columns, we present two sample Mann-Whitney U test (MW-test hereinafter) results that compare differences in the average characteristic of buyers across locations.

The average age of buyers is 40 years with 15 years of experience in the current occupation. 88 percent of buyers work for themselves, 68 percent buys rice by visiting farmer households, and buy around 5,900 kilograms of rice for their businesses every year. When we disaggregate the sample by locations, we observe some heterogeneity in terms of demographics and the amount of rice bought every year. For instance, buyers from the city are significantly more educated, earn a higher income, and buys more rice from farmers every year relative to buyers from multi-ethnic and mono-ethnic villages. Moreover, none of the city buyers buy rice by going door-to-door whereas village buyers mostly buy rice by visiting farmer households. In terms of the frequency of interactions with ethnic minority farmers for business purposes (i.e. buying rice), we see that buyers from multi-ethnic villages interact significantly more than buyers from mono-ethnic villages and the city. We also asked buyers some questions to understand how well they know about the Santali culture, which we call it the level of intercultural competence (Fantini, 2010).[11] We see that multi-ethnic and mono-ethnic village buyers have both scored roughly 0.50 whereas city buyers received an average score of around 0.30, and this difference is statistically significant. Also, there are significantly more Muslim buyers in mono-ethnic villages compared to the other two locations, although these differences are marginal. In terms of land possession, owning the business, and the number of years living in the current residence, we do not find any differences between locations.

In Table 4.2, we provide the summary of rice quality scores and willingness to pay (WTP) that were given by buyers to randomized farmer names. Throughout the rest of the paper, we would address ethnic majority sounding names as Bengali farmers and ethnic minority sounding names as Santal farmers, unless stated otherwise. Out of a score of 10, both Bengali and Santal farmers received a quality score of 6.68 for their rice, which is not significantly different. When disaggregated by location, both multi-ethnic village and city buyers gave statistically the same quality scores to Bengali and Santal farmers, which we test using a two-sample T-test with unequal variances (T-test: p-values are 0.339 and 0.899); however, buyers from mono-ethnic villages gave 0.16 points more to Bengali farmers compared to Santal farmers and this difference is marginally significant (T-test: p-value= 0.081).[12] These suggest that ethnic majority buyers who are only from mono-ethnic villages (marginally) discriminate ethnic minority farmers in terms of the quality of rice produced, whereas buyers from multi-ethnic villages and the city do not show any unfairness in terms of rice quality assessment.

Moving to discrimination in terms of WTP, buyers are willing to pay significantly more to Bengali than to Santal farmers (T-test: p-value= 0.008). Specifically, Santal

[11] We asked 4 simple questions about the Santali culture, e.g. we asked what is the language spoken by Santals, what is their main religious festival called, etc. For each correct answer, we assigned 0.25 points so that 0 would mean having no knowledge and 1 would mean having excellent knowledge. These questions are simplified versions of Fantini's intercultural competence assessment questions that only focus on the "awareness dimensions" of individuals. Please see the survey in the Experimental Instructions and Surveys at the end for all four questions.

[12] We show results using T-tests only as the number of observations are large. Please note that we also carry out a MW-test for robustness and our results remain the same throughout, unless stated otherwise.

TABLE 4.1: Rice Buyer Characteristics

Buyer Characteristics	**All** (*Std. Dev.*)	**A: Multi-Ethnic** (*Std. Dev.*)	**B: Mono-Ethnic** (*Std. Dev.*)	**C: City** (*Std. Dev.*)	**MW-test (A vs B)** ***p*-values**	**MW-test (B vs C)** ***p*-values**	**MW-test (A vs C)** ***p*-values**
Age	40.38	38.76	44.24	38.43	0.046	0.052	0.885
	(12.80)	*(14.14)*	*(11.90)*	*(10.18)*			
Education	7.80	7.22	7.59	9.21	0.735	0.085	0.020
	(3.83)	*(3.67)*	*(4.15)*	*(3.45)*			
Income	14,166	13,286	12,068	18,668	0.882	0.000	0.003
	(7,321)	*(8,517)*	*(3,224)*	*(6,977)*			
Land	24.11	22.94	20.46	31.25	0.628	0.915	0.739
	(43.75)	*(34.72)*	*(30.87)*	*(68.40)*			
% Married	0.90	0.84	0.97	0.93	0.040	0.402	0.244
	(0.30)	*(0.37)*	*(0.16)*	*(0.26)*			
Children	1.92	1.80	2.00	2.04	0.410	1.000	0.476
	(1.35)	*(1.41)*	*(1.22)*	*(1.43)*			
Years in Current Profession	15.46	14.13	16.41	16.82	0.355	0.494	0.107
	(10.41)	*(10.40)*	*(11.65)*	*(8.59)*			
% Own Business	0.88	0.84	0.92	0.93	0.252	0.886	0.244
	(0.32)	*(0.37)*	*(0.28)*	*(0.26)*			
Years Living in Current Location	33.19	33.51	33.84	31.71	0.905	0.715	0.678
	(15.50)	*(17.90)*	*(15.56)*	*(9.59)*			
% Door to Door	0.68	0.82	0.97	0.00	0.026	0.000	0.000
	(0.47)	*(0.39)*	*(0.16)*	*(0.00)*			
Rice Quantity	5,864	2,214	2,733	17,171	0.033	0.004	0.000
	(24,819)	*(3,266)*	*(2,914)*	*(50,090)*			
IC Competence	0.48	0.56	0.48	0.31	0.281	0.037	0.001
	(0.32)	*(0.29)*	*(0.34)*	*(0.32)*			
Business Interaction	0.17	0.31	0.03	0.06	0.000	0.235	0.000
	(0.29)	*(0.35)*	*(0.12)*	*(0.16)*			
% Muslim	0.78	0.73	0.89	0.71	0.057	0.070	0.901
	(0.42)	*(0.45)*	*(0.31)*	*(0.46)*			
Sample Size	120	55	37	28	-	-	-

Note: Age and Education are in years; Income is monthly (in Bangladeshi taka); Land Possession is the amount of land owned in 'katha', where 1 katha = 720 square feet; % Married is the proportion of buyers who are married; Children is the number of children one has; Years in Current Profession is the number of years a buyer is in his current profession; % Own Business is the proportion of buyers who also own their rice buying business; Years Living in Current Location is the number of years one is living in their current village; % Door to Door is the proportion of buyers who buy rice by going door to door; Rice Quantity is the amount of rice (in kilograms) one buys every month for business purpose; IC Competence is the inter-cultural competence score regarding the Santal culture; Business Interaction shows the average frequency of interacting with ethnic minority farmers for business purposes (i.e. buying rice), where 0 means no interaction, 0.33 means some interaction, 0.67 means moderate interaction, and 1 means very frequent interaction; % Muslim is a dummy that equals to 1 if a buyer is a Muslim and 0 otherwise; MW-test is a two-sided Mann-Whitney U test.

farmers are likely to receive 0.58 Taka less for selling one kilogram of rice relative to Bengali farmers. When we break down WTP by locations, we find that buyers from multi-ethnic villages are willing to pay 0.69 Taka less to Santal farmers than what they would pay to Bengali farmers and this difference is marginally significant (T-test: p-value= 0.066). Likewise, buyers from mono-ethnic villages are willing to pay 1.13 Taka less to Santal farmers than to Bengali farmers (T-test: p-value= 0.001), which is almost double the difference we found among buyers from multi-ethnic villages. While we find large gaps in terms of WTP among village buyers, buyers from the city do not discriminate Santal farmers in terms of WTP. Instead, we see that city buyers are willing to pay 0.34 Taka more to Santal farmers than what they would pay to Bengali farmers, although this difference is not statistically significant at conventional levels.

Further to the actual quality scores and WTP given to randomized farmer names, we also look at whether blind assessment of rice quality and WTP differ between actual ethnicity of farmers. We provide these results in Table C4 in Appendix C. Although we have a small number of observations from blind assessments, as expected, we find that blind assessment of rice quality and WTP do not differ across actual ethnicity of farmers, neither overall nor within different locations (MW-test: all p-values$>$ 0.10). This suggests that ethnic majority and minority farmers might be producing rice of very similar qualities and, in a fair world, they would also receive the same price for their product.

4.4.2 Main Results

Using a regression framework, we examine whether the judgment of rice quality and price depends on the ethnic identity of the farmer while also controlling for "actual" rice quality, rice variety and buyer fixed effects. Here we use blind quality scores as a proxy for actual rice quality. Moreover, assuming buyers are fully aware of rice varieties available in the market, controlling for rice variety captures any inference made by buyers about farming quality or skills of farmers. For instance, buyers might have experiences of buying rice variety A mostly from highly skilled farmers and B mostly from low skilled farmers. Hence, knowing the variety might induce buyers to (wrongly) guess the quality or skills of the farmer. Since only the quality of rice and the name of the farmer is visible to the buyer, other farmer characteristics, such as age, years of experience, the location of farming, and so on, that might affect stated quality score and willingness to pay are ambiguous to the assessor. Please also note that all assigned names were male-sounding names, eliminating possibilities of gender discrimination. Also, even though we have randomized names of farmers, adding control variables may give us more precise effects. To capture such effects, we estimate the following OLS regressions for rice quality assessment and willingness to pay:

$$QualityScore_{ij} = \beta Minority_{ij} + \gamma BlindScore_{ij} + v_i + b_j + \epsilon_{ij} \tag{4.1}$$

$$WTP_{ij} = \beta Minority_{ij} + \gamma BlindScore_{ij} + v_i + b_j + \epsilon_{ij} \tag{4.2}$$

TABLE 4.2: WTP and Quality Score Given to Randomized Farmer Names

	All			**Multi-Ethnic**			**Mono-Ethnic**			**City**		
	Bengali (*Std. Dev.*)	**Santal** (*Std. Dev.*)	**T-test *p*-values**	**Bengali** (*Std. Dev.*)	**Santal** (*Std. Dev.*)	**T-test *p*-values**	**Bengali** (*Std. Dev.*)	**Santal** (*Std. Dev.*)	**T-test *p*-values**	**Bengali** (*Std. Dev.*)	**Santal** (*Std. Dev.*)	**T-test *p*-values**
Quality Score	6.68	6.68	0.872	6.65	6.74	0.339	7.12	6.96	0.081	6.18	6.17	0.899
	(1.76)	(1.76)		(1.91)	(1.95)		(1.48)	(1.49)		(1.64)	(1.61)	
WTP	37.92	37.34	0.008	37.64	36.95	0.066	37.75	36.62	0.001	38.70	39.04	0.321
	(6.58)	(6.58)		(7.74)	(7.39)		(5.65)	(6.09)		(4.99)	(5.07)	
Observations	1,800	1,800	-	825	825	-	555	555	-	420	420	-

Note: Bengali (Santal) means a rice sample had a Bengali (Santal) sounding name; Quality Score is the quality score (between 0 to 10) given to a rice sample where 10 corresponds to the highest quality; WTP is a buyer's willingness to pay (in taka) for 1 kilogram of a particular rice sample; T-test is a two-sample t-test with unequal variances.

Table 4.3: Effect of Assigned Ethnicity on Rice Quality Assessment

VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**
Minority	-0.009	-0.034	-0.041	-0.041
	(0.054)	(0.057)	(0.052)	(0.053)
Blind Score	-	0.318***	-0.023	-0.023
		(0.060)	(0.046)	(0.046)
Constant	6.684***	4.697***	7.229***	6.682***
	(0.149)	(0.462)	(0.335)	(0.303)
Rice Variety	No	No	Yes	Yes
Buyer Fixed Effects	No	No	No	Yes
Observations	3,600	3,600	3,600	3,600
R-squared	0.000	0.025	0.141	0.377

Robust standard errors clustered at the shop location level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: OLS regression estimates reported; the dependent variable is quality assessment score given to rice samples (any number between 0 and 10, where 10 corresponds to the highest quality); Minority is a dummy variable that equals 1 if a rice sample was assigned an ethnic minority name and 0 if an ethnic majority name was assigned; Blind Score is the blind (i.e. no name was assigned to rice samples) quality score given to each rice sample; in total, 3,600 rice samples were assessed by 120 rice buyers (each assessed 30 rice samples); 25 shop locations in total.

where $QualityScore$ is the quality score (a number between 0 and 10, where 10 corresponds to the best quality) given to rice sample i by the buyer j, WTP is the buyer's willingness to pay for one kilogram of rice, $Minority$ is a dummy variable that indicates whether a rice sample was assigned an ethnic minority name, $BlindScore$ is the blind quality score given to each rice sample, and v are dummies for each variety of rice. In addition, b is the buyer fixed effects, allowing us to hold the buyers' individual standards fixed. Standard errors are clustered at the shop location level (25 clusters in total), which was the unit of advertising (or initial randomization) in our study.

In Table 4.3, we provide our main regression results of rice quality scores on the ethnicity of farmers. In column 1, we show the results of specification without any control variables. Then we incrementally add controls in the subsequent columns. Specifically, we control for blind quality score of rice in column 2, rice varieties in column 3, and, finally, in column 4 we have a full set of controls that also have buyer fixed effects.

Our results show that buyers do not discriminate Santal farmers by giving them a lower quality score. While adding controls incrementally increases the difference in quality scores between Santal and Bengali farmers, but this difference never reaches statistical significance at conventional levels.[13] In other words, buyers do not seem to discriminate ethnic minority farmers in terms of quality assessment, but it is still to be seen whether this holds true for all locations or whether there are any geographic

[13]Controlling for actual ethnicity of farmers has no significant effect and does minimal to no change to the coefficient of our variable of interest. Although we do not present the results, for robustness, we control for the actual ethnicity in all specifications and none of our results change.

heterogeneity in terms of discrimination that are likely to be driven by differences in interethnic interaction. To explore this, we disaggregate our sample by location and estimate our baseline specification for quality score on each of the locations: multi-ethnic villages, mono-ethnic villages, and the city. We summarize these results in Table 4.5. Columns 1, 2, and 3 show effects on buyers' quality assessment scores in multi-ethnic villages, mono-ethnic villages, and the city respectively. Similar to the raw test results provided in Table 4.2, we find that buyers from mono-ethnic villages give, on average, 0.15 points less to Santal farmers than to Bengali farmers (2% less score), which is significant at the 5% level. However, buyers from multi-ethnic villages and city buyers do not seem to discriminate Santal farmers in terms rice quality assessment and these effect sizes are neither statistically significant nor large in terms of magnitude.[14] To check if these effect sizes statistically differ between locations, we interact locations with the assigned ethnicity of farmers. We do this on a restricted sample where we always leave out a third location that we do not wish to compare, e.g. if the comparison is between multi-ethnic and mono-ethnic villages, then we leave out the city from the analysis. This way, the estimated coefficient of the interaction term would give us the difference between the two effects of ethnicity from two locations (i.e. difference-in-difference). We provide these results in Tables C6-C8 in Appendix C. We find that only the difference in effect sizes between multi-ethnic and mono-ethnic villages is marginally significant at the 10% level. However, we do not find any differences in effect sizes between multi-ethnic villages and the city, and mono-ethnic villages and the city.[15] Therefore, our results suggest that, while the magnitude of discrimination in terms of quality assessment is small within mono-ethnic villages, it is significantly higher than that in multi-ethnic villages but not so much than that in the city.

Table 4.4 summarizes results of our baseline specification for willingness to pay. In column 1, without any control variables, we find that buyers are willing to pay Santal farmers 0.58 Taka less than Bengali farmers (1.5% less), which is significant at the 5% level. Adding control variables in subsequent columns improve the precision of estimates, which finally shows that buyers are willing to pay Santal farmers, on average, 0.70 Taka less than Bengali farmers (2% less) which is also significant at the 5% level (column 5). When we split our sample by location, we find that buyers from both types of villages are willing to pay around 2.5% less to Santal farmers relative to Bengali farmers (both are statistically significant at then 5% level). However, we do not find any effect of assigned ethnicity on willingness to pay by buyers from the city. Therefore, our results suggest that discrimination captured overall are entirely driven by buyers from villages, whereas city buyers do not seem to discriminate ethnic minority farmers in terms of willingness to pay. Instead, although statistically insignificant, they seem to offer more to Santal

[14] However, when we combine both villages together, we do not find any statistically significant effect of assigned ethnicity of farmers on rice quality assessment. This result is shown in Table C5 in Appendix C.

[15] We also compare effect sizes between villages (combined) and the city and found that this difference is also statistically insignificant. This result is shown in Table C9 in Appendix C.

TABLE 4.4: Effect of Assigned Ethnicity on Buyer's Willingness to Pay

VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**
Minority	-0.582**	-0.671**	-0.704**	-0.704**
	(0.257)	(0.264)	(0.264)	(0.268)
Blind Score	-	1.148***	-0.064	-0.064
		(0.102)	(0.095)	(0.097)
Constant	37.920***	30.743***	39.984***	39.155***
	(0.514)	(1.000)	(0.967)	(0.777)
Rice Variety	No	No	Yes	Yes
Buyer Fixed Effects	No	No	No	Yes
Observations	3,600	3,600	3,600	3,600
R-squared	0.002	0.025	0.159	0.493

Robust standard errors clustered at the shop location level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: OLS regression estimates reported; the dependent variable is the buyer's willingness to pay for one kilogram of each rice samples (in Bangladeshi Taka); Minority is a dummy variable that equals 1 if a rice sample was assigned an ethnic minority name and 0 if an ethnic majority name was assigned; Blind Score is the blind (i.e. no name was assigned to rice samples) quality score given to each rice sample; a shop location is the village/locality where a shop is located (25 locations in total); in total, 3,600 rice samples were assessed by 120 rice buyers (each assessed 30 rice samples).

farmers compared to Bengali farmers.[16] Furthermore, while investigating differences in effect sizes between locations, we find that there are no differences between multi-ethnic and mono-ethnic villages (Table C11 in Appendix C). However, effect sizes are significantly different between multi-ethnic villages and the city, and mono-ethnic villages and the city, where both differences are statistically significant (at the 10% level and the 1% level respectively). These results can be found in Tables C12-C13 in Appendix C.[17] Following our raw test results from Table 4.2, our regression results also suggest that Bengali buyers discriminate Santal farmers in terms of how much they are willing to pay for one kilogram of rice and such unequal treatment is only prevalent in the villages.[18]

4.4.3 Underlying Nature of Discrimination

Our design provides two measures of discrimination, one via capturing rice quality scores and another through one's willingness to pay for a kilogram of rice. Through these two measures, we are able to distinguish between the two models of discrimination: a *taste-based* model of discrimination where buyers might have a preference for a certain

[16] Combining both villages together to look at discrimination only within villages, we find that buyers from villages are willing to pay, on average, 0.99 Taka less (or 2.5% less) to Santal farmers compared to Bengali farmers (significant at the 1% level). This result is provided in Table C10 in Appendix C.

[17] The difference in effect sizes between villages (combined) and the city is also statistically significant. This result is shown in Table C14 in Appendix C.

[18] Since we have two regressions with a very similar set of regressors, it is likely that the error terms in the regressions would be correlated. Therefore, to allow for correlation between the error terms across the two regression equations, we also run a seemingly unrelated regression analysis to check if our initial results hold. We find that all our results remain robust throughout.

TABLE 4.5: Effect of Assigned Ethnicity, by Location

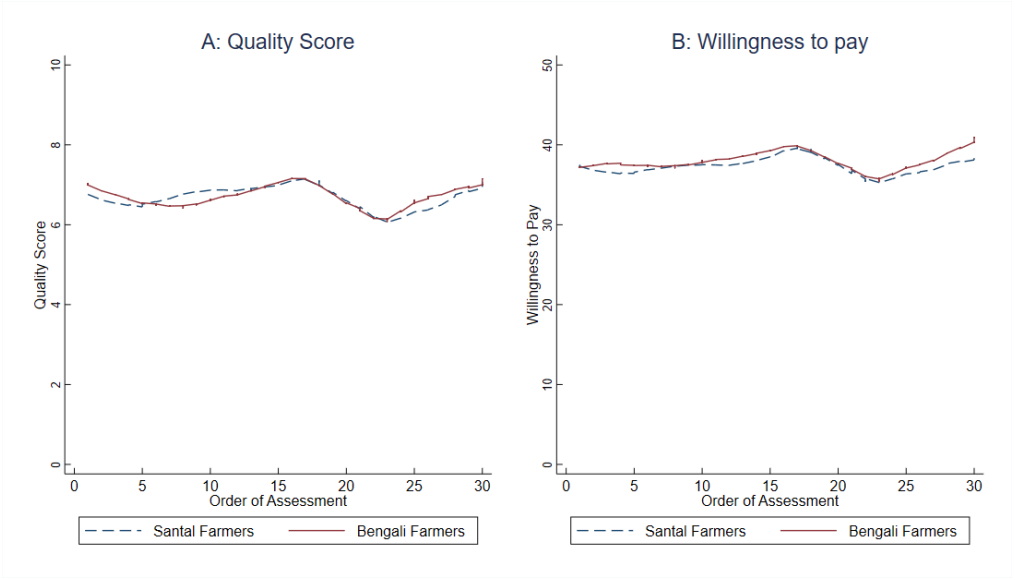
	Quality Score			Willingness to Pay		
VARIABLES	**Multi-ethnic (1)**	**Mono-ethnic (2)**	**City (3)**	**Multi-ethnic (4)**	**Mono-ethnic (5)**	**City (6)**
Minority	0.015	-0.153**	-0.063	-0.968**	-1.138***	0.177
	(0.072)	(0.047)	(0.119)	(0.433)	(0.299)	(0.276)
Blind Score	-0.076	0.040	-0.005	-0.278**	0.310	-0.092
	(0.094)	(0.065)	(0.055)	(0.106)	(0.188)	(0.141)
Constant	6.769***	5.985***	4.893***	41.164***	36.212***	42.530***
	(0.589)	(0.369)	(0.258)	(0.605)	(1.339)	(1.382)
Rice Variety	Yes	Yes	Yes	Yes	Yes	Yes
Buyer Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,650	1,110	840	1,650	1,110	840
R-squared	0.460	0.284	0.379	0.593	0.401	0.340

Robust standard errors clustered at the shop location level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: OLS regression estimates reported; the dependent variable for columns 1-3 is Quality Score and that for columns 4-6 is Willingness to Pay (in Bangladeshi Taka); Minority is a dummy variable that equals 1 if a rice sample was assigned an ethnic minority name and 0 if an ethnic majority name was assigned; Blind Score is the blind (i.e. no name was assigned to rice samples) quality score given to each rice sample; a shop location is the village/locality where a shop is located (25 locations in total); in total, 3,600 rice samples were assessed by 120 rice buyers (each assessed 30 rice samples).

ethnic group (Becker, 1957) or a *statistical* model of discrimination where buyers might use the ethnicity of farmers as a proxy for skills that are unobservable to them (Phelps, 1972; Arrow, 1973). Our first measure of discrimination, rice quality scores, captures a buyer's belief about the skills or competence of farmers in cultivating rice. For instance, if a buyer believes that a rice is of lower quality, hence produced by a low skilled farmer, he would certainly give it a low quality score irrespective of its variety or market price. Therefore, capturing discrimination in terms of rice quality assessment would be consistent with the *statistical* model of discrimination, where buyers' judgments would be entirely driven by stereotypes associated with skills or ability of ethnic minority farmers in terms of rice production. On the other hand, our second measure of discrimination, willingness to pay, captures both buyers' preferences for a certain ethnic group as well as their judgments about skills of that particular group. For example, buyers might be willing to pay less for rice produced by Santal farmers because either they dislike paying more to Santal farmers or they believe Santal farmers produce lower quality rice and, hence, deserves to get a lower price for their product. Therefore, capturing discrimination in terms of willingness to pay would be backing both theories of discrimination. Since our data show that buyers discriminate ethnic minority farmers only in terms of willingness to pay and not in terms of the quality of rice they produce, this certainly isolates *statistical* discrimination from the lot. Therefore, our finding seems consistent with the *taste-based* discrimination model. Although, there still remain concerns about statistical discrimination being present because other beliefs, such as ethnic minority farmers have weaker bargaining abilities and they tend to accept lower prices or the belief that they have relatively less information about market prices of rice are not entirely

FIGURE 4.1: Ethnic Discrimination, by Order of Assessment



Note: This figure shows the relationship between the order in which buyers have assessment rice samples and assigned ethnicity of farmers.

eliminated. In these cases, the observed discrimination in terms of willingness to pay could be partly explained by the *statistical* model of discrimination.

An alternative way to test if discrimination is due to animus or due to making statistical inference about skills is to check the order in which rice samples were assessed (Hanna & Linden, 2012). If there is any correlation between quality scores/willingness to pay and the order of assessment, then that would suggest discrimination is *statistical*. For example, if buyers discriminate at the beginning of the evaluation then that would suggest that buyers use the ethnic identity of farmers as a signal to where the quality of a particular rice sample will end up in the distribution, since the quality distribution is still unknown to buyers at the beginning. Then again, if buyers discriminate at the end of the evaluation process then that would mean boredom/fatigue might be inducing them to reduce the amount of time spent evaluating by quickly predicting rice quality based on farmer's ethnic identity. However, the order of rice assessment would not affect the rice quality scores if discrimination is *taste-based*. This is because, buyers would have a "distaste" for ethnic minority farmers throughout the evaluation process, so their assessment outcomes should be constant regardless of the order. Although this exercise is more desirable when the process of assessment is time-consuming as in Bertrand & Mullainathan (2004) or Hanna & Linden (2012), this alternative test nevertheless would assure that our conclusion regarding the mechanism behind discrimination is robust.

In Figure 4.1, we show the relationship between assessment order and quality score (A)/willingness to pay (B). The x-axis is the order in which rice samples were assessed and the y-axis for graph A is the quality score and that for graph B is the willingness to pay for one kilogram of rice. The solid line is the assessment outcome of Bengali farmers and the dotted line is the assessment outcome of Santal farmers. From both

TABLE 4.6: Effect on Assessment Outcomes, by the Order of Assessment

	Quality Score		**Willingness to Pay**	
VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**
Minority	-0.106	-0.029	-0.806*	-0.632**
	(0.114)	(0.080)	(0.407)	(0.292)
Minority×Order	0.004	-	0.005	-
	(0.007)		(0.018)	
Order	0.005	-	0.045**	-
	(0.004)		(0.017)	
Minority×First Half Order	-	-0.028	-	-0.186
		(0.110)		(0.346)
First Half Order	-	-0.042	-	-0.512**
		(0.065)		(0.248)
Blind Score	-0.028	-0.026	-0.099	-0.099
	(0.046)	(0.047)	(0.091)	(0.091)
Constant	6.653***	6.727***	38.727***	39.657***
	(0.308)	(0.315)	(0.899)	(0.700)
Rice Variety	Yes	Yes	Yes	Yes
Buyer Fixed Effects	Yes	Yes	Yes	Yes
Observations	3,600	3,600	3,600	3,600
R-squared	0.377	0.377	0.495	0.494

Robust standard errors clustered at the shop location level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: OLS regression estimates reported; the dependent variable in column 1 is quality score and that in column 2 is willingness to pay; Minority is a dummy variable that equals 1 if a rice sample was assigned an ethnic minority name and 0 otherwise; Order is the order in which rice samples were assessed; First Half Order is a dummy variable that equals 1 if the order is equal or below 15 and 0 otherwise; a shop location is the village/locality where a shop is located (25 locations in total).

figures, it is quite evident that there is no particular pattern in the assessment order, suggesting discrimination must be constant regardless of the order. To formally test this, we regress quality scores (willingness to pay) on assigned ethnicity, the order of assessment and their interaction, while also controlling for variables from our baseline specifications. This result is provided in Table 4.6. The 'Minority' term shows the average given score (column 1) or stated willingness to pay (column 3) to Santal farmers that are independent of any assessment order. With increasing order, we find that it does not have any effect on neither quality score nor willingness to pay to Santal farmers. Similarly, in columns 2 and 4, we provide results of an interaction between assigned ethnicity and an indicator variable that indicates whether the order of assessment was below or equal to 15 (i.e. first-half). With the interaction term being statistically insignificant, it shows that the difference in assessment outcomes between Santal and Bengali farmers does not differ across the first half and the last half of assessment.

Therefore, our analyses suggest that discrimination might not have been driven by stereotypes associated with skills or ability of ethnic minority farmers. Rather, rice

buyers might have developed a taste for paying less to ethnic minority farmers. Although, there still remain concerns about statistical discrimination being present because other beliefs, such as ethnic minority farmers have weaker bargaining abilities and they tend to accept lower prices or the belief that they have relatively less information about market prices of rice are not entirely eliminated. In these cases, the observed discrimination in terms of willingness to pay could be partly explained by the statistical model of discrimination.

4.4.4 Does Assessment Vary by Buyer Characteristics?

We have found our results to be consistent with *taste-based* discrimination, now it is to be seen whether such discrimination varies by characteristics of buyers, such as their age, years of education, monthly income, years of experience in the current business, level of interaction with ethnic minority farmers, and the level of intercultural competence. For example, older buyers might hold traditional values and might respond to negative social norms associated with ethnic minorities strongly relative to younger buyers who might be more tolerant of minorities. Likewise, having higher interaction with ethnic minority farmers in business might make buyers more welcoming towards the minority people. Similar arguments could also be made for the remaining characteristics. Therefore, here we formally test whether such characteristics have any influence on discrimination.

We present these results in Table C15 (quality score) in Appendix C and in Table 4.7 (willingness to pay). In both tables, column 1 shows estimates for rice samples assessed by buyers who belong to the panel title category, whereas column 2 shows estimates for rice samples assessed by buyers who do not belong to the panel title category. For example, Panel A title category is "Above Median Age", so column 1 (2) shows estimates by buyers who are above (below) the median age. The final column, column 3, shows the estimates of interaction between the buyer's characteristics and farmer's ethnicity (i.e. difference-in-difference). According to Table C15 in Appendix C, we find that buyers above the median age marginally discriminates Santal farmers in terms of quality assessment, while buyers below the median age do not. Instead, buyers below the median age category give a higher score to Santal farmers relative to Bengali farmers, although this is statistically insignificant at conventional levels. Moreover, the gap in discrimination between older and younger buyers is statistically significant at the 5% level (Panel A). Also, having relatively fewer years of experience in the business induces discrimination towards Santal farmers; however, the coefficients of the effects for less experienced buyers do not differ from that for highly experienced buyers (Panel D). In terms of interacting with ethnic minority farmers for business purpose, we find that, although both differences being statistically insignificant, buyers who interact more give higher score to Santal farmers and buyers who interact less give less score to Santal farmers compared to Bengali farmers, and the difference between these two coefficients

TABLE 4.7: Effect on Willingness to Pay by Buyer's Characteristics

	Belongs to panel title category?		
VARIABLES	**Yes (1)**	**No (2)**	**Difference (3)**
Panel A: Above Median Age			
Minority	-0.689**	-0.727	0.032
	(0.302)	(0.421)	(0.470)
Panel B: Above Median Education			
Minority	-0.678*	-0.749**	0.117
	(0.361)	(0.275)	(0.323)
Panel C: Above Median Income			
Minority	-0.414	-0.935**	0.524
	(0.280)	(0.361)	(0.396)
Panel D: Above Median Years in Business			
Minority	-0.339	-1.063**	0.685
	(0.275)	(0.371)	(0.436)
Panel E: Higher Interaction			
Minority	-0.047	-1.031***	1.092***
	(0.229)	(0.347)	(0.360)
Panel F: Above Median IC Competence			
Minority	-1.052**	-0.460*	-0.609
	(0.379)	(0.253)	(0.383)
Panel G: A Muslim			
Minority	-0.753**	-0.545	-0.213
	(0.298)	(0.393)	(0.417)
All Other Controls	Yes	Yes	Yes
Rice Variety	Yes	Yes	Yes
Buyer Fixed Effects	Yes	Yes	Yes

Robust standard errors clustered at the shop location level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: OLS regression estimates reported; the dependent variable is the buyer's willingness to pay for one kilogram of each rice samples (in Bangladeshi Taka); Minority is a dummy variable that equals 1 if a rice sample was assigned an ethnic minority name and 0 if an ethnic majority name was assigned; all panel title categories are dummies where it is equal to 1 if it belongs to the panel title category and 0 otherwise; Column 1 shows estimates for rice samples assessed by buyers who belong to the panel title category; Column 2 shows estimates for rice samples assessed by buyers who do not belong to the panel title category; Column 3 shows the estimates of interaction between buyer's characteristics and farmer's ethnicity (i.e. difference-in-difference); a shop location is the village/locality where a shop is located (25 shops in total); in total, 3,600 rice samples were assessed by 120 rice buyers (each assessed 30 rice samples).

of effects are statistically significant at the 5% level (Panel E). In terms of religious difference (Panel G), we see that neither Muslim nor non-Muslim buyers discriminate Santal farmers. For the rest (Panels B, C, and F), we do not observe ethnic discrimination varying with buyer characteristics.

In terms of discrimination based on willingness to pay (Table 4.7), we find that buyers who are both above and below the median age category discriminate Santal farmers, where younger buyers seem to discriminate more than older buyers, but their difference does not statistically differ (Panel A). In terms years of education, monthly income, years of experience, and the frequency of interaction with ethnic minority farmers, we see a very similar pattern: all buyers who fall below the median level in these characteristics are willing to pay, on average, less to Santal farmers relative to Bengali farmers (Panels B-E). However, differences between above and below categories in all but the frequency of interaction (Panel E, column 3) are not statistically significant at conventional levels. The difference between high and low interacting buyers is large and significant at the 5% level. In terms of intercultural competence level, both well and poorly informed buyers discriminate Santal farmers; however, discrimination seems much larger among well-informed buyers relative to poorly informed buyers, but this difference do not reach statistical significance (Panel F). Finally, with regards to religious differences, we see that Muslim buyers are willing to pay less to Santal farmers than what they would pay to Bengali farmers but we do not observe any bias among non-Muslim buyers (Panel G). Therefore, Muslim buyers seem to have an in-group bias towards ethnic majority farmers (who all have Muslim sounding names) whereas non-Muslims do not. However, the difference observed among Muslim buyers is not statistically different from that among non-Muslim buyers.

4.4.5 Can Ethnic Minority Farmers Avoid Discrimination?

By design, we decided to run experiments on three types of locations: multi-ethnic villages, mono-ethnic villages, and the main city in the district. This not only makes our study more externally valid but also allows us to check for heterogeneity in discrimination that is driven by the different levels of intergroup interaction. Our results suggest that ethnic discrimination in terms of willingness to pay is only present in the villages but not in the city. Moreover, for rice sold by ethnic minority farmers, city buyers seem to be willing to pay significantly higher price relative to buyers from the villages (although this is not statistically significant at conventional levels). Hence, one could argue that ethnic minority farmers from villages might benefit if they avoid selling their products to local buyers and instead go to the city to sell their products at a higher price, where they can receive a higher price while also avoid being discriminated. Therefore, the main aim of this subsection is to analyze this possibility.

A buyer's willingness to pay is the amount that farmers can expect to receive from buyers when they sell one kilogram of rice. Since selling to the village buyers incur no costs of transportation whereas selling to city buyers do, farmers' expected earnings is simply the amount received from buyers minus the cost of transportation (for simplicity, we assume this cost to be fixed).[19] In this scenario, minority farmers are faced with two choices: either (i) sell to the village buyers or (ii) sell to the city buyers. To find the

[19] From Table 4.1, we already know that around 90 percent of village buyers buy rice by visiting farmer households in person, which results in no transportation cost for farmers.

option that would generate the highest monetary benefit to minority farmers, we would compare earnings generated from selling a kilogram of rice at both locations. Through our experiment, we obtained willingness to pay of buyers from both villages and the city (summary available in Table 4.2). Then using the 2015 Bangladesh Integrated Household Survey (BIHS) data, we are able to find the cost of return travel from the city that includes both the opportunity cost of traveling (i.e. forgone income) and the cost of transportation. Assuming this cost to be fixed, from the BIHS data, we could use the difference in income generated from selling to city markets and selling from home or at local village markets as a proxy for the cost of travel.[20] Therefore, if the net earnings for selling one kilogram of rice in the city is higher than that in villages, then it would be beneficial for farmers from villages to sell their products to city buyers.

We present the average willingness to pay and the cost of travel across locations in Table C16 in Appendix C, where column 1 shows the averages in the city and column 2 shows the averages in villages. The raw difference clearly suggests that ethnic minority farmers would make a gain of 1.94 Taka per kilogram of rice if they sell it to city buyers, whereas ethnic majority farmers would make a gain of around 0.69 Taka per kilogram of rice if they sell it to city buyers. As both ethnic majority and minority farmers seem to benefit from selling to city buyers, we now test it formally in a regression framework.

To investigate this, we deduct the cost of travel from city buyers' willingness to pay only (as there are no costs involved when sold to villager buyers) and then regress the 'modified' willingness to pay on the assigned ethnicity of farmers, an indicator variable if the location is a village, and their interactions. Our regression output is given in Table 4.8. Columns 1 and 2 looks at the effects on earnings in villages and city separately, and then the interaction term in column 3 shows the difference-in-difference. As found earlier, Santal farmers are discriminated in the village (column 1) but not in the city (column 2). Here, adding 'Minority×Village' with 'Village' gives the difference in earnings by Santal farmers in the villages relative to the city, which is statistically significant at the 1% level (joint test of 'Village' and the interaction: F-test p-values< 0.01). Whereas for Bengali farmers, it seems they are better off in the villages where they can get higher prices for selling rice (coefficient of 'Village' in column 3). Therefore, selling to city buyers seem to be a profitable strategy for ethnic minority farmers only, as it generates 1.94 Taka more for selling one kilogram of rice. According to the 2015 BIHS data, farmers in the Northwestern part of Bangladesh sell 831.17 kilograms of rice on average each year and have a daily income of 225 Taka. This translates to an increase in earnings of around 1,600 Taka each year, which is equivalent to their week's income.

[20]For other types of costs, such as costs associated with mental stress, the anxiety of finding buyers, cost of searching buyers, etc., we assume that such costs are constant across the two locations as buyers' shops are always located at a central place, e.g. marketplaces, that is easy to find. Please also note that this cost is the same for both ethnic majority and minority farmers.

TABLE 4.8: Effect on Earnings of Farmers, by Location

VARIABLES	Village (1)	City (2)	Difference (3)
Minority	-0.994***	0.177	0.202
	(0.272)	(0.276)	(0.252)
Village	-	-	-0.332*
			(0.192)
Minority×Village	-	-	-1.182***
			(0.384)
Blind Score	-0.043	-0.092	-0.055
	(0.125)	(0.141)	(0.098)
Constant	39.217***	42.220***	39.564***
	(0.973)	(1.382)	(0.811)
Rice Variety	Yes	Yes	Yes
Buyer Fixed Effects	Yes	Yes	Yes
Observations	2,760	840	3,600
R-squared	0.521	0.340	0.492

Robust standard errors clustered at the shop location level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: OLS regression estimates reported; the dependent variable is the expected earnings of farmers for selling one kilogram of rice (in Bangladeshi Taka); Minority is a dummy variable that equals 1 if a rice sample was assigned an ethnic minority name and 0 if an ethnic majority name was assigned; Village is a dummy where it is equal to 1 if a buyer's business is in a village and 0 otherwise; a shop location is the village/locality where a shop is located (25 locations in total); in total, 3,600 rice samples were assessed by 120 rice buyers (each assessed 30 rice samples).

4.5 Discussion

Through our experiment, we find that ethnic majority rice buyers discriminate ethnic minority farmers by willing to pay them significantly less than what they are willing to pay ethnic majority farmers, and the main mechanism behind this behavior could be due to buyers having a *taste* for not paying more to minority farmers relative to majority farmers. With our limited data, we found evidence for the *taste*-based model of discrimination but there still remain concerns about statistical discrimination being present because other beliefs, such as ethnic minority farmers have weaker bargaining abilities and they tend to accept lower prices or the belief that they have relatively less information about market prices of rice are not entirely eliminated. In these cases, the observed discrimination in terms of willingness to pay could be partly explained by the statistical model of discrimination.

So, what could be the reasons behind developing such preferences towards ethnic minority farmers? One plausible reason is socialization of Bengalis from a very young age to dislike ethnic minorities. Ethnic minorities in Bangladesh are usually considered 'inferiors' by the ethnic majority (Bal, 2007), so it is very likely that Bengalis have developed negative attitudes toward minorities both consciously and unconsciously from their parents and the society, where such intolerance is an acceptable social norm. Bengalis socialize in an environment where discrimination and abuse of ethnic minorities across various social domains are not considered a crime, and their exclusion from social,

political, and economic opportunities are seen as customary (AIPP, 2010; Roy, 2012; IMF, 2013). Such views and beliefs have been perpetuated from generations through blind imitation of previous attitudes, and, hence, the ethnic majority buyers might have simply developed a *distaste* towards ethnic minority farmers over time.[21] While our study lack precise data or measures of buyers' socialization from the very young age, we deduce from existing literature on ill-treatment of ethnic minorities in Bangladesh that negative social learning might be a valid reason for developing such prejudicial attitudes (Bandura, 1977).

Another plausible reason is the lack of intergroup interaction. Allport (1954)'s *contact hypothesis* suggests that lack of intergroup contact might induce prejudice, and the most effective way to reduce prejudice is by increasing intergroup contact. Meta-analysis in both Pettigrew & Tropp (2006) and Paluck et al. (2018) show that the majority of studies investigating Allport's intergroup contact theory have found a positive association between intergroup interaction and reduction in prejudice towards out-group members. Brown et al. (2003) tests the hypothesis among Black and White high school student athletes and finds that having more Black athletes in a team improves racial attitudes of White athletes toward Blacks. Moreover, in economics, Rao (2018) induce contact between rich and poor students that improve attitudes of rich students towards poor students. In our paper, we also exploit a natural experiment and a survey measure of social interaction to test this conjecture. Buyers in our study reside and conduct daily businesses in either multi-ethnic villages, mono-ethnic villages or the main city. While residents in multi-ethnic villages are likely to have regular contact and interaction with ethnic minorities, residents in mono-ethnic villages and the city hardly interact with ethnic minorities.[22] Our regression results show that discrimination is more pronounced in mono-ethnic villages than in multi-ethnic villages, but this difference is statistically insignificant at conventional levels.[23] Interestingly, we do not observe any discrimination in the city, where intergroup contacts are also likely to be minimal, but it is highly likely that competitive market pressures have driven out discrimination from the market (Becker, 1957). With rice being a staple food and farmers not always being able to produce the adequate amount of rice each year (mostly due to occasional natural disasters in Bangladesh), buyers in a high competition market, such as in the city that are densely populated, might want to look past any prejudice they may hold to store rice to meet the existing demand. However, in this case, we do not have any measure of competition across locations. Furthermore, when we individually look at buyers' behavior who interact more often and compare it to that who interact less often with ethnic minorities,

[21] In Bangladesh, ethnic minorities also dislike the ethnic majority and have a very bitter relationship that often leads to interethnic violence. Therefore, we also cannot discard the idea that intergroup conflict might have developed prejudice towards ethnic minorities (Sherif, Harvey, White, Hood & Sherif, 1961).

[22] We also confirm this using a survey measure of how often buyers interact with ethnic minority farmers for business purpose (shown in Table 4.1).

[23] We are ruling out possibilities of buyers self-selecting into these locations in order to discriminate against ethnic minority farmers because, from Table 4.1, we see that their average age is 40 years and they have been residing in their present residences for a little over 33 years, and have been in their profession for around 15 years. Therefore, it is very unlikely that they have migrated to these locations at a very young age in order to discriminate against ethnic minority farmers.

we see a significant difference between the two suggesting that buyers who interact more with ethnic minority farmers discriminate less often than buyers who hardly interact. Therefore, in our study, it is also highly likely that interethnic interaction has an impact on the prejudicial attitude formation of ethnic majority buyers.

According to literature in social psychology, other plausible reasons for developing prejudicial attitude towards ethnic minorities could be due to Bengalis having a "deep-seated" personality trait that make them hostile towards people of 'inferior' social status (Adorno, Frenkel-Brunswik, Levinson & Sanford, 1950) or they have developed disliking towards minorities as they "break physical and social patterns", as minorities are very distinct from the majority in terms of race, culture, language, religion, customs, etc. (Gollwitzer, Marshall, Wang & Bargh, 2017). We believe a combination of the reasons that we discuss above might have developed a *distaste* among Bengali buyers towards ethnic minority farmers, wherein ethnic socialization and intergroup interaction play a significant role. Although we cannot explicitly claim which of the reasons truly affect prejudice that we document in this paper, we, nevertheless, find a way ethnic minority farmers could avoid being discriminated. Our analyses show that ethnic minorities would benefit significantly if they sell rice to the city buyers, a strategy that would also generate one week's additional income per year for ethnic minority farmers. While Paluck & Green (2009) and Bertrand & Duflo (2017) provide a range of policies that could be implemented to reduce discrimination, our back-of-the-envelope calculation suggest that ethnic minorities participating into competitive markets would be a good start in reducing discrimination.

4.6 Conclusion

We organized a competition amongst rice farmers followed by a battery of rice evaluation programs in the Northwestern part of Bangladesh where ethnic majority rice buyers were invited to assess rice samples to determine the winner of the competition. To experimentally measure whether the ethnic identity of farmers has any relationship with assessment outcomes, we randomly attached ethnic majority and minority sounding names to each rice sample to implicitly signal buyers about the ethnicity of participants in the rice competition. We find that buyers do not discriminate ethnic minority farmers in terms of the quality of rice they produce; instead, they discriminate them in terms of how much they are willing to pay for their rice. We argue this behavior to be consistent with the *taste-based* model of discrimination. Also, this pattern can only be observed among village buyers, where city buyers do not seem to discriminate ethnic minority farmers.

Exploring our data patterns further, we find that intergroup interaction affects the severity of discrimination. We interpret that a combination of interethnic interaction and socialization of ethnic majority farmers as being the underlying sources of the observed *taste-based* discrimination against ethnic minorities. This provides some directions for future research that could further explore underlying sources of prejudicial

attitude formation. For instance, studying the behavior of individuals who have socialized differently in multi-ethnic and mono-ethnic environments with a varying level of intergroup interactions would certainly disentangle the effect of socialization from intergroup contact on prejudice formation. Similarly, exploring this over different age groups and generations would further elucidate its fundamental sources to help shape existing policies. While our findings suggest that encouraging ethnic minority farmers to seek buyers from the city would be a reasonable step towards circumventing discrimination in this context, other costs associated with leaving family behind, breaking traditions, emotional burden, and so on, might not always make this strategy effective. Therefore, future research that takes more costs into account would certainly shed more light on this strategy.

With recent strands of economic literature suggesting preference being endogenous (Bowles, 1998), it is crucial to pinpoint the main underlying sources behind developing a strict preference against a certain social group. Only then effective policies and interventions could be devised to reduce or eliminate it.

Appendix C: Supplementary Tables and Figures

Table C1: List of Rice Varieties and Market Price

	Rice Variety	Market Price per kg
1.	Atash Grade 1	56
2.	Atash Grade 2	52
3.	Atash Grade 3	50
4.	Aush	45
5.	Gutishorna	42
6.	Jeera	58
7.	Paijam	60
8.	Parija	45
9.	Shorna	40

Note: All prices are in Bangladeshi Taka.

Table C2: List of Bengali and Santal Sounding Names

	Bengali Names	Santal Names
1.	Mohammad Mannan	Horen Hasda
2.	Rafiqul Islam	Hopna Kisku
3.	Jashim Ali	Swapon Murmu
4.	Abul Kalam	Anmel Hasda
5.	Ashraful Islam	Mungla Hembrom
6.	Khairul Islam	Phanichandra Hasda
7.	Mohammad Zakaria	Jogi Murmu
8.	Mazharul Islam	Piuch Tudu
9.	Mohammad Saifuddin	Robi Saren
10.	Imam Hossain	Joydeb Mardi
11.	Rajab Ali	Dhiren Hembrom
12.	Mohammad Rafique	Brijlal Kisku
13.	Borhan Hossain	Niren Mardi
14.	Mohammad Selim	Morme Tudu
15.	Amirul Islam	Philmon Saren

FIGURE C1: A Rice Board

Note: On top of each rice sample, on the left is the rice ID and next to it is the assigned name of the farmer.

TABLE C3: The Evaluation Sheet

Rice ID	**Name of Farmer**	**Quality Score**	**Willing to Pay**

Note: Buyers had to write the rice ID and then the farmer's name, and then give the quality score and write how much they are willing to pay for one kilogram of this particular rice (always in this order).

TABLE C4: Blind WTP and Quality Score Given to Actual Farmers

	All			Multi-Ethnic			Mono-Ethnic			City		
	Bengali (*Std. Dev.*)	**Santal** (*Std. Dev.*)	**MW-test *p*-values**	**Bengali** (*Std. Dev.*)	**Santal** (*Std. Dev.*)	**MW-test *p*-values**	**Bengali** (*Std. Dev.*)	**Santal** (*Std. Dev.*)	**MW-test *p*-values**	**Bengali** (*Std. Dev.*)	**Santal** (*Std. Dev.*)	**MW-test *p*-values**
Blind Quality Score	6.40	6.18	0.453	6.67	6.20	0.350	6.40	6.47	0.983	6.13	5.87	0.590
	(1.03)	(1.40)		(0.98)	(1.32)		(1.24)	(1.51)		(0.83)	(1.41)	
Blind WTP	36.93	36.27	0.122	34.87	35.00	0.770	35.80	34.80	0.161	40.13	39.00	0.200
	(3.60)	(4.20)		(3.18)	(3.23)		(2.62)	(2.24)		(2.61)	(5.29)	
Observations	45	45	-	15	15	-	15	15	-	15	15	-

Note: Bengali (Santal) means a rice sample was collected from a Bengali (Santal) farmer; Blind Quality Score is the quality score (between 0 to 10) given to a rice sample where 10 corresponds to the highest quality; Bind WTP is a buyer's willingness to pay (in taka) for 1 kilogram of a particular rice sample; MW-test is a two-sided Mann-Whitney U test.

TABLE C5: Effect of Assigned Ethnicity on Rice Quality Assessment: Village Only

VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**
Minority	-0.008	-0.031	-0.042	-0.042
	(0.065)	(0.065)	(0.056)	(0.057)
Blind Score	-	0.308***	-0.029	-0.029
		(0.072)	(0.060)	(0.061)
Constant	6.838***	4.914***	7.464***	6.763***
	(0.163)	(0.563)	(0.451)	(0.409)
Rice Variety	No	No	Yes	Yes
Buyer Fixed Effects	No	No	No	Yes
Observations	2,760	2,760	2,760	2,760
R-squared	0.000	0.023	0.166	0.369

Robust standard errors clustered at the shop location level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: OLS regression estimates reported; the dependent variable is quality assessment score given to rice samples (any number between 0 and 10, where 10 corresponds to the highest quality); Minority is a dummy variable that equals 1 if a rice sample was assigned an ethnic minority name and 0 if an ethnic majority name was assigned; Blind Score is the blind (i.e. no name was assigned to rice samples) quality score given to each rice sample; a shop location is the village/locality where a shop is located (25 shops in total); in total, 2,760 rice samples were assessed by 92 rice buyers from the villages (each assessed 30 rice samples).

TABLE C6: Effect of Assigned Ethnicity on Rice Quality Assessment: Multi vs Mono Ethnic Villages

VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**
Minority	-0.155***	-0.187***	-0.153***	-0.153**
	(0.048)	(0.046)	(0.053)	(0.054)
Multi-Ethnic	-0.464*	-0.472*	-0.434*	-0.526***
	(0.233)	(0.235)	(0.229)	(0.045)
Minority×Multi-Ethnic	0.246**	0.261**	0.186**	0.186*
	(0.099)	(0.096)	(0.089)	(0.090)
Blind Score	-	0.309***	-0.030	-0.030
		(0.071)	(0.061)	(0.062)
Constant	7.115***	5.190***	7.732***	6.827***
	(0.127)	(0.488)	(0.380)	(0.421)
Rice Variety	No	No	Yes	Yes
Buyer Fixed Effects	No	No	No	Yes
Observations	2,760	2,760	2,760	2,760
R-squared	0.010	0.033	0.176	0.370

Robust standard errors clustered at the shop location level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: OLS regression estimates reported; the dependent variable is quality assessment score given to rice samples (any number between 0 and 10, where 10 corresponds to the highest quality); Minority is a dummy variable that equals 1 if a rice sample was assigned an ethnic minority name and 0 if an ethnic majority name was assigned; Multi-Ethnic is a dummy variable that equals 1 if the location is a multi-ethnic village and 0 if the location is a mono-ethnic village; Blind Score is the blind (i.e. no name was assigned to rice samples) quality score given to each rice sample; a shop location is the village/locality where a shop is located (25 shops in total); in total, 2,760 rice samples were assessed by 92 rice buyers from multi and mono-ethnic villages (each assessed 30 rice samples).

Table C7: Effect of Assigned Ethnicity on Rice Quality Assessment: Multi-Ethnic vs City

VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**
Minority	-0.014	-0.048	-0.072	-0.072
	(0.101)	(0.134)	(0.133)	(0.135)
Multi-Ethnic	0.470*	0.464*	0.467*	-0.989***
	(0.249)	(0.257)	(0.251)	(0.075)
Minority×Multi-Ethnic	0.105	0.117	0.111	0.111
	(0.136)	(0.160)	(0.148)	(0.150)
Blind Score	-	0.402***	-0.053	-0.053
		(0.056)	(0.059)	(0.060)
Constant	6.181***	3.668***	7.084***	7.444***
	(0.148)	(0.370)	(0.388)	(0.388)
Rice Variety	No	No	Yes	Yes
Buyer Fixed Effects	No	No	No	Yes
Observations	2,490	2,490	2,490	2,490
R-squared	0.018	0.054	0.222	0.419

Robust standard errors clustered at the shop location level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: OLS regression estimates reported; the dependent variable is quality assessment score given to rice samples (any number between 0 and 10, where 10 corresponds to the highest quality); Minority is a dummy variable that equals 1 if a rice sample was assigned an ethnic minority name and 0 if an ethnic majority name was assigned; Multi-Ethnic is a dummy variable that equals 1 if the location is a multi-ethnic village and 0 if the location is the city; Blind Score is the blind (i.e. no name was assigned to rice samples) quality score given to each rice sample; a shop location is the village/locality where a shop is located (25 locations in total); in total, 2,490 rice samples were assessed by 83 rice buyers from multi-ethnic villages and the city (each assessed 30 rice samples).

Table C8: Effect of Assigned Ethnicity on Rice Quality Assessment: Mono-Ethnic vs City

VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**
Minority	-0.014	-0.033	-0.046	-0.046
	(0.102)	(0.118)	(0.108)	(0.110)
Mono-Ethnic	0.934***	0.937***	0.920***	-0.344***
	(0.196)	(0.201)	(0.202)	(0.059)
Minority×Mono-Ethnic	-0.141	-0.145	-0.113	-0.113
	(0.112)	(0.129)	(0.117)	(0.119)
Blind Score	-	0.226***	0.021	0.021
		(0.071)	(0.045)	(0.045)
Constant	6.181***	4.767***	6.181***	6.541***
	(0.148)	(0.422)	(0.291)	(0.265)
Rice Variety	No	No	Yes	Yes
Buyer Fixed Effects	No	No	No	Yes
Observations	1,950	1,950	1,950	1,950
R-squared	0.072	0.087	0.130	0.365

Robust standard errors clustered at the shop location level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: OLS regression estimates reported; the dependent variable is quality assessment score given to rice samples (any number between 0 and 10, where 10 corresponds to the highest quality); Minority is a dummy variable that equals 1 if a rice sample was assigned an ethnic minority name and 0 if an ethnic majority name was assigned; Mono-Ethnic is a dummy variable that equals 1 if the location is a mono-ethnic village and 0 if the location is the city; Blind Score is the blind (i.e. no name was assigned to rice samples) quality score given to each rice sample; Market Price is the actual market price of the rice; a shop location is the village/locality where a shop is located; in total, 1,950 rice samples were assessed by 65 rice buyers from mono-ethnic villages and the city (each assessed 30 rice samples).

Table C9: Effect of Assigned Ethnicity on Rice Quality Assessment: Village vs City

VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**
Minority	-0.014	-0.041	-0.060	-0.060
	(0.100)	(0.125)	(0.123)	(0.125)
Village	0.657***	0.655***	0.648***	-0.412***
	(0.222)	(0.230)	(0.228)	(0.069)
Minority×Village	0.006	0.009	0.025	0.025
	(0.121)	(0.143)	(0.136)	(0.138)
Blind Score	-	0.318***	-0.023	-0.023
		(0.060)	(0.046)	(0.047)
Constant	6.181***	4.194***	6.733***	7.093***
	(0.146)	(0.391)	(0.343)	(0.327)
Rice Variety	No	No	Yes	Yes
Buyer Fixed Effects	No	No	No	Yes
Observations	3,600	3,600	3,600	3,600
R-squared	0.025	0.050	0.166	0.377

Robust standard errors clustered at the shop location level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: OLS regression estimates reported; the dependent variable is quality assessment score given to rice samples (any number between 0 and 10, where 10 corresponds to the highest quality); Minority is a dummy variable that equals 1 if a rice sample was assigned an ethnic minority name and 0 if an ethnic majority name was assigned; Village is a dummy variable that equals 1 if the location is a village and 0 if city; Blind Score is the blind (i.e. no name was assigned to rice samples) quality score given to each rice sample; a shop location is the village/locality where a shop is located (25 shops in total); in total, 3,600 rice samples were assessed by 120 rice buyers (each assessed 30 rice samples).

TABLE C10: Effect of Assigned Ethnicity on Buyer's Willingness to Pay: Village Only

VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**
Minority	-0.864***	-0.952***	-0.994***	-0.994***
	(0.267)	(0.272)	(0.268)	(0.272)
Blind Score	-	1.170***	-0.043	-0.043
		(0.117)	(0.123)	(0.125)
Constant	37.684***	30.367***	39.668***	39.217***
	(0.632)	(1.169)	(1.177)	(0.973)
Rice Variety	No	No	Yes	Yes
Buyer Fixed Effects	No	No	No	Yes
Observations	2,760	2,760	2,760	2,760
R-squared	0.004	0.025	0.174	0.521

Robust standard errors clustered at the shop location level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: OLS regression estimates reported; the dependent variable is the buyer's willingness to pay for one kilogram of each rice samples (in Bangladeshi Taka); Minority is a dummy variable that equals 1 if a rice sample was assigned an ethnic minority name and 0 if an ethnic majority name was assigned; Blind Score is the blind (i.e. no name was assigned to rice samples) quality score given to each rice sample; a shop location is the village/locality where a shop is located (25 locations in total); in total, 2,760 rice samples were assessed by 92 rice buyers from the villages (each assessed 30 rice samples).

TABLE C11: Effect of Assigned Ethnicity on Buyer's Willingness to Pay: Multi vs Mono Ethnic Villages

VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**
Minority	-1.132***	-1.253***	-1.111***	-1.111***
	(0.261)	(0.267)	(0.281)	(0.286)
Multi-Ethnic	-0.116	-0.144	0.009	9.635***
	(1.129)	(1.129)	(1.117)	(0.260)
Minority×Multi-Ethnic	0.447	0.502	0.196	0.196
	(0.481)	(0.482)	(0.511)	(0.520)
Blind Score	-	1.172***	-0.044	-0.044
		(0.116)	(0.124)	(0.126)
Constant	37.753***	30.441***	39.671***	39.284***
	(0.571)	(1.071)	(1.055)	(1.020)
Rice Variety	No	No	Yes	Yes
Buyer Fixed Effects	No	No	No	Yes
Observations	2,760	2,760	2,760	2,760
R-squared	0.004	0.026	0.174	0.521

Robust standard errors clustered at the shop location level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: OLS regression estimates reported; the dependent variable is the buyer's willingness to pay for one kilogram of each rice samples (in Bangladeshi Taka); Minority is a dummy variable that equals 1 if a rice sample was assigned an ethnic minority name and 0 if an ethnic majority name was assigned; Multi-Ethnic is a dummy variable that equals 1 if the location is a multi-ethnic village and 0 if the location is a mono-ethnic village; Blind Score is the blind (i.e. no name was assigned to rice samples) quality score given to each rice sample; a shop location is the village/locality where a shop is located (25 shops in total); in total, 2,760 rice samples were assessed by 92 rice buyers from multi and mono-ethnic villages (each assessed 30 rice samples); the omitted category is mono-ethnic villages.

TABLE C12: Effect of Assigned Ethnicity on Buyer's Willingness to Pay: Multi-Ethnic vs City

VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**
Minority	0.345	0.243	0.186	0.186
	(0.277)	(0.295)	(0.252)	(0.256)
Multi-Ethnic	-1.060	-1.077	-1.031	-0.789***
	(1.201)	(1.193)	(1.172)	(0.259)
Minority×Multi-Ethnic	-1.030*	-0.996*	-1.088**	-1.088*
	(0.496)	(0.504)	(0.510)	(0.519)
Blind Score	-	1.211***	-0.218**	-0.218**
		(0.110)	(0.081)	(0.082)
Constant	38.697***	31.129***	42.106***	41.270***
	(0.607)	(1.135)	(1.129)	(0.712)
Rice Variety	No	No	Yes	Yes
Buyer Fixed Effects	No	No	No	Yes
Observations	2,490	2,490	2,490	2,490
R-squared	0.014	0.037	0.196	0.533

Robust standard errors clustered at the shop location level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: OLS regression estimates reported; the dependent variable is the buyer's willingness to pay for one kilogram of each rice samples (in Bangladeshi Taka); Minority is a dummy variable that equals 1 if a rice sample was assigned an ethnic minority name and 0 if an ethnic majority name was assigned; Multi-Ethnic is a dummy variable that equals 1 if the location is a multi-ethnic village and 0 if the location is the city; Blind Score is the blind (i.e. no name was assigned to rice samples) quality score given to each rice sample; Market Price is the actual market price of the rice; Quality Score is the rice quality assessment score given to each rice sample (any number between 0 and 10); a shop location is the village/locality where a shop is located; in total, 2,490 rice samples were assessed by 83 rice buyers from multi-ethnic villages and the city (each assessed 30 rice samples); the omitted category is the city.

Table C13: Effect of Assigned Ethnicity on Buyer's Willingness to Pay: Mono-Ethnic vs City

VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**
Minority	0.345	0.258	0.212	0.212
	(0.277)	(0.291)	(0.257)	(0.261)
Mono-Ethnic	-0.944	-0.934	-0.999	-0.551**
	(0.838)	(0.834)	(0.845)	(0.189)
Minority×Mono-Ethnic	-1.477***	-1.497***	-1.366***	-1.366***
	(0.383)	(0.393)	(0.372)	(0.378)
Blind Score	-	1.036***	0.137	0.137
		(0.137)	(0.119)	(0.121)
Constant	38.697***	32.222***	38.965***	38.130***
	(0.608)	(1.322)	(1.237)	(0.893)
Rice Variety	No	No	Yes	Yes
Buyer Fixed Effects	No	No	No	Yes
Observations	1,950	1,950	1,950	1,950
R-squared	0.028	0.054	0.144	0.389

Robust standard errors clustered at the shop location level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: OLS regression estimates reported; the dependent variable is the buyer's willingness to pay for one kilogram of each rice samples (in Bangladeshi Taka); Minority is a dummy variable that equals 1 if a rice sample was assigned an ethnic minority name and 0 if an ethnic majority name was assigned; Mono-Ethnic is a dummy variable that equals 1 if the location is a mono-ethnic village and 0 if the location is the city; Blind Score is the blind (i.e. no name was assigned to rice samples) quality score given to each rice sample; a shop location is the village/locality where a shop is located (25 locations in total); in total, 1,950 rice samples were assessed by 65 rice buyers from mono-ethnic villages and the city (each assessed 30 rice samples); the omitted category is the city.

TABLE C14: Effect of Assigned Ethnicity on Buyer's Willingness to Pay: Village vs City

VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**
Minority	0.345	0.249	0.202	0.202
	(0.273)	(0.288)	(0.248)	(0.252)
Village	-1.013	-1.018	-1.027	-0.642***
	(0.906)	(0.899)	(0.896)	(0.192)
Minority×Village	-1.210***	-1.199***	-1.182***	-1.182***
	(0.385)	(0.398)	(0.377)	(0.384)
Blind Score	-	1.148***	-0.055	-0.055
		(0.103)	(0.097)	(0.098)
Constant	38.697***	31.528***	40.710***	39.874***
	(0.600)	(1.042)	(1.102)	(0.811)
Rice Variety	No	No	Yes	Yes
Buyer Fixed Effects	No	No	No	Yes
Observations	3,600	3,600	3,600	3,600
R-squared	0.014	0.037	0.172	0.494

Robust standard errors clustered at the shop location level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: OLS regression estimates reported; the dependent variable is the buyer's willingness to pay for one kilogram of each rice samples (in Bangladeshi Taka); Minority is a dummy variable that equals 1 if a rice sample was assigned an ethnic minority name and 0 if an ethnic majority name was assigned; Village is a dummy variable that equals 1 if the location is a village and 0 if city; Blind Score is the blind (i.e. no name was assigned to rice samples) quality score given to each rice sample; a shop location is the village/locality where a shop is located (25 shops in total); in total, 3,600 rice samples were assessed by 120 rice buyers (each assessed 30 rice samples); the omitted category is the city.

TABLE C15: Effect on Rice Quality Score by Buyer's Characteristics

	Belongs to panel title category?		
VARIABLES	**Yes (1)**	**No (2)**	**Difference (3)**
Panel A: Above Median Age			
Minority	-0.123*	0.035	-0.160**
	(0.061)	(0.062)	(0.058)
Panel B: Above Median Education			
Minority	-0.079	-0.016	-0.059
	(0.072)	(0.062)	(0.079)
Panel C: Above Median Income			
Minority	-0.004	-0.069	0.068
	(0.068)	(0.084)	(0.113)
Panel D: Above Median Years in Business			
Minority	0.030	-0.109*	0.132
	(0.076)	(0.061)	(0.096)
Panel E: Higher Interaction			
Minority	0.077	-0.098	0.193**
	(0.073)	(0.058)	(0.088)
Panel F: Above Median IC Competence			
Minority	-0.079	-0.020	-0.062
	(0.080)	(0.071)	(0.114)
Panel G: A Muslim			
Minority	-0.036	-0.060	0.026
	(0.056)	(0.096)	(0.091)
All Other Controls	Yes	Yes	Yes
Rice Variety	Yes	Yes	Yes
Buyer Fixed Effects	Yes	Yes	Yes

Robust standard errors clustered at the shop location level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: OLS regression estimates reported; the dependent variable is quality assessment score given to rice samples (any number between 0 and 10, where 10 corresponds to the highest quality); Minority is a dummy variable that equals 1 if a rice sample was assigned an ethnic minority name and 0 if an ethnic majority name was assigned; all panel title categories are dummies where it is equal to 1 if it belong to the panel title category and 0 otherwise; Column 1 shows estimates for rice samples assessed by buyers who belong to the panel title category; Column 2 shows estimates for rice samples assessed by buyers who do not belong to the panel title category; Column 3 shows the estimates of interaction between buyer's characteristics and farmer's ethnicity (i.e. difference-in-difference); a shop location is the village/locality where a shop is located (25 locations in total); in total, 3,600 rice samples were assessed by 120 rice buyers (each assessed 30 rice samples).

TABLE C16: Earnings Per Kilogram of Rice for Ethnic Minority Farmers

	Panel A: Santal Farmers		
	City (1)	**Village (2)**	**Difference (3)**
Willingness to Pay	39.04	36.79	2.25
Cost of Travel	0.31	0	0.31
Net Earnings	38.73	36.79	1.94
	Panel B: Bengali Farmers		
Willingness to Pay	38.70	37.70	1.00
Cost of Travel	0.31	0	0.31
Net Earnings	38.39	37.70	0.69

Note: All earnings reported are average earnings and are in Bangladeshi Taka.

Chapter 5

Concluding Remarks

In this thesis, I explore important development issues such as why ethnic minorities fare badly in various socioeconomic outcomes and how the ever-growing religious institutions are shaping economic behaviors and preferences of individuals. I carried out lab-in-the-field and field experiments in Bangladesh to collect data and explore these questions. Through experiments, I measure (i) preference for interethnic competition of an ethnic minority group and the ethnic majority group in my first essay; (ii) altruism, dishonesty, risk-aversion, trust, and cooperation of orphan children studying and living in religious and secular orphanages in my second essay; and (iii) discrimination against ethnic minority farmers by the ethnic majority buyers in the agricultural market in my third essay.

Findings from my first essay show that ethnic minority members are reluctant to compete against ethnic majority members in a group where they are a numerical minority. Likewise, ethnic majority members have an inclination towards competing against ethnic minority members in a group where they are a numerical minority. Both groups in this context have been indoctrinated from a very young age to develop opposing stereotypes about one another: ethnic minorities grow up as inferiors to the ethnic majority and the ethnic majority grow up as a superior to ethnic minorities. So, when ethnic minority members are a numerical minority in a group, the stereotype of being ethnically inferior might have induced them to enter into less competition against the ethnic majority. In a similar manner, when ethnic majority members are a numerical minority in a group, the stereotype of being ethnically superior might have induced them to enter into more competition against ethnic minorities. Therefore, the unwillingness of ethnic minority members to engage in competition with the ethnic majority members might explain why they refrain from investing in education, apply for top-level positions, invest in competitive businesses, etc. – places that require constant competition with the ethnic majority members but also might help them improve their socioeconomic status. Therefore, policies such as improving the self-esteem of ethnic minorities or public awareness to break negative stereotypes might improve preferences for interethnic competition.

Findings from my second essay show that children studying and living in religious

orphan schools are more altruistic and honest compared to children studying and living in secular orphan schools. Also, these behavioral differences are entirely driven by children who have spent 6 or more years in schooling. However, children's behavior does not differ in terms of risk-aversion, trust, trustworthiness, and cooperation across schools. Although exploring behaviors and preferences among orphan children have advantages, such as restricting transmission of beliefs and preferences from parents to children, limited social learning by children after school enrolment, and parents not being able to choose schools for children, there might still remain some concerns regarding endogeneity. To eliminate such concerns, this essay also explores the behavioral differences of younger children across school types and then uses an instrumental variable approach to show that the initial results are robust. This essay highlights that religious education has important features that can affect how people make important economic decisions. Therefore, policymakers could extract important elements from the religious curriculum and introduce it to the secular curriculum to improve economic decision making of children. This is crucial for development because how their economic behaviors and preferences are shaped during childhood would affect how they would make important economic decisions as adults.

Findings from my third essay show that ethnic majority rice buyers discriminate against ethnic minority farmers in terms of how much they are willing to pay for their rice. These buyers' behavior is consistent with the *taste-based* model of discrimination, suggesting that ethnic majority buyers have developed a preference for not paying more to ethnic minority farmers compared to what they are willing to pay to ethnic majority farmers. As agriculture serves as a useful tool in reducing poverty, discrimination of ethnic minority farmers might be one of the reasons why they continue to live in poor conditions. This investigation further highlights geographic heterogeneity in terms of discrimination, where only buyers from the villages discriminate minorities while buyers from the city do not. Moreover, discrimination is also entirely driven by buyers who usually have less interaction with ethnic minority farmers. This further suggests that the underlying source of developing a strong preference against ethnic minority farmers might have emerged from a lack of interethnic interaction. Exploring further also suggest that ethnic minority farmers would benefit significantly by selling their products to the city buyers only. While encouraging minority farmers to travel to the city to sell their rice would be a good policy tool for avoiding discrimination; however, there are many other costs that are associated with traveling to the city that this essay does not take into account. Also, how that might affect the welfare of ethnic majority buyers in the villages are also unknown and exploring it is outside the scope of this essay.

To better inform policies, replication of research is important. Since cultures and social norms would certainly vary across countries and societies, replicating these three studies would further refine our understanding of decision making of individuals across various cultures and societies.

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Experimental Instructions and Surveys

Chapter 2 Instructions

General Instructions

Welcome to this study of decision making. The experiment will take about 60 minutes during which you will be asked to play some basic games. There will be three stages with three different instructions. But all instructions are very simple, and if you follow them carefully, you can earn a considerable amount of money. Out of these three stages, you will only be paid according to one stage, which will be determined at the end using a lottery. For showing up today you will receive 100 Takas. All the money you earn will be paid to you, privately and in cash, at the end after you complete a short exit survey. You will also be told how well you have scored in the payoff stage at the end, in private. This experiment will only be considered complete when you complete the exit survey. If you fail to complete the exit survey, then you will only receive the show-up fee.

The game is to separate lentils from a mix of rice and lentils, and then gather those separated lentils into an empty bowl. Each lentil separated will earn you money but each rice grain picked will lose you money. In short, lentils will win you money but rice will lose you money. So, you have to be careful not to separate rice along with lentils. There will be three stages and each stage will last for 60 seconds or 1 minute. Instructions for each stage are different and will be read aloud before each stage.

If you do not agree to take part in this experiment, then please raise your hand now. If you do not raise your hand, then we will assume you do not have any questions regarding the nature of this study and we will proceed to collect consents. If you do not want to participate, then you will only receive the show-up fee. Only people who participate will receive any money they earn during the experiment along with the show up fee.

Now, to better understand our instructions, we will readout some frequently asked questions and their answers to you. Please listen carefully.

- What do you need to separate from the mix, lentils or rice? [Answer: *Lentils*]

- What will earn you money, lentils or rice? [Answer: *Lentils*]
- What will lose you money, lentils or rice? [Answer: *Rice*]
- How many stages does this experiment have? [Answer: *Three*]
- For how many stages will you be paid at the end? [Answer: *One*]
- How will the payment-stage be determined at the end of the experiment? [Answer: *By a lottery*]
- What do you have to do if you have a question or want to withdraw? [Answer: *Raise hand*]
- Will you receive money if you decide to leave? [Answer: *No*]

Instruction: Stage 1

In this stage, you will have to separate lentils from a mix of rice and lentils which is in the bowl right in front of you. You have to pick lentils and put it into the empty bowl. For each lentil separated, you will earn 5 Takas but, if you also separate rice and put it into the empty bowl, where you are supposed to keep lentils only, then you will lose 5 Takas for each grain of rice. In short, each lentil will earn you 5 Takas and each rice will lose you 5 Takas.

So, if you separate 2 lentils, you will get 10 Takas. If you separate 5 lentils, you will get 25 Takas. If you separate 10 lentils, you will get 50 Takas. But if you separate 10 lentils along with 1 grain of rice, you will get 45 Takas, because you lose 5 Takas for separating 1 grain of rice. If you separate 10 lentils along with 10 grains of rice, you will get 0 Takas or no money, because you lose 50 Takas for separating 10 grains of rice. If you separate 10 lentils along with 11 rice, you still get no money, because you cannot earn less than zero. Therefore, the more lentils you pick, the more money you will earn.

You have 60 seconds to complete this task. We will tell you when to start and when to stop performing the task. Please stop immediately when we ask you to stop. If you do not stop, then you will not earn anything from this stage.

Now, to better understand our instructions, we will readout some frequently asked questions and their answers to you. Please listen carefully.

- If you separate 10 lentils and no rice, what will be your final score? [Answer: *10*]
- If you separate 10 lentils and 1 grain of rice, what will be your final score? [Answer: *9*]
- If you separate 10 lentils and 10 grain of rice, what will be your final score? [Answer: *0*]
- If you separate 10 lentils and 15 grain of rice, what will be your final score? [Answer: *0*]
- How much will you earn per lentil? [Answer: *5 Takas*]
- How much will you lose per grain of rice? [Answer: *5 Takas*]

- What is the duration of this task? [Answer: *60 seconds*]

Do you have any question?

This is the first of three stages, so there will be two more stages after this. At the end, there will be a lottery which will determine the payoff stage out of the three and you will be paid according to your score in that stage only. So, if the lottery determines this stage, then you will be paid according to this stage only. You will be paid in cash at the end. If you have any questions then please raise your hand now. If you do not, then we will proceed with the task.

Instruction: Stage 2

In this stage, you will have to separate lentils from a mix of rice and lentils, which is in the bowl right in front of you. You have to pick lentils and put it into the empty bowl. However, in this task, you will have to outperform all the other members in your group. That means you can only receive money if you pick more lentils than the other 5 members in your group. If you succeed to score the highest and win this task, then for each lentil separated, you will earn 30 Takas but if you also separate rice and put it in the empty bowl, where you are only supposed to keep lentils, then you will lose 30 Takas per grain of rice. In short, if you pick the most number of lentils, then each lentil will earn you 30 Takas. But if you cannot outperform your group members, then you will earn no money from this task. In case of a tie, the winning amount will be divided equally.

So, if you score the highest by picking 10 lentils, with all the other 5 members picking less than 10 lentils, then you will get 300 Takas and others will get no money. If you pick 10 lentils and 1 grain of rice and another member of your group picks 10 lentils but no rice, then it will mean that the other member has picked more lentils than you did, which means you lost the task and you will earn no money. That member, on the other hand, will win the task and will get 300 Takas with other members earning no money. If you score 11 lentils with 1 grain of rice and another member scores 10 lentils with no rice, then your score will be tied with that member and the winning amount will be divided equally. In this case, each tied winner will earn 150 Takas, with others earning no money at all.

Only the winner(s) will earn money, while the losers will get no money from this task. Therefore, the more lentils you pick compared to your group members, the higher your chances will be to win the task and earn 30 Takas per lentil.

You have 60 seconds to complete this task. We will tell you when to start and when to stop performing the task. Please stop immediately when we ask you to stop. If you do not stop, then you will not earn anything from this stage.

Now, to better understand our instructions, we will readout some frequently asked questions and their answers to you. Please listen carefully.

- Will you win the task if you pick the maximum number of lentils in your group? [Answer: *Yes*]
- Will you win the task if your group members pick more lentils than you? [Answer: *No*]
- What do you have to do to win this task and to win money? [Answer: *Pick the highest number of lentils*]
- If you separate 10 lentils and another group member separates 11 lentils, then who will win this task, you or your group member? [Answer: *Group member*]
- If you score 10 lentils and all other group members score 9 lentils or less, then who will win this task, you or your group members? [Answer: *You*]
- If you separate 10 lentils and 1 grain of rice, what will be your final score? [Answer: *9*]
- If you and another group member separate 10 lentils each, then what will happen to the winning money? [Answer: *Money will be divided equally*]
- How much will you earn per lentil if you win? [Answer: *30 Takas*]
- How much will you lose per rice if you win? [Answer: *30 Takas*]
- What is the duration of this task? [Answer: *60 seconds*]

Do you have any questions?

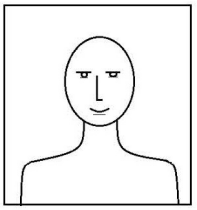
Instruction: Guessing Game

Thank you for completing the first two stages of this experiment. An assistant will privately ask you, one by one, to go to the registration desk and guess your relative rank according to lentils you picked in your group in Stage 2. To guess your rank, you will be shown an image with 6 heads placed vertically and you will have to point out where you think you belong. For example, if you think you have picked the highest number of lentils in your group in Stage 2, then you will have to point to the head at the top. If you think you were the second best then point to the head below the top. Similarly, if you think you performed the worst, then point to the head at the bottom. If you guess correctly, that is, if your guess matches with your actual rank in Stage 2, then you will get 50 Takas. If your guess does not match with your actual score from Stage 2 then you will get no money.

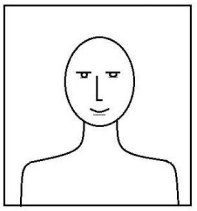
Do you have any questions? If not, then please be seated. An assistant will privately ask you to go to the registration desk where you can make your guesses.

Instruction: Stage 3

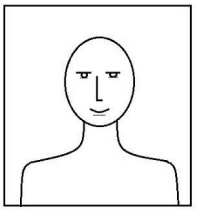
In this stage, you will have to separate lentils from a mix of rice and lentils, which is in the bowl right in front of you. You have to pick lentils and put it into the empty bowl. However, before performing the task, we will now ask you to choose one of the two options according to which you wish to be paid in this stage. The two options are:



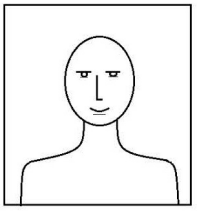
Rank 1



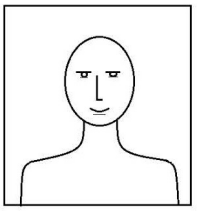
Rank 2



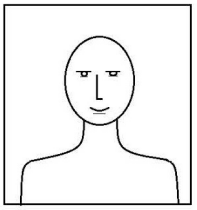
Rank 3



Rank 4



Rank 5



Rank 6

Option 1:

If you choose this option, you will get 5 Takas for each lentil you pick and lose 5 Takas for each rice grain you pick. So, this is exactly like the first task you completed. If you pick 2 lentils, then you will get 10 Takas. If you pick 10 lentils, then you will get 50 Takas. But if you pick 10 lentils along with 1 grain of rice, then you will get 45 Takas, because you lose 5 Takas for picking 1 grain of rice. Therefore, the more lentils you pick, the more money you will earn.

Option 2:

If you choose this option, you will only earn money if your score is higher than your group members' scores from Stage 2. If you succeed to surpass your group members' lentil count from Stage 2, then you will get 30 Takas for each lentil you pick but also you will lose 30 Takas for every rice you pick. If you do not manage to score higher than your group members' scores from Stage 2, then you will earn no money in this task. So, this is very much like the second task you completed, but now you will try to pick more lentils than what your group members picked in the second stage.

So, if you pick 10 lentils, which is also higher than all other 5 members' scores from Stage 2, then you will win this task and you will get 300 Takas. If you pick 10 lentils, which is not higher than all 5 group members' scores from Stage 2, then you will lose this task and you will not get any money. Therefore, the more lentils you pick compared to your group members' score in Stage 2, the higher your chances will be to win the task and earn 30 Takas per lentil.

You have 60 seconds to complete this task. We will tell you when to start and when to stop performing the task. Please stop immediately when we ask you to stop. If you do not stop, you will not earn anything from this stage.

Now, to better understand our instructions, we will readout some frequently asked questions and their answers to you. Please listen carefully.

- In which option do you earn 5 Takas per lentil? [Answer: *Option 1*]
- In which option do you need to score higher than your group members' score from Stage 2 in order to win money? [Answer: *Option 2*]
- In which option do you earn 30 Takas per lentil if you score higher than your group members' scores from Stage 2? [Answer: *Option 2*]
- If you choose Option 1, then how much will you earn per lentil? [Answer: *5 Takas*]
- If you choose Option 2, and pick more lentils than your group members' scores from Stage 2, then how much will you earn per lentil? [Answer: *30 Takas*]
- If you choose Option 2, and pick less lentils than your group members' scores from Stage 2, then will you win any money? [Answer: *No*]
- What is the duration of this task? [Answer: *60 seconds*]

This is the final stage of this experiment. After this task, there will be a lottery which will determine the payoff stage and you will be paid according to your score in that stage only. You will be told how well you have performed in that stage and will be paid in cash at the end. If you have any questions, please raise your hand. If you do not, then we will proceed with the task.

Do you have any questions? If not, then an experimenter will ask you privately to go to the registration desk and make your choice on how you want to be paid: according to Option 1 or Option 2?

Instruction: Risk Game

Welcome to this study of decision-making. This is a bonus game which will take about 15 minutes. The instructions are simple, and if you follow them carefully, you can earn a considerable amount of money. All the money you earn is yours to keep and will be paid to you, in cash, immediately after the experiment ends.

At the beginning of this experiment, you will receive 20 Takas. You are asked to choose the portion of this amount (between 0 and 20) that you wish to invest in a risky option. The rest of the money will be accumulated in your total balance.

The risky investment: there is an equal chance that the investment will fail or succeed. If the investment fails, you lose the amount you invested. If the investment succeeds, you receive 6 times the amount invested.

How do we determine if you win? After you have chosen how much you wish to invest, we will toss a coin to determine whether you win or lose. If the coin comes up heads, you win 6 times the amount you chose to invest. If the coin comes up tails, you lose the amount invested.

Examples

- If you choose to invest nothing, you will get the 20 Takas for sure. That is, the coin flip would not affect your profits.
- If you choose to invest all of the 20 Takas, then if the coin comes up heads, you win 120 Takas, and if the coin comes up tails, you win nothing and end up with 0.
- If you choose to invest 10 Takas, then if the coin comes up heads, you win 70 Takas, and if the coin lands on tails, you end up with 10 Takas.

Do you have any questions? If not, then an experimenter will ask you privately to go to the registration desk and make your choice on how much you want to bet in the lottery.

Chapter 2 Survey

ID Number: ☐

Age (in years): ☐

Ethnicity (Tick one): Santal ☐ ; Bengali ☐

Handedness (Tick one): Right ☐ ; Left ☐ ; Ambidextrous ☐

Marital status (Tick one): Single ☐ ; Married Monogamous ☐ ; Married Polygamous ☐ ; Widow ☐ ; Divorced ☐ ; Other ☐

How many times have you been married: ☐

Years of education: ☐

Highest level of education completed: ☐

Father's education: ☐

Mother's education: ☐

Occupation of father:
Occupation of mother:
Income of father (if known):
Income of mother (if known):
Number of children (if any):
Do your children go to school or work:
Your work type:
Your daily wage (in Taka):
Your monthly income (in Taka):
Amount of land own (in katha):
Distance to nearest school:
Distance to nearest medical centre:
When did you or your family last migrated:
Which village are you from:
Do you cook at home?
Have you ever cooked rice or lentils?
Were you familiar with the task?
Could you identify properly who was Santal/Bengali in your group?
How did you identify?
How many Santals/Bengalis were in your group? Name all five.

Did you (personally) know anyone from your group?

Intercultural Competence Questions

- What is the language spoken by Santals (Bengalis)?
- Do you speak that language?
- What is their major religion?
- What is their major religious festival?

Thank you for completing the survey!

Chapter 3 Instructions

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 Takas worth of delicious food in it so you should not feel bad about not earning money if you leave.

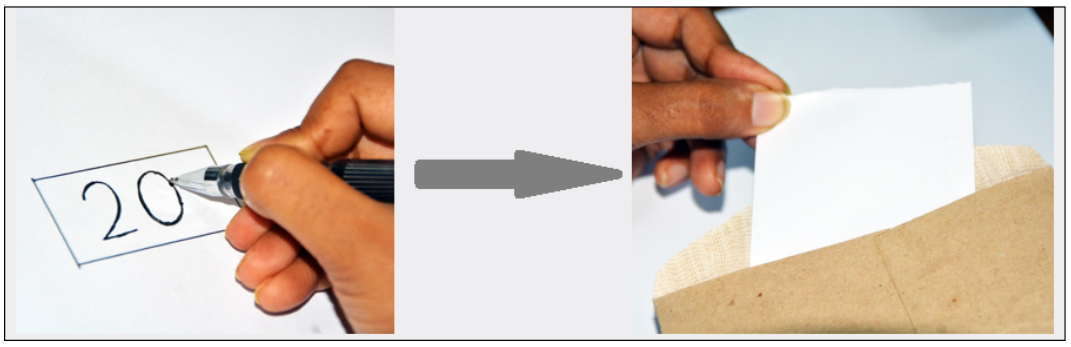
Now we will explain the rules of the first game. Before we do that, do you have any questions? [*pause*] Are you happy with the rules that I just told you? [*pause*] If you are not happy or do not wish to take part then you can raise your hand at any time to say so.

Game 1 (Donation)

For participating in this game, we will give you 50 Takas. Now you can decide how much of this 50 Takas 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 Takas 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 Takas for yourself.
- If you donate all money or 50 Takas then the orphan school will receive 50 Takas from you but you will have no money for yourself.
- If you donate 10 Takas then the orphan school will receive 10 Takas from you but you will have 40 Takas for yourself.
- If you donate 25 Takas then the orphan school will receive 25 Takas from you but you will have 25 Takas for yourself.
- If you donate 45 Takas then the orphan school will receive 45 Takas from you but you will have 5 Takas 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 Takas 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 Takas 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 Takas.

Game 2 (Investment)

For participating in this game, we will give you 50 Takas. Now you can decide how much of this 50 Takas that you just earned you wish to invest in a risky lottery. This can be any amount between 0 and 50 Takas. 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 Takas 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.

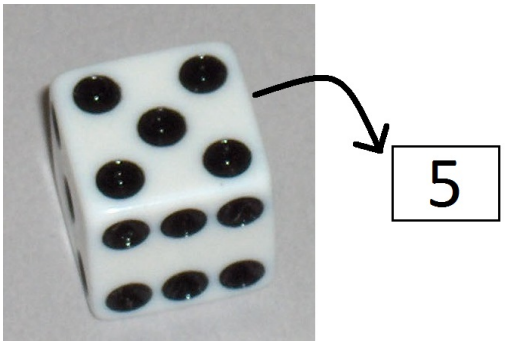
Risky lottery: In this lottery there is an equal chance you will fail or succeed, which will be determined by a coin toss. After you choose how much you wish to invest in this lottery, we will do a coin toss to determine whether you win or lose. If the coin comes up heads, you win 3 times the amount 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 Takas for sure. That means, we will not do any coin flips for you.
- If you choose to invest all of 50 Takas, then if the coin comes up heads, you win 150 Takas, and if it comes up tails, you win nothing and end up with 0.
- If you choose to invest 20 Takas, then if the coin comes up heads, you win 60 Takas from this lottery which will then be added to the remaining 30 Takas which you did not invest. But if it comes up tails, you only end up with 30 Takas which you did not invest.
- If you choose to invest 5 Takas, then if the coin comes up heads, you win 15 Takas from this lottery which will then be added to the remaining 45 Takas which you did not invest. But if it comes up tails, you only end up with 45 Takas 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.

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 Takas 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 Takas from this game. However, if you are unlucky and get all 1's in all 10 rolls, the you will receive 10 Takas 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.

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 Takas. 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 Takas. 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 Takas. Therefore, if you both decide to keep the money to yourselves then both of you will have 50 Takas. However, if you both decide to transfer all to one another, then both of you will end up with 100 Takas 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 Takas from the other person and the person you are playing with will receive 100 Takas from you.
- If none of you transfer your money, then you will be able to keep the 50 Takas to yourself and the other person will also be able to keep his 50 Takas 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 Takas from you, that you transferred, and 50 Takas that he did not send you. In total he will end up with 150 Takas 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 Takas and the doubled amount of 100 Takas from the other person, in total 150 Takas. 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.

Game 5 (Trust)

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

the other person's outcome will be 200 Takas.

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.

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

- Imagine the other person transfers all of his 50 Takas to you. Then you will receive the tripled amount, 150 Takas from him. Now, you have 150 Takas plus 50 Takas, or 200 Takas, and the other person has nothing. If you send nothing back to that person, then your outcome will be 200 Takas and the other person's outcome will be 0 Takas.

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 Takas 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.

Answer Sheets

Donation amount: ☐

Dice Throw Outcomes

Rolls	1 ↓	2 ↓	3 ↓	4 ↓	5 ↓	6 ↓	7 ↓	8 ↓	9 ↓	10 ↓
Outcomes										

Transfer amount (*cooperation*): ☐

Transfer amount (*trust*): ☐

Received amount (*trust*): ☐

Chapter 4 Instructions and Adverts

Advert for Rice Competition (Farmers)

[While talking to the male head of the household who is a farmer]

The NGO *Ashrai* is organizing a rice competition in your village. The idea is to reward the farmer who has cultivated the "best" rice in this region during the last rice growing season. To participate, all you have to do is submit 500 grams of rice that you cultivated during the most recent season. Your rice will be judged by rice buyers from various locations but they will not be from your own village. Based on buyers' scores, the farmer with the highest total score would receive a cash prize of 2,000 Taka.

If you wish to take part then please submit 500 grams of your cultivated rice. We would only contact the winner after 6 weeks. If you have any questions then you can either ask me now or you can call Abu Siddique at [phone number].

Thank you!

Advert for Rice Assessment (Buyers)

We are organizing a competition on rice quality produced by local farmers in the Rajshahi region. Farmers have already submitted their rice for the competition. Now, we need rice buyers to assess these rice samples to determine the winner. Based on your assessment, the farmer who receives the highest total score would receive a monetary reward. Also, by taking part, you will receive 200 Taka in cash. In addition, you will have a chance to earn 150 Taka by assessing rice samples. Therefore, by taking part, you can earn up to 350 Taka for 60 minutes of your time.

If you wish to take part, then please go to [location] on the [date] at [time].

If you have any questions then you can either ask me now or you can call Abu Siddique at [phone number]. Further details will be provided at the time of the assessment.

Thank you!

Instructions (Evaluation Program)

Welcome to our rice quality assessment program. This session will last for 50 minutes during which you will be asked to assess rice quality of 30 different rice samples produced by 30 different farmers from villages of the Rajshahi region. These farmers are participants in a rice competition that we are organizing, where the farmer who

cultivated the best rice during the most recent rice season will win a cash prize of 2,000 Taka. We have recruited you to determine the winner of the competition.

We will give you a big board which will have 30 different rice samples attached to it [*show them a board*]. Each rice sample will have a rice ID and the name of the farmer who has cultivated that particular rice. All you have to do is to look at each rice sample closely to check its quality and then give a quality score of between 0 to 10 for each rice sample, where 0 is the lowest score (indicating the rice quality is extremely bad) and 10 is the highest score (indicating the rice quality is very good). Then for that same rice sample, you will also have to say how much you are willing to pay for one kilogram of that rice. In short, you will analyze each rice sample and then give that rice a quality score and a price that you are willing to pay per kilogram on a separate piece of paper that we will provide. Before writing scores and prices, you will have to copy the rice ID and the name of the farmer for each sample. For a final score, we would give 50% of the weight to quality score and the remaining 50% to price. Therefore, both quality score and price are equally important to determine the winner. In the end, the farmer with the highest overall score will win a cash prize of 2,000 Taka. Please see the example below:

Example: If you think a rice with rice ID 01 produced by [First Name] [Surname] is of excellent quality then you could give this sample a score of, for example, 8 or 9 or 10 and state how much you are willing to pay, for example, 38 or 48 or 58 Taka per kilogram of this rice. In that case, you will have to first copy the rice ID, the name of the farmer, then write the quality score and then state the price that you are willing to pay. You always have to write it in this order (from left to right):

Rice ID	**Name of Farmer**	**Quality Score**	**Willing to Pay**
01	[First Name] [Surname]	10	58

This is only an example. You can give any score or state any price you like. Please raise your hand if you have any questions.

Along with the participation fee of 200 Taka, you can also earn 5 Taka for assessing each rice sample. That means you can earn up to 150 Taka when you assess all 30 rice samples. After completing this task, we will ask you to fill out a short survey that will not take more than 10 minutes. You can leave blank any question that you are not willing to answer.

Please do not talk to other buyers or show them your scores. Please assess rice samples privately.

Do you have any questions?

Now we will distribute the boards with rice samples.

Chapter 4 Survey

(*Please leave blank if you do not want to answer a question*)

ID Number:
Age (in years):
Marital Status:
Number of children (if any):
Ethnicity (tick one): Adivasi / Bengali
Maximum education obtained:
Occupation of mother (if known):
Occupation of father (if known):
Income of mother (if known):
Income of father (if known):
Education of mother (if known):
Education of father (if known):
Years in current occupation:
Is rice-buying your own business or do you work for someone else (tick one)? own / someone else
What is your monthly income (in Taka):
How much land do you own (in Katha):
When did your family last migrate (in years):
Which village are you from:
In which market is your rice shop:
How often do you buy rice from Adivasi farmers in a month (tick one):
very often / often / not that often / not at all
How often Adivasi farmers bargain while selling rice (tick one):
very often / often / not that often / not at all
How often Bengali farmers bargain while selling rice (tick one):
very often / often / not that often / not at all
Do you buy rice by going door-to-door (tick one)? Yes / No
What is the main feature you look at while assessing rice quality (tick one):
shape / colour / chalkiness / size / proportion of damaged grains / something else
How much rice do you buy in a month (in kilograms):

Thank you for completing the survey!