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Nikoloz Kudashvili

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Dissertation Committee:

Randall K. Filer (CERGE-EI, Charles University; Hunter College, City University of New York; Chair)

Michal Bauer (CERGE-EI, Charles University; Chair)

Nikolas Mittag (CERGE-EI, Charles University)

Jan Zápal (CERGE-EI, Charles University)

Referees:

Sigrid Suetens (Tilburg University)

Basit Zafar (University of Michigan)

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Abstract

In the first chapter, I study the sources of discrimination. Preference-based discrimination is usually deep-seated and takes a long time to be dealt with. By contrast, statistical discrimination can be eliminated or reduced in a relatively shorter period. The latter type of discrimination is based on stereotypes, which can be overcome by providing relevant information. I conduct a field experiment to measure the extent and nature of discrimination in the Georgian land market. The experiment is designed to uncover sources of statistical discrimination due to different beliefs about foreign investors. Discrimination is measured by the difference in price offers to foreign and Georgian investors. I find that the magnitude of discrimination shrinks significantly once foreign investors signal their willingness to search and pay the lease price in advance. This suggests that discrimination is largely driven by stereotypes about search costs and the payment reliability of foreign investors - leaving no or very little preference-based discrimination. Knowing the source of discrimination can be helpful to policy makers in framing anti-discriminatory legislation.

In the second chapter, we study minorities' response to discrimination. Discrimination against minorities is pervasive in many societies, but little is known about strategies that minorities apply to minimize discrimination. In our trust game with 758 high-school students in Georgia, ethnic Georgian trustors discriminate against an ethnic Armenian minority group. We introduce an initial signaling stage to investigate Armenians' willingness to hide their ethnicity to avoid expected discrimination. 43 percent of Armenian trustees untruthfully signal that they have a Georgian name. Signaling behavior is driven by expected transfers and identity-based motives. This strategic misrepresentation of ethnicity increases Georgian trustors' expected back transfers and eliminates their discriminatory behavior.

In the third chapter, we examine the effects of informational treatment on student educational choices. Students may hold inaccurate beliefs about earnings and employment opportunities when making their education decisions. This paper analyzes the effects of information provision on students' intended and actual college major choices in Georgia. Secondary school students in our experiment systematically overestimated the earnings and unemployment rates of college graduates. We find that 10 percent more students who received information on actual earnings and unemployment changed their final college major choices than others. The changes in their majors are partly driven by differences in the perceived and actual unemployment rates, whereas the earning differences do not appear to play a role. We also estimate spillover effects on students who do not receive information directly, and show that they matter, but only for older students who are closer to high school graduation. Importantly, we find that the immediate changes in the intended choices are not linked to the final major choices, suggesting that measuring the effects of information on immediately expressed intentions may not be sufficient to understand how information affects actual real-life decisions. We find that both direct and indirect information provision have sizable effects on student college major choices.

Abstrakt

V první kapitole tržní diskriminace je dobře zdokumentována empirickými studiemi. Field experimenty jsou naproti tomu méně úspěšné při identifikaci zdrojů diskriminace. Tento článek předkládá experimentální důkazy o rozsahu a povaze diskriminace na trhu s pozemky v Gruzii. Cílem experimentu je odhalit možné zdroje statistické diskriminace v důsledku rozdílných názorů na zahraniční investory. Diskriminace je měřena rozdílnými cenami pro domácí a zahraniční investory. Zjistili jsme, že síla diskriminace významně klesá s ochotou zahraničních investor důkladněji prozkoumat trh a platit nájem předem. To naznačuje, že diskriminace je ovlivněna především stereotypy o nákladech na průzkum trhu a schopnosti zahraničních investorů platit nájem předem. Vliv diskriminace na základě preferencí mezi zahraničními a domácími investory se zdá být zanedbatelný. Znalost původu diskriminace může být užitečná při tvorbě antidiskriminačních zákonů.

V druhé kapitole diskriminace vůči menšinám je přítomna v mnoha společenských skupinách, ale málo se ví o strategiích, které mohou menšiny použít k minimalizaci diskriminace. Využíváme experiment typu trust game se 758 žáky středních škol z Gruzie, kde etničtí Gruzínci diskri-minují etnické Arményi. Zavádíme úvodní fázi signalizace k prozkoumání ochoty Arménů skrýt svůj etnický původ a vyhnout se tak očekávané diskriminaci. 43 procent arménských účastníků falešně signalizovalo, že mají gruzínské jméno. Falešná signalizace je motivována očekávanými transfery a nefinančními důvody. Strategické klamání o etnické příslušnosti zvyšuje gruzínským účastníkům očekávané zpětné transfery a eliminuje diskriminující cho-vání.

Ve třetí kapitole studenti mohou mít při rozhodování o vzdělání nepřesné představy o budoucích výdělcích a uplatnění. Tento článek zkoumá vliv informování na zamýšlenou a výslednou volbu studijního oboru v Gruzii. Středoškolští studenti v našem experimentu systematicky nadhodnocují příjmy a nezaměstnanost vysokoškolských absolventů. Zjišťujeme, že o 10 % více studentů, kteří byli informováni o skutečných příjmech a nezaměstnanosti, změnilo svou volbu studijního oboru oproti studentům, kteří informováni nebyli. Změny ve volbě studijního oboru jsou částečně dány rozdílem mezi přibližnou představou o nezaměstnanosti a skutečnou nezaměstnaností. Naproti tomu se zdá, že rozdíly v příjmech nehrají roli. Také

odhadujeme a nacházíme vliv na studenty, kteří informaci nezískali přímo. Nepřímý vliv se projevuje pouze u starších studentů, kteří jsou blíže ukončení střední školy. Zjišťujeme, že okamžité změny v zamýšlené volbě oboru nejsou propojeny s konečnou volbou oboru, což naznačuje, že měření vlivu informace na bezprostřední úmysly nemusí být postačující k pochopení, jak informace ovlivní skutečné životní volby. Zjišťujeme, že informování má značný přímý i nepřímý vliv na volbu univerzitního oboru.

Introduction

In this dissertation, I study the extent and sources of discrimination, minorities' strategic response to discrimination, and the effects of information provision on individuals' education choices. This dissertation is related to studies on discrimination and the effects of information on various economic outcomes. While experimental evidence on discrimination is vast, this literature *mostly* measures the extent of discrimination without uncovering the sources of discrimination or studying minorities' responses to discrimination. My coauthors and I contribute to the literature by introducing novel experimental designs to shed light on the real impact of information provision and minorities' strategic responses. The first chapter focuses on the discrimination against foreign investors and examines whether the magnitude of discrimination can be reduced by providing relevant signals aimed at eliminating stereotypes. The second chapter discusses the strategic behavior of minorities to the observed discrimination and their willingness to hide their ethnicity. The third chapter studies the actual effects of the information on student college major choices.

Methodologically, all chapters use field experiments to study the extent of discrimination, or the effects of information on the outcomes discussed above. In Harrison and List's taxonomy (2004), the first chapter is classified as a "natural field experiment" as subjects do not know that they are participants in an experiment and they make decisions in their naturally occurring environments. The second and third chapters employ lab-in-the-field experiments, which are classified as "framed field experiments" in Harrison and List (2004). In all three studies, we randomly assign subjects into different experimental groups (treatment, control or spillover) to study the effects of the information and draw causal inferences. Methodologically, we extend the existing literature by introducing a signaling stage in the standard trust game (Berg et al. 1995) to explore minorities' strategic behavior and we study spillover effects by modelling an experiment with indirect information transmission. These novel experimental approaches are exclusively designed to study the economics of discrimination and the process of human capital formation. In the first and third chapters, we study the change in the economic behavior upon arrival of the relevant information. Our experimental settings allow us to identify the sources of discrimination and draw conclusions on the causal effects of information. In the third chapter, we also propose alternative, indirect methods of information provision to facilitate efficient human capital formation (college major decisions). The second paper has an inbuilt, experimental manipulation element to study the strategic response of the minority group members and its consecutive impact on efficiency. This study also provides important insights on the extent of the measured discrimination in the situations in which minorities are (can be) allowed to adjust their behavior. Our experimental design allows us to draw conclusions on the intrinsic costs associated with denying self-defined ethnic group affiliation.

The first chapter provides evidence on the sources of discrimination. In general, discrimination against a particular group of people may be driven by animosity (hatred) or stereotypes. The former is known as preference- or taste-based discrimination (Becker 1971), whereas the latter is referred to as statistical discrimination (Phelps 1972, Arrow 1973). Preference-based discrimination is usually deep-seated and takes a long time to be eliminated, whereas statistical discrimination can be eliminated easily. Discrimination is measured by the difference in price offers to foreign and Georgian investors. The results of this study suggest that there is discrimination against foreign investors in Georgia. I consider four stereotypes that could potentially form statistical discrimination based on focus group discussions and meetings with experts in the field. To measure the sources of discrimination, I extend the experimental design implemented in Gneezy, List and Price (2012). Castillo, Petrie, Torero and Vesterlund (2013) employ a similar experimental design to identify the source of discrimination. I consider four signals that are designed to address all stereotypes that are beliefs against foreign investors. I find that over 75% of the discrimination is explained by stereotypes about foreign investors. The magnitude of discrimination shrinks significantly once foreign investors signal their willingness to search and pay the lease price in advance. This suggests that discrimination is largely driven by stereotypes about search costs and the payment reliability of foreign investors - leaving no or very little preference-based discrimination. The other two stereotypes about wealthy investors and the extractive technologies used by them turn out to be unimportant in explaining the observed discrimination. One of the factors impeding foreigners to invest in developing countries can be discrimination. In this study, I show that in some occasions, discrimination can be easily eliminated by providing relevant signals. Elimination of discrimination could increase the likelihood of the transaction occurrence and the production of goods and services. Knowing the source of discrimination can be helpful for policy makers in designing anti-discriminatory legislation. For instance, local authorities could publish average price quotes that would be available to every interested party including foreign investors. According to this study search costs account for over 50% of the observed discrimination.

The second chapter, co-authored with Philipp Lergetporer, contributes to the literature by studying minorities' strategic response to observed discrimination. We use a lab-in-the-field experiment in Tbilisi, Georgia to examine whether minorities hide their ethnicity to reduce discrimination. We show that the minorities hide their names to circumvent anticipated discrimination - this behavior substantially improves the well-being (profits earned in the modified trust game) of both trustors and trustees. Our study complements the large body of experimental studies on ethnic discrimination. This literature mostly measures discrimination without accounting for potential victims' strategic responses to reduce discrimination. Our results show that adjusting for minority-group members' optimizing behavior halves the magnitude of discrimination. Therefore, we argue that minorities' strategic behavior has important consequences for the magnitude and interpretation of detected discrimination (Heckman 1998, Bertrand and Duflo 2017). Next, we contribute to the literature (Jia and Persson, 2020) studying the trade-off between discrimination costs and identity costs associated with name-changing decisions. We show that name-changing behavior is strategic, and that minorities face a trade-off between pecuniary and non-pecuniary factors when taking signaling (name-changing) decisions. Our findings suggest that strategic misrepresentation of ethnicity increases trustors' expected back transfers and eliminates their discriminatory behavior.

The third chapter, co-authored with Gega Todua, contributes to the literature on educational choices. College major choices, like other decisions, made under imperfect information based on the subjective beliefs may be inefficient for individuals and society as a whole (Jensen 2010). We find that students in Georgia, similar to other developed and developing countries, are not well informed about labor market conditions (wages and unemployment rates for a given college major/specialization). Using a lab-in-the-field experiment, we examine the effects of direct and indirect information provision on student college major choices. We also shed light on the impact of the information in terms of immediate and final effects on the student college major choices. We find that 10 percent more students who received information on actual earnings and unemployment changed their actual college major choices than others. The changes in their majors are partly driven by differences in the perceived and actual unemployment rates, whereas the earning differences do not appear to play a role. We also estimate spillover effects on students who do not receive information directly, and show that they matter, but only for older students who are closer to high school graduation. Importantly, we find that the immediate changes in the intended choices are not linked to the final major choices, suggesting that measuring the effects of information on immediately expressed intentions may not be sufficient to understand how information affects actual real-life decisions. We find that both direct and indirect information provision have sizable effects on student college major choices.

My research is motivated by ideas and mechanisms that assist in identifying ways to eliminate or reduce the magnitude of discrimination, increase trust levels and hence economic transactions, and identify ways to help individuals make better-informed decisions. This dissertation sheds light on these issues and provides insights on how to lower the discrimination, increase economic transactions and make informed choices. These avenues may be interesting for policy makers in their efforts to design policies and for researchers in academia to extend the evidence on these issues.

Chapter 1

1 Sources of Statistical Discrimination: Experimental Evidence from Georgia[1]

Nikoloz Kudashvili

1.1 Introduction

A growing body of research suggests that discrimination against nonwhites, ethnic minorities, women and people with disabilities is common in the marketplace. Field experiments have been widely used to test for discrimination in the labor and housing markets.[2] In addition, many empirical studies effectively document the extent of discrimination, though less progress has been made in identifying the sources of discrimination (List 2004).

Economic literature distinguishes between two types of discrimination - preference-based and statistical-based. Preference, or taste-based discrimination is due to animosity against a specific group of people. This type of discrimination can be against race, color, ethnicity, religion or a minority group. In this case individuals are willing to give up monetary gains in the form of profits or wages in accordance with their prejudice (see Becker 1971), and thus prejudice enters an individuals' utility function. Preference-based discrimination can be reflected in lower wage

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[2]See Bertrand and Duflo (2016) for excellent overview of field experiments.

rates or higher housing prices offered to a certain group of people who are discriminated against. Statistical discrimination, on the other hand exists when individuals do not have complete information about a specific person (see Phelps 1972, Arrow 1973). Lack of information leads to a reliance on stereotypes and beliefs about average, standardized group behavior. Various stereotypes can constitute statistical discrimination that in turn affects an individual's decision. Statistical discrimination is likely to disappear once the discriminator is given relevant information about the minority group member.

This paper addresses not only the extent of discrimination, but also provides insights into its sources. We conducted a series of field experiments in Georgia. Our experimental design allows the identification of whether Georgian landlords discriminate against foreign investors when leasing land. Once discrimination is found, we disentangle different sources of statistical discrimination. This study addresses the following central questions:

(a) Is discrimination present in the Georgian land market?

(b) Is this discrimination based on beliefs or preferences?

(c) If there is statistical discrimination, which stereotypes drive it?

In general, field experiments model various interactions between subjects and objects and observe the subject's behavior over the object type (see List and Rasul 2011 for an excellent overview of empirical studies in labor economics). We take a similar route to measure the extent of discrimination. In this study the experimental subjects are Georgian landowners who are willing to lease their land. We observe how Georgian landowners' behavior changes given the nationality of the investor (object).

In our experiment, randomly selected Georgian farmers who were willing to lease their plot of land are contacted by cell-phone by a representative of a Georgian or a foreign investor. Both investors are identical; the only difference is in nationality. Landowners were asked to state their price quotes if they were to lease a plot land to either a Georgian or Indian investor. The first experimental treatment manipulates the nationality of the investor and the extent of discrimination is calculated as the relative difference between the proposed price given by the landowner to foreigners and Georgians.

With regard to question (a) we find that foreign investors are quoted roughly a 50% higher lease price compared to Georgian investors when no signal is provided. Furthermore, we consider four stereotypes that could potentially form the statistical discrimination, based on focus group discussions.

One possibility is that Georgian landowners discriminate against foreign farmers because they think that foreigners have higher land search costs, and therefore have fewer choices when searching for agricultural land.

A second possibility is that statistical discrimination is due to stereotypes about a foreign farmer's wealth. In other words, if Georgian landowners perceive that foreigners have, on average, more income, then foreigners may be willing to pay a higher price.

A third possibility is that Georgian landowners think that foreign investors will extract more resources from their land and hence the land will be less productive in subsequent periods. Specifically, if landowners believe that foreigners deplete land more than Georgians due to the use of different technology, then foreigners may be charged a higher rent price than Georgian farmers for the same plot.

The final possibility investigated is that Georgian landowners perceive that it might be harder to enforce a contract when dealing with a foreign farmer. Landowners may think that foreigners delay payment or can leave the country without actually paying the rent. Hence, foreign investors are treated differently.

We build on the experimental approach of Gneezy, List and Price (2012) to address each of these four stereotypes. Experimental results in Gneezy et al. (2012) suggest that individuals with disabilities are discriminated against in the automobile repair market. The authors conduct an additional experiment to identify the source of discrimination where both disabled and non-disabled drivers provide a signal[3] about their knowledge of car fixing prices. Provision of the signal lowers the price quotes for both non-disabled and disabled agents, and the difference in price offers becomes statistically insignificant. Thus, disabled drivers face higher price quotes as car repair

[3] Both drivers clearly state that they are getting a few price quotes.

shop staff believe that non-disabled individuals have lower search costs. Therefore, discrimination observed in the automobile repair market is consistent with a model of statistical discrimination.

We extend Gneezy et al. (2012) and consider four potential sources of statistical discrimination. In these experimental treatments four different types of signals are provided to landowners to tackle the common stereotypes described above. All experimental treatments are orthogonal - the first experimental manipulation is over the nationality of the investor, while the second varies by the type of the signal provided to the landowners by both Georgian and Indian investors. In order to address the first stereotype, the representative of both Georgian and Indian investors states that they are considering leasing a piece of land in a neighborhood when phoning the landowner. This signal is designed at eliminating the stereotype about search cost differences between local and foreign investors. In the next treatments, the representative of both investors either notes that the company is a family business or plans to produce bio products with no use of fertilizers. These two signals are aimed at eliminating stereotypes about differences in investor wealth and extractive usage of the technology, respectively. Finally, differences in landowner beliefs about payment reliability between two investors are addressed by proposing an advance payment at the beginning of the contract period.

After measures designed to reduce the price difference, we find that the total discrimination markup shrinks significantly. Only differences in search costs and payment reliability turn out to be significant determinants of statistical discrimination. Thus, statistical discrimination is amplified by stereotypes regarding information availability to foreign investors and beliefs about the reliability of foreign investors.

Once the representative manager of interested investors provides signals to Georgian landowners about the land prices available to foreign investors, statistical discrimination vanishes. A similar effect is found once the representative manager provides information to Georgian landowners about the reliability of foreign investors. The drop in the discrimination markup is largest when providing a joint signal on search effort and reliability. In this case discrimination shrinks from 48% to 12%, implying that the largest portion of discrimination is due to statistical grounds, rather than preference driven. Thus, our experimental results suggest that at least 75% of discrimination occurs due to the channels of statistical discrimination identified, while the remaining part is likely to be preference-based discrimination. Nevertheless, there might be some other channels of statistical discrimination which are not captured in this study.

1.2 Related Literature

This paper is related to the studies that uncover sources of discrimination. Fershtman and Gneezy (2001) use a laboratory experimental setting to show that individuals with eastern origins are systematically discriminated against in Israeli society by both Eastern and Ashkenazic Jews. They employ trust and dictator games to disentangle the source of discrimination. Interestingly, they find no evidence for preference-based discrimination against Eastern Jews. Experimental results in Fershtman and Gneezy (2001) suggest that stereotypes about Eastern Jews drive discrimination; therefore, the mistrust observed against Eastern Jews is consistent with a model of statistical discrimination.

Another strand of literature investigates sources of discrimination in the market- place. List (2004) finds that minorities (women, nonwhite and older players) receive inferior initial and final offer prices compared to the majority group in a sports-card market. Similar to Fershtman and Gneezy (2001), a dictator game is played to identify whether the discrimination detected is due to animus. List (2004) finds no evidence for taste-based discrimination; instead the discrimination observed is driven by stereotypes. A complementary experimental treatment is implemented to provide further insights into the nature of discrimination. In particular, dealers treat minority and majority buyers equally when reservation values are drawn randomly. This implies that dealers "knowingly perform statistical discrimination" against minority buyers.[4]

Two field studies effectively identify the source of discrimination in well-functioning markets. We use a similar experimental design to provide signals to experimental subjects and these studies are therefore the closest to our work. The first paper is by Castillo, Petrie, Torero and Vesterlund (2013) who conducted a field experiment in the Lima taxi market. Surprisingly, Castillo et al. (2013) find that men face higher prices and rejection rates from taxi drivers. Trained male and female taxi passengers signal their valuation to identify the source of discrimination. The

[4] This occurs when dealers do not know that reservation values are drawn randomly.

trained taxi passengers reject the taxi hailed first, and that is seen by the taxi queued behind it. The rejection of the first taxi hailed is used to signal the passenger's low valuation. Castillo et al. (2013) show that men and women receive similar price offers - implying that differential gender treatment vanishes once the signal on the fare valuation is sent to taxi drivers. According to the study, men face higher prices and rejection rates, as taxi drivers perceive that men value taxi rides at a higher price than women. Experimental results suggest that discrimination is entirely driven by a stereotype about a passenger's willingness to pay. Therefore, the discrimination observed against men is consistent with statistical discrimination, leaving no room for preference-based discrimination.

The second study that distinguishes the source of discrimination is by Gneezy, List and Price (2012) who analyze whether there is discrimination against individuals with disabilities in the car repair market. In this field experiment, control and treatment groups are formed to capture the magnitude of discrimination. The treatment group consists of six disabled agents who drive a specialized vehicle, while the control group is formed by six non-disabled agents. Each type drives to six car repair shops and asks for a price quote.

The cars have only visible problems[5] and the same car is used by all the non-disabled agents. Similarly, the same specialized car is driven by disabled individuals. Therefore, differences in price quotes can only be attributable to the disability status of the agent and differential price treatment could only be due to the disability status of the individual.

In the first treatment, Gneezy et al. (2012) find that drivers with disabilities are discriminated against. Specifically, disabled agents receive about a 30% higher price quote compared to their non-disabled counterparts. Gneezy et al. (2012) further investigate whether discriminatory behavior is due to sellers' beliefs about drivers' knowledge of car repair prices. This is equivalent to testing whether car repair shop workers think that individuals with disabilities have a higher search cost and hence less precise information about car repair prices. In the second treatment, both disabled and non-disabled drivers clearly state that they have received several price quotes from other car repair shops. In this way both types of drivers disclose that they have

[5] The problem is not related to the type of vehicle (specialized vs non-specialized); therefore, repair prices should be the same for both types of cars.

information about the current car repair prices. This treatment is aimed at eliminating the statistical discrimination that arises due to search cost differences between two groups of drivers. Once the sellers receive a signal that disabled drivers have information about car repair prices, the magnitude of the discrimination falls. Experimental results suggest that prices offered to both disabled and non-disabled drivers are not statistically different after the signal provision. Therefore, differences in price quotes can be explained by car repair shop workers' stereotypes about search costs. Our experimental design is similar to Gneezy et al. (2012). However, we consider more stereotypes which can constitute the statistical discrimination.

Another strand of literature documents the extent of discrimination in the housing and labor markets. For instance, Bertrand and Mullainathan (2004) find that individuals with African-American sounding names are discriminated against in the U.S. labor market. The field experiment was conducted in Boston and Chicago, and fictitious resumes were randomly assigned to either African-American or White-sounding names. Resumes were then sent in response to employment advertisements. Discrimination is measured as the difference between callback rates for African-American and White-sounding names. Bertrand and Mullainathan (2004) find that job applicants with African-American sounding names receive about 50% fewer callbacks for interviews. Furthermore, differences in callback rates for African-American and White-sounding names is higher once high-quality resumes are sent in response to advertisements.

Bartoš, Bauer, Chytilová and Matějka (2016) study how limited attention and lack of information about minorities lead to their inferior treatment. They find that ethnic minorities are treated differently in both the labor and rental housing markets. Their experimental results suggest that employers invest less effort in inspecting resumes of the minority-sounding individuals, whereas landlords devote more time to reading them. Attention discrimination is captured with the use of novel experimental tools to observe information acquisition in the labor and rental housing markets. Interestingly, the observed inferior treatment of ethnic minorities in the labor market can explain their low earnings and educational attainment.

1.3 Field Experiment

1.3.1 Short Overview of the Georgian Land Market

Agricultural land ownership is very fragmented in Georgia. Specifically, the average size of a plot of land is about 0.96 hectares and only 5% of individuals own land that is larger than 2 hectares (Kan, Kimhi and Lerman 2004).

A significant share of Georgian agricultural land is not utilized. Prasad (2012) reports that half of the arable land in Georgia is not farmed. Foreign farmers, particularly from Asia and South Africa have, however, expressed increasing interest in investing in the Georgian agricultural sector (Prasad 2012).

This study is important in the context of the development of the Georgian agricultural sector, as over 50% of all workers in Georgia are still employed in this sector (Geostat 2012). Given the foreign investors' interest in this sector, it is important to know factors that could impede foreigners from leasing Georgian land.

1.3.2 Data

The experiment took place in the summer of 2015. Leasing land in summer is typical for lessors and most of the lease contracts occur in summer. 384 randomly chosen farmers were phoned by a representative of both foreign and Georgian investors. The representative for both types of investors was the same person, to avoid a contamination effect. The data contain details about landowners who had posted their lease offers. We stratified by region[6] to randomly select farmers who were later contacted by the representative manager. The randomization was performed within each stratum: Kakheti and Kartli. We limit our analysis to these regions as the largest plot of agricultural land is located there (Geostat 2014). We also collected data for other characteristics of the land, such as the irrigation system, location and productivity. Foreign

[6] Land quality and prices could potentially be different across regions.

investors were specifically identified as Indian in our experiment.[7] Indian farmers have recently become widespread participants in the Georgian agriculture sector (Prasad 2012).

We only consider plots of lands up to two hectares. Agricultural land ownership is fragmented in Georgia and therefore there would be very few plots of lands larger than two hectares. In particular, according to Kan, Kimhi and Lerman (2006) 95.4% of farms are smaller than 2 hectares.

The food and Agriculture Organization of the United Nations (FAO) distinguishes two types of crops: permanent and temporary. Temporary crops have a growing cycle of less than one year. These types of crops are usually planted or sown every year for further production. In contrast, permanent crops are planted once every few decades. Thus, most vegetables and grains are in the temporary crop category, while grapes and some fruit production belong to the permanent crop category. Production of permanent crops requires long-term investments; hence it is not very common to lease a plot of land for a year to produce permanent crops. Therefore, in this experiment the representative manager asked for the annual price per hectare suitable for temporary crop production.

All plots of land are located in different parts of the region. We constructed a distance variable to control for the location of the land (note that the exact distance from the land to the town was not available). Distance to the city takes a value of either zero or one. Zero is used for values up to 15 kilometers from the city and one for those in excess of 15 kilometers.

Access to irrigation is similarly valued. Limited or no access to irrigation corresponds to zero, while high access to irrigation corresponds to one.

The third covariate controls the type of land. If a plot of land was fallowed previous year the fallow is valued one, and zero otherwise.

The lease price offer that is stated by the landowners during the phone conversation is the outcome variable.[8] Lease prices are quoted per hectare on an annual basis.

[7] Lease price offers could potentially be different for foreigners who are culturally closer to locals.
[8] See telephone conversation scripts in Appendix A2.

Randomization checks are performed for each covariate. We find that mean covariates are not statistically different for Indian and Georgian investors in both experimental manipulations (see Table 1.2).

1.3.3 Experimental Design

The first experimental manipulation relates to the nationality of the investor, while the second varies by the type of the signal provided to landowners by both Georgian and Indian investors. All experimental treatments are orthogonal, as representative managers of both Georgian and Indian investors provide exactly the same signal to both treatment and control groups in each experiment; the only difference is the reported nationality of the investor.

1.3.3.1 Manipulating the Identity of Investors

The experiment was based on phoning the landowners expressing an interest in leasing their land. To avoid contamination, each landowner was called only once. In addition to the lease price, the representative manager also asked questions regarding the location, irrigation and productivity of the land, to confirm the figures given in the lease ad. Any additional questions asked by the landowner were responded to as follows: "we will answer all your questions when we visit the land". Thus, the representative manager provides exactly the same set of information to each landowner.

A similar procedure is repeated by a representative manager of a foreign firm when calling another landowner - the only difference is the stated nationality of the investor. The representative of a foreign manager conducting all calls for the experiment is the same person and speaks in Georgian.

The representative manager of both Georgian and foreign companies clearly states that he will be the person dealing with the landowner during the contract. This excludes any potential differences in lease price quotes due to language barriers. The script of the conversation can be found in Appendix A2.

1.3.3.2 Manipulating the Provision of Information

Five different treatment groups are formed to disentangle different sources of statistical discrimination. Georgian farmers reporting a lease price offer to Georgian investors represent a control group. Note that the representative manager of Georgian and Indian firms discloses exactly the same piece of information to the landowners in all five treatments.

We conducted preliminary research to identify the reasons foreigners were charged higher prices. Firstly, we surveyed farmers who were not subjects in the experiment. Secondly, we discussed stereotypes with experts in the field: farmers' association representatives, ISET-PI policy institute experts, and other relevant experts. Based on our discussions with these parties, we identified four stereotypes that could explain differential lease price offers for locals as versus for foreigners. These stereotypes[9] are defined below, in respective treatments.

1. *Search Cost Treatment*

During the phone conversation, the representative manager of a foreign firm discloses information to the landowner that the foreign investor has several other leasing offers in the surroundings and is considering leasing some of them.

Hence, if the high rental price was due to perceived higher search costs for foreigners, then the extent of discrimination should shrink. This is the result of the elimination of the high search cost stereotype.

2. *Investor Wealth Treatment*

The representative manager of a foreign firm also discloses information about the size of the foreign investor to the landowner during the phone conversation. In particular, to eliminate the stereotype that foreigners are willing to pay more, the manager states that the foreign firm carries out small scale farming in Georgia.

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[9] It would be interesting to investigate whether the stereotypes above are accurate. Note that the accuracy of these beliefs was not studied in this paper.

Hence, if the high rental price was due to perceived beliefs about the foreign investors' wealth, then the extent of discrimination should shrink. This is the result of the elimination of the "wealthy foreigners" stereotype.

3. *Technology Used Treatment*

According to the third hypothesis, foreigners are charged a high price because landowners believe that foreigners depreciate agricultural land more. In this treatment the representative managers clearly state the production technology which is going to be used during the leasing period. In other words, randomly chosen farmers are told that both Georgian and foreign farmers have identical plans of land usage.

Hence, if the high rental price was due to perceived overuse of land, then the extent of discrimination should shrink. This is the result of the elimination of the "foreigners depreciate our lands more" stereotype.

4. *Payment Reliability Treatment*

According to the fourth hypothesis, foreigners are charged a high price because landowners believe that they may not pay the rent in a timely fashion or it is difficult to enforce the leasing contract. In this treatment the representative managers offer to pay the rent in advance, at the beginning of the leasing period.

Hence, if the high rental price for foreigners was due to perceived differences in reliability or difficulty in enforcing the contract, then the extent of discrimination should shrink. This is the result of the elimination of the "foreigners may not pay or pay late" stereotype.

5. *Search Cost and Payment Reliability Treatment: Joint Signal Provision*

We form a multichannel treatment that is composed of signals one and four. In this treatment representative managers signal their readiness to pay the rent in advance and state that they have considered leasing other land in the surroundings.

We estimate the following equation

$$P_i = \beta_0 + \beta_1 \boldsymbol{I} + \gamma X_i + M_i + \sum_{\boldsymbol{j=1}}^{\boldsymbol{5}} \left(\alpha_{j0} T_j + \alpha_{j1} T_j \boldsymbol{I}\right) + \boldsymbol{\epsilon_i}$$

where P_i is the lease price offered to investors in region i. X_i is a vector of control variables in each region. M_i is a stratum dummy and takes a value of one if the land is located in Kakheti, and zero otherwise. Similarly, investor nationality $\boldsymbol{I}$ takes a value of one if the investor is Indian and zero otherwise.

T_j denotes the provision of the signal j and equals one when signal j is provided and zero otherwise. For instance, $T_1 = 1$ corresponds to the search effort signal provision, $T_2 = 1$ corresponds to the investor wealth signal provision, $T_3 = 1$ corresponds to the technology usage signal provision, $T_4 = 1$ corresponds to the payment reliability signal provision, and finally, $T_5 = 1$ corresponds to the joint signal on search effort and payment reliability provision.

The treatment effect, i.e. the relative difference between the landowner's proposed price to Indians and Georgians, is captured by the coefficient β_1 when no signal is provided. We test the hypothesis whether there is a difference in lease price offers between Georgian and Indian investors. According to the H_0 hypothesis there is no discriminationin terms of lease price quotes, that is $\beta_1 = 0$. In contrast, according to the alternative hypothesis H_a there is discrimination against Indian investors in terms of lease price offers, that is $\beta_1 = 0$.

α_{j0} shows how the provision of signal j affects the price reported to Georgian investors. For instance, α_{10} shows the change in the conditional mean lease price offer to Georgian investors when a search effort signal is provided. In contrast, α_{j1} shows the change in discrimination markup after providing signal j. For instance, α_{11} shows the change in the magnitude of discrimination once a search effort signal is provided. Coefficient estimates are reported in Table 1.1.

1.4 Results

First, we find that foreign investors receive a higher price offer compared to Georgian investors in the baseline treatment when no signal is provided. The result is shown in Table 1.1. The estimated coefficient is about 115, suggesting that an Indian investor receives an approximately 115 GEL[10] greater lease price offer compared to the Georgian investor per hectare per year. The estimated treatment effect is significant at the $p < 0.001$ level in all model specifications. Coefficient estimates in Table 1.1 imply that Indian investors receive a 48.35% higher price offer compared to Georgian investors.

The left panel of Figure 1.1 shows mean price offers for Georgian and Indian investors when no signal is provided. The mean lease price offer for Georgians is about 240 GEL, while the mean price offer is about 355 GEL for Indian investors.

Second, we find that the price discrimination against foreign investors can be partially explained by search cost differences. Coefficient estimates in Table 1.1 suggest that a significant part of the total discrimination is driven by search cost differences. A coefficient estimate of α_{10} is insignificant at the $p < 0.05$ level, implying that the mean lease offer to Georgian investors does not change. Georgian investors receive similar lease price offers as in the case of no signal provision. This indicates that landowners have beliefs about search cost differences between Georgian and Indian investors. In contrast, the mean lease price offer to Indian investors drops by about 62 GEL. The estimated treatment effect of the search effort signal α_{11} is significant at the $p < 0.001$ level.

A decrease in the magnitude of discrimination is illustrated in Figure 1.1, which depicts mean lease prices. Indian investors receive about a 52 GEL higher lease price offer compared to Georgian investors once in this treatment, as opposed to the baseline of 115, indicating that landowners offer higher lease prices to foreigners as lessors perceive that foreigners have high search costs. This result is consistent with the model of statistical discrimination (Phelps 1972) and the findings of Gneezy et al. (2012). Coefficient estimates in Table 1.1 imply that Indian investors receive a 22.58% higher price compared to Georgian investors. Therefore, over half of the discrimination markup is due to perceived higher search costs.

Third, we do not find any significant interaction effect for the treatment in which we state that the investor carries out small scale farming, suggesting that stereotypes about

[10] GEL stands for the Georgian Lari, the average exchange rate for 2015-2016 was roughly $1=2.2 GEL.

foreign investors' wealth cannot explain their observed differential treatment.

Coefficient estimates in Table 1.1 suggest that discrimination is not driven by the stereotype about foreigners' wealthiness; that is, "foreigners are wealthy and therefore we should charge a higher lease price." Coefficient estimates of α_{20} and α_{21} are insignificant at the $p < 0.05$ level, suggesting that lease price offers do not change for either Georgian or Indian investors. Under the provision of the wealth signal, Indian investors receive a 45.32% higher price offer compared to Georgian investors. This implies that foreigners receive similar lease price offers as in the baseline when providing a signal about their wealth. Figure 1.1 also confirms that the magnitude of discrimination is similar to that in the baseline treatment when no signal is provided.

Fourth, we do not find any significant interaction effect for the treatment in which we state that the investor only wants to do organic farming, suggesting that stereotypes about foreign investors relating to their intended production technology cannot explain their observed differential treatment.

Coefficient estimates of α_{30} and α_{31} in Table 1.1 suggest that discrimination is not driven by the stereotype about higher land depletion by foreigners; that is, "foreigners use more extractive technologies and deplete our land." Both coefficients are insignificant at the $p < 0.05$ level. Under the provision of the technology signal, Indian investors receive a 47.13% higher price offer compared to Georgian investors. Hence, the magnitude of discrimination is similar to the one in the baseline treatment when no signal is provided (see Figure 1.1). Therefore, differences in the technology usage cannot account for differential price treatment of foreign investors.

Fifth, we find that the price discrimination against foreign investors can be partially explained by the stereotype about foreigners' reliability. Coefficient estimates in Table 1.1 suggest that the total discrimination is partly driven by the stereotype of foreigners being less reliable. The coefficient estimate α_{40} is insignificant at the $p < 0.05$ level, suggesting that Georgian investors receive similar price offers as in the baseline treatment. In contrast, the mean lease price reported to foreign investors falls by about 64 GEL. In this case, foreigners receive only a 51 GEL higher lease price offer as compared to 115 GEL in the baseline treatment.

The results are consonant with Georgian landowners' beliefs about foreign investors being less reliable. Discrimination shrinks significantly once the manager of the Indian investor expresses his willingness to pay the lease price in advance (see Figure 1.1). Under the provision of the reliability signal, Indian investors receive a 22.94% higher price offer compared to Georgian investors. The estimated treatment effect of the reliability channel α_{41} is significant at the $p < 0.001$ level.

Finally, we examine the effects of the joint signal and find that most of the discrimination is explained by search cost and reliability channels. Sixth, we find that more than 75% of the price discrimination against foreign investors can be explained by the interaction of search cost and investor reliability stereotypes. Once the representative manager of the Indian investor states that his client has obtained a few price quotes and wants to pay the lease price in advance, the disparate treatment effect shrinks significantly. The coefficient estimate of α_{51} is significant at the $p < 0.05$ level, suggesting that Georgian investors receive about an 18.5 GEL lower lease price offer compared to the baseline treatment. Indian investors receive about an 88.9 GEL lower price offer, implying an approximately 75% drop in discrimination. The estimated treatment effect of the search cost and reliability channels α_{51} is significant at the $p < 0.001$ level. Under the provision of the joint signal on search effort and reliability, Indian investors receive only a 12.10% higher price offer compared to Georgian investors. Thus, discrimination under joint signal provision is about four times smaller compared to under no signal provision.

Figure 1.1 summarizes the change in mean lease price offers to Georgian and Indian investors. Our findings suggest that at least 75% of apparent discrimination is based on statistical grounds. The remainder could be driven either by preference-based discrimination or other stereotypes that are not captured in this study. Our experimental results suggest that discrimination shrinks significantly once landowners receive relevant information about foreigners' search efforts and willingness to pay in advance.

1.5 Conclusion

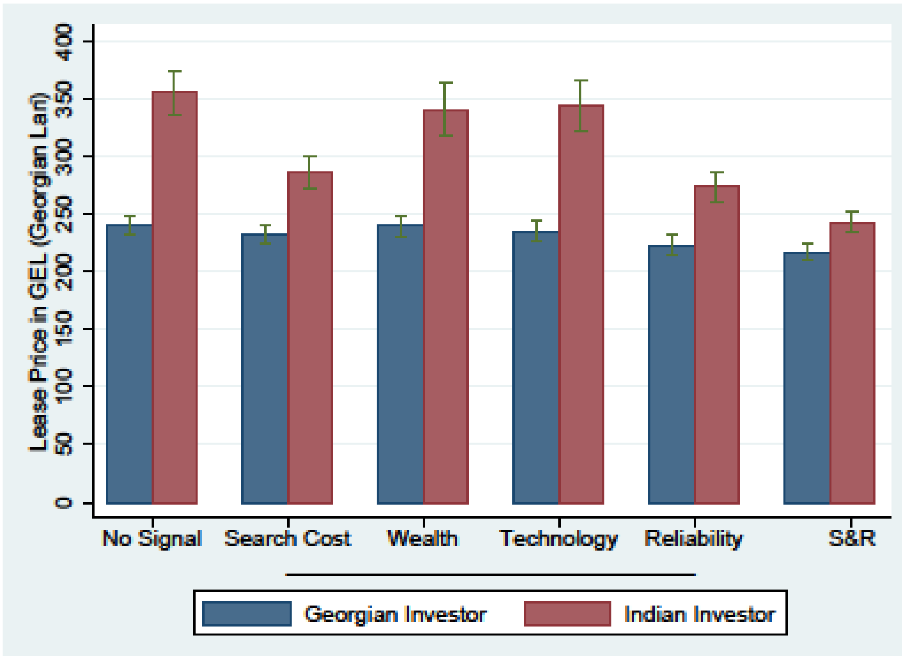
This paper contributes to the existing studies on discrimination in the land market by experimentally separating the extent and source of discrimination. Our novel design allows us to uncover different stereotypes that form the statistical discrimination. Individuals disclose specific signals which are aimed to eliminate agents' stereotypes about foreign investors. We find that foreign investors systematically receive approximately 50% higher lease price offers compared to Georgian investors. Next, we show that discrimination is largely driven by stereotypes about search costs and the payment reliability of foreign investors - leaving no or very little preference-based discrimination. Interestingly, we find no evidence that stereotypes about foreigners' wealth and technology usage contributes to inferior treatment of foreign investors. Overall, we suggest that at least 75% of discrimination is on statistical grounds, given a joint signal on foreigners' search effort and payment reliability. Our results may be useful for policy makers when crafting their anti-discriminatory legislation. In particular, knowing the source of discrimination helps decision makers to form desirable policies. The government could make lease price offers publicly available, thus attenuating the first source of statistical discrimination due to perceived search cost differences. With respect to the reliability of foreign investors, government could help local lessees by providing information about law enforcement or making an advance lease payment obligatory.

This paper suggests a further policy implication for developing countries aiming at higher investment inflows. Foreign investment inflows might be impeded by high price offers, leaving the factors of production underutilized. Thus, curbing discrimination could potentially lead to a higher level of investment inflow, which in turn creates employment opportunities for locals.

Policies aiming to curb preference-based discrimination are usually costly. For instance, better educational systems and institutional development could gradually eliminate this type of discrimination. In contrast, curbing statistical discrimination requires an emphasis on better information movement within societies. Our findings show how the arrival of different informational signals can change price offers to foreigners, thus pointing to potential policies that aim to affect the cause of the discrimination rather than the outcome.

Figures and Tables

Figure 1.1: Average lease price for Indian and Georgian investors



Notes: the figure shows the lease price offers for the Georgian and Indian investors. Blue and red bars represent the lease price offers for Georgian and Indian investors respectively. The figure also shows the changes in the price quotes in *Search Cost, Investor Wealth, Technology, Reliability and Search Cost & Reliability* treatments. Error bars show 95% confidence intervals.

Table 1.1: Effects of signals on the lease price offers

	(1)	(2)	(3)	(4)	(5)	(6)
Indian (I)	115.1***	114.7***	115.1***	114.5***	114.8***	115.4***
	(9.329)	(8.244)	(10.42)	(10.10)	(8.057)	(7.396)
Search Cost		-6.529				
		(8.252)				
I*Search Cost		-62.33***(11.66)				
Wealth			-2.102			
			(10.43)			
I*Wealth			-8.595			
			(14.76)			
Technology				-2.037		
				(10.13)		
I*Technology				-3.449		
				(14.29)		
Reliability					-15.33	
					(8.057)	
I*Reliability					-63.66***(11.38)	
S&R						-18.52*(7.460)
I*Search Cost *Reliability						-88.86***(10.47)
Constant	207.8***	215.5***	214.5***	217.3***	219.3***	221.8***
	(14.90)	(9.704)	(13.12)	(12.15)	(9.855)	(9.641)
Observations	64	128	128	128	128	128
Adjusted R^2	0.715	0.700	0.645	0.674	0.727	0.776

Notes: OLS in all columns. The dependent variable is the lease price received by the representative manager. Indian investor = 1 if the investor is Indian, 0 otherwise. Standard errors are in parentheses beneath coefficient estimates. Region, Distance, Irrigation, and Fallow are included in all six specifications. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 1.2: Georgian Land Market - Randomization Check

	Investor Nationality			Signal Provided															
Experimental Manipulation	Georgian	Indian	t-test p-value	No Signal		Search Cost		Investor Wealth		Technology Used		Payment Reliability		Search Cost & Reliability		F-test P-value			
	(1)	(2)		(3)		(4)		(5)		(6)		(7)		(8)					
				(G)	(I)	(G)	(I)	(G)	(I)	(G)	(I)	(G)	(I)	(G)	(I)				
Distance	0.32	0.33	0.83	0.31	0.25	0.37	0.37	0.25	0.31	0.31	0.37	0.31	0.31	0.37	0.37	0.92			
	(0.03)	(0.03)		(0.08)	(0.07)	(0.08)	(0.08)	(0.07)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)				
Irrigation	0.79	0.78	0.80	0.81	0.81	0.81	0.87	0.87	0.81	0.75	0.75	0.75	0.75	0.75	0.68	0.65			
	(0.03)	(0.02)		(0.07)	(0.07)	(0.07)	(0.05)	(0.05)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.08)				
Fallow	0.42	0.38	0.53	0.50	0.44	0.43	0.43	0.43	0.31	0.37	0.37	0.43	0.37	0.31	0.37	0.93			
	(0.04)	(0.04)		(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)				
Obs.	192	192		32	32	32	32	32	32	32	32	32	32	32	32	32	32	32	32

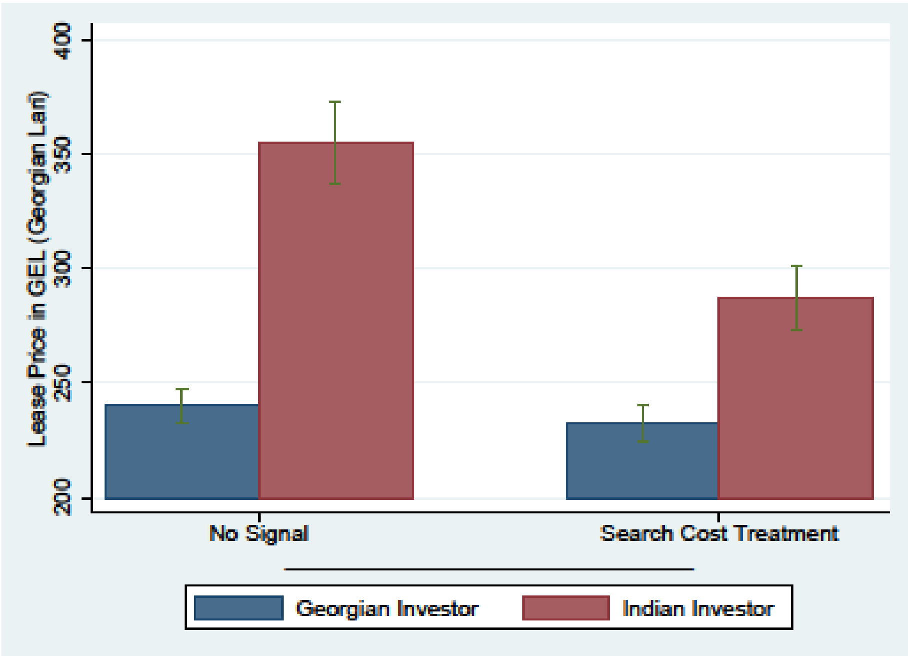
Notes: Standard errors are in parentheses beneath mean estimates. We also report the p-values for a t-test testing the null hypothesis that the means are equal for Georgian and Indian investors and an F-test testing the null hypothesis that the means are equal across all six treatments are also reported

Appendix A

A1. Figures

Figure A1.1: Average lease price for Indian and Georgian investors

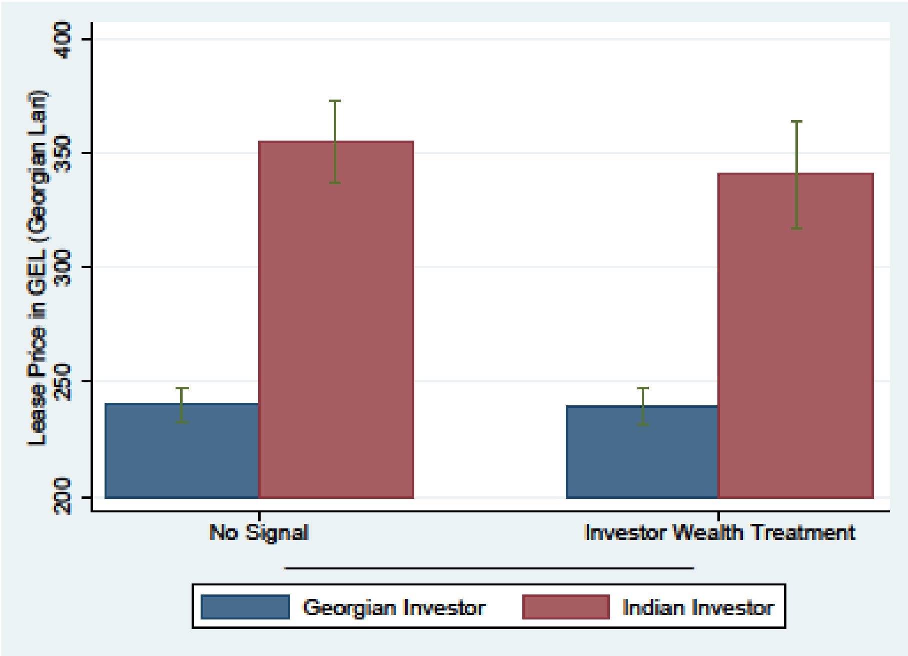
Search Cost Treatment



Notes: the figure shows the lease price offers for the Georgian and Indian investors. Blue and red bars represent the lease price offers for Georgian and Indian investors respectively. The figure illustrates a significant decrease in the magnitude of discrimination after the search effort signal provision. Error bars show 95% confidence intervals.

Figure A1.2: Average lease price for Indian and Georgian investors

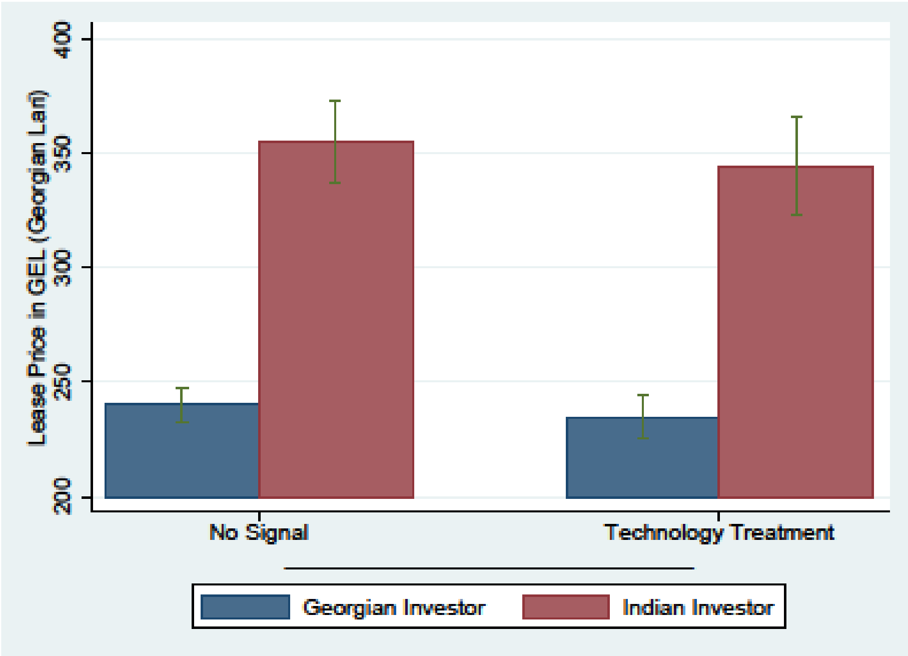
Investor Wealth Treatment



Notes: the figure shows the lease price offers for the Georgian and Indian investors. Blue and red bars represent the lease price offers for Georgian and Indian investors respectively. The figure illustrates an insignificant decrease in the magnitude of discrimination after the investor wealth signal provision. Error bars show 95% confidence intervals.

Figure A1.3: Average lease price for Indian and Georgian investors

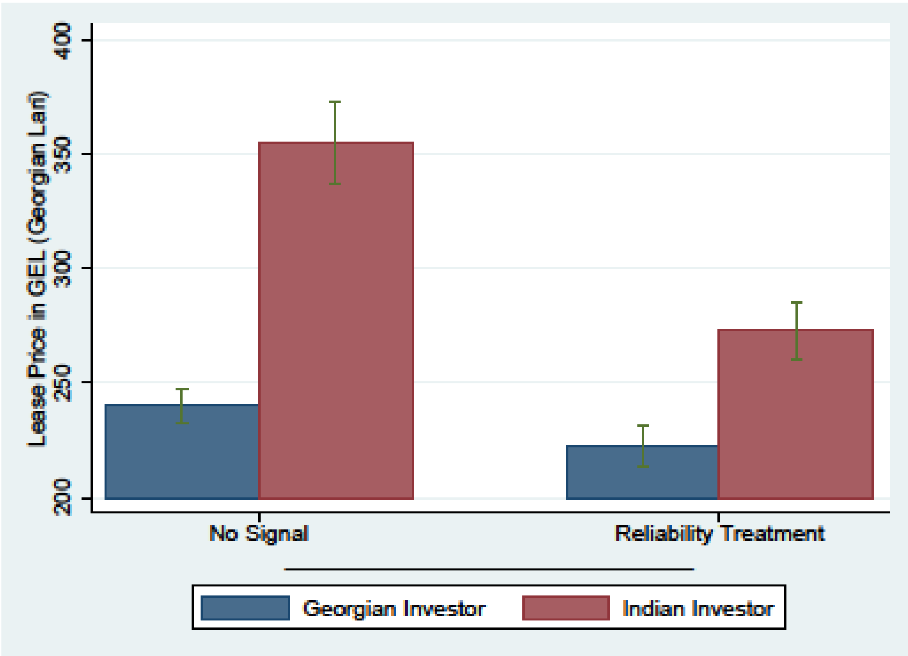
Technology Treatment



Notes: the figure shows the lease price offers for the Georgian and Indian investors. Blue and red bars represent the lease price offers for Georgian and Indian investors respectively. The figure illustrates an insignificant decrease in the magnitude of discrimination after the technology signal provision. Error bars show 95% confidence intervals.

Figure A1.4: Average lease price for Indian and Georgian investors

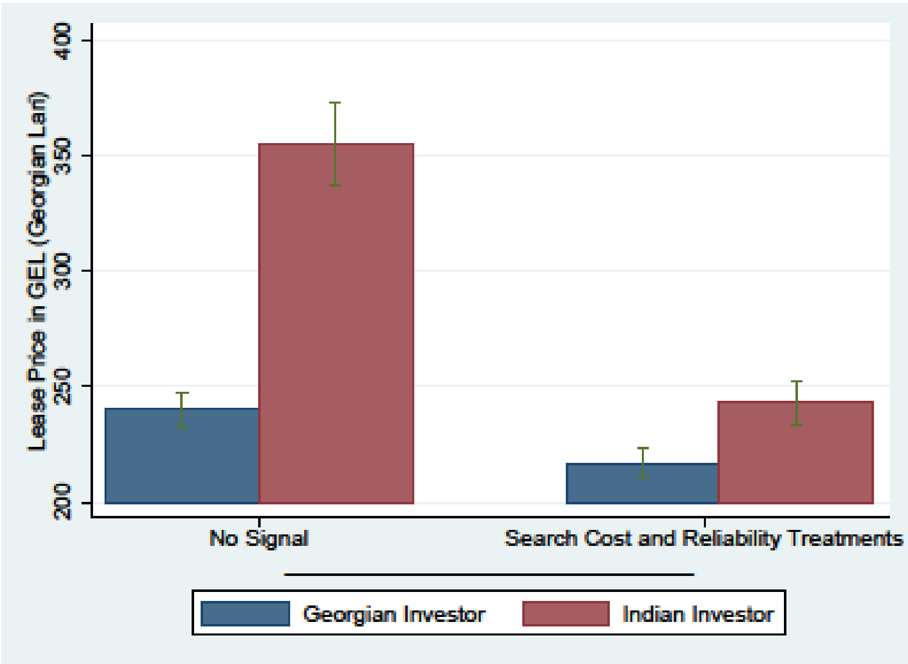
Reliability Treatment



Notes: the figure shows the lease price offers for the Georgian and Indian investors. Blue and red bars represent the lease price offers for Georgian and Indian investors respectively. The figure illustrates a significant decrease in the magnitude of discrimination after the reliability signal provision. Error bars show 95% confidence intervals.

Figure A1.5: Average lease price for Indian and Georgian investors

Search Cost and Reliability Treatments



Notes: the figure shows the lease price offers for the Georgian and Indian investors. Blue and red bars represent the lease price offers for Georgian and Indian investors respectively. The figure illustrates a significant decrease in the magnitude of discrimination after the joint search effort and reliability signal provision. Error bars show 95% confidence intervals.

A2. Telephone Conversation Scripts

Extent of Discrimination

A2.1: Baseline Treatment (No Signal)

Good morning Mr. X,

My name is Giorgi and I am a manager of a Georgian (Indian) investor. The company is based in Tbilisi under the name "Agro."

I am calling you because I am very interested in leasing your land. Could you please confirm us whether you are leasing the land located in Kakheti (Kartli)?

Could you please confirm for us figures on the location of your land, productivity (whether it was fallowed or not) and access to irrigation?

We plan to produce temporary crops such as vegetables or fruit if we lease your land. If we agree on a price I will be communicating with you and representing "Agro" until the end of the lease contract.

What would be the price per hectare for a year? Please state the minimum possible price so that we consider a visit to see your land. I will report the price to my boss and call you back if the price is acceptable for him.

Thank you very much for your time.

Have a nice day!

A2.2: Search Cost Treatment

Good morning Mr. X,

My name is Giorgi and I am a manager of a Georgian (Indian) investor. The company is based in Tbilisi under the name "Agro."

I am calling you because I am very interested in leasing your land. Could you please confirm us whether you are leasing the land located in Kakheti (Kartli)?

Could you please confirm for us figures on the location of your land, productivity (whether it was fallowed or not) and access to irrigation?

We plan to produce temporary crops such as vegetables or fruit if we lease your land. If we agree on a price I will be communicating with you and representing "Agro" until the end of the lease contract.

Our company has a few other price quotes from other Georgian landlords in your neighborhood.

What would be the price per hectare for a year? Please state the minimum possible price so that we consider a visit to see your land. I will report the price to my boss and call you back if the price is acceptable for him.

Thank you very much for your time.

Have a nice day!

A2.3: Investor Wealth Treatment

Good morning Mr. X,

My name is Giorgi and I am a manager of a Georgian (Indian) investor. The company is based in Tbilisi under the name "Agro."

I am calling you because I am very interested in leasing your land. Could you please confirm us whether you are leasing the land located in Kakheti (Kartli)?

Could you please confirm for us figures on the location of your land, productivity (whether it was fallowed or not) and access to irrigation?

We plan to produce temporary crops such as vegetables or fruit if we lease your land. If we agree on a price I will be communicating with you and representing "Agro" until the end of the lease contract.

The company is a family business and only carries out small scale farming in Georgia.

What would be the price per hectare for a year? Please state the minimum possible price so that we consider a visit to see your land. I will report the price to my boss and call you back if the price is acceptable for him.

Thank you very much for your time.

Have a nice day!

A2.4: Technology Treatment

Good morning Mr. X,

My name is Giorgi and I am a manager of a Georgian (Indian) investor. The company is based in Tbilisi under the name “Agro.”

I am calling you because I am very interested in leasing your land. Could you please confirm us whether you are leasing the land located in Kakheti (Kartli)?

Could you please confirm for us figures on the location of your land, productivity (whether it was fallowed or not) and access to irrigation?

We plan to produce temporary crops such as vegetables or fruit if we lease your land. If we agree on a price I will be communicating with you and representing “Agro” until the end of the lease contract.

The company plans not to use pesticides or chemical fertilizers, as we want to produce bio products.

What would be the price per hectare for a year? Please state the minimum possible price so that we consider a visit to see your land. I will report the price to my boss and call you back if the price is acceptable for him.

Thank you very much for your time.

Have a nice day!

A2.5: Investor Reliability Treatment

Good morning Mr. X,

My name is Giorgi and I am a manager of a Georgian (Indian) investor. The company is based in Tbilisi under the name “Agro.”

I am calling you because I am very interested in leasing your land. Could you please confirm us whether you are leasing the land located in Kakheti (Kartli)?

Could you please confirm for us figures on the location of your land, productivity (whether it was fallowed or not) and access to irrigation?

We plan to produce temporary crops such as vegetables or fruit if we lease your land. If we agree on a price I will be communicating with you and representing “Agro” until the end of the lease contract.

The company plans to pay the lease at the beginning of the contract period, just right after we make a deal over the land you lease.

What would be the price per hectare for a year? Please state the minimum possible price so that we consider a visit to see your land. I will report the price to my boss and call you back if the price is acceptable for him.

Thank you very much for your time.

Have a nice day!

A2.6: Investor Reliability Treatment

Good morning Mr. X,

My name is Giorgi and I am a manager of a Georgian (Indian) investor. The company is based in Tbilisi under the name “Agro.”

I am calling you because I am very interested in leasing your land. Could you please confirm us whether you are leasing the land located in Kakheti (Kartli)?

Could you please confirm for us figures on the location of your land, productivity (whether it was fallowed or not) and access to irrigation?

We plan to produce temporary crops such as vegetables or fruit if we lease your land. If we agree on a price I will be communicating with you and representing “Agro” until the end of the lease contract.

The company plans to pay the lease at the beginning of the contract period, just right after we make a deal over the land you lease.

What would be the price per hectare for a year? Please state the minimum possible price so that we consider a visit to see your land. I will report the price to my boss and call you back if the price is acceptable for him.

Thank you very much for your time.

Have a nice day!

A2.7: Search Cost and Investor Reliability Treatments

Good morning Mr. X,

My name is Giorgi and I am a manager of a Georgian (Indian) investor. The company is based in Tbilisi under the name "Agro."

I am calling you because I am very interested in leasing your land. Could you please confirm us whether you are leasing the land located in Kakheti (Kartli)?

Could you please confirm for us figures on the location of your land, productivity (whether it was fallowed or not) and access to irrigation?

We plan to produce temporary crops such as vegetables or fruit if we lease your land. If we agree on a price I will be communicating with you and representing "Agro" until the end of the lease contract.

Our company has a few other price quotes from other Georgian landlords in your neighborhood. Additionally, our company plans to pay the lease at the beginning of the contract period, just right after we make a deal over the land you lease.

What would be the price per hectare for a year? Please state the minimum possible price so that we consider a visit to see your land. I will report the price to my boss and call you back if the price is acceptable for him.

Thank you very much for your time.

Have a nice day!

Chapter 2

2 Minorities' Strategic Response to Discrimination: Experimental Evidence[1]

Nikoloz Kudashvili and Philipp Lergetporer[2]

2.1 Introduction

Discrimination against minorities is pervasive in many societies around the world. Over the past decades, an impressive body of research in economics and related fields has documented discrimination in various market- and non-market settings (for surveys of the field, lab-, and non-experimental literature, see Arrow 1998, Yinger 1998, Altonji and Blank 1999, Riach and Rich 2002, Anderson et al. 2006, List and Rasul 2011, and Bertrand and Duflo 2017). Discrimination can be based on different attributes of the groups discriminated against (e.g., ethnicity, gender, or religion), and materializes in decisions related to hiring, pricing, letting,

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[2] Technical University of Munich and ifo institute; philipp.lergetporer@tum.de.

or allocating attention (e.g., Bartoš et al. 2016).[3] The experimental literature to date has mainly focused on majority-group members' decisions to discriminate, e.g., by studying the existence of discrimination, or the effectiveness of anti-discrimination policies.[4] In contrast, minorities' strategic responses to discrimination have received little scholarly attention. In particular, very little is known about the effectiveness of minority-group members' strategies to circumvent anticipated discrimination. This is the research gap that we address in this paper.

We focus on minorities' strategic misrepresentation of their ethnicity to avoid being discriminated against. Existing models of discrimination usually assume that minorities' ethnic affiliation is perfectly observable. In reality, however, many interactions are characterized by asymmetric information, where majority-group members observe only a noisy signal of the ethnicity of minority-group members. Consequently, minority-group members may have some discretion over what ethnicity-revealing signals to send. For instance, job applicants can decide whether to include racial cues in their résumés (Kang et al. 2016), students can decide whether to perform rituals which identify their religious affiliation (Lavy et al. 2018), car dealers can decide whether to reveal their ethnic names in advertisements (Zussman 2013), immigrants can decide whether to adopt typical names of the host-country population (Arai and Thoursie 2009, Biavaschi et al. 2017), and minority university students can decide whether to use ethnic-majority names when emailing their professors (Zhao and Biernat 2017). In fact, the existing literature documents that misrepresentation of ethnicity is a common phenomenon among minorities, but the economic consequences of such behavior have hardly been studied.[5]

We experimentally study the causes and consequences of minorities' strategic signaling behavior in the context of the marginalized Armenian minority in the country of Georgia. Georgia provides an ideal setting for two reasons: First, as in many other societies, names are

[3] Victims of discrimination face, for instance, lower likelihood of educational success (Alesina et al. 2018), higher probabilities of being assigned monetary bail (Arnold et al. 2018) and being convicted (Anwar et al. 2012), worse labor- and rental-market outcomes (Bertrand and Mullainathan 2004, Ahmed and Hammarstedt 2008), and higher consumer prices (Gneezy et al. 2012).

[4] Potential policies to mitigate discrimination (or its consequences) include enhancing majority-group members' contact to minority-group members (Boisjoly et al. 2006), raising awareness of racial bias or stereotypes (Pope et al. 2018, Alesina et al. 2018), introducing anonymous application procedures (Goldin and Rouse 2000), and implementing affirmative-action programs (Holzer and Neumark 2000).

[5] For instance, Biavaschi et al. (2017) show that 31 percent of U.S. immigrants in the early twentieth century engaged in name Americanization, Zussman (2013) reports that 30 percent of Arab car dealers in Israel hide their ethnic names in advertisements, and Kang et al. (2017) finds that 31 percent (40 percent) of black (Asian) students conceal racial cues in their résumés.

unambiguous identifiers of ethnicity. Second, recent historical accounts suggest that some Armenians have adopted Georgian-sounding names to avoid discrimination (see section 2.2.3 for historical and cultural background information).[6]

We conduct our randomized lab-in-the-field experiment with a total of 758 high school students (aged 12 to 17 years) from six high schools in Tbilisi, Georgia.[7] Studying the determinants and consequences of strategic name-signaling behavior with observational data is extremely challenging because credible exogenous variation to identify causal effects and high-quality data on behaviors and beliefs are usually unavailable. To sidestep these identification challenges, we use a modified version of the trust (or "investment") game (Berg et al. 1995) to measure discrimination in our experiment. The trust game is a two-player game in which the first player ("trustor") is endowed with a fixed number of tokens, and has to decide how many tokens, if any, to transfer to the second player ("trustee"). The experimenter multiplies the transfer and hands it over to the trustee, who then decides how many of the received tokens, if any, to transfer back. Back transfers are not multiplied. We use this experimental paradigm to address the following three research questions: First, do Georgians discriminate against the Armenian minority in the trust game? Second, do Armenians expect discrimination and do they misrepresent their ethnicity to avoid being discriminated against? Third, is Armenians' strategic signaling behavior effective in reducing discrimination?

Focusing on Georgians in the role of trustors and Armenians in the role of trustees, we implement four between-subject treatments in which first names serve as indicators for the interaction partners' ethnic affiliation. In the first treatment, the trustor is ethnic Georgian and the trustee is also ethnic Georgian. In the second treatment, the trustor is ethnic Georgian and the trustee is ethnic Armenian. These two treatments do not include a signaling stage and serve

[6] Names are the most common way to manipulate perceived minority traits in correspondence studies (Bertrand and Duflo 2017). The literature documents various ways individuals can "choose" majority-sounding names strategically: One set of options includes official naming decisions, such as legally binding name changes (e.g., Arai and Thoursie 2009, Biavaschi et al. 2017), or parents choosing names for their children (e.g., Abramitzky et al. 2016, 2019). Another set of options concern situations where individuals have the possibility to choose names which do not necessarily correspond to their official names. For instance, Lieberson (2000) lists the original and stage names of entertainers in the U.S., and argues that stage names are often chosen to hide ethnic or religious origins. Similarly, new technologies such as email or social media provide a particularly easy way to manipulate perceived minority status (e.g., Zhao and Biernat 2017). For instance, anecdotal evidence suggests that teenagers of Arabic descent in Germany choose Latino-sounding names on Facebook to improve their mating probabilities (Bayerischer Rundfunk, 8 September 2016, https://www.br.de/puls/themen/leben/aus-muslim-wird-latino-100.html [accessed 11 January 2019]).

[7] In Harrison and List's (2004) taxonomy, our study is classified as a "framed field experiment".

as our benchmark to measure Georgians' trust discrimination against Armenians. In the third treatment, instead of informing the trustor about the ethnicity of the trustee, the trustee had the option to send a message about her ethnicity to the trustor. The trustee can decide between sending (i) a truthful message signaling that her name is ethnic Armenian, (ii) an untruthful message signaling that her name is ethnic Georgian, or (iii) no message. Comparing Georgian trustors' transfers to Armenian trustees across treatments with and without the signaling stage reveals the extent to which Armenian trustees' signaling behavior mitigates discrimination. Finally, the fourth treatment is identical to the third treatment except that the trustor is also Armenian. Comparing Armenian trustees' signaling behavior toward Georgian versus Armenian trustors allows us to assess whether signaling behavior is strategic in the sense that it depends on the ethnicity of the trustor.

We have three main findings. First, there is pronounced discrimination by Georgian trustors against Armenian trustees: While Georgian trustors transfer, on average, 5.2 tokens to Georgian trustees in the first treatment, transfers to Armenian trustees are significantly lower by 1.2 tokens ($p<0.01$). This discrimination is based on Georgians' correct belief about lower back transfers from Armenian trustees. Second, Armenians anticipate this discrimination, and many react to it by misrepresenting their names: In the third treatment with the signaling stage, 43 percent of trustees send the untruthful message that their name is Georgian. Signaling behavior is driven by both expected transfers and non-pecuniary considerations such as ethnic in-group attachment, which suggests that subjects face a trade-off between monetary and identity-based factors when choosing a signal. The fact that none of the Armenian trustees sends a Georgian name signal when the trustor is also Armenian in the fourth treatment shows that minority members use the signaling device strategically to avoid discrimination. Third, Armenians' signaling behavior is effective in reducing discrimination: On average, the magnitude of Georgian trustors' discrimination halves when introducing the signaling stage, which increases overall efficiency (and Armenian trustees' profits). Scrutinizing the underlying mechanisms of these effects, we suggest that the signal alters trustors' transfers by raising their expectations about trustees' back transfers.

Our results show that minorities' strategic behavior can strongly reduce observed discrimination, which highlights the importance of accounting for such strategic responses when measuring discrimination, especially in observational data. The rest of the paper is organized as follows. In section 2.2, we discuss the related literature and highlight our contributions. Section 2.3 provides a brief account of the historical and cultural background of the Armenian minority in Georgia. Section 2.4 introduces the experimental design and procedure. Section 2.5 presents the results, and section 2.6 concludes.

2.2 Related Literature

Our paper contributes to several strands of the economic literature. At the most basic level, it complements the large body of experimental studies on ethnic discrimination. While experimental evidence on discrimination is vast, this literature mostly measures discrimination without accounting for potential victims' strategic responses to circumvent anticipated discrimination. As our results show, adjusting for minority-group members' optimizing behavior (in the form of strategic ethnic signaling) halves the magnitude of trust discrimination. Of course, the extent to which adjusted or unadjusted discrimination rates are more meaningful depends on the specific context and research question at hands. In any case, our results highlight that whether or not one accounts for minorities' strategic behavior has important consequences for the magnitude and interpretation of measured discrimination.[8]

Few papers examine minorities' strategic behavior in response to discrimination. Most prominently, Parsons et al. (2011) investigate pitchers' strategic behavior in Major League Baseball. Pitchers correctly anticipate that strikes are called less often if umpires are of a different ethnicity. Consequently, pitchers throw pitches that allow other-ethnicity umpires fewer subjective judgements, which biases minorities' performance downward. Complementing this paper, we show causal evidence that minorities' strategic behavior to avoid discrimination extends to ethnicity-signaling decisions.

We are aware of only three papers that study signaling behavior in the presence of possible discrimination. Zussman (2013) provides descriptive evidence that Arab car dealers in Israel are more likely to leave the name fields of their advertisements blank than Israeli dealers are. The author suggests that Arab car dealers obfuscate their ethnic identity to avoid being

[8] Relatedly, a major criticism against audit- and correspondence studies is that they assume that job seekers apply for positions in a random fashion. In reality, however, real job seekers optimize their behavior during the search process, for instance, by not applying for certain positions, or by strategically highlighting or hiding some of their characteristics in their application material. Thus, these types of studies measure average differences in hiring probabilities, but not discrimination at the margin, i.e. after minority-group members adjusted their behavior strategically to the realities of the specific market (see Heckman 1998, and section 1.5 in Bertrand and Duflo 2017).

discriminated against.[9] Relatedly, Alston's (2018) working paper shows that experimental subjects in the role of workers (mistakenly) assume that managers will discriminate against females in their hiring decisions, and that female workers therefore are willing to give up their earnings for not revealing their gender in their résumé. Finally, Kang et al. (2016) find that black and Asian job applicants engage in "résumé whitening", i.e. concealing racial cues in résumés, to avoid anticipated discrimination. We extend this small evidence base by identifying the causal effect of minorities' strategic signaling behavior on majority-group members' propensity to discriminate in the trust game.

On a more general level, our paper provides an experimental micro-foundation for studies investigating the effects of minorities' name changing behavior on their economic success. The first paper to study this phenomenon is Arai and Thoursie (2009), who show for Sweden that immigrants from Asian, African, and Slavic countries experience substantial earnings increases after changing their surnames to Swedish-sounding names. Focusing on U.S. immigrants in the early twentieth century, Biavaschi et al. (2017) find that the Americanization of first names is associated with substantial occupational upgrading.[10] These studies speculate that minorities adopt majority-sounding names to avoid name-based discrimination, which implies a trade-off between discrimination costs and identity costs associated with name-changing decisions (Jia and Persson, 2020).[11] Our results support the notions that minority-group members misrepresent their ethnicity strategically to avoid discrimination, and that they face a trade-off between pecuniary and non-pecuniary factors when taking signaling decisions.

Methodologically, our paper is part of the literature which investigates discrimination using trust games (Berg et al. 1995). In their seminal contribution, Fershtman and Gneezy (2001) study ethnic discrimination between Ashkenazic (Western) and Eastern Jews in Israel. Using typical ethnic names as signals for ethnicity, they detect systematic distrust toward men

[9] Zussman (2013) focuses on the existence of (taste-based versus statistical) discrimination against Arab buyers and sellers on the Israeli online market for used cars, and not on Arab's strategic response to avoid discrimination. While he shows experimentally that leaving the name field blank (as opposed to signaling an Arab name) yields higher callback rates, his evidence on Arab's name-signaling choice is non-experimental. We advance this observational evidence by showing that minorities' signaling choice is strategic in the sense that it causally depends on the opponent's ethnicity.

[10] Relatedly, Abramitzky et al. (2016, 2019) investigate immigrants' first-name choices for their children in the U.S., and show that choosing American-sounding names relates to improved economic outcomes, e.g., in terms of educational and labor market success. A related strand of economic research analyzes name choice rather than its effect on outcomes (e.g., Goldin and Shim 2004, Fryer and Levitt 2004, Algan et al. 2013).

[11] Note that observed name changes do not necessarily reflect strategic motives. It might well be that they arise from minorities' efforts to assimilate into the majority society, or from general preferences for majority-sounding names, for instance.

of Eastern ethnicity. Our experimental design extends Fershtman and Gneezy (2001) with an initial ethnicity-signaling stage in which trustees can send an (un)truthful message about their name. To our knowledge, the only other paper to introduce such a signaling stage in the trust game is Heyes and List (2016). When they allow their subjects to decide whether or not to pay to send a picture of themselves to their opponent, they find that a substantial proportion of players is willing do so, and this does (does not) increase the tokens sent by the trustee (trustor). We extend Heyes and List's (2016) design in two key dimensions: First, we introduce the possibility to send an incorrect signal. Second, we exogenously vary trustors' ethnicity, which allows us to uncover the strategic motives behind signaling decisions.

Finally, we add to the growing literature on economic behavior of children and adolescents (see Sutter et al. 2019 for an overview), in particular to lab-in-the-field experiments on discrimination in dictator- and cooperation games (e.g., Fehr et al. 2008, 2013, Angerer et al. 2016, List et al. 2017, Barron et al. 2020, Bindra et al. 2020). While this literature scrutinized the development of discriminatory behavior early in life, our contribution is to investigate minority adolescents' strategic reactions to anticipated discrimination.

2.3 Brief Historical and Cultural Background

This section provides a brief description of our study's setting – the country of Georgia – and of the relationship between Georgians and ethnic Armenians in the country (see Appendix B.2 for additional information).

Georgia is a small country in the Caucasus region with a population of 3.7 million and a GDP per capita of \$ 9,702 in 2017 (PPP adjusted).[12] The capital of Georgia, Tbilisi, is the largest city with a population of over 1 million. Georgia is a multiethnic state with ethnic minorities accounting for about 15 percent of the population. Armenians are the second largest minority group in Georgia (after Azeris) and live mostly in Tbilisi and the Javakheti region in the country's south.[13] Both Georgians and Armenians are typically Christian, though Georgians are usually Orthodox and Armenians belong to the Armenian Apostolic Church. The two groups differ somewhat in appearance, but appearance is not an unambiguous identifier of

[12] World Bank, https://data.worldbank.org/country/georgia [accessed 5 June 2019].
[13] Armenians accounted for 4.5 percent of the country's population, and for 4.8 percent of Tbilisi's population, in 2014 (see Appendix Table B1.1).

ethnicity. Monthly average income from paid employment, educational attainment, but also unemployment rates are higher among Georgians than among members of the Armenian minority. The Armenian language differs widely from Georgian, but over 96 percent of the Armenian minority in Tbilisi speak Georgian (Osepashvili 2013). The Armenian minority in Tbilisi is concentrated in the central districts of the city, and are not segregated from ethnic Georgians. Tbilisi has a total of 294 schools, which are segregated along ethnic lines. Most schools are Georgian (and cater to Georgian children), and a small minority of ten schools are Armenian or Russian (and cater to Armenian children). Language of instructions in all public schools is Georgian.

The relationship between Georgians and Armenians is characterized by a long history of mistrust and rivalry, which culminated in the Georgian-Armenian war in 1918.[14] After the collapse of the Soviet Union, Georgia saw a rise of nationalism, and minorities in Georgia were increasingly considered a threat to national security (Jones 1996). While the relationship between Georgians and the Armenian minority has improved over the years, negative perceptions of and mistrust toward Armenians still prevail in Georgian society (Osepashvili 2013). For instance, the 2017 Caucasus Barometer finds that only 68 percent of Georgians approve of members of their ethnicity doing business with Armenians living in Georgia (Caucasus Research Resource Centers 2017).[15] Similarly, only 31 percent of Georgian subjects in our sample state that they trust Armenians “a lot” or “a bit” in our post-experimental questionnaire, while trust toward other Georgians is high at 74 percent (see Appendix Figure B1.1).[16]

In Georgia, names are unambiguous identifiers of ethnicity. For instance, most Georgian surnames end with the suffix *shvili, dze, ava, ia, ua, or iani*, while Armenian surnames end with *ian*. Similarly, first names are ethnicity-specific. Reportedly, some Armenians in Georgia have changed their names to Georgian-sounding names to avoid being discriminated against (Public Defender’s Office of Georgia 2008). Name changes peaked in the 1990s, which coincides with the rise of nationalism in Georgia after the fall of the Soviet

[14] See Rohner et al. (2013a, 2013b) for theory and evidence on the eroding effects of conflict on trust.

[15] The approval rate of doing business with other Georgians is much higher at 94 percent. Similarly, only 40 percent of respondents to the Caucasus Barometer 2017 approve of women of their ethnicity marrying an Armenian living in Georgia (Caucasus Research Resource Centers 2017).

[16] Armenian subjects’ trust toward both Georgians and Armenians is equally high at 75 percent and 74 percent, respectively. Asali et al. (2018) provide correspondence-study evidence for labor-market discrimination against Armenians in Georgia.

Union.[17] Today, name changes remain a common phenomenon in Georgia: The webpage of Georgia's Ministry of Justice states that "A citizen of Georgia [...] has the right to change his/her name or/and surname" and offers name changes at low fees from 55 Georgian Lari (app. 20 USD; current exchange rate) (Ministry of Justice, https://sda.gov.ge/?page_id=7429&lang=en [accessed 7 June 2019]).

2.4 Experimental Design and Procedure

2.4.1 The Modified Trust Game

Our experimental design is based on Berg et al.'s (1995) standard trust game, which consists of two players (trustor and trustee) and two stages. The trustor is endowed with 10 Experimental Currency Units (ECU). In the first stage, she has to decide what amount $T \in [0,10]$ to transfer to the trustee. The experimenter then triples the transferred amount. In the second stage, the trustee observes the trustor's transfer and decides upon $B \in [0, 3T]$, i.e., the number of ECUs to transfer back to the trustor. Back transfers are not tripled. Transfers from the trustor (T) are usually interpreted as "trust", whereas back transfers from the trustee (B) are usually interpreted as "trustworthiness".[18] The Nash equilibrium of the game with self-regarding agents is that the trustor sends nothing (T=0) and, consequently, the trustee returns nothing (B=0). However, the socially optimal outcome (in terms of the total number of tokens produced) is that the trustor transfers his entire endowment (T=10). In contrast to zero-sum games, an important advantage of the trust game is that it enables the researcher to quantify the efficiency implications of discrimination.

As is standard for the trust game, we use the strategy method (Selten 1967) to elicit trustees' decisions, i.e., trustees have to specify their back transfer (B) for each possible level of the trustors' transfer (T).[19] To scrutinize the motivation behind subjects' choices, we also

[17] Qualitative evidence suggests that Armenians in Georgia change their names to Georgian names in order to avoid disadvantages, for instance on the labor market (Osepashvili 2013). Unfortunately, data on actual name changes in Georgia is not available to researchers.
[18] See Houser et al. (2010) and Cox et al. (2016) for detailed discussions of the interpretation of trust and trustworthiness.
[19] Casari and Cason (2009) show that the strategy method yields somewhat lower levels of trustworthiness compared to the direct-response method. Brandts and Charness (2000) find no difference in behavior across both elicitation methods in two-person sequential games. Note that we keep the elicitation method constant across treatments, so that it does not affect treatment-effect estimates.

elicit trustors' beliefs about trustees' back transfers (B) and trustees' beliefs about trustors' transfers (T) after experimental decisions are made. We incentivize beliefs to foster truthful reporting.[20]

The goal of this paper is to study trust discrimination by Georgian trustors against Armenian trustees. Therefore, our four treatments (which we describe in detail below) differ with respect to the ethnicity of trustors and trustees. Exploiting the fact that names are unambiguous identifiers of Georgian or Armenian ethnicity, we follow the standard approach from previous experiments and use names to induce perceptions about ethnicity. Specifically, we compiled name lists of ten common Georgian respectively Armenian first names (five male and five female names) from our sample and inform each trustor that her interaction partner's first name is "among the list of names" that we handed out (see Appendix B3 for the instructions).[21] A major advantage of conducting our experiment in schools is that they provided us with complete name lists in advance. This allowed us to create individualized name lists for each subject, and to induce perceptions about subjects' ethnicity without deception. This approach, using name lists, as opposed to single names, is similar to the strategy applied in Bauer et al. (2018), and has the advantages that (i) the risk of losing anonymity is much smaller with name lists, (ii) and false attributions of names to ethnicities are less likely.[22] As a manipulation check, we elicited subjects' beliefs about their interaction partners' ethnicity in our post-experimental questionnaire, and we find that the name lists work as intended.[23]

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[20] Subjects receive two extra ECUs if their stated belief is exactly correct, and one extra ECU if it is only one or two units away from the true value. While there are more sophisticated methods to incentivize beliefs (e.g., Trautmann and van de Kuilen 2015), we deliberately opted for a simpler incentive scheme to foster comprehension among our sample of adolescents.

[21] Names are one of the most common ways to manipulate perceived minority traits in experiments on discrimination (e.g., Fershtman and Gneezy 2001, Bertrand and Mullainathan 2004, Cettolin and Suetens 2019, Bertrand and Duflo 2017). While it would also be interesting to investigate gender-specific ethnic discrimination in our setting, our ex-ante power calculations revealed that we were underpowered to split our sample to study gender differences. Therefore, we decided to avoid the possibility of gender-specific ethnic discrimination by using mixed-gender name lists.

[22] One might be concerned that using name lists, as opposed to single names, makes it more obvious for the subjects that the experiment is about ethnicity, which, in turn, might trigger experimenter-demand effects. We consider this concern unlikely for several reasons. First, it is a priori not clear whether the focus on ethnicity is less obvious when ethnicity is signaled through single names. Second, we employ a between-subject design which is less susceptible to experimenter-demand effects when measuring discrimination than within-subject designs (e.g., Angerer et al., 2016). Third, at the most basic level, de Quidt et al. (2018) provide evidence that experimenter demand effects barely affect choices in economic games.

[23] All trustors correctly associate the Georgian (Armenian) name list with trustees' Georgian (Armenian) ethnicity. For the sake of simplicity, we induce trustees' beliefs about trustors' ethnicity by directly informing them that their interaction partner has a Georgian/Armenian first name. Note that this asymmetry between trustors and trustees in how ethnic perception is induced (name lists versus direct information) is kept constant across treatments, and therefore does not affect treatment-effect estimates.

To study whether Armenian trustees strategically misrepresent their ethnicity to avoid being discriminated against, some of our treatments include a pre-play signaling stage. In these signaling treatments, Armenian trustees can send a signal about their ethnicity, and trustors observe the signal before deciding upon their transfers (T). Specifically, an Armenian trustee has to choose one of three options: 1. Sending the truthful message that he or she has an Armenian name. 2. Sending the untruthful message that she has a Georgian name. 3. Sending no message. Importantly, we made it clear in the trustor's instructions of the signaling treatments that the name list is a message from the trustee, and not information provided by the experimenters. In particular, we told trustors in the signaling treatments "[…] we want to inform you that the student you are matched with had the option to send you a message about his or her first name." In principle, one might expect that trustors do not believe the trustees and therefore fail to internalize the information provided in the message. In section 2.5.2 we show that this is not the case. The text of the signal reads "My first name is among the names listed below". For the truthful message, the list comprises 10 Armenian names, including the real name of the trustee. For the untruthful message, the name list comprises 10 Georgian names. Appendix Table B1.2 depicts an example of the trustees' message space. All trustors in these treatments were informed that the trustee had the option to send a message about his or her first name, but we deliberately did not inform the trustor about the trustee's message space (signaling an Armenian name, a Georgian name, or sending no signal). We took this design choice – which reflects the standard correspondence-study design where the ethnicity-signal space is not revealed to the employer either – to resemble everyday-life interactions with asymmetric information about interaction partners' ethnicity as closely as possible. In many everyday-life situations, people are also not explicitly informed that interaction partner might not reveal their true ethnicity. Also note that our experimental design does not involve any deception on the part of the experimenter. Instead, it is similar to the experimental literature on deception in that it allows experimental subjects to deceive each other (e.g., Gneezy 2005). In these treatments, we elicit trustees' beliefs about trustors' transfers using the strategy method. Thus, each trustee has to state what transfer (T) she expects upon signaling an Armenian name, a Georgian name, and when sending no signal. To incentivize truthful reporting, we told subjects that we would randomly choose one of their beliefs and compare it to the average transfers of trustors who received the respective signal. These beliefs allow us to investigate the extent to which differences in expected transfers can explain trustees' signaling behavior.[24] Furthermore, the post-experimental questionnaire elicits trustees' second-order ethnic beliefs in the signaling treatments, i.e. what beliefs they expect the trustors to hold about their ethnicity.

2.4.2 Treatment Groups

We implement four between-subject treatments to identify the extent of discrimination among majority-group members, and strategic responses of the discriminated minority. In the first treatment, G-G, both the trustor and the trustee are ethnic Georgians. In the second treatment, G-A, the trustor is ethnic Georgian and the trustee Armenian. The third treatment, G-A Signal, is identical to the second treatment, with the exception that the Armenian trustee has the possibility to send a signal about his or her name as described above. Finally, the fourth treatment, A-A Signal, is identical to treatment G-A Signal, except that both the trustor and the trustee are ethnic Armenians. Below, we describe the treatment contrasts we focus on in our analysis.

Measuring the extent of discrimination without signaling: G-G versus G-A

We first measure the extent of discrimination by Georgian trustors against Armenian trustees without signaling. To do so, we compare trustors' transfers between treatments *G-G* and *G-A*. Both treatments differ only in the ethnicity of the trustees, so that differences in trustors' transfers can be causally attributed to trustees' ethnicity. The contrast between *G-G* and *G-A* serves as our benchmark to assess how Armenian trustees' strategic signaling behavior affects the extent of discrimination. Further, we compare the amounts of transfers which trustees expect to receive across treatments to investigate whether subjects hold correct beliefs about the extent to which Georgians trust them.

Measuring strategic signaling behavior: G-A Signal versus A-A Signal

The main innovation of our experimental design is to introduce a pre-play signaling stage which allows Armenian trustees to misrepresent their ethnicity. Before trustors decide upon their transfers (T), Armenian trustees in these treatments can decide between truthfully signaling an Armenian name, untruthfully signaling a Georgian name, or sending no signal at

[24] Note that we do not impose extrinsic costs on sending a signal. However, our results in section 2.5.2 suggest that sending an untruthful signal is associated with significant intrinsic identity costs for our subjects.

all. We are particularly interested in the share of subjects who decide to signal their Armenian name in treatment *G-A Signal*. Note, however, that untruthful name signaling in this treatment cannot be interpreted as direct evidence of *strategic* signaling behavior: It may well be that Armenians do not signal their ethnicity because they have privacy concerns or preferences for mimicking Georgian ethnicity, or because they are indifferent between messages and therefore pick a message at random. Therefore, we implement treatment *A-A Signal*, which is identical to treatment *G-A Signal* except that the trustor is Armenian rather than Georgian. Comparing trustees' signaling behavior across these two treatments enables us to assess the extent to which Armenians choose signals strategically. Finally, we analyze what transfers Armenian trustees expect to receive upon sending different signals. This within-subject comparison reveals whether subjects expect discrimination-reducing effects from not revealing their Armenian ethnicity.

2.4.3 Subject Pool and Experimental Procedure

The experiment was conducted in the fall of 2017 in 41 classes at six high schools (22 classes in three Georgian schools and 19 classes in three Armenian schools) in Tbilisi, Georgia. All subjects in Georgian schools had ethnic Georgian first names. In Armenian schools, three students had ethnic Georgian names, and all other students had ethnic Armenian names. We excluded those three students so that first names in our sample are unambiguous identifiers of ethnicity. In total, 758 students aged 12 to 17 years (grades 7 to 12) participated. High schools in Georgia are comprehensive up to grade 12, which means that our sample is not selective with respect to educational track choice. Table 2.1 shows the distribution of participants across treatments and roles.[25] The study was pre-registered in the AEA RCT Registry (trial 2522) and approved by the schools' principals and teachers. The experiments were conducted in classes during regular school hours. Of course, participation was voluntary, and all subjects consented to participate.

Each session lasted about 60 minutes, including the post-experimental questionnaire. The experiment was explained to the whole class in detail, following a fixed script. Consistent

[25] Note that the treatment contrast between G-G and G-A Signal is particularly important for estimating the discrimination-reducing effects of the signaling stage. To maximize statistical power for these groups, we randomly assigned them higher numbers of observations. The remaining small imbalances in the numbers of observations in Table 2.1 are due to natural class-size fluctuations.

with the language of instructions in public schools, the experimental instructions were in Georgian.[26] We phrased our instructions as simply as possible and used visual support to assure comprehension. Prior to the decision phase, participants answered control questions privately. If a subject failed to answer questions correctly, the instructions were explained again in private until comprehension was achieved. See Appendix B3 for the instructions and the post-experimental questionnaire.[27] The unit of randomization was at the class level. Therefore, we cluster standard errors at the class level in our analyses. Appendix Table B1.3 assesses the balance of observable characteristics across experimental groups. Six of 60 comparisons show significant differences; however, our results remain robust when we control for these covariates. Closer inspection reveals that there are in fact no significant differences in observable characteristics across Georgian trustors (see columns 1 to 3) or across Armenian trustees (see columns 5 to 7).[28] Trustors and trustees were matched one-to-one across schools in order to minimize the risk of losing subjects' anonymity. For practical reasons, we first collected all decisions of the trustees using the strategy method and then elicited trustors' decisions.

We incentivized choices using gift vouchers from a well-known office-supplies chain. Each token was worth two Georgian Lari (app. 0.8 USD). In our post-experimental questionnaire, almost all participants (99.5 percent) stated that they liked the gift voucher, which indicates that the incentives were meaningful for them. In addition to the tokens earned during the experiment, participants received a show-up fee of a gift voucher worth two Georgian Lari. While the show-up fee was paid immediately after the experiment, payment for subjects' experimental decisions was delayed one week because the decisions of trustors and trustees had to be matched to calculate earnings. Delayed payments were made in sealed envelopes marked with an anonymized ID. According to our post-experiment questionnaire,

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[26] One concern with conducting our experiment in Georgian language is that Armenians may shy away from sending the untruthful signal of a Georgian name because of experimenter-demand effects, which would render the misrepresentation-rate in section 2.5.2 a lower bound. We consider this possibility unlikely, however, because all choices were taken in private.

[27] In our post-experimental questionnaire, we asked subjects to rate how well they understood the instructions on an 11-point scale (from 0 = "Did not understand at all" to 10 = "Understood very well"). The median (mean) answer to the question is 10 (9.65), and only four subjects gave an answer of 5 or below. Excluding those subjects from the analysis does not change our qualitative results (results available upon request).

[28] Note that the focus of this paper is on Georgian trustors' discrimination and Armenian trustees' response to anticipated discrimination, where all covariates are balanced. Imbalances prove to be concentrated among Georgian trustees who are, on average, more likely to be male, older, more risk tolerant and to have a better understanding of the instructions.

almost all subjects (99.5 percent) trusted that they would receive the delayed payment.[29]

2.5 Results

We present our results in three steps. First, we analyze Georgian trustors' transfers, Armenian trustees' back transfers, and beliefs without the signaling stage. Second, we investigate Armenians' strategic name-signaling behavior. Third, we evaluate its effects on Georgian trustors' discrimination.

2.5.1 Discrimination against Armenian Trustees without Signaling

We begin with a depiction of Georgian trustors' transfers and beliefs without signaling. Figure 2.1 shows their average transfers to Georgian and Armenian trustees in treatments G-G and G-A, respectively (see the left panel of the figure). On average, Georgian trustors transfer 5.2 tokens of their 10-token endowment to Georgian trustees (see bar "Treatment G-G"). Transfers to Armenian trustees are significantly lower at 4.0 tokens (about 77 percent of the average transfer to a Georgian trustee; see bar "Treatment G-A"). Columns 1 and 2 of Table 2.2 present OLS regressions of Georgian trustors' transfers on an indicator for treatment G-A (omitted category: treatment G-G).[30] The coefficient on treatment G-A in column 1 shows that the difference in transfers to Georgian versus Armenian trustees of 1.2 tokens is highly statistically significant, and it barely changes when we control for standard covariates in column 2 (covariates include gender, age, number of siblings, and self-reported risk tolerance and patience). Thus, we find robust evidence of pronounced discrimination by Georgian trustors against Armenian trustees which, by the nature of the trust game, decreases overall efficiency.

To scrutinize the motivations behind trust discrimination against Armenians, we next investigate trustors' expected back transfers from Georgian and Armenian trustees. In columns 3 and 4 of Table 2.2, we regress our incentivized measure of expected back transfers on treatment indicator G-A, controlling for trustors-transfer dummies. On average, Georgian

[29] Excluding those few subjects who (i) do not like the gift voucher or (ii) do not trust that they will receive the delayed payment does not change our qualitative results (results available upon request).
[30] All models in this paper are estimated as linear regression models. (Ordered) probit models yield qualitatively identical results (results available upon request).

trustors expect to receive a back transfer of 5.6 tokens from Georgian trustees (see control mean). The significant and negative treatment coefficient in column 3 shows that Georgian trustors' expected back transfers from Armenian trustees are significantly lower, by 0.8 tokens.[31]

When we compare Georgian trustors' beliefs about trustees' back transfers with actual back transfers in columns 1 and 2 of Table 2.3, we find that the trustors' beliefs are very well-calibrated: While Georgian trustees on average transfer 5.7 tokens back to Georgian trustors, Armenian trustees' back transfers are significantly lower, by more than 0.5 tokens.[32] Figure 2.2 depicts Armenian and Georgian trustees' back transfers for each possible trustor transfer. The pattern that back transfers strictly increase with trustor transfers shows that both Armenian and Georgian trustees act reciprocally. Most importantly, however, Armenian trustees' back transfers are lower than Georgians' for each possible trustor transfer. Thus, Georgian trustors have accurate beliefs about Georgians' and Armenians' trustworthiness. This finding suggests that trust discrimination against Armenians is (at least partially) driven by statistical discrimination (Arrow 1972, 1973, Phelps 1972) in the sense that Georgian trustors correctly expect lower back transfers from Armenian trustees and therefore transfer less to them.[33, 34]

Finally, we investigate the transfer amounts trustees expect to receive. Comparing Armenian and Georgian trustees' beliefs (see columns 3 and 4 of Table 2.3) with actual

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[31] In additional analyses in Appendix Table B1.4, we regress trustors' beliefs on treatment indicator *G-A*, trustors' transfer, and their interaction. Results show that the gap in expected back transfers from Georgian versus Armenian trustees increases with trustors' transfers (see the significant coefficient on the interaction term and the corresponding Wald tests at the bottom of the table). Note also that, on average, expected back transfers increase with trustors' transfers, which is an intuitive result.

[32] The dependent variable in columns (1) and (2) of Table 2.3 is the actual back transfers. Since we used the strategy method to elicit ten back-transfer decisions from each trustee (i.e., one decision for each possible trustor transfer), columns 1 and 2 of Appendix Table B1.5 instead use each trustee's average back transfer as a dependent variable. The results in Table 2.3 are robust to this alternative definition of the dependent variable.

[33] One obvious concern with the analysis of trustors' beliefs is self-serving bias (e.g., Gino et al. 2016): Georgian trustors might state biased beliefs about Armenian trustees' back transfers to justify own low transfers. The fact that we incentivized the accuracy of beliefs mitigates this concern (see Bullock et al. 2015 and Prior et al. 2015 for evidence that monetary incentives reduce self-serving bias in stated beliefs). It is interesting to note that subjects' incentivized beliefs are uncorrelated with their answers to the general risk question (results available upon request), since subjects' risk preferences have been theorized to affect the ability of incentive schemes to foster truthful reporting (e.g., Trautmann and van de Kuilen 2015).

[34] Our results are in line with Falk and Zehnder (2013), who show that trust discrimination against people from different districts in Zurich is based on accurate beliefs about their relative trustworthiness. This result is in contrast to Fershtman and Gneezy (2001) who argue that discrimination against Eastern Jews in Israel is largely based on downward-biased beliefs about their trustworthiness. As in most of the literature that studies discrimination between natural groups, we note that it might well be that Georgians' discrimination is also based on (perceived) characteristics of Armenians, such as income or education. Bohren et al. (2019) provide a recent careful discussion of the role of (biased) beliefs in explaining discrimination, and advocate distinguishing between accurate and inaccurate statistical discrimination.

transfers reveals that trustees hold very accurate beliefs about trustors' behavior: On average, Georgian trustees expect that trustors will transfer 5.11 tokens (see control mean). As the negative coefficient on the treatment indicator in column 3 shows, Armenian trustees' expectations are significantly lower, by 1.79 tokens. This difference is robust to controlling for covariates in column 4. The comparison between Armenian and Georgian trustees shows that beliefs are consistent with the actual transfers made by trustors. Note, however, that this descriptive analysis is not informative about whether individual Armenian trustees anticipate discrimination. In the next section, we analyze the extent to which Armenian trustees anticipate discrimination when deciding to send different signals about their ethnicity.

Having established that (i) Georgians discriminate against Armenians in the trust game without signaling, and (ii) that trustors and trustees hold accurate beliefs about each others' transfers, we next investigate Armenian trustees' strategic name-signaling behavior.

2.5.2 Strategic Signaling by Armenian Trustees

Signals sent to Georgian trustors

Figure 2.3 and Table 2.4 present the distribution of signals sent by Armenian trustees to trustors in treatments G-A Signal and A-A Signal. In treatment G-A Signal, where the trustor is Georgian, 56 percent of trustees send a truthful message that they have an Armenian name. The share of untruthful messages of a Georgian name is 43 percent, and only one single Armenian (0.89 percent) chooses to send no signal. Thus, while a sizable share of Armenians misrepresents their ethnicity when interacting with a Georgian trustor, the majority truthfully signals having an Armenian name. It is noteworthy that the option not to send any signal is very unpopular, despite the fact that it represents a middle path in the sense that it (i) conceals Armenian ethnicity but (ii) does not involve untruthful signaling.

To scrutinize the motivations behind Armenian trustees' signaling behavior, we next investigate what transfers they expect to receive upon sending different signals to the Georgian trustor. After experimental decisions were made, we therefore elicited trustees' beliefs about trustors' transfers for the three possible signals using the strategy method. On average, trustees expected a transfer of 3.12 tokens from Georgian trustees when they signaled an Armenian name (see the left panel of Figure 2.4). When they signaled a Georgian name, expected transfers are significantly higher, at 5.34 tokens ($p < 0.01$, Wilcoxon signed rank test). Put differently, 78 percent of Armenian trustees expect a higher transfer when they signal a Georgian rather than an Armenian name, 22 percent expect equal transfers, and not a single one expects that signaling an Armenian name will pay off more. Expected transfers when sending no signal are 3.1 tokens, which is significantly lower than when signaling a Georgian name, and statistically indistinguishable from expected transfers when signaling an Armenian name ($p < 0.01$ respectively $p = 0.638$, Wilcoxon signed rank tests).

The observation that 78 percent of Armenian trustees expect higher transfers when signaling a Georgian name, but only 43 percent actually send a Georgian name signal, raises the question of what drives signaling behavior. The probability of sending a Georgian name is positively correlated with expecting higher transfers from sending this signal (within-person correlation = 0.46, p=0.000), which indicates that pecuniary considerations partly explain signaling behavior. Yet, a sizable share of subjects who expect higher transfers when signaling a Georgian name still signal an Armenian name, which indicates that non-pecuniary considerations – such as identity-based preferences (Akerlof and Kranton 2000) – matter as well. To explore the motivations behind signaling decisions more systematically, Table 2.5 regresses a dummy variable coded 1 if the trustee in treatment G-A Signal signals a Georgian name, and 0 otherwise, on different explanatory variables collected in the post-experiment questionnaire. While column 1 shows that pride in Armenian ethnicity does not affect signaling decisions, those who consider observable markers (such as language or names) important to being "truly Armenian" are significantly less likely to signal a Georgian name (column 2).[35] Adding expected transfers in column 3 shows that beliefs about trustors' transfers when signaling a Georgian name are significantly and positively related to sending such a signal. Column 4 adds two measures of attachment to the Armenian ingroup: the share of subjects' Armenian friends among the total number friends, and a hypothetical allocation decision between two strangers, one Armenian and one Georgian. Both measures of Armenian ingroup attachment are positively related to the probability of signaling a Georgian name.[36] Finally, adding further control variables in column 5 shows that risk tolerance is negatively associated

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[35] The variable "importance of ethnic markers" is the mean response to the following questions on how important different markers are to be "truly Armenian": "How important do you think it is to be able to speak Armenian?" "How important do you think it is to have an Armenian name?" (see footnote of Table 2.5 and Appendix B3 for the exact questions). Including both measures separately in the regressions shows that, while both coefficients are negative, only the one on language reaches statistical significance (results available upon request).

[36] One possible explanation for this finding is that other-regarding preferences toward Georgian trustors decrease with in-group attachment, which makes it more acceptable for Armenians to send a signal which might reduce a Georgian trustors' payoff.

with sending a Georgian signal. Of course, this descriptive analysis is not exhaustive since additional unobserved factors – such as intrinsic costs of lying (e.g., Abeler et al. 2014) – may also matter.[37] Our results do indicate that signaling decisions are not driven only by pecuniary considerations, but also by non-pecuniary motivations (which is in line with Hett et al. 2020, for instance).

Signals sent to Armenian trustors

From the signals sent in treatment *G-A Signal*, it is not entirely clear whether Armenians use the signaling device *strategically* to avoid being discriminated against. For instance, some Armenians might simply prefer to present themselves as Georgian, independent of their interaction partner's ethnicity. Treatment *A-A Signal*, where the trustor is Armenian, reveals that signaling behavior is in fact strategic: In this treatment, all but one trustee (99 percent) send the truthful signal that they have an Armenian name, and not a single subject sends the untruthful message that they have a Georgian name (see Figure 2.3 and Table 2.4). The signaling differences between *G-A Signal* and *A-A Signal* are highly statistically significant (see column 5 of Table 2.4) and show that Armenian participants condition their signals on the trustors' ethnicity. Consistently, Figure 2.4 and Appendix Table B1.6 show that trustees' expected transfers when signaling an Armenian (Georgian) name are significantly higher (lower) in treatment *A-A Signal* than in treatment *G-A Signal*.[38]

Having established that Armenian trustees use the signaling device strategically to misrepresent their ethnicity, we now investigate whether trustors believe the signals sent. To this end, our post-experimental questionnaire collected trustors' binary beliefs about trustees' ethnicity (Georgian or Armenian), as well as trustees' second-order beliefs about trustors' beliefs. Appendix Table B1.7 depict the share of trustors who believe that the trustee's ethnicity

[37] While we did not measure cost of lying directly, the fact that only one subject chose the option not to send any signal – which conceals Armenian ethnicity without explicitly lying to the trustor – suggests that lying costs are unlikely to be a driving factor of signaling behavior in our context. Assessing the exact role of directly measured lying costs is an interesting avenue for future research. Interestingly, self-assessed understanding of the instructions does not correlate with signal choice, which suggests that strategic sophistication is not a key determinant of signaling behavior (see Fe and Gill 2018, for evidence on how cognitive skills and strategic sophistication emerge in children).

[38] In treatment *A-A Signal*, expected transfers are 5.98 tokens when signaling an Armenian ethnicity, 3.69 tokens when signaling a Georgian name ($p<0.01$, Wilcoxon signed rank test), and 3.21 when sending no signal ($p=0.108$ and $p<0.01$ in comparison to signaling an Armenian or Georgian name, respectively). A natural interpretation of this finding is that Armenian trustees expect that both Armenian and Georgian trustors exhibit endophilia toward interaction partners from their own ethnicity (e.g., Feld et al. 2016).

is Armenian (column 1) or Georgian (column 2). On average, 56 percent of Georgian trustors in treatment *G-A Signal* believe that the trustee is ethnic Armenian, and 44 percent believe that s/he is Georgian. Inspecting beliefs by the name-signal received, we find that trustors believe the signals: Georgian trustors who received the message that the trustee has a Georgian (Armenian) name think that she is ethnic Georgian (Armenian). Similarly, Armenian trustors believe the signal they received. The fact that trustors fully believe trustees is particularly interesting given that we emphasized that the name signal is a *message* from the trustee, and not a piece of factual information provided by the experimenter.[39] Trustees, in turn, hold correct beliefs about the impacts of their signal on trustors' beliefs (see columns 3 and 4): Those who signal an Armenian (Georgian) name correctly expect trustors to believe that they are ethnic Armenians (Georgians).[40]

In sum, this section shows that trustees use the pre-play name-signaling stage to strategically misrepresent their ethnicity, that trustors will believe the messages sent, and that trustees anticipate that trustors believe their messages. In the next section, we investigate the extent to which strategic signaling affects Georgian trustors' transfers, beliefs, and profits.

2.5.3 Effects of Name-Signaling on Discrimination against Armenian Trustees

Figure 2.1 shows that Georgian trustors transfer, on average, 4.44 tokens to Armenian trustees in treatment G-A Signal (see the right panel of the figure). This number falls between transfers to Georgian and Armenian trustees without signaling in treatments G-G and G-A, respectively. OLS regressions in Table 2.6 show that differences in Georgian trustors' transfers between treatments G-G and G-A Signal, and between treatments G-A and G-A Signal are statistically significant. Thus, allowing Armenian trustees to send a signal about their ethnicity halves the magnitude of trust discrimination in our setting. Going beyond these reduced-form

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[39] Given that our instructions and message sheet clearly indicate that the name signal is a message from the interaction partner, it is very unlikely that trustors' lack of understanding can explain their beliefs. Even if some trustors were unaware that the signal stems from the trustee and is therefore potentially subject to untruthful reporting, we consider this a natural reflection of everyday-life interactions, where the possibility that interaction partners will manipulate signals about their ethnicity is often not salient or apparent.

[40] Sutter (2009) shows that a significant percentage of senders in cheap-talk sender-receiver games with asymmetric information try to deceive the receiver by (i) sending a truthful message, and (ii) expecting the receiver not to believe the message. Note that Armenian trustees' second-order beliefs reveal that such considerations do not drive trustees' signaling behavior.

effects, we next investigate the causal effect of receiving a Georgian name signal on trustor behavior.

Figure 2.1 shows that Georgian trustor transfers vary strongly by the signal received in treatment G-A Signal. While trustors who receive a signal that the trustee has a Georgian name transfer 5.19 tokens on average, transfers are much lower, at 3.88, if no such signal is received.[41] Note that these transfers are remarkably similar to those in treatments without signaling (G-G and G-A, respectively). Columns 1 and 2 of Table 2.7 show that this difference is highly significant and robust to controlling for standard covariates.

To understand the mechanisms behind the effect of Georgian name signals on trustors' transfers, we next investigate how the signal affects trustors' expected back transfers from the trustee. Columns 3 and 4 of Table 2.7 regress trustors' expected back transfers from the trustee on a dummy indicating receipt of a Georgian name signal, controlling for trustor-transfer dummies. The significant and positive coefficients show that receiving a Georgian name signal has a strong and positive effect on trustors' expected back transfers. This finding suggests that a Georgian name signal increases trustors' transfers through altering their expected back transfers. Note that Georgian trustees received 0.018 fewer tokens in the G-A signal treatment than in the G-A treatment. However, the difference is statistically insignificant.

Turning to Armenian trustee transfers, we find that Georgian trustors overestimate actual back transfers: Columns 5 and 6 of Table 2.7 show that Armenian trustees' actual back transfers are unrelated to the signal they sent.[42] The same picture emerges when looking at back transfers for each possible trustor transfer in Appendix Figure B1.2.[43] Thus, Armenian trustees seem to increase Georgian trustors' transfers by inducing an incorrectly high level of expected back transfers.

Finally, we are interested in how Armenian trustees' strategic signaling behavior affects profits. Table 2.8 presents OLS regressions of Georgian trustors' and Armenian trustees' profits on an indicator on whether a Georgian name was signaled in treatment *G-A Signal*. While

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[41] Note that differences in trustor outcomes by the signal received can be interpreted as the causal effect of the signal, because trustees' actual signals were randomly assigned to trustors. Since random assignment of signals was implemented at the individual level (and not on the class level), we do not cluster standard errors at the class-level in this analysis.

[42] Again, using average back transfers from our strategy-method elicitation instead of actually implemented back transfers yields the same results (see columns 3 and 4 of Appendix Table B1.5).

[43] The strictly increasing pattern of the figure reveals that reciprocal motivations for back transfers are prevalent in treatment *G-A Signal*, which is similar to the back-transfer pattern in treatments without signaling (see Figure 2.2).

receiving a Georgian name signal has no overall effect on Georgian trustors' profits (see columns 1 and 2), signaling Georgian ethnicity significantly increases Armenian trustees' profits: Armenian trustees who signal a Georgian name earn 2.26 tokens, or about 27 percent, more than those who do not (see column 3). This effect is robust to controlling for standard covariates (see column 4). In sum, introducing a pre-play signaling stage increases overall efficiency. This is because Armenian trustees' strategic signals mitigate Georgian trustors' inefficient discriminatory behavior. Since Armenian trustees do not increase their back transfers when sending a Georgian name signal, they are able to capture the extra "pie" produced.

2.6 Conclusion

Ethnic discrimination is a pervasive phenomenon in many societies. However, while majority-group members' decisions to discriminate have been subjected to close scientific scrutiny over past decades, (experimental) evidence on minority-group members' strategic behavior to avoid experiencing discrimination is extremely scarce. We address this research gap by running a lab-in-the-field experiment with more than 750 high-school students in the country of Georgia, where the Armenian minority typically experiences discrimination from the ethnic Georgian majority. In our modified trust game, we implement a pre-play signaling stage in some treatments to study whether Armenian trustees misrepresent their ethnicity to avoid being discriminated against, and whether strategic signaling is effective to mitigate discrimination.

Our results show that Georgian trustors discriminate against Armenian trustees by transferring significantly fewer tokens than they transfer to Georgian trustees. When we allow Armenian trustees to send an (un)truthful signal about their ethnicity, Armenians' strategic signaling behavior halves the Georgians' trust discrimination and thereby increases overall efficiency. Our rich choice-, beliefs-, and background data allows us to study pecuniary and non-pecuniary motivations for (strategic) signaling behavior, as well as the channels through which the signaling stage increases Georgians' transfers.

A generic concern in experimental research with human subjects is that treatment effects may be driven by unintended effects such as experimenter-demand effects or social-desirability bias. We consider it unlikely that our results are due to such biases for several reasons: First, we implemented our experiment in a between-subject design, which is less susceptible to experimenter demand-effects (Charness et al. 2012). Second, all decisions were taken in private and anonymously, i.e., using anonymized ID-codes instead of names, which reduces social-desirability bias (see, e.g., Coffman et al. 2017 and references therein). Third, experimental decisions and stated beliefs were incentivized, which has also been shown to reduce report bias (e.g., Bullock et al. 2015, Prior et al. 2015, Grewenig et al. 2021). Fourth, more generally, de Quidt et al. (2018) provide evidence that experimenter demand effects only barely affect choices in economic games. Finally, the fact that our results differ very little between males and females (females are generally more likely to give "desired" answers in surveys; see Kuziemko et al. 2015) provides some indirect evidence that unintended effects like experimenter-demand effects or social-desirability biases do not drive our main results.

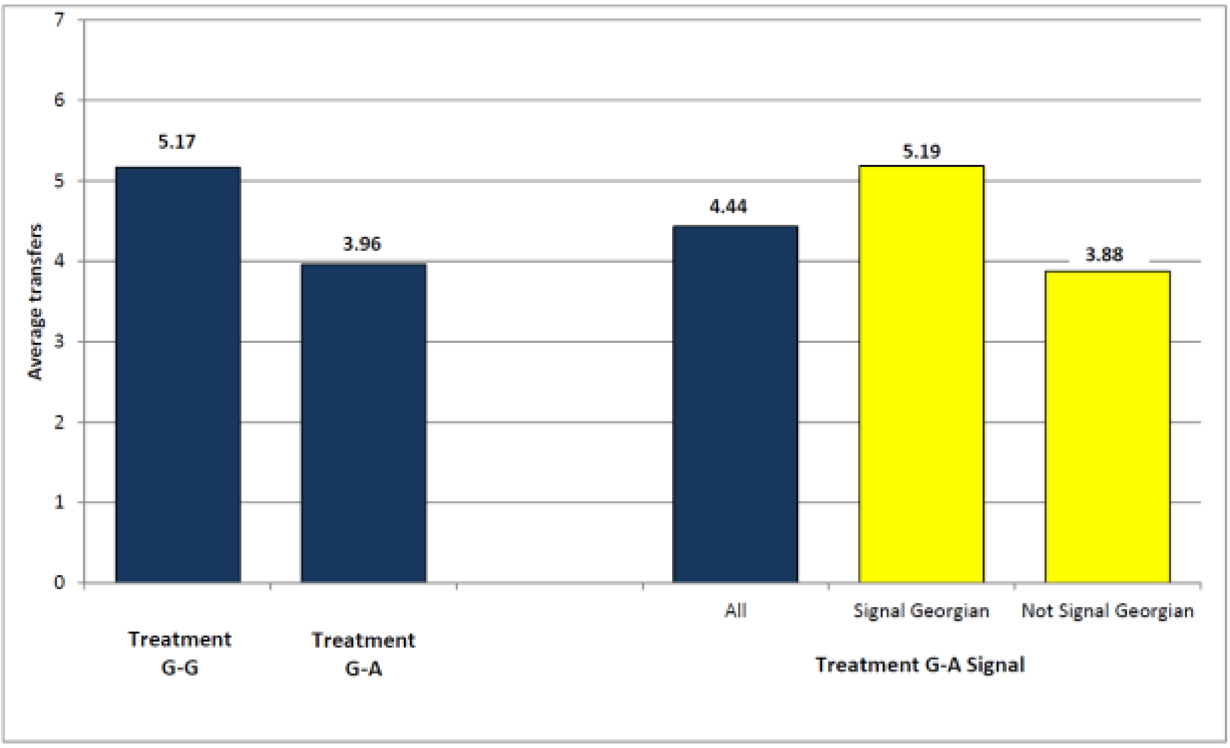
Our results are relevant for situations in which ethnic affiliation is not perfectly observable and minorities have some discretion over what ethnicity-revealing signals they send. Examples abound and include, for instance, written job applications, naming decisions, and decisions on whether to wear typical ethnic markers in everyday-life (e.g., clothing and accessories). Previous literature shows that minorities misrepresenting their ethnicity in such situations is not a marginal phenomenon: For instance, more than 30 percent of minorities misrepresent their ethnicity in Zussman (2013), Biavashi et al. (2017), and Kang et al. (2017). A particularly interesting area for which our results are relevant are new communication technologies. Recent studies show that discrimination can be based on self-reported information on social media (e.g., Tjaden et al. 2018, Acquisti and Fong 2020). It is clearly very easy to manipulate profile information on social media, or to use majority-sounding names in emails, to alter one's perceived minority status.

Our results have implications for the interpretation of experimental studies on discrimination. While these studies usually abstract from minorities' optimizing behaviors when measuring discrimination (e.g., by sending out fictitious job applications with randomized applicant characteristics), our findings show that minorities' strategic behavior can affect measured discrimination, and therefore the transferability of experimental estimates of discrimination to everyday-life settings.

Our results suggest the following policy recommendations. In principle, the fact that observed differences between ethnic groups (e.g., in labor, housing, or goods markets) are likely to be mitigated by minorities' strategic behavior should be taken into account when designing and evaluating anti-discrimination policies. In addition, interpreted at face value, our findings suggest that expanding the practice of anonymous application procedures (e.g., Goldin and Rouse 2000) to allow minority-group members to choose what signals to reveal about their ethnicity might mitigate discrimination. However, this policy conclusion has at least three clear limitations. First, the ability to misrepresent ethnicity is limited to situations in which ethnic group affiliation is not perfectly observable. Second, encouraging minorities to misrepresent their ethnicity may backfire in the long run, as it may further undermine majority-group members' trust in them. Third, as we discuss in section 2.5.2, the decision to deny one's minority-group membership may impose additional costs on minorities (e.g., lying costs or identity costs). Therefore, we view allowing minority-group members to misrepresent their ethnicity as, at best, a second-best solution to the problem of interethnic discrimination. The first-best solution is to address the discriminatory tendencies directly. Drawing on the economics literature on life-cycle (non-) cognitive skill formation, (e.g., Cunha et al. 2006), the most promising approach may be to implement anti-discrimination interventions in early education programs to tackle discrimination before it is internalized.

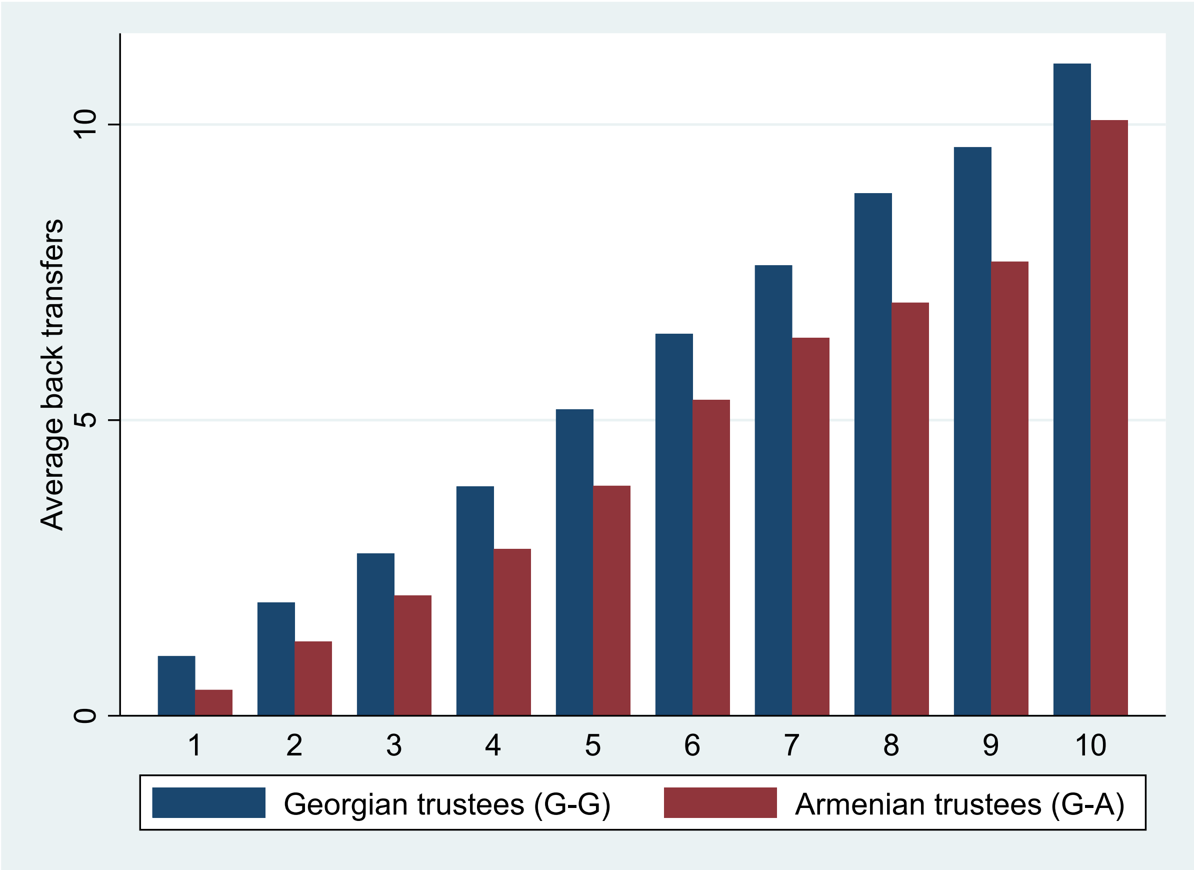
Figures and Tables

Figure 2.1: Georgian trustor's average transfers



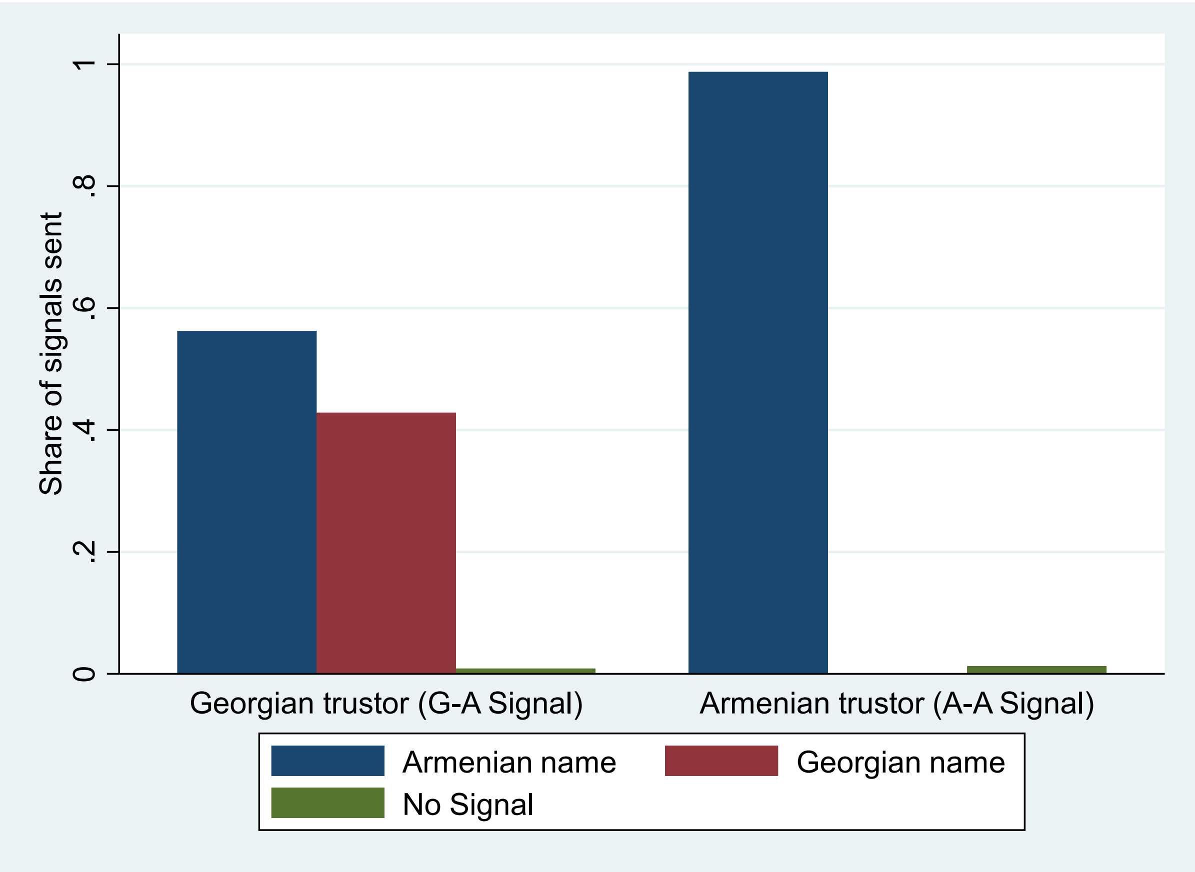
Notes: The figure shows transfers by Georgian trustors in treatments *G-G*, *G-A*, and *G-A Signal*. Dark bars represent average transfers per treatment, the light bars represent average transfers of trustors who did and did not receive the signal that the trustee has a Georgian name.

Figure 2.2: Trustees' back transfers without signaling



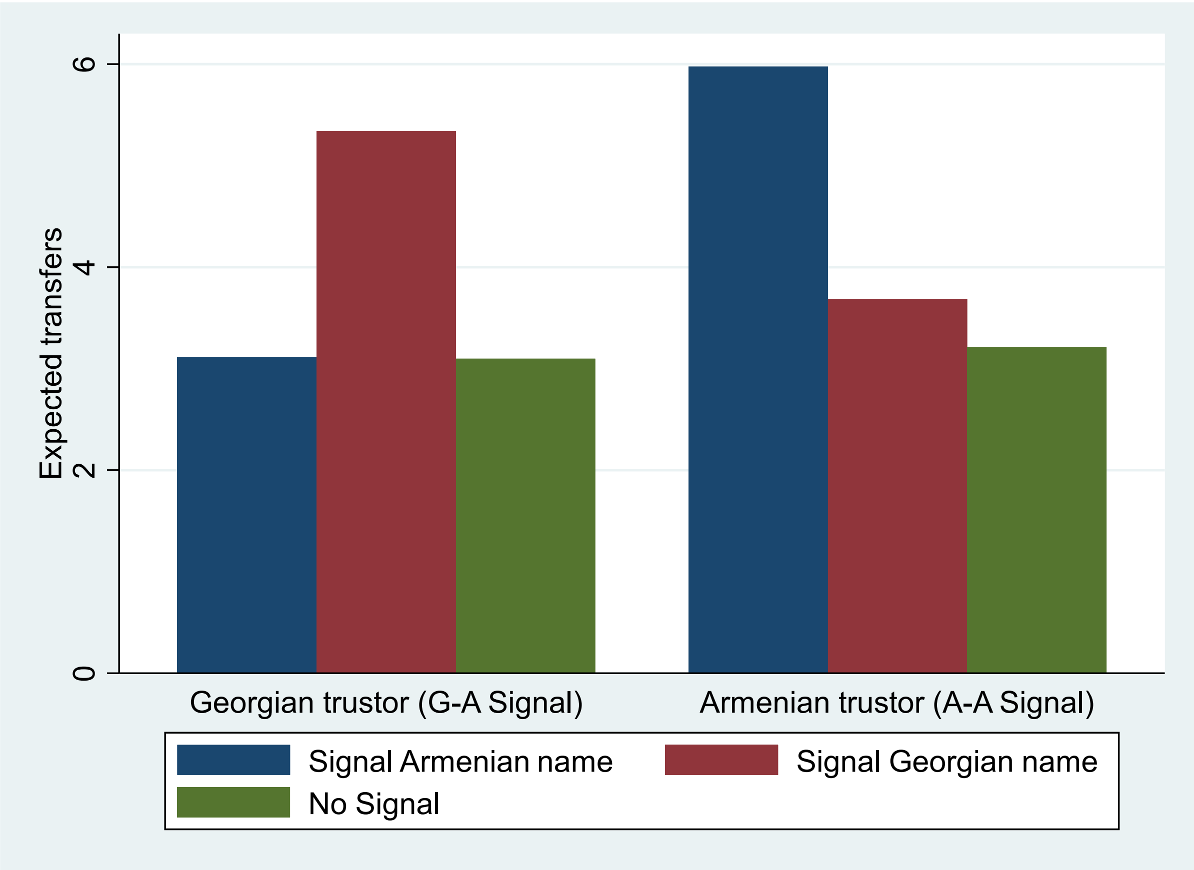
Notes: The figure shows actual back transfers by Georgian (blue bars) and Armenian (red bars) trustees to Georgian trustors. Each trustee reported a back-transfer decision for each possible trustor transfer (strategy method).

Figure 2.3: Signals sent from Armenian trustees to Georgian and Armenian trustors



Notes: The figure shows the shares of Armenian trustees who signal Armenian names (truthfully), Georgian names (untruthfully), or send no signal to the trustor.

Figure 2.4: Armenian trustees' expected transfers upon sending different signals



Notes: The figure shows the amount of transfers Armenian trustees expect to receive from Georgian trustors (left panel) and Armenian trustors (right panel) upon sending a Georgian name signal (blue), an Armenian name signal (red), and no name signal (green).

Table 2.1: Number of participants by treatments

Treatment	Role		
	Trustors	Trustees	Total
G-G	105	105	210
G-A	82	82	164
G-A Signal	112	112	224
A-A Signal	80	80	160
Total	379	379	758

Table 2.2: Extent of Georgian trustors' discrimination and beliefs without signaling

	Trustors' transfers		Trustors' beliefs about trustees' back transfers	
	(1)	(2)	(3)	(4)
Treatment G-A	-1.208***	-1.273***	-0.829***	-0.651**
	(0.291)	*(0.225)*	*(0.205)*	*(0.221)*
Trustors-transfer dummies	n.a.	n.a.	Yes	Yes
Control mean (G-G)	5.171		5.648	
Covariates	No	Yes	No	Yes
Observations	187	186	187	186
R^2	0.095	0.150	0.662	0.691

Notes: OLS regressions. Dependent variable: Col. (1)-(2): Transfers from Georgian trustors; col. (3)-(4): Georgian trustors' beliefs about trustees' transfers. Control mean: mean of the outcome variable in treatment *G-G*. Covariates: gender, age, number of siblings, risk tolerance, and patience. Robust standard errors clustered at the class-level in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 2.3: Trustees' back transfers and beliefs without signaling

	Trustees' back transfers		Trustees' beliefs about trustors' transfers	
	(1)	(2)	(3)	(4)
Treatment G-A	-0.512*	-0.773**	-1.785**	-2.279**
	(0.262)	*(0.279)*	*(0.645)*	*(0.714)*
Trustors-transfer dummies	Yes	Yes	n.a.	n.a.
Control mean (G-G)	5.724		5.114	
Covariates	No	Yes	No	Yes
Observations	187	185	187	185
R^2	0.594	0.621	0.106	0.143

Notes: OLS regressions. Dependent variable: Col. (1)-(2): Trustees' actual back transfers; col. (3)-(4): trustees' beliefs about trustors' transfers. Control mean: mean of the outcome variable in treatment *G-G*. Covariates: gender, age, number of siblings, risk tolerance, and patience. Robust standard errors clustered at the class-level in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 2.4: Name signaling behavior

	G-A Signal		A-A Signal		Difference
	(1)	(2)	(3)	(4)	(5)
Signal	#	%	#	%	p-values
Armenian name	63	56.25%	79	98.75%	0.000
Georgian name	48	42.86%	0	0.00%	0.000
No signal	1	0.89%	1	1.25%	0.810
Total	112	100%	80	100%	

Notes: The table depicts the signals sent by Armenian trustees to Georgian and Armenian trustors. The figures represent the absolute and relative numbers of Armenian trustees who signal Armenian names (truthfully), Georgian names (untruthfully), or send no signal. Col. (1)-(2): Georgian trustors; col. (3)-(4): Armenian trustors. P-values in column 5 stem from Chi-squared tests.

Table 2.5: What predicts the signaling behavior?

	Signaling Georgian name				
	(1)	(2)	(3)	(4)	(5)
Pride about Armenian ethnicity[a]	0.006	0.060	0.062	0.009	-0.001
	(0.076)	(0.086)	(0.083)	(0.062)	(0.064)
Importance of ethnic markers[a]		-0.203**	-0.223**	-0.136*	-0.144*
		(0.086)	(0.088)	(0.077)	(0.076)
Expected transfers when …					
… signaling Georgian name			0.055*	0.056**	0.062**
			(0.031)	(0.024)	(0.025)
… signaling Armenian name			-0.032	-0.006	-0.026
			(0.030)	(0.025)	(0.028)
… sending no signal			-0.029	-0.040*	-0.037*
			(0.025)	(0.021)	(0.022)
Ingroup attachment					
Share of Armenian friends				0.553***	0.513***
				(0.150)	(0.151)
Hypothetical ingroup allocation[c]				0.105***	0.102***
				(0.018)	(0.019)
Female					-0.003
					(0.081)
Age					0.038
					(0.027)
Siblings					-0.013
					(0.055)
Risk tolerance					-0.030**
					(0.012)
Patience					-0.012
					(0.009)
Constant	0.399	1.046**	1.017*	-0.257	-0.342
	(0.359)	(0.492)	(0.522)	(0.471)	(0.559)
Observations	112	112	112	111	111
R^2	0.000	0.046	0.078	0.374	0.434

Notes: Linear probability models. Sample: Armenian trustees in treatment *G-A Signal*. Dependent variable: categorical variable coded 1 if subject signals a Georgian name, 0 otherwise. Robust standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

[a] Pride about Armenian ethnicity: response to the following question: "How proud are you to be Armenian?" Answer categories ranged from 1="not at all proud" to 5="very proud".

[b] *Importance of ethnic markers*: average response to the following two questions: "Some people say that the following things are important for being truly Armenian. Others say they are not important. How important do you think it is to be able to speak Armenian? How important do you think it is to have an Armenian name?" Answer categories ranged from 1="very unimportant" to 5="very important".

[c] *Hypothetical ingroup allocation*: "Please consider the following situation: You have to decide how to split 10 Lari between two strangers. One stranger is Georgian, the other is Armenian. How would you split the money?"

Table 2.6: Georgian trustors' transfers with and without signaling

	Trustors' transfers	
	(1)	(2)
Treatment G-A	-1.208***	-1.271***
	(0.285)	*(0.229)*
Treatment G-A Signal	-0.734***	-0.766***
	(0.233)	*(0.218)*
Control mean (G-G)	5.171	
Covariates	No	Yes
Observations	299	297
R^2	0.063	0.110
Wald-Test		
H_0: $\beta_{G\text{-}A} - \beta_{G\text{-}A\ Signal}=0$	-0.474*	-0.505**

Notes: OLS regressions. Dependent variable: Transfers from Georgian trustors to trustees. Control mean: mean of the outcome variable in treatment *G-G*. Covariates: gender, age, number of siblings, risk tolerance, and patience. Robust standard errors clustered at the class-level in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 2.7: Georgian trustors' transfers, expected back transfers and Armenian trustees' actual back transfers with signaling

Treatment *G-A Signal*

	Trustors' transfers		Trustors' beliefs about trustees' back transfers		Trustees' back transfers	
	(1)	(2)	(3)	(4)	(5)	(6)
Signal Georgian name (=1)	1.312***	1.197***	1.033***	1.095***	0.108	0.0210
	(0.346)	*(0.357)*	*(0.347)*	*(0.368)*	*(0.473)*	*(0.470)*
Trustors-transfer dummies	n.a.	n.a.	Yes	Yes	Yes	Yes
Control mean (not signal Georgian)	3.875		3.656		3.272	
Covariates	No	Yes	No	Yes	No	Yes
Observations	112	111	112	111	112	112
R^2	0.116	0.140	0.678	0.683	0.615	0.652

Notes: OLS regressions. Sample: treatment *G-A Signal*. Dependent variable: Col. (1)-(2): Transfers from Georgian trustors to trustees; col. (3)-(4): Georgian trustors' beliefs about trustees' transfers; col. (5)-(6): trustees' actual back transfers. Independent variable: coded 1 if Armenian trustee signals a Georgian name, 0 otherwise. Control mean: mean of the outcome variable without signaling a Georgian name. Covariates: gender, age, number of siblings, risk tolerance, and patience. Robust standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 2.8: Effects of signals on Georgian trustors' and Armenian trustees' profits

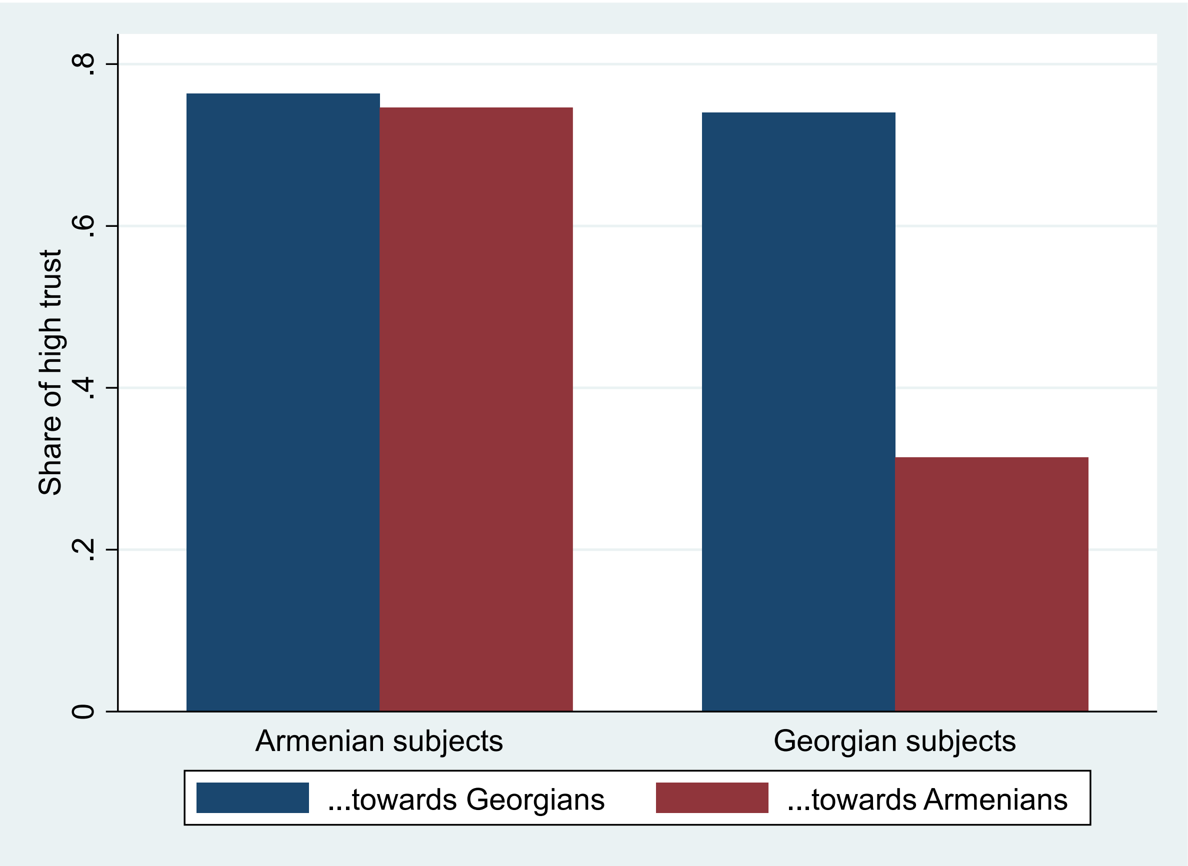
	Georgian trustors' profits		Armenian trustees' profit	
	(1)	(2)	(3)	(4)
Signal Georgian name (=1)	0.365	0.358	2.260***	2.287***
	(0.436)	(0.449)	(0.741)	(0.779)
Control mean (not signal Georgian)	9.531		8.219	
Covariates	No	Yes	No	Yes
Observations	112	111	112	111
R^2	0.006	0.047	0.078	0.112

Notes: OLS regressions. Sample: treatment *G-A Signal*. Dependent variable: Col. (1)-(2): Georgian trustors' profits; col. (3)-(4): Armenian trustees' profits. Independent variable: coded 1 if Armenian trustee signals a Georgian name, 0 otherwise. Control mean: mean profits without signaling a Georgian name. Covariates: gender, age, number of siblings, risk tolerance, and patience. Robust standard errors in parentheses. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Appendix B

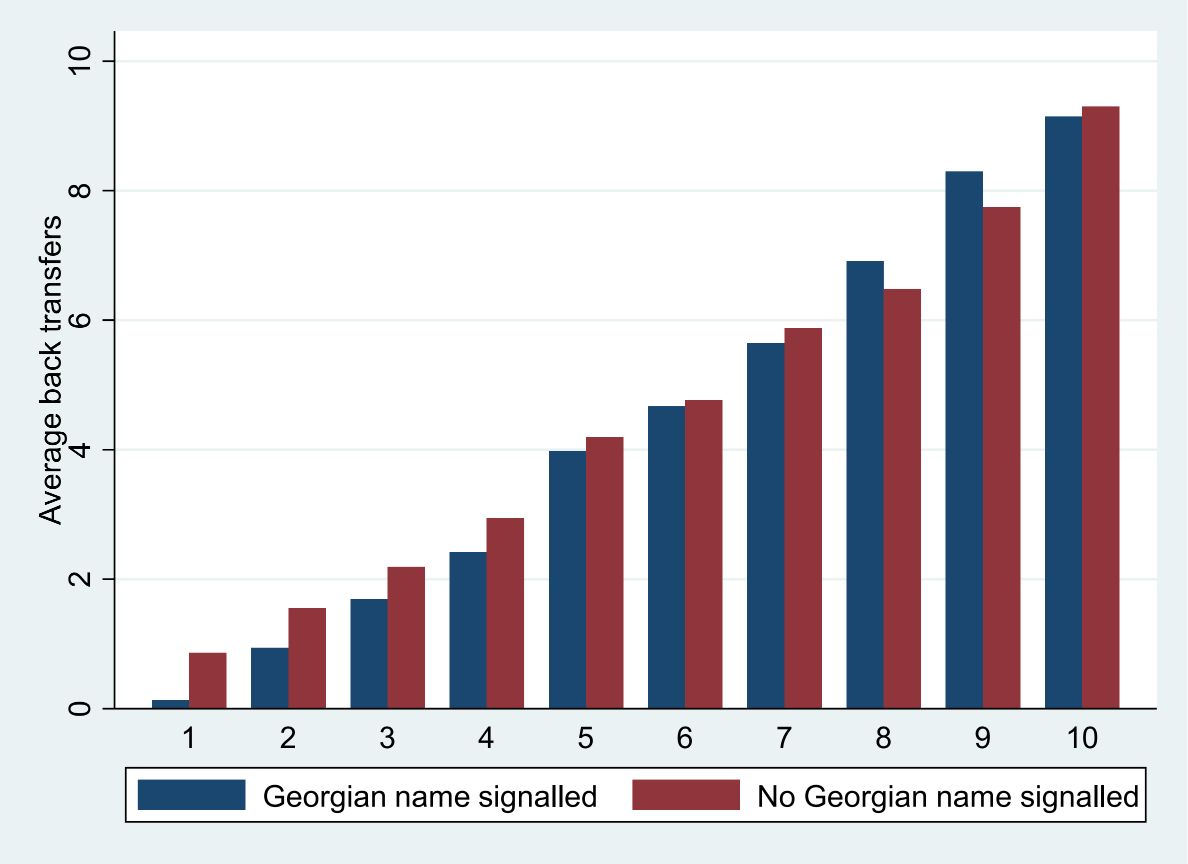
B1. Supplementary Figures and Tables

Figure B1.1: General trust within and between ethnicities in our sample



Notes: Survey questions: „Generally speaking, how much do you trust [Georgians/Armenians]". Answer categories: "No trust at all", "Little trust", "Quite a bit of trust", "A lot of trust". The figure depicts the share of subjects who trust a lot or a bit.

Figure B1.2: Armenian' trustees' back transfers with signaling



Notes: The figure shows actual back transfers by Armenian trustees who did and did not signal a Georgian name (blue bars and red bars, respectively) in treatment *G-A Signal*. Each trustee reported a back-transfer decision for each possible trustor transfer (strategy method).

Table B1.1: Ethnic groups in the country of Georgia (1989-2014)

			Armenians		Azeris	
	Census	Total pop.	Total	% of pop.	Total	% of pop.
	(1)	(2)	(3)	(4)	(5)	(6)
Georgia	1989	5,400,841	437,211	8,1%	307,512	5,69%
	2002	4,371,535	248,929	5,69%	284,761	6.51%
	2014	3,713,804	168,102	4.53%	233,024	6.27%
Tbilisi	1989	1,246,936	150,138	12,04%	17,986	1,44%
	2002	1,081,679	82,586	7.63%	10,942	1,01%
	2014	1,108,717	53,409	4.82%	15,187	1.37%

Notes: Data source: National Statistics Office of Georgia, https://www.geostat.ge/en/modules/categories/316/population-and-demography [accessed 5 June 2019].

Table B1.2: Sample message space of Armenian trustee (treatments *G-A signal* and *A-A signal*)

Option 1	Option 2	Option 3
"My first name is among the names listed below Armen Samvel Artur Karen Levon Armine Susanna Gaiane Ruzanna Karine"	"My first name is among the names listed below Daviti Giorgi Leqso Nikoloz Luka Mariam Nino Salome Kato Tamar"	Send no message

Notes: "Option 1" represents the truthful (untruthful) message of signaling Armenian (Georgian) ethnicity. "Option 1" contains the subjects' true name. The names in the lists were selected with respect to their frequency in our subject pool.

Table B1.3: Summary statistics and balancing tests

	Georgian subjects				Armenian subjects			
	Mean	Difference			Mean	Difference		
	G-G	**G**-A	**G**-A Signal	G-**G**	G-**A**	G-**A** Signal	A-**A** Signal	**A**-A Signal
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	TRUSTORS			TRUSTEES	TRUSTEES			TRUSTORS
Female	0.429	-0.051	-0.009	-0.190***	0.500	0.054	0.062	-0.025
Age	13.733	-0.050	-0.211	-0.848***	13.829	-0.017	-0.017	0.614***
Siblings	1.533	0.076	-0.060	-0.067	1.439	-0.037	-0.039	0.048
% of Georgian friends	0.945	0.003	-0.004	-0.020	0.212	0.002	-0.046	-0.048
% of Armenian friends	0.005	0.005	0.004	0.011	0.727	0.029	0.031	0.002
Risk tolerance	6.952	0.157	0.021	-1.362***	7.200	-0.352	-0.275	-0.150
Patience	6.192	0.503	0.584	0.008	5.232	-0.357	-0.119	1.381**
Understood instructions	9.419	0.203	0.135	0.286**	9.646	0.059	0.166	0.129
Trusts in receiving presents	0.990	0.010	0.010	0.010	1.000	0.000	0.000	0.000
Likes incentives	1.000	0.000	0.000	0.000	1.000	0.000	0.000	0.000
Observations	105	82	112	105	82	112	80	80

Notes: "Difference" displays the difference in means between the reference groups (trustors in treatment *G-G* for Georgian subjects, see column 1; trustees in treatment *G-A* for Armenian subjects, see column 5) and the groups. **Highlighted** letters indicate the role (trustor or trustee) which is represented in the respective column. Significance levels of "Difference" stem from linear regressions of the respective background variable on treatment dummies. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table B1.4: Georgian trustors' beliefs without signaling, by trustor's transfers

	Trustors' beliefs about trustees' back transfers	
	(1)	(2)
Treatment G-A	0.747	1.034
	(0.553)	(0.583)
Transfer	1.281***	1.323***
	(0.0711)	(0.0631)
*Treatment G-A*Transfer*	-0.364**	-0.402**
	(0.150)	(0.156)
Covariates	No	Yes
Observations	187	186
R^2	0.651	0.674
Treatment effect for (Wald tests):		
Trustor's transfer = 0	0.747	1.034
Trustor's transfer = 1	0.383	0.632
Trustor's transfer = 2	0.019	0.23
Trustor's transfer = 3	-0.345*	-0.172
Trustor's transfer = 4	-0.709***	-0.574**
Trustor's transfer = 5	-1.073***	-0.976***
Trustor's transfer = 6	-1.437***	-1.378***
Trustor's transfer = 7	-1.801***	-1.78**
Trustor's transfer = 8	-2.165**	-2.182**
Trustor's transfer = 9	-2.529**	-2.584**
Trustor's transfer = 10	-2.893**	-2.986**

Notes: OLS regressions. Dependent variable: Georgian trustors' beliefs about trustees' transfers. The Wald tests test H_0: $\beta_{\text{Treatment G-A}} + \beta_{\text{Treatment G-A*Transfer}} = 0$. Covariates: gender, age, number of siblings, risk tolerance, and patience. Robust standard errors clustered at the class-level in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table B1.5: Trustees' back transfers (strategy-method averages)

	Trustees' back-transfers			
	Treatments *G-G/G-A*		Treatment *G-A Signal*	
	(1)	(2)	(3)	(4)
Treatment G-A	-1.137**	-1.223***		
	(0.398)	*(0.319)*		
Signal Georgian name (=1)			-0.208	-0.309
			(0.432)	*(0.445)*
Control mean (G-G)	5.829		n.a.	
Control mean (Not signal Georgian)	n.a.		4.589	
Covariates	No	Yes	No	Yes
Observations	187	185	112	112
R^2	0.055	0.118	0.002	0.074

Notes: OLS regressions. Dependent variable: Trustees' back transfers, averaged across each possible trustor's transfer (strategy-method elicitation). Control mean: Col. (1)-(2): mean back-transfer in treatment *G-G*; col. (3)-(4): mean back-transfer without signaling a Georgian name. Covariates: gender, age, number of siblings, risk tolerance, and patience. Robust standard errors clustered at the class-level in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table B1.6: Armenian trustees' expected transfers, by signal

	Expected transfers after sending …					
	Armenian name		Georgian name		No signal	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment A-A Signal	2.859***	2.868***	-1.652***	-1.640***	0.114	0.110
	(0.393)	*(0.187)*	*(0.357)*	*(0.335)*	*(0.232)*	*(0.216)*
Control mean (G-A Signal)	3.116		5.339		3.098	
Covariates	No	Yes	No	Yes	No	Yes
Observations	192	192	192	192	192	192
R^2	0.290	0.356	0.133	0.148	0.001	0.043

Notes: OLS regressions. Dependent variable: Col. (1)-(2): trustees' beliefs about trustors' transfers when signaling an Armenian name; col. (3)-(4): trustees' belief about trustors' transfers when signaling a Georgian name; col. (5)-(6): trustees' belief about trustors' transfers when sending no signal. Control mean: mean of the outcome variables in treatment *G-G Signal*. Covariates: gender, age, number of siblings, risk tolerance, and patience. Robust standard errors clustered at the class-level in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table B1.7: Beliefs about Armenian trustees' ethnicity with signaling

	Beliefs about trustees' ethnicity			
	Georgian trustors' beliefs		Armenian trustees' second-order beliefs	
	Armenian	Georgian	Armenian	Georgian
Treatment	(1)	(2)	(3)	(4)
G-A Signal	56.25%	43.75%	58.33%	41.67%
Armenian name signaled	100.00%	0.00%	100.00%	0.00%
Georgian name signaled	0.00%	100.00%	0.00%	100.00%
A-A Signal	100.00%	0.00%	100.00%	0.00%
Armenian name signaled	100.00%	0.00%	100.00%	0.00%

Notes: Col. (1)-(2): Trustors' beliefs about Armenian trustees' ethnicity, by treatment and name signal received; col (3)-(4): Armenian trustees' second-order beliefs about trustors' beliefs about their ethnicity. Note that the following categories are not shown because of their very low numbers of observations: no signal in treatment *G-A* Signal (N=1), Georgian name signal in treatment *A-A Signal* (N=0), and no signal in Treatment *A-A Signal* (N=1).

B2. Additional Background on the Country of Georgia

Armenians and Azeris are the largest ethnic minority groups in Georgia. As Appendix Table B1.1 shows, they accounted for 4.5 percent and 6.3 percent of the entire country's population in the 2014 census, and for 4.8 percent and 1.4 percent of the population in Tbilisi. Note that the number of Armenians in the country, and the city, has been decreasing sharply since 1989 (see Appendix Table B1.1).

There were two major waves of Armenian immigration to Georgia in the past two millennia. The first wave was initiated by Georgian kings who encouraged Armenians to populate remote areas and towns in Georgia in the fifth, eleventh and eighteenth century to increase the Christian population after periods of Arab and Persian dominance. The second wave was a result of the Russo-Turkish wars, when Armenians migrated to Georgia's southern region of Meskhet-Javakheti and Tbilisi between 1828 and 1915. As a result, the Armenian population in Tbilisi increased to 125,000 in the beginning of the twentieth century (see Jones 1996). After the collapse of the Ottoman and Russian empires in the wake of World War I, Armenia and Georgia were independent states before becoming part of the Soviet Union from 1921 to 1991. The period of independence was not peaceful for either country. Conflicts between them culminated in the Armenian-Georgian war in December 1918 when Armenia tried to capture Georgian territories populated by an Armenian majority. The current phase of the Georgian-Armenian relationship started in 1991, when both countries declared independence from the Soviet Union. The collapse of the Soviet Union was generally followed by rising nationalism in the post-soviet countries, exemplified, for instance, by the election of the nationalist party in Georgia in 1990. Reportedly, in the wake of rising nationalism, minorities in Georgia were increasingly considered a threat to national security (Jones 1996).

Today, Armenians in Georgia live mostly in Tbilisi and the Javakheti region in the country's south. Georgians and Armenians are both Christians, though Georgians are Orthodox, while Armenians generally belong to the Armenian Apostolic Church. They often differ in appearance, although this is not always apparent. Monthly average income from hired full-time employment is 747 Georgian Lari (app. 311 USD; 2015 exchange rate) among Georgians and 560 Georgian Lari (app. 233 USD; 2015 exchange rate) among Armenians living in Georgia. The university completion rate is 39 percent among Georgians and 31 percent among Armenians (bachelor or master degree), and the unemployment rate is 11.6 percent among Georgians and 10 percent among Armenians.[1] The Armenian language differs widely from the Georgian language, but over 96 percent of the Armenian minority in Tbilisi are fluent in Georgian. This is in contrast to the Armenian minority in the Javakheti region, where only 25 percent know Georgian well (Osepashvili, 2013). The Armenian minority in Tbilisi is concentrated in the central districts of the city, but they are not segregated from ethnic Georgians. Tbilisi has a total of 294 schools, which are segregated along ethnic lines. Most of the schools are Georgian (and cater to Georgian children), and a small minority of ten schools are Armenian or Russian (and cater to Armenian children). The number of Armenian schools in Tbilisi declined markedly from 60 schools in 2005 to only 10 in 2019 (Ministry of Education, Science, Culture and Sports of Georgia, http://mes.gov.ge/ [accessed 5 June, 2019]). In Georgia, each school provides primary, lower secondary and upper secondary education. Except for a small set of private schools (where the language of instruction is English or German, for instance), the language of instruction in all public schools is Georgian.

The relationship between Georgians and the Armenian minority has improved over the past years. In 1995, the Georgian government gradually began to build democratic institutions and promote equal rights for all citizens irrespective of their ethnicity. Still, minorities remain underrepresented in Georgia's political life, and Georgian politicians are often discredited by allegations that they have Armenian origins (Democracy & Freedom Watch, 18 October 2015, https://dfwatch.net/unchallenged-stereotypes-blight-georgian-armenian-relations-38678 [accessed 7 June 2019]). Negative perceptions and mistrust towards Armenians still prevail in Georgian society today (e.g, Osepashvili 2013).

[1] The State Department for Statistics of Georgia. Integrated Household Survey Databases 2015. Dataset downloaded from https://www.geostat.ge/en/modules/categories/128/databases-of-2009-2016-integrated-household-survey-and-2017-households-income-and-expenditure-survey on 2 April 2019.

B3. Experimental Instructions and Post-Experiment Questionnaire[2]

Treatment G-G/G-A: Trustor

Welcome to our game and thank you for participating. My name is XXX and I come from Charles University. The game we will play today will take about one hour (60 min) in total and you can earn money in the game. You are asked to make choices during the game and the amount of money you earn is influenced by your own decisions. For this reason, it is very important that you properly understand the rules. Please raise your hand if you have any questions. We will come to your desk to answer your questions privately. It is very important that you do not talk to your neighbor or any other participant during the whole game. (*The first-time persons don't adhere to this rule, announce that you will deduce one token from the participant for each warning.*) In today's game you can earn these tokens. Each token is worth 0.5 GEL, so two tokens are worth one GEL. Here is what you can do with the tokens: Later, you will receive a gift voucher for the amount of the tokens which you earn in this game. The gift voucher is valid for three months and you can use it to buy office supplies (e.g., pencils, pens, notebooks). *Show example items. Importantly, do not comment on what the prices of single items are* - experimenter shows example items which can be bought with the voucher, such as pencils and pens. You can buy these items with the tokens you earn. The more tokens you earn, the more presents you can buy. You will receive the vouchers equivalent to the tokens you earn in this game in a sealed envelope labeled with your anonymized ID-code in one week. I will explain your anonymized ID-code later. For the delivery of your vouchers, we will re-visit your school in one week and hand over the envelopes personally. After this you can visit the office supply shop and buy the supplies you like. In the case that you are not present when we re-visit you, we will come again until we find you. You can therefore be assured that you really will receive the vouchers from today's game and be able to buy presents with it.
Do you have any questions so far? (*If questions come up, answer privately at the desk of the student.*)

In addition to the presents which you will receive later for the tokens earned in the game, you will also receive a present as a Thank You for participating today, right after the game is finished. Therefore, I brought a voucher worth of two GEL.

All decisions in this game are, of course, anonymous. Nobody can connect the decisions you make with you as a person. This is possible because we use anonymized ID-codes. I will now show you how you can create your anonymized ID-code. Take the sheet "Instruction for ID-code" and build your ID-code. It is very important that you add your ID-code to all sheets you fill out during the game. Only in this way can we guarantee that you will receive the correct payment.

You play this game together with another randomly selected student from another school in Tbilisi who attends the same grade as you. It may be a girl or a boy. You don't know who exactly you are playing with, but it is important to remember that the student attends another school in Tbilisi. That student can also earn tokens in the game which he or she can exchange for presents in the experimental shop. There are two different roles in this game. The role of Student A (this is your role) and the role of Student B (this is the role of the student you are playing with). Next, I will explain the game in detail. Do you have any questions so far? (*If questions come up, answer privately at the student's desk.*)

The game works as follows:
At the beginning of the game you will receive 10 tokens. Student B will not receive any tokens. The game has two steps.
In the **first step**, you are asked to decide whether you wish to send any amount of the 10 tokens to the student you are matched with and if so, how many. You can send any amount from 0 to 10. We will triple the amount you send and give it to Student B. That is, for every token that you send, Student B will receive 3 tokens.
In the **second step** we will ask Student B to decide if he or she wants to return any of the tokens he or she received (three times what you sent); and if so, how many. This amount will not be tripled. After the second step, the game is concluded.

[2] These instructions were translated from Georgian.

I brought a poster which illustrates the game (*hang poster on whiteboard/wall so that everybody can see it*). You also find the illustration on your desk. You are Student A (*point to Student A on poster*) and the other student is Student B (*point to student B on poster*).
(*Repeat instructions and point to the relevant parts on the upper part of the poster.*)
Do you have any questions so far? (*If questions come up, answer privately at the student's desk.*)
This is how you calculate the earnings of the game (*point to relevant parts on the lower part of the poster*):
Your earnings: 10 tokens which you receive at the beginning MINUS the number of tokens which you send to Student B (if any) PLUS the number of tokens which Student B returns to you.
Earnings of Student B: The number of tokens which you send to Student B TIMES THREE minus the number of tokens which Student B returns to you.
Let's make some examples now about how the earnings are calculated. Please complete the quiz which I am going to handout right now (*hand out quiz*). Please answer each question. When you have finished, please turn the sheet over and raise your hand. I will then come and check whether you correctly completed the quiz (*Let participants fill out the quiz; if a participant indicates s/he is finished, go to his/her desk and check (i) whether the ID-code has been entered, and (ii) whether the answers are correct. If the answers are correct: collect the answer sheet; if an answer is not correct, go through the example together with the participant until she understands the game).* Thank you for completing the quiz. Note that these were only examples on how to calculate earnings. These examples do not tell you, of course, which decisions you should take in the game or how the other student might decide.
Do you have any questions so far? (*If questions come up, answer privately at the student's desk.*)

<u>Information on Student B:</u>
Before you play the game, we want to inform you about the first name of the student you are matched with. The name of Student B is among the list of names which we hand out now (*hand out list of names*). Please have a close look at the list and read each of the names in the list quietly. Please read the list of names carefully now and raise your hand when you are finished (*individuals who finished reading are instructed to wait until the game continues*).
Similarly, Student B you are matched with is informed that you have a Georgian first name. We don't tell Student B your exact name, however.
Do you have any questions so far? (*If questions come up, answer privately at the student's desk.*)

<u>Decision of Student A:</u>
Now I ask you to make your decision about how many of your 10 tokens you wish to send to Player B. Note that your decision is anonymous and don't forget to put your ID-Code on the decision sheet (*hand out decision sheet*). When you have taken your decision, turn the decision sheet over and wait for further instructions. *At this stage, it is very important that subjects don't communicate. After decisions have been made, collect decision sheets. At the experimenter's desk, check that (i) each participant made a decision and (ii) that each decision sheet contains an ID-code. Do not check this in front of the participant (anonymity!). If (i) or (ii) is missing, go back to the participant and ask her to fill out the sheet correctly; if the sheet is completed, archive it into the provided folder.*

<u>Beliefs of Student A about Student B:</u>
(*Continue when all decision sheets are collected. Hand out belief sheets.*) Thank you for your decision. I have handed out a new sheet in which I ask you to state your guess about the following question: How many tokens do you think Student B will send you back? If your guess is correct, you will receive **two extra tokens**. If your guess is almost correct (one or two tokens above or below the true number of tokens sent back), you will receive **one extra token**. Thus, the better your guess is, the more likely it is that you receive extra tokens.
Do you have any questions so far? (*If questions come up, answer privately at the student's desk.*)
When you have completed the sheet, turn the sheet over and wait for further instructions. *At this stage, it is very important that subjects don't communicate. After belief sheets are completed, collect belief sheets. At the experimenter's desk, check that (i) each participant completed the belief sheet, (ii) that the answers are readable, and (iii) that each decision sheet contains an ID-code. Do not check this in front of the participant (anonymity!). If (i), (ii), or (iii) does not apply, go back to the participant and ask her to fill out the sheet correctly; if the sheet is completed, archive it into the provided folder.*

<u>Survey and end of session:</u>
(*Continue when all belief sheets are collected. Hand out survey.*) Thank you for your guesses. We are not at the end of the game. I have now handed out a survey which I ask you to complete. Please put your ID-Code on the survey and complete the survey carefully. After you have completed the survey, please come forward to the experimenter's desk

with your completed survey and all remaining sheets which are on your desk. Make sure that your ID-Code is on all sheets. After you hand in the sheets, you will receive a voucher as a Thank You for your participation today.

You will receive the voucher equivalent to the tokens you earned in the game when we re-visit you in one week.

Treatment G-G/G-A: Trustee

Welcome to our game and thank you for participating. My name is XXX and I come from Charles University. The game we will play today will take about one hour (60 min) in total and you can earn money in the game. You are asked to make choices during the game and the amount of money you earn is influenced by your own decisions. For this reason, it is very important that you properly understand the rules. Please raise your hand if you have any questions. We will come to your desk to answer your questions privately. It is very important that you do not talk to your neighbor or any other participant during the whole game. (*The first-time persons don't adhere to this rule, announce that you will deduce one token from the participant for each warning.*) In today's game you can earn these tokens. Each token is worth 0.5 GEL, so two tokens are worth one GEL. Here is what you can do with the tokens: Later, you will receive a gift voucher for the amount of the tokens which you earn in this game. The gift voucher is valid for three months and you can use it to buy office supplies (e.g., pencils, pens, notebooks). *Show example items. Importantly, do not comment on what the prices of single items are* - experimenter shows example items which can be bought with the voucher, such as pencils and pens. You can buy these items with the tokens you earn. The more tokens you earn, the more presents you can buy. You will receive the vouchers equivalent to the tokens you earn in this game in a sealed envelope labeled with your anonymized ID-code in one week. I will explain your anonymized ID-code later. For the delivery of your vouchers, we will re-visit your school in one week and hand over the envelopes personally. After this you can visit the office supply shop and buy the supplies you like. In the case that you are not present when we re-visit you, we will come again until we find you. You can therefore be assured that you really will receive the vouchers from today's game and be able to buy presents with it.
Do you have any questions so far? (*If questions come up, answer privately at the desk of the student.*)

In addition to the presents which you will receive later for the tokens earned in the game, you will also receive a present as a Thank You for participating today, right after the game is finished. Therefore, I brought a voucher worth of two GEL.

All decisions in this game are, of course, anonymous. Nobody can connect the decisions you make with you as a person. This is possible because we use anonymized ID-codes. I will now show you how you can create your anonymized ID-code. Take the sheet "Instruction for ID-code" and build your ID-code. It is very important that you add your ID-code to all sheets you fill out during the game. Only in this way can we guarantee that you will receive the correct payment.

You play this game together with another randomly selected student from another school in Tbilisi who attends the same grade as you. It may be a girl or a boy. You don't know who exactly you are playing with, but it is important to remember that the student attends another school in Tbilisi. That student can also earn tokens in the game which he or she can exchange for presents in the experimental shop. There are two different roles in this game. The role of Student A (this is the role of the student you are playing with) and the role of Student B (this is your role). Next, I will explain the game in detail. Do you have any questions so far? (*If questions come up, answer privately at the student's desk.)*

<u>The game works as follows:</u>
At the beginning of the game Student A will receive 10 tokens. You will not receive any tokens. The game has two steps.
In the **first step**, Student A is asked to decide whether he or she wishes to send any number of the 10 tokens to you and if so, how many. Student A can send any amount from 0 to 10. We will triple the amount Student A sends and give it to you. That is, for every token that Student A sends, you will receive 3 tokens.
In the **second step** we will ask you to decide if you want to return any of the tokens which you received; and if so, how many. This amount will not be tripled. After the second step, the game is concluded.

I brought a poster which illustrates the game (*hang poster on whiteboard/wall so that everybody can see it*). You also find the illustration on your desk. You are Student B (*point to Student B on poster*) and the other student is Student A (*point to student A on poster*).
(*Repeat instructions and point to the relevant parts on the upper part of the poster*)
Do you have any questions so far? (*If questions come up, answer privately at the student's desk.*)
This is how you calculate the earnings of the game (*point to relevant parts on the lower part of the poster*):
Your earnings: The number of tokens which you receive from Student A (i.e., the number of tokens sent by Student B TIMES THREE) minus the number of tokens which you return to Student B.
Earnings of Student A: 10 tokens which he or she receives at the beginning MINUS the number of tokens which he or she sends to you PLUS the number of tokens which you return to Student B.
Let's see some examples now about how the earnings are calculated. Please complete the quiz which I am going to hand out right now (*hand out quiz*). Please answer each question. When you have finished, please turn the sheet over and raise your hand. I will then come and check whether you correctly completed the quiz (*Let participants fill out the quiz; if a participant indicates s/he is finished, go to her/his desk and check (i) whether the ID-code has been entered, and (ii) whether the answers are correct. If the answers are correct: collect the answer sheet; if an answer is not correct, go through the example together with the participant until she understands the game).* Thank you for completing the quiz. Note that these were only examples on how to calculate earnings. These examples do not tell you, of course, which decisions you should take in the game or how the other student might decide.
Do you have any questions so far? (*If questions come up, answer privately at the student's desk.*)

<u>Information on Student A:</u>
Before you play the game, we want to inform you about the first name of the student you are matched with. Student A has a Georgian first name.
Student A you are matched with receives information about your first name through a name list before making his or her decision. The list contains ten first names. One of the names on the list is your first name, the other names are similar Georgian/Armenian (*depending on treatment*) first names. Student A is told that the first name of the student he or she is matched with (this is you) is on the list. However, we don't tell Student A which exact name on the list is yours. We now hand out the list which Player A will receive from us. Please read the list of names carefully now and make sure that your name is on the list. Raise your hand when you are finished (*important: here we need personalized lists; every student needs to receive a list which includes his/her name. If this fails for some reason, tell Players B that the list will be corrected to contain their names; individuals who finished reading are instructed to wait until the game continues*). Do you have any questions so far? (*If questions come up, answer privately at the student's desk.*)

<u>Decision of Student B:</u>
Now I ask you to make your decision about how many tokens you wish to return to Player A. Importantly, we don't know yet how many tokens Student A will send. Therefore, we ask you to tell us how many tokens you would return to Student A for each possible number of tokens which he or she might send you. I will explain this more explicitly on the decision sheet which I hand out now (*hand out decision sheet*). Importantly, do not write anything on the decision sheet before I finished my explanation.
On the decision sheet there are 11 decision lines in total (*Point to column "Decision Line"*). In each decision line, you are asked how many tokens you want to return to Student A in case he or she sends you zero, one, two, three, four, five, six, seven, eight, nine, or all ten of her tokens (*point to each line when saying the numbers*). As an example, look at Decision Line 3. In this case, Student A sends you two of his or her tokens (*show number "2" at decision sheet*). In this case, how many tokens do you receive from Student A (*ask people in the classroom; correct answer is 6*)? Correct, it is 6 tokens because we triple each token sent by Student A. Decision Line 3 therefore asks you how many of these six tokens you want to return (*point at figure "6" in decision row "Decision 3", column 3 of decision sheet*). This is, of course, also the maximum number of tokens you can return. Now look at Decision Line 8. How many tokens does Student A send you in this case? (*correct answer: 7*) Correct, it is seven (*point at respective figure in the decision sheet*). And how many tokens do you receive in this case from Student A? (*correct answer: 21*) Correct, it is

21. Therefore, in Decision Line 8, you are asked how many of 21 tokens you will return (*point at respective figure in third column*). For each possible decision of Student A, you need to decide how many tokens you return to Student A. Do you have any questions so far? (*If questions come up, answer privately at the student's desk.*)
There is one very important question: What Decision Line do we use for calculating your earnings? The rule is as follows: The relevant Decision Line is determined by the decision of Student A. For example, if Student A sends you two tokens, your choice in Decision Line 3 is relevant and earnings are calculated accordingly (*point at respective line in decision sheet*). Your other decisions don't count. As another example, if Student A sends you 7 tokens, what Decision Line is used? (*correct: decision 8*) Correct, your choice in Decision Line 8 is used for calculating earnings, all other decisions are irrelevant.
Do you have any questions so far? (*If questions come up, answer privately at the student's desk.*)
It is important that you provide a choice in all 11 Decision Lines, because you don't know yet how many tokens Student A will send you (i.e., what line will be relevant for calculating earnings). If you don't provide a choice in the Decision Line which is used for calculating earnings, you will receive no payment. Therefore, you need to provide a choice in each Decision Line.
Importantly, Player A does not know your choices when he or she decides how many tokens to send you.
Do you have any questions so far? (*If questions come up, answer privately at the student's desk.*)
If there are no more questions, you can now fill out your decision sheet. Note that your decisions are anonymous and don't forget to put your ID-Code on the decision sheet. Also, don't forget to provide a choice in each Decision Line. Please write as clearly as possible, since you receive no earnings if we can't read what your choices are. When you have taken your decisions, turn the decision sheet over and wait for further instructions. (*At this stage, it is very important that subjects don't communicate. After decisions have been made, collect decision sheets. At the experimenter's desk, check that (i) each participant made a decision in each decision line, (ii) that all decisions are readable, and (iii) that each decision sheet contains an ID-code. Do not check this in front of the participant (anonymity!). If (i), (ii) or (iii) does not apply, go back to the participant and ask her to fill out the sheet correctly; if the sheet is completed, archive it into the provided folder.*)

Beliefs of Student B about Student A:
(*Continue when all decision sheets are collected. Hand out belief sheets.*) Thank you for your decision. I have now handed out a new sheet in which I ask you to state your guess about the following question: How many of Student A's 10 tokens do you think will Student A send you? Please state your guess as the number of tokens before they are tripled! If your guess is correct, you will receive **two extra tokens**. If your guess is almost correct (one or two tokens above or below the true number of tokens sent back), you will receive **one extra token**. Thus, the better your guess is, the more likely it is that you will receive extra tokens.
Do you have any questions so far? (*If questions come up, answer privately at the student's desk*).
When you completed the sheet, turn the sheet over and wait for further instructions. (*At this stage, it is very important that subjects don't communicate. After belief sheets are completed, collect them. At the experimenter's desk, check that (i) each participant completed the belief sheet, (ii) that the answers are readable, and (iii) that each decision sheet contains an ID-code. Do not check this in front of the participant (anonymity!). If (i), (ii), or (iii) does not apply, go back to the participant and ask her to fill out the sheet correctly; if the sheet is completed, archive it into the provided folder.*)

Survey and end of session:
(*Continue when all belief sheets are collected. Hand out survey.*) Thank you for your guesses. We are not at the end of the game. I have now handed out a survey which I ask you to complete. Please put your ID-Code on the survey and complete the survey carefully. After you have completed the survey, please come forward to the experimenter's desk with your completed survey and all remaining sheets which are on your desk. Make sure that your ID-Code is on all sheets. After you hand in the sheets, you will receive a voucher as a Thank You for your participation today.

You will receive the voucher equivalent to the tokens you earned in the game when we re-visit you in one week.

Treatment G-A Signal/A-A Signal: Trustor

Welcome to our game and thank you for participating. My name is XXX and I come from Charles University. The game we will play today will take about one hour (60 min) in total and you can earn money in the game. You are asked to make choices during the game and the amount of money you earn is influenced by your own decisions. For this reason, it is very important that you properly understand the rules. Please raise your hand if you have any questions. We will come to your desk to answer your questions privately. It is very important that you do not talk to your neighbor or any other participant during the whole game. (*The first-time persons don't adhere to this rule, announce that you will deduce one token from the participant for each warning.*) In today's game you can earn these tokens. Each token is worth 0.5 GEL, so two tokens are worth one GEL. Here is what you can do with the tokens: Later, you will receive a gift voucher for the amount of the tokens which you earn in this game. The gift voucher is valid for three months and you can use it to buy office supplies (e.g., pencils, pens, notebooks). *Show example items. Importantly, do not comment on what the prices of single items are* - experimenter shows example items which can be bought with the voucher, such as pencils and pens. You can buy these items with the tokens you earn. The more tokens you earn, the more presents you can buy. You will receive the vouchers equivalent to the tokens you earn in this game in a sealed envelope labeled with your anonymized ID-code in one week. I will explain your anonymized ID-code later. For the delivery of your vouchers, we will re-visit your school in one week and hand over the envelopes personally. After this you can visit the office supply shop and buy the supplies you like. In the case that you are not present when we re-visit you, we will come again until we find you. You can therefore be assured that you really will receive the vouchers from today's game and be able to buy presents with it.
Do you have any questions so far? (*If questions come up, answer privately at the desk of the student.*)

In addition to the presents which you will receive later for the tokens earned in the game, you will also receive a present as a Thank You for participating today, right after the game is finished. Therefore, I brought a voucher worth of two GEL.

All decisions in this game are, of course, anonymous. Nobody can connect the decisions you make with you as a person. This is possible because we use anonymized ID-codes. I will now show you how you can create your anonymized ID-code. Take the sheet "Instruction for ID-code" and build your ID-code. It is very important that you add your ID-code to all sheets you fill out during the game. Only in this way can we guarantee that you will receive the correct payment.

You play this game together with another randomly selected student from another school in Tbilisi who attends the same grade as you. It may be a girl or a boy. You don't know who exactly you are playing with, but it is important to remember that the student attends another school in Tbilisi. That student can also earn tokens in the game which he or she can exchange for presents in the experimental shop. There are two different roles in this game. The role of Student A (this is your role) and the role of Student B (this is the role of the student you are playing with). Next, I will explain the game in detail. Do you have any questions so far? (*If questions come up, answer privately at the student's desk.*)

<u>The game works as follows:</u>
At the beginning of the game you will receive 10 tokens. Student B will not receive any tokens. The game has two steps.
In the **first step**, you are asked to decide whether you wish to send any amount of the 10 tokens to the student you are matched with and if so, how many. You can send any amount from 0 to 10. We will triple the amount you send and give it to Student B. That is, for every token that you send, Student B will receive 3 tokens.
In the **second step** we will ask Student B to decide if he or she wants to return any of the tokens he or she received (three times what you sent); and if so, how many. This amount will not be tripled. After the second step, the game is concluded.
I brought a poster which illustrates the game (*hang poster on whiteboard/wall so that everybody can see it*). You also find the illustration on your desk. You are Student A (*point to Student A on poster*) and the other student is Student B (*point to student B on poster*).
(*Repeat instructions and point to the relevant parts on the upper part of the poster.*)
Do you have any questions so far? (*If questions come up, answer privately at the student's desk*).
This is how you calculate the earnings of the game (*point to relevant parts on the lower part of the poster*):

Your earnings: 10 tokens which you receive at the beginning MINUS the number of tokens which you send to Student B (if any) PLUS the number of tokens which Student B returns to you.
Earnings of Student B: The number of tokens which you send to Student B TIMES THREE minus the number of tokens which Student B returns to you.
Let's see some examples now about how the earnings are calculated. Therefore, please complete the quiz which I am going to hand you out right now (*hand out quiz*). Please answer each question. When you have finished, please turn the sheet and raise your hand. I will then come and check whether you correctly completed the quiz (*Let participants fill out the quiz; if a participant indicates s/he finished, go to his or her desk and check (i) whether the ID-code has been entered, and (ii) whether the answers are correct. If the answers are correct: collect the answer sheet; if an answer is not correct, go through the example together with the participant until she understands the game).* Thank you for completing the quiz. Note that these were only examples on how to calculate earnings. These examples do not tell you, of course, which decisions you should take in the game or how the other student might decide.
Do you have any questions so far? (*If questions come up, answer privately at the student's desk*).

Information on Student B:
Before you play the game, we want to inform you that the student you are matched with had the option to send you a message about his or her first name. We now hand out the message sheets which Student B sent you (*hand out message sheets*). Please first put your ID-Code on the message sheet you received. Have a close look at the message and read through it quietly and carefully. Please raise your hand when you are finished (*individuals who finished reading are instructed to wait until the game continues*).
Student B you are matched with is informed that you have a Georgian first name. We don't tell Student B your exact name, however.
Do you have any questions so far? (*If questions come up, answer privately at the student's desk*).

Decision of Student A:
Now I ask you to make your decision about how many of your 10 tokens you wish to send to Player B. Note that your decision is anonymous and don't forget to put your ID-Code on the decision sheet (*hand out decision sheet*). When you have taken your decision, turn the decision sheet over and wait for further instructions. (*At this stage, it is very important that subjects don't communicate. After decisions have been made, collect decision sheets and message sheet. At the experimenter's desk, check that (i) each participant made a decision and (ii) that each decision sheet contains an ID-code. Do not check this in front of the participant (anonymity!). If (i) or (ii) is missing, go back to the participant and ask her to fill out the sheet correctly; if the sheet is completed, archive it into the provided folder.*)

Beliefs of Student A about Student B:
(*Continue when all decision sheets are collected. Hand out belief sheets.*) Thank you for your decision. I have handed out a new sheet in which I ask you to state your guess about the following question: How many tokens do you think Student B will send you back? If your guess is correct, you will receive **two extra tokens**. If your guess is almost correct (one or two tokens above or below the true number of tokens sent back), you will receive **one extra token**. Thus, the better your guess is, the more likely it is that you receive extra tokens.
Do you have any questions so far? (*If questions come up, answer privately at the student's desk*).
When you have completed the sheet, turn the sheet over and wait for further instructions. (*At this stage, it is very important that subjects don't communicate. After belief sheets are completed, collect them. At the experimenter's desk, check that (i) each participant completed the belief sheet, (ii) that the answers are readable, and (iii) that each decision sheet contains an ID-code. Do not check this in front of the participant (anonymity!). If (i), (ii), or (iii) does not apply, go back to the participant and ask her to fill out the sheet correctly; if the sheet is completed, archive it into the provided folder.*)

Survey and end of session:
(*Continue when all belief sheets are collected. Hand out survey.*) Thank you for your guesses. We are not at the end of the game. I have now handed out a survey which I ask you to complete. Please put your ID-Code on the survey and complete the survey carefully. After you have completed the survey, please come forward to the experimenter's desk with your completed survey and all remaining sheets which are on your desk. Make sure that your ID-Code is on all sheets. After you hand in the sheets, you will receive a voucher as a Thank You for your participation today.

You will receive the voucher equivalent to the tokens you earned in the game when we re-visit you in one week.

Treatment G-A Signal/A-A Signal: Trustee

All decisions in this game are, of course, anonymous. Nobody can connect the decisions you made with you as a person. This is possible because we use anonymized ID-codes. I will now show you how you can create your anonymized ID-code. Therefore, take the sheet "Instruction for ID-code" and build your ID-code. It is very important that you add your ID-code to all sheets which you fill out during the game. Only in this way we can guarantee that you receive the correct payment.

You play this game together with another randomly selected student from another school in Tbilisi who attends the same grade as you. It may be a girl or a boy. You don't know who exactly you are playing with, but it is important to remember that the student attends another school in Tbilisi. The student can also earn tokens in the game which he or she can exchange for presents in the experimental shop. There are two different roles in this game. The role of Student A (this is the role of the student you are playing with) and the role of Student B (this is your role). Next, I will explain the game in great detail. Do you have any questions so far? (*If questions come up, answer privately at the student's desk*).

<u>**The game works as follows:**</u>
At the beginning of the game Student A will receive 10 tokens. You will not receive any tokens. The game has two steps.
In the **first step**, Student A is asked to decide whether he or she wishes to send any amount of the 10 tokens to you and if so, how many. Student A can send any amount from 0 to 10. We will triple the amount Student A sends and give it to you. That is, for every token that Student A sends, you will receive 3 tokens.
In the **second step** we will ask you to decide if you want to return any of the tokens which you received; and if so, how many. This amount will not be tripled. After the second step, the game is concluded.
I brought a poster which illustrates the game (*hang poster on whiteboard/wall so that everybody can see it*). You also find the illustration on your desk. You are Student B (*point to Student B on poster*) and the other student is Student A (*point to student A on poster*).
(*Repeat instructions and point to the relevant parts on the upper part of the poster*)
Do you have any questions so far? (*If questions come up, answer privately at the student's desk*).
This is how you calculate the earnings of the game (*point to relevant parts on the lower part of the poster*):
Your earnings: The number of tokens which you receive from Student A (i.e., the number of tokens sent by Student B TIMES THREE) minus the number of tokens which you return to Student B.
Earnings of Student A: 10 tokens which he or she receives at the beginning MINUS the number of tokens which he or she sends to you PLUS the number of tokens which you return to Student B.
Let's see some examples now about how the earnings are calculated. Please complete the quiz which I am going to hand out right now (*hand out quiz*). Please answer each question. When you have finished, please turn the sheet over and raise your hand. I will then come and check whether you correctly completed the quiz (*Let participants fill out the quiz; if a participant indicates s/he is finished, go to his or her desk and check (i) whether the ID-code has been entered, and (ii) whether the answers are correct. If the answers are correct: collect the answer sheet; if an answer is not correct, go through the example together with the participant until she understands the game).* Thank you for completing the quiz. Note that these were only examples on how to calculate earnings. These examples do not tell you, of course, which decisions you should take in the game or how the other student might decide.
Do you have any questions so far? (*If questions come up, answer privately at the student's desk*).

<u>**Information on Student A:**</u>
Before you play the game, we want to inform you about the first name of the student you are matched with. Student A has a Georgian/Armenian (*depending on treatment*) first name.

There is yet one important step in the game. Student A did not take his or her decision yet. You have the option to send Student A you are matched with a message about your first name. Student A will receive this information before he or she makes her decision.
I will explain the messages you can send more explicitly on the message sheet which I hand out now (*hand out decision sheet*). Importantly, do not write anything on the decision sheet before I finished my explanation.
On the message sheet, you have three options. Option 1 is to tell Student A that your name is among the list of names provided in the first column (*show this option*). Please now read the name list of Option 1 carefully (*give some time for reading*). Option 2 is to tell Student A that your name is among the list of names provided in the second column (*show this option*). Please now read the name list of Option 2 carefully (*give some time for reading*). Option 3 is not to send either of the messages.
Importantly, we will only show the Option you selected to Student A before he or she makes her decisions. This is, we will cut out the respective message and give it to him or her. Thus, Student A will, of course, also not see your code! The message you select is the only information Student A will receive about your name. He or she will never know whether the message you sent is correct or incorrect.
Do you have any questions so far? (*If questions come up, answer privately at the student's desk*).
If there are no more questions, you can now fill out your message sheet. Note that your decision is anonymous and don't forget to put your ID-Code on the decision sheet. When you have taken your decision, turn the message sheet and wait for further instructions. (*At this stage, it is very important that subjects don't communicate. After decisions have been made, collect message sheets. At the experimenter's desk, check that (i) each participant ticked one box, and (ii) that each message sheet contains an ID-code. Do not check this in front of the participant (anonymity!). If (i), or (ii) does not apply, go back to the participant and ask her to fill out the sheet correctly; if the sheet is completed, archive it into the provided folder.*)

<u>Decision of Student B:</u>
Now I ask you to make your decision about how many tokens you wish to return to Player A. Importantly, we don't know yet how many tokens Student A will send you. Therefore, we ask you to tell us how many tokens you would return to Student A for each possible number of tokens which he or she might send you. I will explain this more explicitly on the decision sheet which I hand out now (*hand out decision sheet*). Importantly, do not write anything on the decision sheet before I finished my explanation.
On the decision sheet there are 11 decision lines in total (*Point to column "Decision Line"*). In each decision line, you are asked how many tokens you want to return to Student A in case he or she sends you zero, one, two, three, four, five, six, seven, eight, nine, or all ten of her tokens (*point to each line when saying the numbers*). As an example, look at Decision Line 3. In this case, Student A sends you two of his or her tokens (*show number "2" at decision sheet*). In this case, how many tokens do you receive from Student A (*ask people in the classroom; correct answer is 6*)? Correct, it is 6 tokens because we triple each token sent by Student A. Decision Line 3 therefore asks you how many of these six tokens you want to return (*point at figure "6" in decision row "Decision 3", column 3 of decision sheet*). This is, of course, also the maximum number of tokens you can return. Now look at Decision Line 8. How many tokens does Student A send you in this case? (*correct answer: 7*) Correct, it is seven (*point at respective figure in the decision sheet*). And how many tokens do you receive in this case from Student A? (*correct answer: 21*) Correct, it is 21. Therefore, in Decision Line 8, you are asked how many of 21 tokens you will return (*point at respective figure in third column*). For each possible decision of Student A, you need to decide how many tokens you return to Student A.
Do you have any questions so far? (*If questions come up, answer privately at the student's desk*).
There is one very important question: What Decision Line do we use for calculating your earnings? The rule is as follows: The relevant Decision Line is determined by the decision of Student A. For example, if Student A sends you two tokens, your choice in Decision Line 3 is relevant and earnings are calculated accordingly (*point at respective line in decision sheet*). Your other decisions don't count. As another example, if Student A sends you 7 tokens, what Decision Line is used? (*correct: decision 8*) Correct, your choice in Decision Line 8 is used for calculating earnings, all other decisions are irrelevant.
Do you have any questions so far? (*If questions come up, answer privately at the student's desk*).

It is important that you provide a choice in all 11 Decision Lines, because you don't know yet how many tokens Student A will send you (i.e., what line will be relevant for calculating earnings). If you don't provide a choice in the Decision Line which is used for calculating earnings, you will receive no payment. Therefore, you need to provide a choice in each Decision Line.
Importantly, Player A does not know your choices when he or she decides how many tokens to send you.
Do you have any questions so far? (*If questions come up, answer privately at the student's desk*).
If there are no more questions, you can now fill out your decision sheet. Note that your decisions are anonymous and don't forget to put your ID-Code on the decision sheet. Also, don't forget to provide a choice in each Decision Line. Please write as clearly as possible, since you receive no earnings if we can't read what your choices are. When you have made your decisions, turn the decision sheet over and wait for further instructions. (*At this stage, it is very important that subjects don't communicate. After decisions have been made, collect them. At the experimenter's desk, check that (i) each participant made a decision in each decision line, (ii) that all decisions are readable, and (iii) that each decision sheet contains an ID-code. Do not check this in front of the participant (anonymity!). If (i), (ii) or (iii) does not apply, go back to the participant and ask her to fill out the sheet correctly; if the sheet is completed, archive it into the provided folder.*)

Beliefs of Student B about Student A:

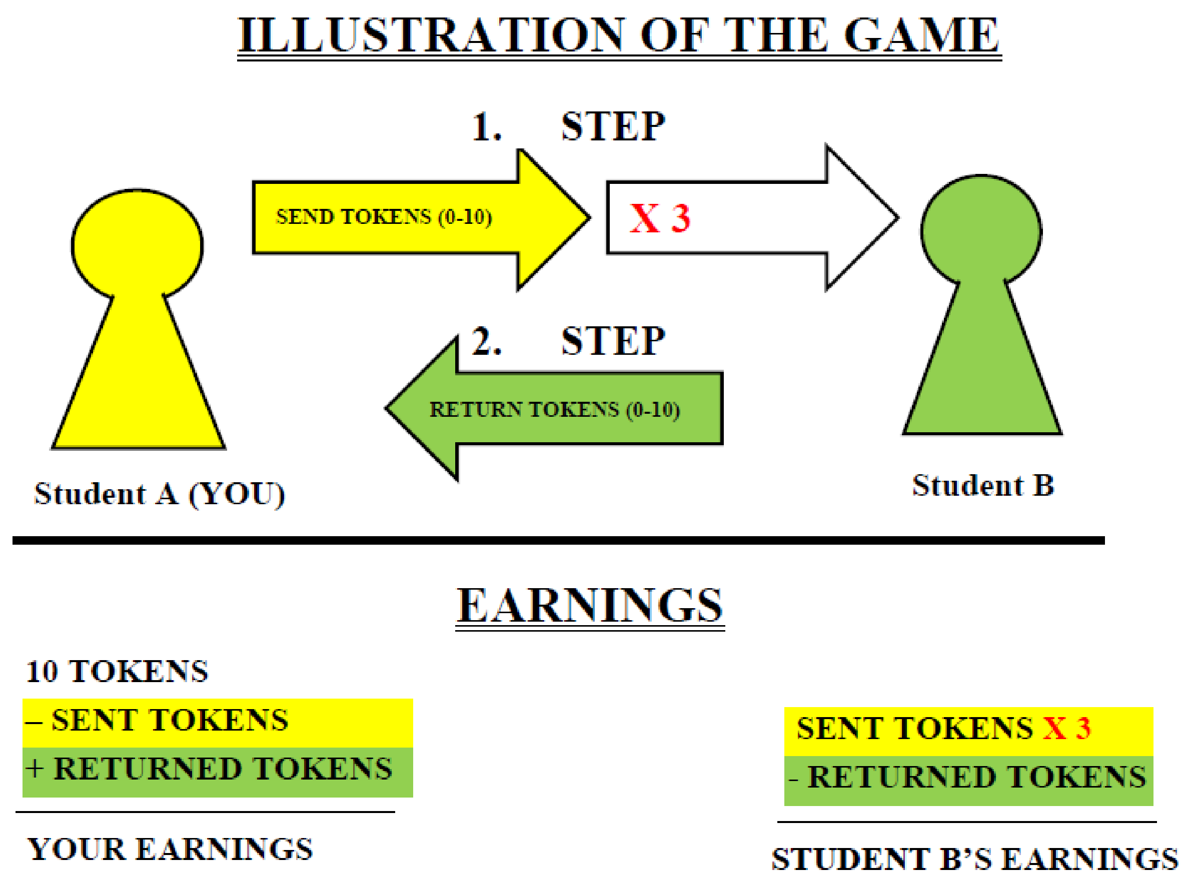
(*Continue when all decision sheets are collected. Hand out belief sheets.*) Thank you for your decision. I have now handed out a new sheet in which I ask you to state your guesses about the following question: How many of Student A's 10 tokens do you think will Student A send you? Please state your guess as the number of tokens before they are tripled! If your guess is correct, you will receive **two extra tokens**. If your guess is almost correct (one or two tokens above or below the true number of tokens sent back), you will receive **one extra token**. Thus, the better your guess is, the more likely it is that you will receive extra tokens.
Do you have any questions so far? (*If questions come up, answer privately at the student's desk*).
We would like to know your guesses for each of the three messages possible messages (Options 1, 2 and 3). On the sheet, please first state the message that you actually sent (*point to respective part of decision sheet*). Then, we would like to known your guesses about how many tokens Student A sends you if you sent message OPTION 1 (*point to first box on the decision sheet*), OPTION 2 (*point to second box on the decision sheet*), and OPTION 3 (*point to the third box on the decision sheet*). Importantly, you need to give your best guess in each of the three boxes, independent of the message you actually sent. When calculating your earnings, we will randomly select one of the three guessing questions to be relevant for your payment. To determine whether your guess is correct in the randomly selected box (and thus, whether you receive extra tokens for your guess), we will compare your guess to the average number of tokens which students in the role of Student A send if they receive the message of Option 1, Option 2, and Option 3, respectively. Thus, it is important that you give your best guess in all three questions.
Do you have any questions so far? (*If questions come up, answer privately at the student's desk*).
When you completed the sheet, turn the sheet over and wait for further instructions. (*At this stage, it is very important that subjects don't communicate. After belief sheets are completed, collect them. At the experimenter's desk, check that (i) each participant completed the belief sheet, (ii) that the answers are readable, and (iii) that each decision sheet contains an ID-code. Do not check this in front of the participant (anonymity!). If (i), (ii), or (iii) does not apply, go back to the participant and ask her to fill out the sheet correctly; if the sheet is completed, archive it into the provided folder.*)

Survey and end of session:

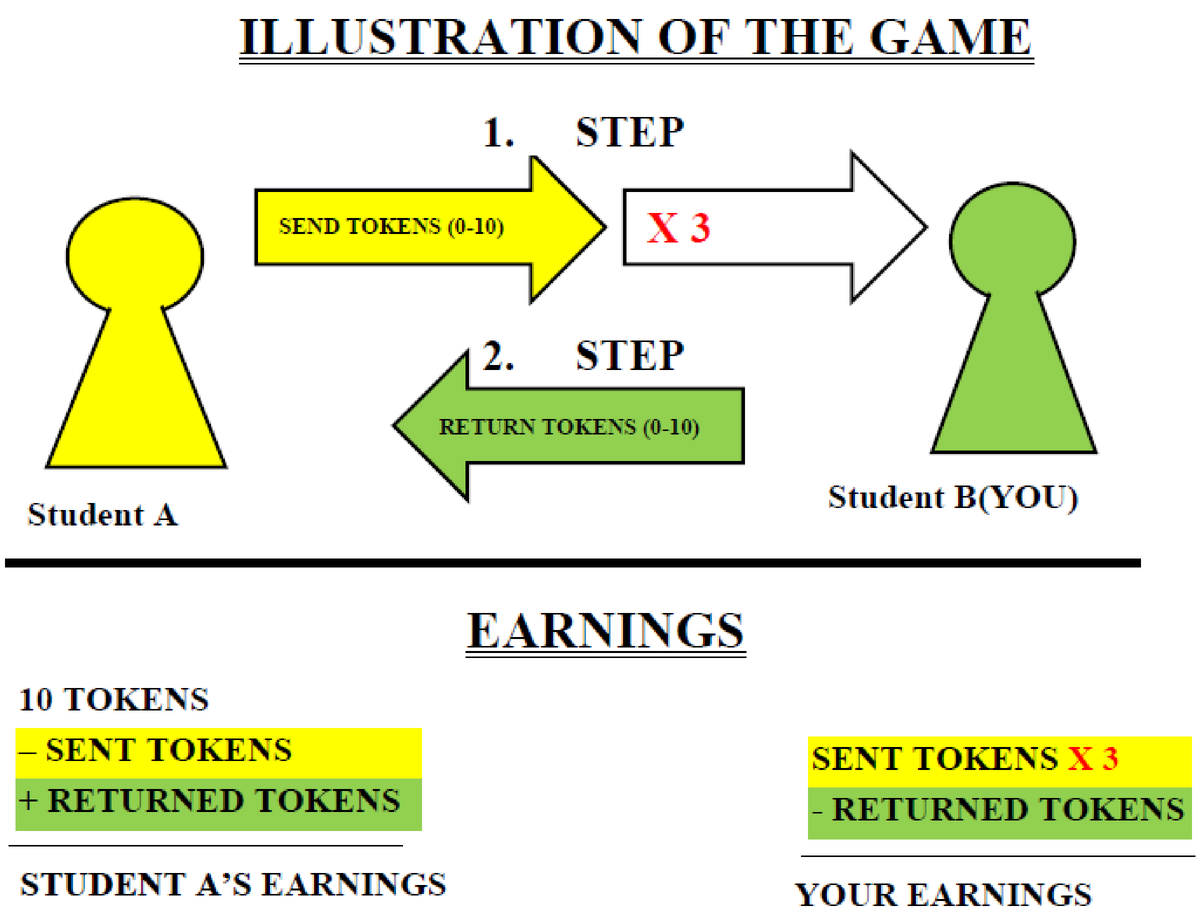
(*Continue when all belief sheets are collected. Hand out survey.*) Thank you for your guesses. We are not at the end of the game. I have now handed out a survey which I ask you to complete. Please put your ID-Code on the survey and complete the survey carefully. After you have completed the survey, please come forward to the experimenter's desk with your completed survey and all remaining sheets which are on your desk. Make sure that your ID-Code is on all sheets. After you hand in the sheets, you will receive a voucher as a Thank You for your participation today.

You will receive the voucher equivalent to the tokens you earned in the game when we re-visit you in one week.

Visual illustration of the trust game: Trustor



Visual illustration of the trust game: Trustee



Post-experiment questionnaire

ID _____ _____ _____ _____ _____ _____ _____
1 2 3 4 5 6 7

Questionnaire	
1.	Your school name: ____________________________
2.	Your class name (for example IX^3) : ____________________________
3.	Your gender: ☐ Male ☐ Female
4.	Your birthdate (DD/MM/YYYY): ________________________
5.1	How many brothers do you have? _________________________
5.2	How many sisters do you have? _________________________
6.1	How many friends do you have? When counting your friends, think of people who you would call your friend and who would call you their friend. **I have ________________ friends**
6.2	How many of them are Georgian? ___________________
6.3	How many of them are Armenian? ___________________
6.4	How many of them have another ethnicity? __________________ What is their ethnicity? __________________
7.	How well did you understand the instructions of Today's game? 0="Did not understand at all" 10="Understood very well" **0** ☐ **1** ☐ **2** ☐ **3** ☐ **4** ☐ **5** ☐ **6** ☐ **7** ☐ **8** ☐ **9** ☐ **10** ☐
8.	We promised you that you will receive the presents from today's game at a later point in time. Do you trust us that you really receive the presents? ☐ No ☐ Yes
9.	Do you like the presents which you can win in the game? ☐ No ☐ Yes
10.1	Remember that you received information about Student A/B you were matched with. What is your best guess about the ethnicity of Student A/B? ☐ I think student A is Georgian ☐ I think student A is Armenian ☐ I think student A is __________
10.2	How sure are you about your guess being correct (close to correct)? "Very unsure" "Very sure" **0** ☐ **1** ☐ **2** ☐ **3** ☐ **4** ☐ **5** ☐ **6** ☐ **7** ☐

11.1	Remember that you had the option to send a message to Student A - only for the second movers in G-A signal and A-A signal. What is your best guess, what does student A/B think about your ethnicity? ☐ I think Student A/B thinks that I am Georgian ☐ I think Student A/B thinks that I am Armenian ☐ I think Student A/B thinks that I am ______________
11.2	How sure are you about your guess being correct (close to correct)? "Very unsure" "Very sure" **0** ☐ **1** ☐ **2** ☐ **3** ☐ **4** ☐ **5** ☐ **6** ☐ **7** ☐
12.	What is the ethnicity of your parents? ☐ Georgian ☐ Armenian ☐ Other __________________
13.	How proud are you to be Georgian/Armenian (Armenian - if the player is Armenian)? ☐ Not at all proud ☐ Not very proud ☐ Neither nor ☐ Quite proud ☐ Very proud
14.	Some people say that the following things are important for being truly Georgian. Others say they are not important. *How important do you think it is to be able to speak Georgian?* ☐ Very important ☐ Important ☐ Neither important nor unimportant ☐ Unimportant ☐ Very unimportant *How important do you think is to have a Georgian name?* ☐ Very important ☐ Important ☐ Neither important nor unimportant ☐ Unimportant ☐ Very unimportant
15.	In comparison to others, are you a person who is generally willing to give up something today in order to benefit from that in the future or are you not willing to do so? *Please use a scale from 0 to 10, where a 0 means you are "completely unwilling to give up something today" and a 10 means you are "very willing to give up something today". You can also use the values in-between to indicate where you fall on the scale* 0="Completely unwilling to give up something today" 10="Very willing to give up something today" 0 ☐ 1 ☐ 2 ☐ 3 ☐ 4 ☐ 5 ☐ 6 ☐ 7 ☐ 8 ☐ 9 ☐ 10 ☐

16.	How do you see yourself: are you a person who is generally willing to take risks, or do you try to avoid risks? *Please use a scale from 0 to 10, where a 0 means you are "completely unwilling to take risks" and a 10 means you are "very willing to take risks". You can also use the values in-between to indicate where you fall on the scale.* 0="Completely unwilling to take risks" … 10="Very willing to take risks" 0 ☐ 1 ☐ 2 ☐ 3 ☐ 4 ☐ 5 ☐ 6 ☐ 7 ☐ 8 ☐ 9 ☐ 10 ☐
17.	Generally speaking, how much do you trust Georgians? ☐ No trust at all ☐ Little trust ☐ Quite a bit of trust ☐ A lot of trust
18.	Generally speaking, how much do you trust Armenians? ☐ No trust at all ☐ Little trust ☐ Quite a bit of trust ☐ A lot of trust
19.	Please consider the following situation: You have to decide how to split 10 Lari between two strangers. One stranger is Georgian, the other is Armenian. How would you split the money? On the following scale, the first number always refers to the amount for the Georgian, the second number always refers to the amount for the Armenian. *You can also use the values in between to indicate where you fall on the scale.* 0/10="0 for the Georgian, 10 for the Armenian" … 10/0="10 for the Georgian, 0 for the Armenian" 0/10 ☐ 1/9 ☐ 2/8 ☐ 3/7 ☐ 4/6 ☐ 5/5 ☐ 6/4 ☐ 7/3 ☐ 8/2 ☐ 9/1 ☐ 10/0 ☐

Chapter 3

3 Information, Perceived Returns and College Major Choices[1]

Nikoloz Kudashvili, Gega Todua[2]

3.1 Introduction

College major choices represent an important specialization-specific human capital investment and can largely determine an individual's future earnings and career prospects (Hastings et al. 2013). However, little is known about how students choose college majors (Kirkeboen et al. 2016). A large number of studies have emphasized the roles of factors affecting students' choices including tastes, parental education, credit constraints, and pecuniary and non-pecuniary benefits. Pecuniary benefits are an important consideration and are largely determined by future salary and employment opportunities. However, many students likely make their major choices based primarily on their subjective beliefs (Arcidiacono et al. 2012 and Montmarquette et al. 2002) as the information on the salaries and unemployment rates for each major may not be perfectly observed by the students. Indeed, a growing body of studies has shown that students do not hold accurate beliefs about earnings conditional on a college major (Betts 1996, Conlon 2020,

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[2] CERGE-EI, a joint workplace of Center for Economic Research and Graduate Education, Charles University and the Economics Institute of the Czech Academy of Sciences; gega.todua@cerge-ei.cz.

Jensen 2010, Nguyen 2008, Wiswall and Zafar 2015b). Furthermore, Hastings et al. (2016), Oreopoulos and Dunn (2014), and Wiswall and Zafar (2015a) find that students in Chile, Canada, and the US overestimate returns to post-secondary or higher education. In contrast, Conlon (2020) and Jensen (2010) find that students underestimate returns in the US and Dominican Republic, respectively. These studies illustrate that there is large heterogeneity in students' perceived earnings.

Due to a dearth of accurate information students may make suboptimal educational decisions based on perceived potential earnings and employment opportunities. Therefore, college major choices made under imperfect information may be inefficient for students and the society as a whole.[3] In such settings, policy interventions providing relevant information could help students to make better-informed choices (Bettinger et a. 2012, Conlon 2020, Dinkelman and Martínez 2014, Hastings and Weinstein 2008, Jensen 2010, Nguyen 2008). Nevertheless, little is known about the mechanisms through which information affects students' educational choices.

Using a novel experimental design, we focus on direct and indirect effects of information provision on students' college major choices. We extend the literature on educational choices in two ways. Firstly, we investigate the effects of indirect information provision, i.e., allowing for information spillover effects on specialization choices.[4] Secondly, we focus on the immediate (intended) and actual changes in students' college major choices in response to the provision of information. A handful of studies consider the immediate effects of information, however, these changes in behavior may not concretely inform real-life choices. For this reason, we study the persistency in terms of both direct and indirect information effects in terms immediate and actual (final) outcomes. Additionally, we study whether responsiveness to information depends on age. Observing the intended and actual changes for the younger and older students, we discuss possible alternative timing of the information provision and the effectiveness of such interventions.

The experiment was conducted in three rounds in 2017-2019 on tenth and eleventh grade students in Tbilisi, Georgia. At the time of the experiment, tenth grade students had two years to make an actual college major decision, while eleventh grade students had only one year. In the

[3] Information provision could have a stronger impact on choices and efficiency in less developed countries with few or limited possibilities for students to access accurate and relevant information. Information on earnings and unemployment rates are not available in Georgia, rather Georgian Statistical Office publishes wages by industry.
[4] We use the terms specialization and college major choices interchangeably throughout the paper.

first round, we elicited students' *baseline intended college major choices* and beliefs about the average earnings and unemployment rates of individuals for every field of study, and the same for those workers with no tertiary education. Further, we implemented a belief elicitation mechanism by providing incentives to students to truthfully report their major choices.[5] Then, schools were randomly divided into control and treated groups. A randomly selected half of the classes in the treated schools received information on the population earnings and unemployment figures (direct provision - treatment group). The remaining half in the treated schools were not given any information (indirect provision - spillover group). However, their peers from the same school could pass the information on to them (indirect provision). In the second round, a month after the first round, we surveyed students and collected *revised intended college major choices*. In the third round, we collected their *actual college major choices*.[6] Hence, the two main outcome variables of the experiment are *the revised intended and actual college major choices*.

This study answers the following key questions: (i) Do students respond to the information provided? Do they hold accurate beliefs about earnings and unemployment? (ii) When is the ideal time to intervene, i.e., is the information more relevant to the older (younger) students? (iii) Does the information have to be provided directly or can it be effectively passed on by other students? This would be easier and less costly for policy makers to implement. (iv) How do the treatment and spillover effects differ in terms of the intended and actual college major choices? (v) What are the channels through which information affects college major choices?

We find that the students in our sample overestimate wages and unemployment rates for all study fields, and underestimate the salaries of individuals with no tertiary education. Using actual major choices data, we find that students with the information we provide are 10 percent more likely to change their actual college majors. Interestingly, the treatment effect is largely driven by the eleventh-grade students. Thus, too early intervention may be less effective. Furthermore, the spillover effect is significant but smaller than the impact of direct information.[7]

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[5] We made an announcement that we were sending specific major information to students after the first round (see section 3.2.3).

[6] We followed up with the eleventh-grade students after a year, while tenth grade students were followed up after two years, when their actual college major choices were finalized.

[7] The spillover effect becomes insignificant in the full sample after controlling for covariates. However, the effect remains significant in the subsample of the eleventh-grade students.

The structure of specialization revisions differs when we compare the intended and actual college major choices. 82 percent of the actual choice revisions are made by the students who did not revise their intended choices, i.e., their baseline and revised intended choices were identical. The treatment effect is 1 percent smaller in the actual choices sample compared to the intended choices. Unlike the treatment effect, the spillover effect is 3 percent higher in the actual choices sample. We argue that studies that consider only immediate effects of an intervention and ignore the final outcomes may not be accurately analyzing treatment effects on real-life decisions.

We find that the differences between the *actual and perceived unemployment rates* have a significant effect on actual major choices. At first glance, this result may be puzzling as these students changed their majors in favor of the specializations with high unemployment rate. Why would students revise their choices in this way? Students' perceived difference between the unemployment rates for the two choices they were considering was large, and so they opted for the major they thought offered significantly better chances for employment. [8] However, when they learned via the informational leaflet that the gap between the two majors was not as large as they imagined, the cost of changing to the major they actually preferred was smaller than they had believed. Initially, this group of students overestimated the potential unemployment cost of changing their major. Thus, revision of the major toward the more desired specialization for these students, would not result in a drastic decrease in their employment opportunities. We refer to this as 'the relative unemployment rate channel' to explain the pattern in college major revisions. In addition to this channel, preferences and other unobserved factors must be behind the complex decision-making behavior regarding college majors (Wiswall and Zafar, 2015a).

However, we do not find evidence that students revise their choices toward higher wages or higher expected earnings.[9] The same is true for the differences between actual and perceived earnings. This can be explained by the relative importance of actual wages. For instance, a student may not find average earnings data relevant for her future earnings if she considers herself a high ability student. Alternatively, students may perceive that earnings distributions by major will

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[8] Consider a student with a baseline intended major choice of medical sciences with an unemployment rate of 10%, who ultimately chooses the major 'exact and natural sciences', which has a higher unemployment rate of 12.6%. Hence, the actual cost of changing the major in the form of a higher unemployment rate is 2.6% (see Table 3.3, column 4). Prior to the intervention, students perceived that the cost of changing the major in this case was 5%, nearly twice as high in actuality (see Table 3.3, column 5).

[9] Expected earnings are calculated as the product of the wage and employment rate for any given college major. Note that the employment rate equals (1-unemployment rate).

change considerably by the time they graduate. Third, students may consider average wages less informative as the calculations still include the individuals with the Soviet education. Students may logically assume that a current tertiary education offers better career prospects. There may be still other reasons that the population earnings figures may not necessarily be relevant and informative.

Our study is related to research evaluating the effects of information provision on belief updating and actual educational choices where agents have inaccurate information or hold biased beliefs. In particular, Jensen (2010) finds that high school students in the Dominican Republic underestimate the earnings of individuals who completed secondary school. Provision of information on the true returns to secondary school education[10] had large and significant positive effects on two outcomes – students revised their perceived returns upwards and completed about 0.2-0.35 more years of schooling. Similarly, Nguyen (2008) finds that the intervention improved students' school attendance and average test scores during the first few months following an experiment in Madagascar. Interestingly, Nguyen (2008) shows that a role model (a person from a poor/rich background presenting her/his success story) had a larger impact on student school attendance and performance than statistics provision. Wiswall and Zafar (2015a) show that students updated their beliefs on major-specific salaries after observing true earnings. Perceived earnings and abilities, along with heterogonous tastes, were the main drivers of specialization choices in a sample of New York University undergraduate students. Granguli et al. (2020) show that doctoral students were overly optimistic about their chances on the academic job market and updated their beliefs after information treatment. However, the study does not find any evidence of doctoral students changing their subsequent academic career plans (doing a postdoc or deciding on an academic job market placement).

Our results have implications for policymakers – both direct and indirect information provision have effects on intended and actual major choices. Both treatment and spillover effects are driven by older students implying that early interventions are less effective. The treatment effect is consistently stronger than the spillover effect in both actual and intended choice samples.

[10] In these studies, actual (true population) salaries and unemployment rates are either given by the respective government statistics bureaus of a country, private organizations, or are calculated by the authors based on household surveys conducted prior to the experiment. The latter is usually used in cases of limited or no data availability (Jensen 2010, Nguyen 2008).

Additionally, we find that the composition of the changes, treatment and spillover effects vary significantly in the actual and intended choices samples. Further research is needed to complement our findings on immediate and actual changes.

The remainder of the paper is organized as follows. Section 3.2 describes the data and field experiment. Section 3.3 presents the main results, section 3.4 concludes.

3.2 Field Experiment

3.2.1 Short Overview of the Georgian Education System

Georgia is a small country in the Caucasus region with a population of 3.7 million and a GDP per capita of $ 9,702 in 2017 (PPP adjusted).[11] The degree of urbanization is 58%. The capital of Georgia, Tbilisi, is the largest city, with a population of 1.1 million, and with over public 250 schools providing elementary, primary, and secondary education.

School education in Georgia consists of elementary (age 6-12), basic (age 12-15), and secondary (age 15-18) studies (Ministry of Education of Georgia). Students receive a Full General Certificate upon passing school leaving examinations at the end of the twelfth grade. Students with a school-leaving certificate have access to the higher education. University admissions have been centrally administered by the National Assessment and Examination Center (NAEC) of Georgia since 2005. All students wishing to enter accredited universities have to pass standardized written exams conducted by NAEC. Note that entrance examination subjects vary by major. For instance, entering a university with a major in Economics and Business would require a student to pass four examinations: Georgian language and literature, mathematics, general skills, and foreign languages.

The demand for each specialization at accredited universities in 2017 appears in Table C1.1. The demand for each major is defined as student's first desired specialization choice. All accredited Georgian universities were able and willing to admit nearly 50,000 students, while there were about 40,000 applicants in 2017. We aggregated the various university majors into six

[11] World Bank, https://data.worldbank.org/country/georgia [accessed 5 June 2019].

groups: (i) exact and natural sciences, (ii) medical sciences, (iii) economics and business, (iv) social sciences, (v) arts and humanities, and (vi) law. According to Table C1.1, the majority of applicants chose humanities, exact and natural sciences, and economics and business as their first college major choice in 2017.

3.2.2 Data

The experiment was conducted in three rounds in Tbilisi during 2017-2019. In the first round, 2015 students aged between 15 and 17 participated.[12] First round was administered at twenty-two randomly selected schools during regular school hours in April 2017. Students were asked to report their *baseline intended college major choices*, and their beliefs about what unemployment rates and earnings are for persons with a university diploma in that field. They were also asked to report on their individual and household characteristics in the baseline survey.[13] Each session lasted approximately 55 minutes.

The experiment was conducted on tenth and eleventh grade students. Twelfth grade students who intend to enter a university fill out their university applications in March every year. A student's university application is a combination of specialization and university choices submitted during the final year of secondary school. Although the formal decision about the major choice occurs in March, twelfth grade students make informal decisions at the beginning of their final year of studies. A student's informal major decision results in extensive private tutoring sessions in the subjects required for the unified entrance examinations. It is very common that students and their parents decide to pay additional fees for extensive tuition sessions for courses that are relevant to their college major choices. For instance, 78.3% of the students in our experiment reported that they either already had or intended to have a tutor to prepare for the unified examinations. Tutoring may increase their chances of being admitted at top universities and/or receiving merit-based state scholarships. As expected, the share of such students is higher in the eleventh grade (81.3%) than in the tenth grade (75.9%). This can be explained by the remaining time before the university enrollment - tenth grade students had about two years to go

[12] Note that two students were 18 years old.
[13] See Appendix C2.3.

before making their major choices, hence they may have been less certain about their need to have a tutor. In contrast, eleventh grade students are about to start their preparation for the unified entrance examinations over the final year of their studies in a secondary school (twelfth grade). According to common practice, eleventh grade students and their parents usually search for tutors in the spring and summer for the upcoming September.[14]

A second survey of the students was conducted one month later (May 2017). Similar to first round, students were again asked to report their specialization choices; we refer to these second round choices as *revised intended college major choices*. The first and second round surveys were conducted using a pen and a paper. A third follow-up survey on major choices was conducted in September 2018 and 2019, by which time the students' final major choices were realized, i.e., students were admitted to universities.[15] Applicants usually learn about their test scores and university admissions in late August, therefore September was the earliest possibility to track actual college major choices, the real-life outcomes in this case. In the third round, *actual major choices* were collected using telephone and email surveys, as students were no longer in the high schools where the experiment was originally administered.

The timeline of three experimental rounds appears in Table 3.1. Overall, we were able to obtain 95.9% follow-up responses in the May 2017 survey. In the third round, most of the responses were recorded via a phone communication - there were only four email responses that were not documented via phone call. This may be explained by low popularity of email communication, or students might have changed their school email addresses. In our experiment, 1,290 students provided their cell numbers, which is 67.4% of round-two observations.[16] We were able to track a large majority of student major choices. Indeed, the phone response rate was 89.7%. Overall, we were able to obtain follow-up information on 1,157 students in the September 2018 and 2019 phone survey – 27 students reported that they had not applied for the universities at all.

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[14] We did not consider students in their final year of studies (twelfth grade) in our experiment. Twelfth grade students are generally unlikely to change their majors for two reasons. Firstly, they have already attended tutor sessions in the subjects required for the major and hence, there are sunk costs in the form of tuition. Secondly, even if they wanted to change their majors, students would have little time to prepare for the new exam(s) for the different major.

[15] Students were also asked whether their desired major choices were different from the realized university major decisions that are dependent on test scores. Note that none of the students reported that they picked a different major choice due to the insufficient exam scores (Round 3). Thus, all the major choices were students' own decisions and were not driven by their exam scores.

[16] Note that students optionally filled in their cell numbers in the questionnaire in the first round.

Thus, we were able to track 1,130 students and record their major choices three times (baseline intended, revised intended and actual college major choices) for the period of 2017-2019. The overall attrition rate is 42%, hence, we further study whether the attrition is correlated with the treatment or spillover effect. Table C1.7 shows that neither treatment nor spillover effects are correlated with the attrition. However, we find that tenth-grade students are more likely to be missing in the final round (actual choices collection) than the eleventh-grade students. This effect is expected, as we followed up with eleventh- and tenth-grade students after one and two years respectively.

Tables 3.2.1, 3.2.2[17] and 3.2.3 show that there were no systematic differences in covariates across treatment, spillover, and control groups. Table 3.2.4 reports the school characteristics. These groups differ in terms of the information provision discussed below in detail.

3.2.3 The Intervention

In this section, we describe our experimental design to study the effects of direct and indirect information provision on college major choices. Our three experimental treatment groups differ with respect to the information provided to each group. Firstly, the schools were randomly divided into the control (C) and treated (T) schools. Students in the control schools (C) did not receive any information. Secondly, students in the treated schools were divided in treatment (TT) and spillover groups (TS).[18] Students in the TT group received information on earnings and unemployment rates by specialization; students in the spillover group did not receive any information. The control group included students from seven schools, and treatment and spillover groups included students from fifteen schools. Classes in each grade in every treated school were randomly divided into treatment and spillover groups. Thus, the randomization unit was at the class level in the treated schools. Note that, for this reason, student characteristics in three experimental groups may differ. Overall, 1,429 students were surveyed in the treated schools and

[17] We also run the randomization checks in the actual choices sample – we do not find any statistical differences across control, spillover and treatment groups (see Appendix Table C1.8).

[18] TT- students received an information leaflet in the treated schools; TS – students did not receive an information leaflet in the treated schools. Thus, by our design, students in TT group could reveal information on earnings and unemployment rates to their peers in the spillover group (TS). Note that both TS and TT classes were located in the same school building.

586 students in the control schools. There were 752 and 677 students in the TT and TS groups. First, students were asked to report their baseline college major choices. Next, we elicited student beliefs about the average earnings and unemployment rates of university graduates from each field and collected other relevant data (baseline survey). After the baseline survey, the intervention took place.

At the end of the first survey session, each student in the TT group was given the information on earnings and unemployment by specialization, calculated by the authors based on a household survey conducted by the statistical office of Georgia in 2015 (see Table 3.3). Overall, 98.52% of the students in the TT group found the information leaflet helpful for their choice of major decisions (see Appendix C2.2). In the second round, students were asked to state whether they discussed their major choices with their parents. More than 78% of students stated that they discussed their major choices with their parents in all three experimental groups.

To study whether information on earnings and employment affects their choices of major, we track the choices over three rounds – baseline intended, revised intended, and actual college major choices. We first measure the treatment effect by comparing the revision of major choices across the TT and C groups. Further, we examine the major choices revision rate across the TS and C groups, to identify any spillover effects. We incentivized students to truthfully report their baseline major choices. Students were told that they would receive an email with the major specific information. The major specific information included details about university application procedures and deadlines, admission requirements, top universities, and degree of competition (chances of being admitted) for each major. We emphasized that the information was major specific, i.e., students would benefit by indicating their 'truly desired' specialization and would receive relevant information by email. The sample information was shown to students but not distributed in the beginning of the experiment. 98.17% students provided their email addresses and over 99% students reported that they were interested in the major-specific information to be sent by email later. Thus, our incentivization scheme worked as intended. However, we are aware that some students might still misreport their major choices, particularly those who were less certain about entering the university at all.

Table 3.3 reports average monthly salaries and unemployment rates for each college major choice, including individuals with no university education. Students in the treatment group were provided with the information (see Appendix C2.2). The earnings and unemployment figures were accompanied by an explanatory sheet explaining the differences in wages and employment likelihood for each major. Students were informed that they could ask questions straight away or send an email with a question if the leaflet was not clear. On the one hand, providing the unemployment rate could be interpreted as positive news for the students, because they overestimated unemployment for all major choices. On the other hand, providing actual earnings data could be perceived as negative news, because students overestimated wages for every major choice listed.

3.3 Results

First, we present the differences between student beliefs and actual data to scrutinize the motivation behind their college major choice revisions. Second, we investigate the effect of the information provided on major choices. In particular, we examine the patterns of college major choice revisions in treatment, spillover, and control groups. Third, we investigate channels rationalizing the revision of the major choices.

3.3.1 Perceived Earnings and Unemployment Rates

Do students hold accurate beliefs about earnings and unemployment rates? - We start the analysis by presenting the key differences between the perceived and actual figures. Table 3.3 shows actual and perceived mean monthly wages and unemployment rates for individuals with and without tertiary education. First, individuals with a tertiary education earn about 59 percent more than workers with only a high school diploma. However, the difference is only 51 percent when comparing the expected wages that considers the higher unemployment rate among individuals with a tertiary education. Workers with a university degree in *law, and economics and business administration* earn the most. Second, students systematically overestimate[19] earnings for each major except for the earnings of individuals with no university education. Their overestimation is the highest for individuals with degrees in *medical sciences* and least for

[19] Note that the beliefs were elicited before we provided the leaflet, to avoid contamination. High perceived returns in our sample can be ascribed to the experimental setting - the experiment was conducted in an urban area, where wages are generally higher than overall country wages.

graduates in *exact and natural sciences*. Students perceive that workers with no tertiary education earn about 25% less than actual earnings. Unlike the findings in Jensen (2010) and Nguyen (2008), students in our sample, perceive that returns to tertiary education are large. Overestimation of tertiary education returns could partly explain high enrollment rates in the universities. Unsurprisingly, the percentage of the labor force with tertiary education in Georgia is high, at 31 percent, higher than most advanced European countries. This figure is even more pronounced in urban areas, where every second worker has a higher education diploma (World Bank report 72824, 2013). Students hold nearly accurate beliefs regarding expected earnings for the following specializations: *exact and natural sciences, and arts and humanities*.

Third, students overestimate the unemployment rate for all workers. Students perceive that the highest unemployment is among individuals with no tertiary education, followed by workers with a degree in *arts and humanities*. Interestingly, the perceived unemployment rate (46 percent) for individuals with no university education is 4.5 times higher than the actual unemployment rate (8 percent). In fact, the individuals with no tertiary education have the lowest unemployment rate (Table 3.3). One of the reasons for the overestimated unemployment rates may be connected to the peculiar employment structure in Georgia. Over 50 percent of workers are employed in the agricultural sector - contributing less than ten percent of the country's GDP. Rutkowski describes this strange phenomenon: "*while not contributing substantially to the economy overall, agriculture provides employment of last resort for those who cannot find jobs elsewhere, and eventually work as subsistence farmers*" (World Bank report 72824, 2013). Differences in employment trends are also observed in unemployment rates in the rural and urban areas – the latter being 28 percent, three times higher than in rural areas. This further reinforces the argument of the hidden unemployment in rural areas. Further, unlike the majority of European countries, highly educated individuals are more likely to remain unemployed over the long-term in Georgia. For instance, over 40 percent of unemployed individuals have higher education, and highly educated workers account for over 70 percent of the long-term unemployed (World Bank report 72824, 2013).[20]

The gap in the unemployment rate between workers with tertiary and secondary education is in sharp contrast with most EU countries. For instance, the EU28 unemployment rate for

[20] Individuals are considered long-term unemployed if they have been unemployed longer than twelve months according to ILO.

individuals with tertiary education in 2018 was only 3.9 percent, and 12.5 percent for individuals with no tertiary education.[21] However, workers with tertiary education in Georgia experience higher unemployment (13%) compared to workers with only secondary education (8%). Higher long-term unemployment among educated individuals and systematic underemployment for their skills are associated with losses in investments into human capital. Thus, providing the information about earnings and employment opportunities may help students to make more optimal educational choices.

3.3.2 Changes in the College Major Choices

First, we report the effects of information provision on intended major choices. We document the changes in the intended major choices across control, spillover, and treatment groups. Second, we present changes in the realized (actual) major choices. Our primary analysis is based on actual major choices, as they represent real-life outcomes, i.e., actual major choices collected after university admission decisions. Next, we explore the mechanism explaining the revision of major choices by looking at differences in perceived and actual earnings and unemployment rates for the baseline intended and actual major choices.

Do Students Revise Their Intended Major Choices Upon Observing Actual Earnings and Unemployment Rates? - Figure 3.1 shows that students in TT (treatment) and TS (spillover) groups revise their majors more frequently than do their peers in the control schools. Thus, information provision both directly and indirectly alters the main outcome variable to a greater extent in the TT and TS groups than in the control group. Students in the treatment and spillover groups revise their major choices by 11 percent and 4 percent more, respectively, and both effects are significant at 5 percent. Thus, the information has a significant effect on intended college major choices reported by students a month after the intervention. Table 3.4 (columns 3 and 4) shows that the treatment and spillover effects remain robust after controlling for covariates.[22]

Next, we analyze the revision patterns in the treatment and spillover groups across two grades, and notice significant differences. Interestingly, students in the tenth grade, including those

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[21] https://ec.europa.eu/eurostat/web/products-eurostat-news/-/DDN-20190920
1?inheritRedirect=true&redirect=%2Feurostat%2Fhome%3F

[22] The results remain significant in the probit model specification as well (Table 3.4, columns 5-6).

in the control group, revise their major choices more than eleventh grade students. This can be explained by less information availability or higher uncertainty about their future major choices. Why do revision rates differ by grade? Eleventh grade students had to decide about college majors within a year and therefore, they may logically have considered their major choices seriously beforehand and they were more certain about their major choices. This is indicated by relatively lower revision rates by the eleventh-grade students. By contrast, tenth grade students had nearly two years to choose a major, so their choices fluctuated more. Overall, the total revision rate across all three experimental groups in the tenth-grade students is 16 percent, compared to 13 percent in the eleventh grade. The revision rate differentials across the two grades is more salient for students in the control group. Indeed, Table 3.4 shows that 9 percent of tenth grade and 5 percent of eleventh grade students in the control group revised their majors. In fact, unstable choices undermined both the treatment and spillover effects in the sample of tenth grade students – the spillover effect is nearly zero; the treatment effect is significant but smaller than the one found in the sample of eleventh grade students. Thus, we conclude that changes in the intended major choices were more pronounced in the eleventh-grade students, and overall changes are also driven by older students.

Next, we present our analysis of *actual college major choices*. Both treatment and spillover effects are calculated by comparing changes between the actual and baseline intended college major choices. In line with our findings on the intended choices, we find that the treatment effect is positive and statistically significant at the 1 percent level. Figure 3.2 shows that students in the treatment group revise their major choices 10 percent more often than their peers in the control group. Table 3.5 derives similar results – the treatment effect is more pronounced in the sample of eleventh grade students. Overall, the spillover effect is 7 percent and significant at the 10 percent level, however, the effect is stronger at 11 percent and statistically more significant at the 1 percent level among the eleventh-grade students. Furthermore, we find that 47 percent of the tenth-grade students revised their actual majors, compared to 23 percent of the eleventh-grade students. Hence, almost every second tenth grade student revised her/his choice. For this reason, there is a cleaner revision pattern across treatment, spillover and control groups for the eleven grade students. The treatment effect is 14 percent when controlling for covariates (Table 3.5). Thus, both the direct and indirect information have a significant and strong effect on actual specialization choices.

Our results shed the light on the intervention's timing. Both treatment and spillover effects are largely driven by the eleventh-grade students. That is, both direct and indirect provision of the information, a year before the university entry date, has a larger impact on actual choices.

Now we turn to the revision patterns in terms of the intended and actual changes in the college major choices. Are changes in the actual and intended college major choices consistent with each other; if a student revised her intended choice, did she also revise the actual choice? We find that the structure of revisions differs largely across intended and actual major choice samples. Most students who revised their actual choices compared to baseline, had not changed their intended choices.[23] 82 percent of the changes in the actual major choices were made by students whose baseline intended and revised intended choices were identical.[24] Thus, intended choices are less suggestive in predicting the effect of information on real life outcomes. We report the results based on real life outcomes (actual major choices) below.

Result 1

Students revise their major choices upon observing actual earnings and unemployment figures. Students with information are 10 percent more likely to revise their actual majors. The effect is significant and robust to all model specifications in the full sample. The treatment effect is more pronounced in the sub-sample of eleventh grade students.

Result 2

The spillover effect is positive and significant in all model specifications in the subsample of eleventh-grade students, but the effect is insignificant in the whole sample after controlling for

[23] Had not changed their intended choices in round 2 but did make a change in round 3.

[24] Note that we recorded 1,913 intended choices (round 2) and 1,130 actual major choices (round 3). Could attrition drive the differences? Appendix Table C1.5 shows that there are no significant differences across treatment, spillover, and control groups for the students who did not report their actual choices (participated in the round 2, but did not participate in round 3). Moreover, the means in Appendix Table C1.5 are similar to those in Table 3.2.2. Next, we run an analysis of the intended choices sample on the round three sample and find that the treatment effect is significant but smaller than in the original round 2 sample (Appendix Table C1.6). Similarly, the spillover effect is insignificant in the whole sample but positive and significant in the subsample of eleventh-grade students. Thus, we do not find any evidence that attrition drives the differences between the analyses across the revised intended choices and actual choices data. Appendix Table C1.7 shows that the attrition is not correlated with the spillover or treatment effects. However, the attrition rate is 10% higher for tenth-grade students, which is intuitive – we tracked actual choices of the tenth-grade students after two years, as opposed to one year for the eleventh-grade students.

covariates. Thus, indirect information provision has a real impact on the choices of the older students.

3.3.3 Determinants of the College Major Choice Revisions

Next, we explore the channels that explain the changes in college major choices. Students in the treatment group were given the leaflet displaying the monthly earnings and unemployment figures for each major (Appendix C2). Existing literature emphasizes the role of expected earnings and employment opportunities when deciding between specializations (Wiswall and Zafar, 2015a). A specialization with higher wages and lower unemployment could make this major more attractive. Provision of the information is a mixture of good and bad news. Intuitively, the earnings statistics can be treated as negative news, as students perceived that wages were higher than the actual ones, however, the unemployment statistics should be treated as positive news, as students largely underestimated graduates' employment chances.

Our analysis suggests that changes in the student specialization choices are explained by the differences between the actual and perceived unemployment rates. We refer to this as the '*relative unemployment rate*' channel.

How does this channel rationalize the changes in the college major choices? Consider students who revised their majors from medical sciences (baseline intended choice) to the exact and natural sciences (actual choice). Table 3.3 reports the actual and perceived unemployment rates of individuals with a degree in medical sciences, 10% and 25%, respectively. The same figures for the exact and natural sciences diploma holders are 12.6% and 30%. This implies that the *actual cost* [25] of changing one's major from medical sciences to exact and natural sciences is 2.6%. This is in stark contrast with the perceived costs of the same change – indeed, the *perceived cost* of this change is 5%. Thus, students in our sample overestimated their *cost* of changing the major in the form of lower employment opportunities. In fact, they only would give up 2.6% if they chose *exact and natural sciences* instead of *medical sciences.* However, they perceived that the revision would be associated with an increase in their unemployment by 5%, much larger than the actual difference, 2.6%.

[25] Cost is defined as the reduced chance of finding a job, i.e., higher unemployment rate.

Figure 3.7 displays the fraction of students who revised their actual major choices toward one with a lower relative unemployment rate, defined as the difference between actual and perceived unemployment rates associated with the actual and baseline intended major choices respectively.

Figure 3.7 shows the share of students whose revision (college major changes) behavior satisfies the following rule:

$$\Delta U_{Actual} - \Delta U_{Perceived} < 0,$$

where U stands for the unemployment rate, and the differences between actual and perceived unemployment rates are defined as follows:

$$\Delta U_{Actual} = U_{Actual}\,(Actual\ Major) - U_{Actual}\,(Baseline\ Intended\ Major)$$

$$\Delta U_{Perceived} = U_{Perceived}\,(Actual\ Major) - U_{Perceived}\,(Baseline\ Intended\ Major)$$

The following rationale explains the students' revision behavior - they learned that they would not be as much at risk of unemployment as they had previously believed if they changed their majors. We find that a higher share of students follows this pattern in the treatment group than in the control group. Coefficient estimates in Table 3.7 suggest that 20 percent more students in the treatment group revise toward 'lower relative unemployment rate' compared to the control group; the effect is significant at 1 percent.[26] The effect is more pronounced at 30 percent in the sample of eleventh-grade students. Both coefficients remain robust after controlling for the covariates in all model specifications. Unlike with the treatment effect, he 'lower relative unemployment' argument does not explain the effect of indirect provision of information.

Result 3

The revisions are driven by the differences between the perceived and actual unemployment rates across baseline and actual specialization choices. Therefore, changes in specialization choices can be explained by the differences between the perceived and actual employment opportunities.

[26] Note that the regression analysis is conducted on the sample of students whose actual college major choices differ from the baseline choices ones.

Surprisingly, we find no evidence of earnings explaining changes in the college major choices, i.e., students do not change their specializations toward higher wages.[27] Moreover, we show that students do not change their choices toward majors with higher expected earnings and lower unemployment rates. An extensive analysis of all these channels with reference to appropriate tables and figures can be found in Appendix C3.

3.4 Conclusion

This paper studies the effects of information provision on the college major choices of high school students in Georgia. We find that information strongly affects educational choices – 10 percent more students in the treatment group chose a different college major after information was provided. Interestingly, the treatment effect is more pronounced in the older students. We implement a novel experimental design and contribute to related literature by measuring the effects of indirect information provision. We find that the spillover effect is significant but smaller than the treatment effect. Similarly to the direct treatment effect, indirect provision of the information mainly affects the choices of older students. Our results suggest that both direct and indirect channels of information provision can be used to nudge individuals' behavior. However, from policy perspective, too-early intervention may be ineffective.

We argue that immediate effects of the information may not be translated into real life choices. Our findings indicate that immediate changes in the intended choices are not necessarily linked to the final major choices. Interestingly, only 18 percent of the students who initially changed their intended choices did so again at the end, i.e., revised their actual college major choices. We conclude that analyzes based on immediate effects may be less informative of the effects of interventions on real-life outcomes. Further research is needed to supplement our findings on the immediate and actual changes in different experimental settings.

Our paper sheds light on the mechanisms through which information affects students' college major choices. We find that the differences between the actual and perceived unemployment rates have significant effects on major choices. This suggests that some students may have initially overestimated the cost of changing their college major, in the form of high

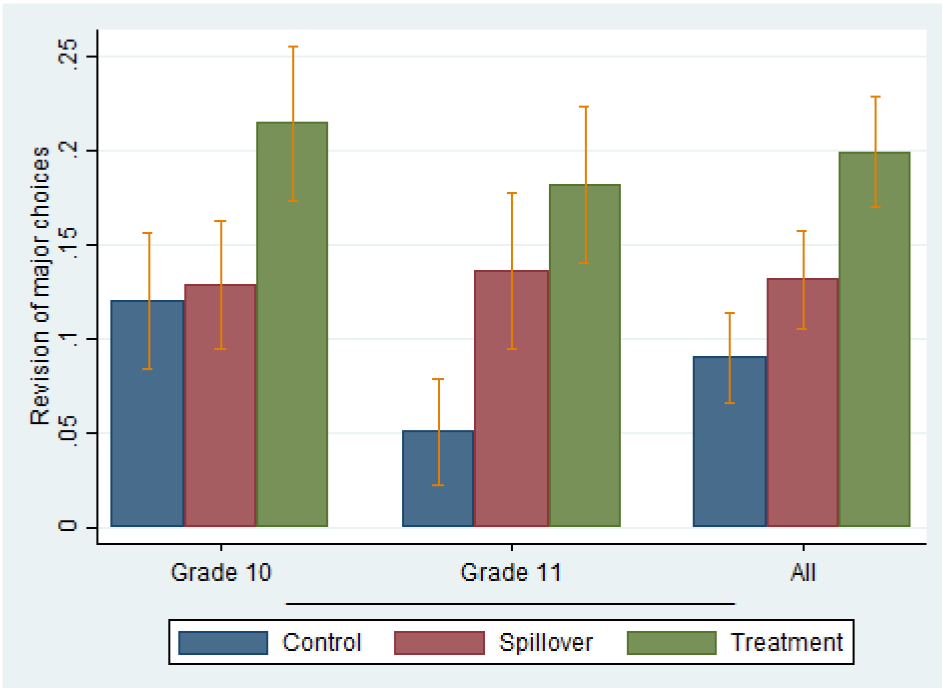
[27] We find no support for this hypothesis: $W_{baseline\ intended\ choices} < W_{actual\ choices}$.

unemployment rate. However, upon observing the information, they learned that actual unemployment is lower than they believed and changed their baseline major choice in the end. We do not find any evidence that students revise their choices toward majors associated with higher wages, higher expected earnings, or lower unemployment rates. Further, the differences between the actual and perceived earnings do not explain the revisions.

This study provides information about average wages and unemployment rates for each major, however, in reality, there are other factors that contribute to students' final decisions, not all of which would be measurable or observable. For instance, one could consider designing experiments providing the distribution of salaries and unemployment, opportunities to work or continue studies, or work abroad opportunities, and information on the differences between urban and rural areas. Furthermore, non-pecuniary aspects of the specialization can be highly relevant to the students (Wiswall and Zafar, 2018). These aspects are interesting directions for future research.

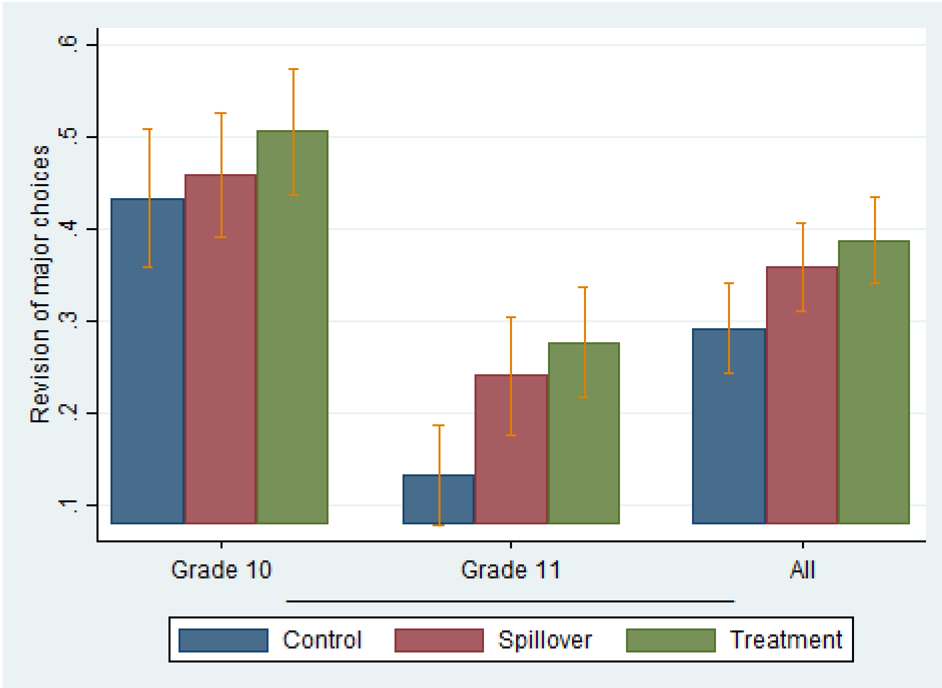
Figures and Tables

Figure 3.1: Revision of intended college major choices



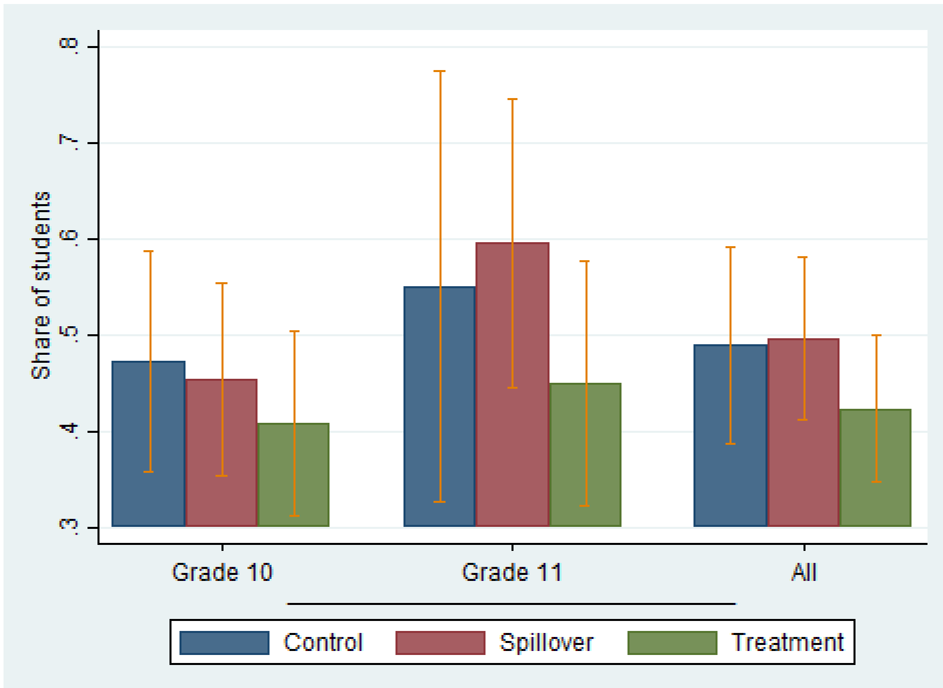
Notes: the figure shows revisions of the intended choices by the control (blue bars), spillover (red bars), and treatment groups (green bars). The revision rate represents the fraction students whose revised intended choices differ from their baseline intended major choices. The revision of the major choices is presented for the tenth grade, eleventh grade, and full sample. We find that students in the treatment group revise their intended major choices more often than do their peers in the control group. The difference is statistically significant at the p<0.01 significance level (see Table 3.4).

Figure 3.2: Revision of actual college major choices



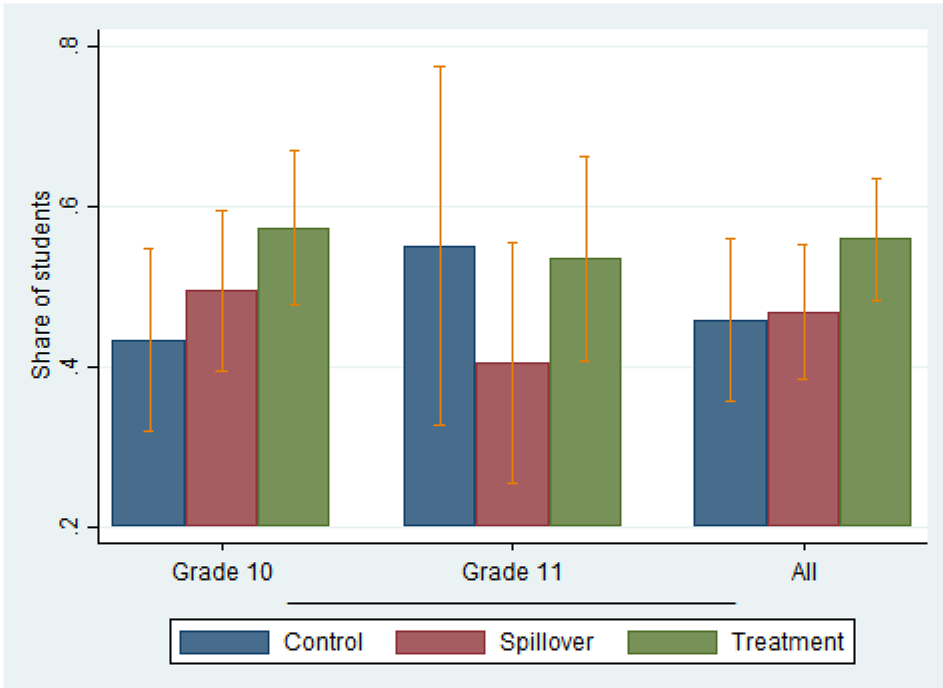
Notes: the figure shows revisions of the actual choices by the control (blue bars), spillover (red bars), and treatment groups (green bars). The revision rate represents the fraction students whose revised actual choices differ from their baseline intended major choices. The revision of the major choices is presented for the tenth grade, eleventh grade, and full sample. We find that students in the treatment group revise their intended major choices more often than do their peers in the control group. The difference is statistically significant at the p<0.01 significance level (see Table 3.5).

Figure 3.3: Do students revise their actual choices toward college majors associated with higher wages?



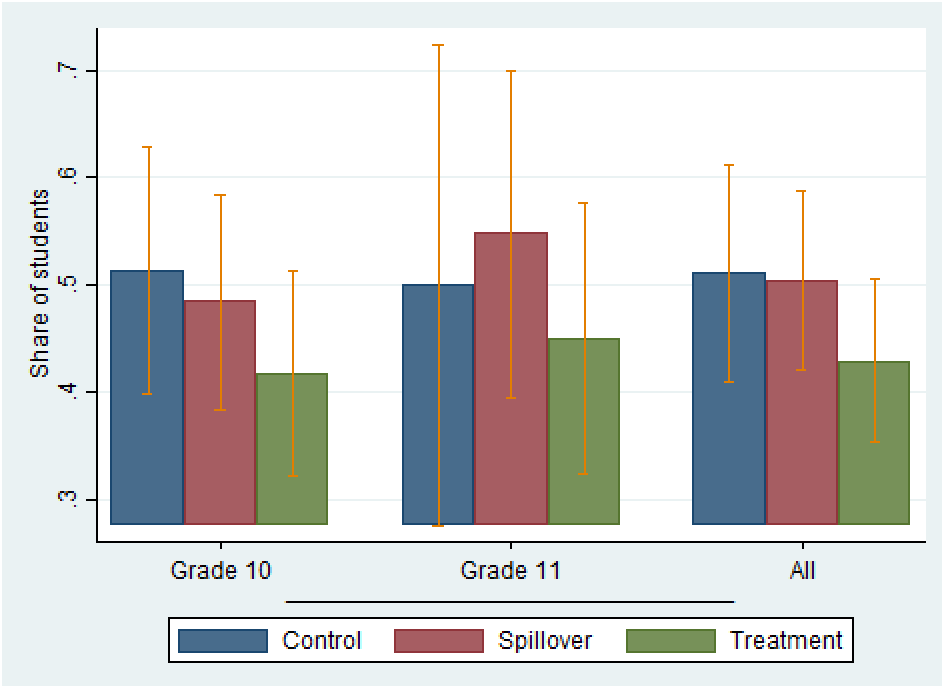
Notes: the figure shows the share of the students who revised their actual major choices toward those associated with higher wages in the control (blue bars), spillover (red bars) and treatment groups (green bars). Note that the actual wages for each major are given in Table 3.3. The vertical bars represent the share of students for whom the real wage differences between the actual and baseline intended major choices are positive. About 40% of the informed students (treatment group) revise their specialization choices toward majors associated with higher wages – much less than the students in the control group. Indeed, the share of the students who revise their actual major choices toward those associated with higher wages is larger in the control group than in the treatment group. Thus, the treatment effect is negative and statistically insignificant at the $p<0.05$ level for the eleventh-grade students.

Figure 3.4: Do students revise their actual choices toward college majors associated with lower rates of unemployment?



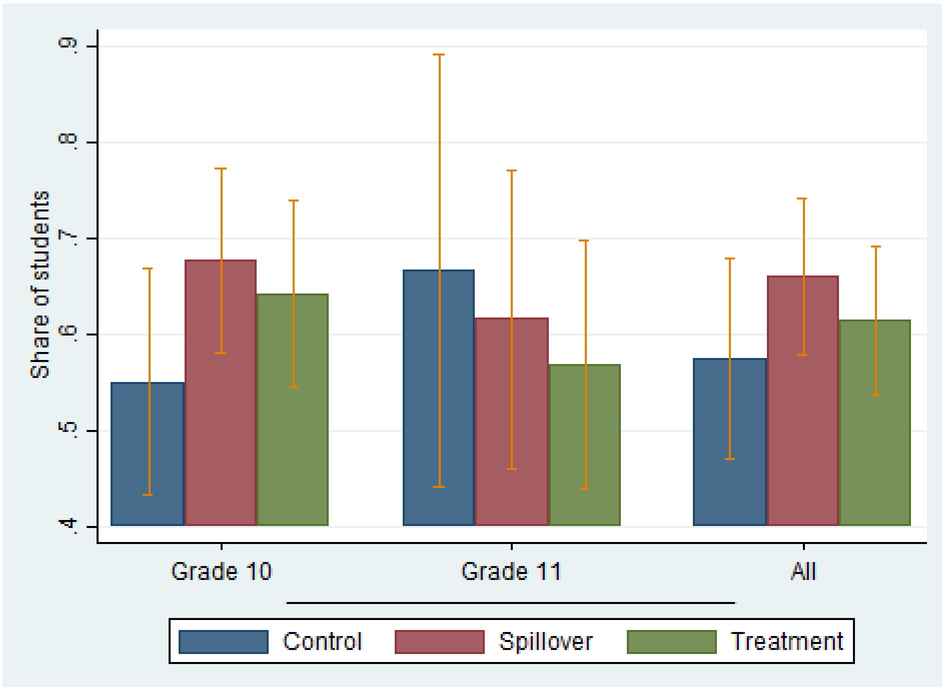
Notes: the figure shows share of the students who revised their actual major choices toward majors associated with lower unemployment rates in the control (blue bars), spillover (red bars), and treatment groups (green bars). Note that the actual unemployment rates for each major are given in Table 3.3. Nearly 11% more students revise toward majors with lower rates of unemployment in the treatment group than in the control group. Overall, the difference is statistically insignificant at the p<0.05 level (see Table 3.6).

Figure 3.5: Do students revise their actual choices toward college majors associated with higher expected earnings?



Notes: the figure shows share of the students who revised their actual major choices toward majors associated with higher expected earnings in the control (blue bars), spillover (red bars), and treatment groups (green bars). Expected earnings are calculated as the product of the wage rate and employment rate for each major. The employment rate for each specialization is calculated as one minus the unemployment rate. Note that expected earnings for each major are given in Table 3.3. Fewer of the informed students (treatment group) revise their specialization choices toward majors with higher expected earnings. By contrast, expected earnings are higher for students who revised in the control group than those in the treatment group. However, the difference is statistically insignificant at p<0.05 level (see Table 3.6).

Figure 3.6: Revision of actual college choices toward college majors associated with higher relative wages: perceived vs actual wage differences



Notes: the figure shows share of the students who revised their choice toward majors associated with higher relative wages in the control (blue bars), spillover (red bars), and treatment groups (green bars). Relative wage is defined as the difference between actual and perceived wages associated with actual and baseline intended major choices, respectively. Actual wages are the population mean earnings given in Table 3.3, while the perceived wages are measured in the baseline survey before the provision of the information. The vertical bar shows the share of students whose revision behavior satisfies the following condition:

$$\Delta W_{Actual} - \Delta W_{Perceived} > 0,$$

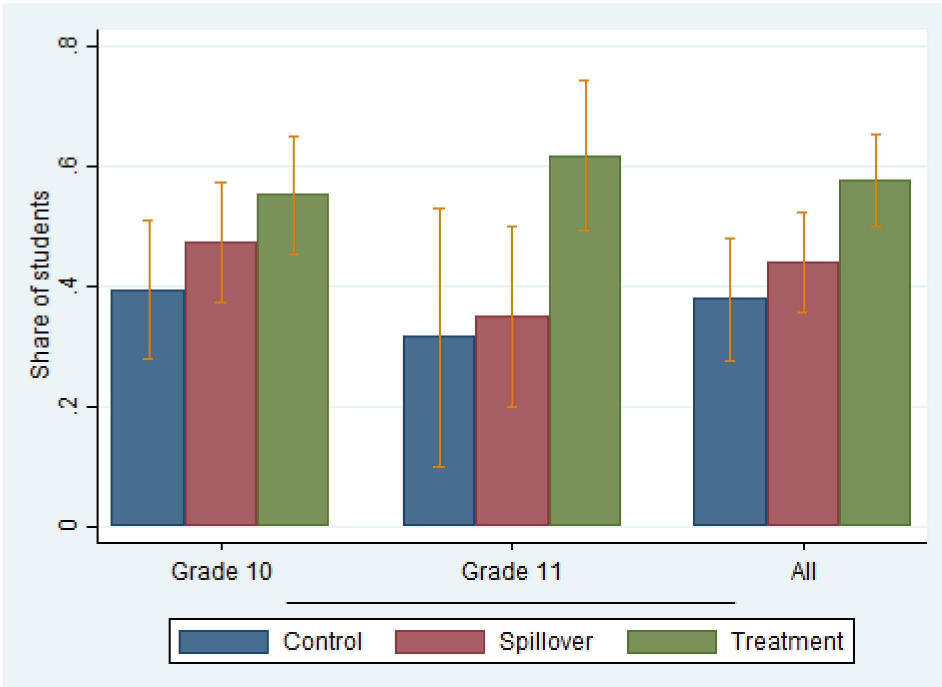
where W stands for the wage and the differences between actual and perceived wages are defined as follows:

$$\Delta W_{Actual} = \frac{W_{Actual}\,(Actual\ Major) - W_{Actual}\,(Baseline\ Major)}{W_{Actual}\,(Baseline\ Choice)}$$

$$\Delta W_{Perceived} = \frac{W_{Perceived}\,(Actual\ Major) - W_{Perceived}\,(Baseline\ Major)}{W_{Perceived}\,(Baseline\ Major)}$$

The following mechanism explains the students' revision behavior - they learned that they would not be likely to face significantly lower earnings by changing their specialization as they had initially perceived. We find that a higher share of students follows this pattern in the treatment group than in the control group, however, this difference is insignificant (see Table 3.6).

Figure 3.7: Revision of actual choices toward college major choices associated with lower relative rates of unemployment: perceived vs actual unemployment rate differences



Notes: the figure shows share of students who revise their major toward those associated with lower relative unemployment rate in the control (blue bars), spillover (red bars), and treatment groups (green bars). Relative unemployment rate is defined as the difference between actual and perceived unemployment rates associated with the actual and intended baseline major choices respectively. Actual unemployment rates are the population unemployment rates given in Table 3.3, while the perceived unemployment rates are measured in the baseline survey before the provision of the information. The vertical bar shows the share of students whose revision behavior satisfies the following rule:

$$\Delta U_{Actual} - \Delta U_{Perceived} < 0,$$

where U stands for the unemployment rate and the differences between actual and perceived unemployment rates are defined as follows:

$$\Delta U_{Actual} = U_{Actual}\,(Actual\ Major) - U_{Actual}\,(Baseline\ Major)$$

$$\Delta U_{Perceived} = U_{Perceived}\,(Actial\ Major) - U_{Perceived}\,(Baseline\ Major)$$

We find that a higher share of students follows this pattern in the treatment group than those in the control group, i.e., perceived unemployment differences exceed the actual ones. The following mechanism explains the students' revision behavior - they learned that they would not sacrifice much of their employment opportunities by changing their specialization, as they had initially perceived. Overall, 20% more students revise toward lower unemployment rates in the treatment group than in the control group. This difference is statistically significant at p<0.01 level in all model specifications (see Table 3.7)

Table 3.1: Timeline of the experiment

	Round 1	Round 2	Round 3	
	Baseline Intended Choices *Information Intervention*	Revised Intended Choices	Actual Choices	
	April 2017	May 2017	September 2018	September 2019
	(1)	(2)	(3)	(4)
Grade 10	Yes	Yes	No	Yes
Grade 11	Yes	Yes	Yes	No
Total	2015	1913	543	587

Notes: Columns (1), (2), (3) and (4) report the number of student responses in rounds 1-3. Both tenth-grade and eleventh-grade students were surveyed in May and April 2017. In the baseline survey, twenty students either did not report any specialization choice or selected the ‘no university’ choice, thus we recorded 1,995 responses with stated college major choices. In the second round, we collected 1,913 revised intended college major choices. Revised intended choices are their updated intended choices. In the third round, we were able to follow-up 543 grade 10 and 587 grade 11 students (1,130 in total) and collect actual major choices.

Table 3.2: Summary statistics

Table 3.2.1: Comparison of the means in the *baseline sample*

	Control	Spillover	Treatment	F-test p-value
Age	16.17	16.15	16.21	0.29
	(0.67)	(0.67)	(0.67)	
% of male students	45.6	44.49	47.85	0.43
	(49.85)	(49.73)	(49.99)	
Number of brothers	0.65	0.68	0.62	0.39
	(0.89)	(0.74)	(0.72)	
Number of sisters	0.66	0.59	0.66	0.15
	(0.81)	(0.68)	(0.78)	
% of students having a tutor	79.08	78.5	81.61	0.31
	(40.71)	(41.12)	(38.77)	
% of students having a computer	90.75	91.71	91.92	0.75
	(29)	(28.1)	(27.26)	
Subjective ranking in the school	36.77	35.30	33.82	0.23
	(31.45)	(30.63)	(28.25)	
Beliefs about own earnings†	1,174.62	1,070.75	1,074.09	0.23
	(1,342)	(1,127)	(1,114)	
Class Size	17.55	14.61	15.83	0.24
	(6.6)	(14.61)	(7.51)	
Observations	744	672	579	

Notes: Standard deviations are in parentheses beneath mean estimates in columns (1)-(3); Column (4) reports the p-value of an F-test testing the null hypothesis that means are equal across control, spillover, and treatment groups. Data are from the baseline survey of tenth and eleventh grade students, conducted by the authors in April 2017. Control and spillover groups did not receive any information, the treatment group received earnings and unemployment figures. Treatment and spillover groups both represent the treated schools, hence the students from treatment groups could spread the information to their peers in the spillover group.

† Beliefs about their own potential earnings are measured in Georgian Lari, GEL and represent the student's expected monthly salaries after university graduation.

Table 3.2.2: Comparison of the means for major choices and beliefs in the *baseline sample*

Educational Attainment	Baseline Major Choice				Beliefs on Monthly Earnings (GEL)				Beliefs on Unemployment Rate (%)			
	Control	Spillover	Treatment	F-test p-value	Control	Spillover	Treatment	F-test p-value	Control	Spillover	Treatment	F-test p-value
No Uni. Education	NA	NA	NA	NA	373	373	392	0.32	47.51	45.19	45.10	0.04
					(339.45)	(220.23)	(232.78)		(18.48)	(18.84)	(19.18)	
Exact and Natural Sc.	14.51%	15.77%	14.78%	0.80	907	928	1,459	0.16	29.87	30.06	29.47	0.81
	(35.25)	(36.48)	(35.52)		(552.22)	(673.13)	(703.64)		(16.82)	(17.13)	(17.45)	
Medical Sciences	15.54%	15.03%	15.59%	0.95	1,336	1,482	1,459	0.02	24.52	24.96	24.72	0.89
	(36.26)	(35.76)	(36.3)		(878.26)	(1017.88)	(845.91)		(16)	(16.76)	(16.06)	
Econ. and Business	28.84%	28.87%	28.90%	1.00	1,627	1,682	1,760	0.08	27.21	27.38	28.01	0.67
	(45.34)	(45.35)	(45.36)		(928.24)	(1099.79)	(853.15)		(17.16)	(17.5)	(17.26)	
Social Sciences	7.77%	9.52%	7.39%	0.31	1,176	1,206	1,248	0.29	30.61	30.18	29.38	0.44
	(26.8)	(29.38)	(26.18)		(762.68)	(842.03)	(853.15)		(17.51)	(18.03)	(17.8)	
Art and Humanities	13.82%	14.14%	16.40%	0.34	808	832	878	0.05	35.02	33.54	34.34	0.39
	(34.54)	(34.87)	(37.05)		(509.77)	(482.58)	(576.33)		(19.31)	(18.04)	(19.41)	
Law	19.52%	16.67%	16.94%	0.35	1,515	1,498	1,635	0.02	30.11	28.30	28.82	0.22
	(39.67)	(37.3)	(37.53)		(928.17)	(934.68)	(1023.75)		(18.88)	(18.49)	(18.42)	
Obs.	579	672	744		534	649	723		564	666	733	

Notes: Standard deviations are in parentheses beneath mean estimates in columns (1)-(3), (5)-(7) and (9)-(11); Columns (4), (8) and (12) report p-values for a F-test testing the null hypothesis that the means are equal for all three groups. Data are from the baseline survey of tenth and eleventh grade students, conducted by the authors in April 2017.

Table 3.2.3: Comparison of the means for the parental education in the *baseline sample*

Educational Attainment	Mother's Education				Father's Education			
	Control	Spillover	Treatment	F-test p-value	Control	Spillover	Treatment	F-test p-value
No Uni. Education	6.22%	6.11%	5.94%	0.98	1.58%	3.93%	3.51%	0.04
	(24.17)	(23.97)	(23.65)		(12.48)	(19.44)	(18.42)	
Exact and Natural Sc.	11.05%	11.33%	11.34%	0.98	32.28%	26.74%	28.11%	0.09
	(31.38)	(31.72)	(31.72)		(46.8)	(44.29)	(44.98)	
Medical Sciences	24.18%	25.34%	26.45%	0.64	5.26%	4.98%	6.08%	0.64
	(42.85)	(43.53)	(44.14)		(22.35)	(21.78)	(23.91)	
Econ. and Business	17.79%	18.93%	14.71%	0.09	22.63%	22.51%	22.03%	0.96
	(38.28)	(39.2)	(35.44)		(41.88)	(41.79)	(41.47)	
Social Sciences	12.78%	11.18%	14.98%	0.10	19.65%	21.00%	18.65%	0.54
	(33.42)	(31.53)	(35.71)		(39.77)	(40.76)	(38.98)	
Art and Humanities	24.18%	22.95%	19.70%	0.12	8.07%	8.46%	8.38%	0.97
	(42.85)	(42.08)	(39.8)		(27.26)	(27.85)	(27.73)	
Law	3.97%	4.17%	6.88%	0.02	10.53%	12.39%	13.24%	0.32
	(19.55)	(20.01)	(25.33)		(30.72)	(32.97)	(33.92)	
Obs.	579	671	741		570	662	740	

Notes: Standard deviations are in parentheses beneath mean estimates in columns (1)-(3), (5)-(7); Columns (4) and (8) report p-values for a F-test testing the null hypothesis that the means are equal for all three groups. Data are from the baseline survey of tenth and eleventh grade students, conducted by the authors in April 2017.

Table 3.2.4: Comparison of the means: school characteristics

School Characteristics	Control Schools	Treated Schools	t-test *p-value*
Total number of students	1693.97	1553.33	0.58
	(671.62)	(482.83)	
Total number of teachers	106	98	0.60
	(44.11)	(26.8)	
% of Schools located in the city center	33.12	38.08	0.75
	(18.37)	(39.04)	
Class size	21.95	18.60	0.17
	(13.58)	(8.81)	
Observations	7	15	

Notes: Standard deviations are in parentheses beneath mean estimates in columns (1)-(2); Column (3) reports p-values for a t-test testing the null hypothesis that the means are equal for all three groups. The data cover the schools where the experiment was carried out in April 2017. The data on the total number of students and teachers were retrieved from the website of the Georgia Ministry of Education in 2017. The data on locations and class sizes were collected by the authors.

Table 3.3: Actual vs perceived earnings and unemployment rates in the *baseline sample*

Educational Attainment	Mean Earnings			Unemployment Rate			Expected Earnings		
	Actual	Belief	Bias	Actual	Belief	Bias	Actual	Belief	Bias
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
No University Education	504	381	-25%	8.3%	46%	450%	462	206	-55%
Tertiary Education	802	1,280	60%	13.1%	29%	123%	697	921	31%
Exact and Natural Sciences	771	940	22%	12.6%	30%	137%	673	660	-2%
Medical Sciences	673	1,432	113%	10%	25%	149%	606	1,078	78%
Economics and Business	890	1,696	91%	19.2%	28%	43%	719	1,229	71%
Social Sciences	872	1,213	39%	13.3%	30%	125%	756	849	12%
Art and Humanities	654	843	29%	8.5%	34%	303%	599	554	-7%
Law	953	1,555	63%	15.1%	29%	92%	809	1,104	36%

Notes. Columns (1)-(2) report the actual and perceived mean monthly earnings in Georgia. Columns (4)-(5) report the actual and perceived unemployment rates. Columns (7)-(8) report the expected monthly earnings calculated as the product of mean monthly earnings and employment rates. Employment rates are calculated as one minus the unemployment rate. Both actual and perceived earnings are given in Georgian Lari, and the average exchange rate in 2017 was approximately $1=2.4 GEL. Mean monthly earnings and unemployment rates for individuals with tertiary education are calculated as the weighted average earnings and unemployment rates of individuals having a degree in one of the majors: exact and natural sciences, medical sciences, economics and business administration, social sciences, arts and humanities, and law. Columns (3), (6) and (9) calculate the difference between the perceived and actual figures in percentage terms. The bias is calculated as follows: $Bias = \frac{Belief - Actual}{Actual} * 100$. Actual earnings and unemployment rates are calculated using the 2015 Household Survey conducted by the National Statistics Office of Georgia. For the calculation of earnings, we considered only full-time employees. Unemployment rates are defined in line with the International Labor Organization (ILO) strict criteria (see page 6). https://ilo.org/wcmsp5/groups/public/---dgreports/---stat/documents/publication/wcms_675155.pdf

Table 3.4: Revision of intended college major choices

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.11***	0.13***	0.10***	0.12***	0.10***	0.16***
	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.04)
Spillover	0.04**	0.09***	0.04*	0.09***	0.05**	0.13***
	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.04)
Grade10		0.07**	0.04**	0.10***	0.04**	0.13***
		(0.03)	(0.02)	(0.02)	(0.02)	(0.04)
Treatment ×		-0.04		-0.04		-0.09**
Grade10		(0.04)		(0.04)		(0.04)
Spillover ×		-0.08*		-0.10***		-0.13***
Grade10		(0.04)		(0.03)		(0.04)
Covariates†	No	No	Yes	Yes	Yes	Yes
Constant	0.09***	0.05**	0.01	-0.01		
	(0.01)	(0.02)	(0.05)	(0.06)		
Observations	1,913	1,913	1,668	1,668	1,668	1,668
R^2	0.02	0.02	0.04	0.04		

Notes: (1)-(4) Linear probability models, (5)-(6) probit models (marginal effects). Sample: 10th and 11th grade students who reported their intended major choices. Dependent variable: categorical variable coded 1 if a student's revised intended major choice differs from her/his baseline intended college major choice. Standard errors in parentheses. Robust standard errors clustered by class for the linear probability models. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

† Covariates: gender, age, beliefs on population earnings and unemployment rate by specialization, beliefs about personal earnings upon university graduation, baseline intended specialization choice, number of siblings, a dummy variable indicating whether a student has a private tutor, beliefs about their own ranking, parental education, class size.

Table 3.5: Revision of actual college major choices

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.10***	0.14***	0.09**	0.14***	0.09**	0.16***
	(0.04)	(0.05)	(0.04)	(0.04)	(0.04)	(0.05)
Spillover	0.07*	0.11**	0.04	0.12***	0.04	0.14***
	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Grade10		0.30***	0.26***	0.35***	0.25***	0.36***
		(0.05)	(0.03)	(0.05)	(0.03)	(0.07)
Treatment ×		-0.07		-0.09		-0.13
Grade10		(0.07)		(0.07)		(0.08)
Spillover ×		-0.08		-0.15**		-0.17**
Grade10		(0.07)		(0.07)		(0.09)
Covariates†	No	No	Yes	Yes	Yes	Yes
Constant	0.29***	0.13***	0.25**	0.20**		
	(0.03)	(0.04)	(0.1)	(0.09)		
Observations	1,130	1,130	995	995	995	995
R^2	0.01	0.07	0.1	0.11		

Notes: (1)-(4) Linear probability models, (5)-(6) probit models (marginal effects). Sample: 10th and 11th grade students who reported their actual major choices. Dependent variable: categorical variable coded 1 if a student's actual major choice differs from her/his baseline intended college major choice. Standard errors in parentheses. Robust standard errors clustered by class for the linear probability models. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

† Covariates: gender, age, beliefs on population earnings and unemployment rate by specialization, beliefs about personal earnings upon university graduation, first round reported specialization choice, number of siblings, a dummy variable indicating whether a student has a private tutor, beliefs about their own ranking, parental education, class size.

Table 3.6: Determinants of actual college major choice revisions: wage, unemployment rate, expected earnings and relative wages

	Actual Wage		Actual Unemployment Rate		Actual Expected Earnings		Relative Wage	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	-0.07	-0.1	0.1	-0.02	-0.08	-0.05	-0.03	-0.12
	(0.06)	(0.13)	(0.06)	(0.13)	(0.06)	(0.13)	(0.07)	(0.13)
Spillover	0.01	0.05	0.01	-0.15	-0.01	0.05	0.05	-0.11
	(0.07)	(0.14)	(0.07)	(0.14)	(0.07)	(0.14)	(0.07)	(0.14)
Grade10		-0.08		-0.12		0.01		-0.17
		(0.13)		(0.13)		(0.13)		(0.13)
Treatment × Grade10		0.03		0.16		-0.05		0.09
		(0.15)		(0.15)		(0.15)		(0.15)
Spillover × Grade10		-0.06		0.21		-0.08		0.2
		(0.16)		(0.16)		(0.16)		(0.16)
Constant	0.49***	0.55***	0.46***	0.55***	0.51***	0.50***	0.59***	0.72***
	(0.05)	(0.11)	(0.05)	(0.11)	(0.05)	(0.11)	(0.05)	(0.12)
Observations	396	396	396	396	396	396	372	372
R^2	0	0.01	0.01	0.01	0.01	0.01	0.01	0.01

Notes: Linear probability models. Sample: 10^{th} and 11^{th} grade students whose actual major choices differ from their baseline intended major choices. Dependent variable: (1)-(2) a categorical variable coded 1 if a student changed her/his specialization choice toward one associated with a higher wage, and 0 otherwise; (3)-(4) a categorical variable coded 1 if a student changed her/his specialization choice toward one associated with a lower unemployment rate, and 0 otherwise; (5)-(6) a categorical variable coded 1 if a student changed her/his specialization choice toward one associated with a higher expected earning, and 0 otherwise; (7)-(8) a categorical variable coded 1 if a student changed her/his specialization choice toward one associated with a relatively higher wage, and 0 otherwise. Standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table C1.2 displays the same analysis with the dependent variable being *the differences* in actual wages, unemployment rates, expected earnings and relative wages. Appendix Table C1.3 displays the same analysis with the dependent variable being *the percentage differences* in actual wages, unemployment rates, expected earnings and relative wages.

Table 3.7: Determinants of the actual college major choice revisions: relative unemployment rate

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.20***	0.30**	0.24***	0.40***	0.06***	0.05*	0.08***	0.07**
	(0.07)	(0.13)	(0.06)	(0.12)	(0.02)	(0.03)	(0.02)	(0.03)
Spillover	0.06	0.03	0.10*	0.06	0.01	-0.01	0.01	-0.02
	(0.07)	(0.14)	(0.06)	(0.14)	(0.02)	(0.03)	(0.02)	(0.03)
Grade10		0.08	0.14	0.22		-0.02	0	-0.03
		(0.13)	(0.1)	(0.14)		(0.03)	(0.02)	(0.02)
Treatment × Grade10		-0.14		-0.24*		0.01		0.02
		(0.15)		(0.13)		(0.04)		(0.04)
Spillover × Grade10		0.05		0.07		0.04		0.06
		(0.16)		(0.14)		(0.04)		(0.04)
Covariates†	No	No	Yes	Yes	No	No	Yes	Yes
Constant	0.38***	0.32***	0.04	-0.06				
	(0.05)	(0.11)	(0.21)	(0.23)				
Observations	385	385	334	334	1,119	1,119	995	995
R^2	0.03	0.03	0.18	0.2				

Notes: (1)-(4) Linear probability models. Sample: 10th and 11th grade students whose actual major choices differ from the baseline intended major choices. Dependent variable: a categorical variable coded 1 if a student changed her/his specialization choice toward one associated with a *relatively* lower unemployment rate, and 0 otherwise. Standard errors in parentheses: robust standard errors clustered by school. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

(5)-(8) Ordered probit models, marginal effects for switching toward lower relative unemployment rate. Sample: 10th and 11th grade students who reported their actual major choices. Dependent variable: a categorical variable coded 1 if a student changed her/his specialization choice toward one associated with *a relatively lower unemployment rate*, -1 if a student changed her/his specialization toward one associated with a relatively higher unemployment rate, and 0 if a student did not change her/his specialization. The table reports marginal effects only for switching toward a major associated with lower relative unemployment rate

† Covariates: gender, age, beliefs about personal earnings upon university graduation, first round reported specialization choice, number of siblings, a dummy variable indicating whether a student has a private tutor, beliefs about their own ranking, parental education, class size.

Appendix C

Appendix C1

Table C1.1: Offered places and demand for college majors

College Major	Offered Places	1st choice	1st choice (%)
	(1)	(2)	(3)
Exact and Natural Sciences	10,868	9,550	23.56%
Medical Sciences	2,917	3,264	8.05%
Economics and Business	14,575	8,807	21.73%
Social Sciences	4,267	2,314	5.71%
Arts and Humanities	10,955	11,413	28.16%
Law	6,121	5,182	12.79%
Total	49,703	40,530	

Notes: The table shows the supply of each specialization (offered places) by accredited universities in Georgia and the demand for each major (first desired choice stated by the applicants) in 2017. Column (1) reports the maximum number of places offered by the accredited universities in Georgia. Column (2) reports the number of applicants willing to continue their studies with a given major choice. Column (3) reports the demand for each major in percentage terms. Note that top ranked universities are highly selective and competition is high, although the overall number of offered places exceed the demand. Columns (1) and (2) are constructed based on the information provided by NAEC.

Table C1.2: Determinants of actual college major choice revisions: differences in wages, unemployment rates, expected earnings and relative wages

	Actual Wage		Actual Unemployment Rate		Actual Expected Earnings		Relative Wage	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	-10.92	17.72	-0.94	0.72	-1.51	10.45	-108.02	47.15
	(22.46)	(44.9)	(0.81)	(1.63)	(14.59)	(29.17)	(167.02)	(336.34)
Spillover	25.71	63.68	0.09	1.79	21.8	41.08	109.98	155.44
	(23.16)	(47.24)	(0.84)	(1.71)	(15.04)	(30.69)	(171.77)	(355.21)
Grade10		39.24		1.74		20.46		242.57
		(43.82)		(1.59)		(28.47)		(329.93)
Treatment × Grade10		-35.68		-2.19		-13.9		-182.64
		(52.13)		(1.89)		(33.87)		(389.87)
Spillover × Grade10		-49.38		-2.22		-25		-34.03
		(54.33)		(1.97)		(35.3)		(406.7)
Constant	-3.88	-34.77	0.48	-0.89	-7.15	-23.26	246.06*	53.68
	(17.89)	(38.88)	(0.65)	(1.41)	(11.62)	(25.26)	(133.36)	(293.82)
Observations	396	396	396	396	396	396	372	372
R^2	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01

Notes: OLS in all columns. Sample: 10th and 11th grade students whose actual major choices differ from the baseline major choices. Dependent variable: (1)-(2) the actual wage difference between the actual and baseline major choices; (3)-(4) the actual unemployment rate difference between the actual and baseline major choices; (5)-(6) the actual expected earnings difference between the actual and baseline major choices; (7)-(8) the relative wage difference between the actual and baseline major choices. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table C1.3: Determinants of actual college major choice revisions: percentage differences in wages, unemployment rates, expected earnings and relative wages

	Actual Wage		Actual Unemployment Rate		Actual Expected Earnings		Relative Wage	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	-1.22	2.65	-4.2	4.95	-0.31	1.88	-8.2	-9.4
	(2.92)	(5.84)	(6.71)	(13.41)	(2.14)	(4.28)	(16.15)	(32.59)
Spillover	3.23	8.04	1.94	10	3.08	6.27	13.18	10.93
	(3.02)	(6.15)	(6.92)	(14.11)	(2.21)	(4.5)	(16.61)	(34.42)
Grade10		5.62		11.22		3.63		-2.61
		(5.7)		(13.09)		(4.18)		(31.97)
Treatment × Grade10		-4.74		-11.72		-2.57		1.2
		(6.79)		(15.57)		(4.97)		(37.78)
Spillover × Grade10		-6.17		-10.11		-4.09		2.87
		(7.07)		(16.23)		(5.18)		(39.41)
Constant	0.32***	0.32***	0.32***	0.32***	0.32***	0.32***	0.32***	0.32***
	(2.33)	(5.06)	(5.34)	(11.61)	(1.7)	(3.71)	(12.9)	(28.47)
Observations	396	396	396	396	396	396	372	372
R^2	0.01	0.01	0	0	0.01	0.01	0.01	0.01

Notes: OLS in all columns. Sample: 10^{th} and 11^{th} grade students whose actual major choices differ from their baseline major choices. Dependent variable: (1)-(2) the actual wage difference (in %) between the actual and baseline intended major choices; (3)-(4) the actual unemployment rate difference (in %) between the actual and baseline intended major choices; (5)-(6) the actual expected earnings difference (in %) between the actual and baseline intended major choices; (7)-(8) the relative wage difference in (%) between the actual and baseline intended major choices. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table C1.4: Do baseline beliefs predict the changes in the major choices?

	Actual major choices		Baseline intended major choices	
	(1)	(2)	(3)	(4)
Treatment	0.0818*	0.0927*	0.14***	0.12***
	(0.05)	(0.05)	(0.03)	(0.02)
Spillover	0.03	0.00	0.07**	0.06***
	(0.05)	(0.06)	(0.03)	(0.02)
Unemp. Bias[a]	0.000	0.001	0.002**	0.002*
	(0.00)	(0.00)	(0.00)	(0.00)
Treatment ×	0.001	-0.000	-0.002*	-0.002
Unemp. Bias	(0.00)	(0.00)	(0.00)	(0.00)
Spillover ×	0.003	0.003	-0.002	-0.002
Unemp. Bias	(0.00)	(0.00)	(0.00)	(0.00)
Covariates[b]	No	Yes	No	Yes
Constant	0.2843***	0.2776***	0.063***	0
	(0.03)	(0.1)	(0.02)	(0.05)
Observations	1,108	995	1881	1668
R^2	0.01	0.11	0.02	0.04

Notes: Linear probability models. (1)-(2) Sample: 10th and 11th grade students who reported their actual major choices. The dependent variable: a categorical variable coded 1 if a student's actual major choice differs from the baseline intended choice. (3)-(4) Sample: 10th and 11th grade students who reported their intended major choices. Dependent variable: a categorical variable coded 1 if a student's intended major choice differs from the baseline one. Robust standard errors clustered by class in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

[a] *Unemp. Bias*: variable Unemployment bias is defined as a difference between the perceived and true unemployment rate (in percentage points) for the baseline college major. Note that the beliefs were elicited before the leaflet was provided. *Unemp. Bias* × *Treatment* (*Unemp. Bias* × *Spillover)* stands for the interaction term between the unemployment bias and the Treatment (Spillover) dummy variable.

[b] Covariates: gender, age, beliefs about personal earnings upon university graduation, baseline college major choice, number of siblings, a dummy variable indicating whether a student has a private tutor, beliefs about their own ranking, parental education, class size.

Table C1.5: Comparison of the means in the *attrition sample*

Educational Attainment	Baseline Intended Major Choice				Beliefs on Monthly Earnings (GEL)				Beliefs on Unemployment Rate (%)			
	Control	Spillover	Treatment	F-test p-value	Control	Spillover	Treatment	F-test p-value	Control	Spillover	Treatment	F-test p-value
No Uni. Education	NA	NA	NA	NA	350	374	356	0.35	48.28	43.89	44.32	0.01
					(214.22)	(212.01)	(187.01)		(17.81)	(19.47)	(19.55)	
Exact and Natural Sc.	14.79%	15.79%	14.86%	0.93	832	884	1,288	0.50	30.08	29.47	27.73	0.21
	(35.57)	(36.53)	(35.63)		(469.2)	(612.43)	(621.19)		(16.92)	(16.83)	(16.58)	
Medical Sciences	15.95%	15.09%	15.48%	0.96	1,205	1,405	1,288	0.03	24.17	24.10	24.32	0.99
	(36.69)	(35.86)	(36.23)		(768.64)	(977.05)	(739.62)		(15.24)	(16.38)	(16.09)	
Econ. and Business	29.18%	28.07%	28.79%	0.96	1,571	1,695	1,593	0.30	27.15	27.81	27.62	0.90
	(45.55)	(45.01)	(45.35)		(894.95)	(1062.39)	(810.97)		(16.82)	(18.15)	(16.85)	
Social Sciences	7.78%	10.18%	6.81%	0.31	1,095	1,121	1,137	0.82	30.00	30.32	28.90	0.58
	(26.84)	(30.29)	(25.23)		(726.45)	(773.9)	(810.97)		(16.86)	(17.79)	(18.19)	
Art and Humanities	13.62%	14.04%	17.03%	0.44	774	785	835	0.31	35.14	33.01	32.51	0.23
	(34.37)	(34.8)	(37.65)		(493.33)	(407.05)	(588.93)		(19.88)	(17.77)	(19.29)	
Law	18.68%	16.84%	17.03%	0.83	1,405	1,431	1,504	0.37	30.10	27.43	27.38	0.16
	(39.05)	(37.49)	(37.65)		(838.48)	(819.37)	(933.14)		(19.32)	(18.88)	(18.61)	
Obs.	257	285	323		231	273	316		252	283	320	

Notes: Sample: 10th and 11th grade students who were present in the baseline survey but have not reported their actual college major choices. Standard deviations are in parentheses beneath mean estimates in columns (1)-(3), (5)-(7) and (9)-(11); Columns (4), (8) and (12) report p-values for a F-test testing the null hypothesis that the means are equal for all three groups. Data are from the survey of tenth and eleventh grade students who were present in the baseline survey round, but have not reported their actual choices throughout the final stage of the survey.

Table C1.6: Revision of intended college major choices in the *actual choices sample*

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.06***	0.06*	0.05**	0.05	0.05**	0.06
	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.04)
Spillover	0.01	0.06*	0.00	0.06**	0	0.07*
	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.04)
Grade10		0.07**	0.05**	0.08**	0.05***	0.09**
		(0.03)	(0.02)	(0.03)	(0.02)	(0.04)
Treatment ×		0.02		0.02		-0.01
Grade10		(0.04)		(0.05)		(0.05)
Spillover ×		-0.10**		-0.10**		-0.12**
Grade10		(0.04)		(0.04)		(0.05)
Covariates [a]	No	No	Yes	Yes	Yes	Yes
Constant	0.06***	0.03	0.00	-0.01		
	(0.02)	(0.02)	(0.05)	(0.06)		
Observations	1,130	1,130	995	995	995	995
R^2	0.01	0.02	0.04	0.05		

Notes: (1)-(4) Linear probability models, (5)-(6) probit models (marginal effects). Sample: 10th and 11th grade students who reported their actual major choices. Note that this table is identical to Table 3.4 with the difference of the sample. This table analyzes the intended choices of the students who reported their college major choices on all three occasions: in the *baseline* survey*; intended choice*s survey and *actual choices* survey. Dependent variable: categorical variable coded 1 if a student's revised intended major choices differs from her/his baseline intended major choice. Robust standard errors in parentheses. Robust standard errors clustered by class for the linear probability models. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

[a] Covariates: gender, age, beliefs on population earnings and unemployment rate by specialization, beliefs about personal earnings after university graduation, baseline intended specialization choice, number of siblings, having a private tutor, beliefs about their own ranking, parental education, class size.

Table C1.7: Is attrition correlated with the treatment or spillover?

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-0.01	-0.02	0.03	0.03	0.03	0.03
	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.06)
Spillover	-0.02	-0.03	0	0.03	0	0.03
	(0.03)	(0.04)	(0.04)	(0.05)	(0.03)	(0.05)
Grade10		0.10**	0.11***	0.13***	0.11***	0.13**
		(0.04)	(0.03)	(0.05)	(0.03)	(0.06)
Treatment ×		0.03		0		0
Grade10		(0.06)		(0.06)		(0.08)
Spillover ×		0.01		-0.05		-0.05
Grade10		(0.06)		(0.06)		(0.07)
Covariates [a]	No	No	Yes	Yes	Yes	Yes
Constant	0.42***	0.36***	0.28***	0.27***		
	(0.02)	(0.03)	(0.07)	(0.07)		
Observations	1,913	1,913	1,668	1,668	1,668	1,668
R^2	0	0.01	0.05	0.05		

Notes: (1)-(4) Linear probability models, (5)-(6) probit models (marginal effects). Sample: 10th and 11th grade students who reported their revised intended major choices. Dependent variable: categorical variable coded 1 if a student's actual major choice is missing (attrition) and 0 otherwise. Robust standard errors in parentheses. Robust standard errors clustered by class for the linear probability models. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

[a] Covariates: gender, age, beliefs on population earnings and unemployment rate by specialization, beliefs about personal earnings after university graduation, baseline intended specialization choice, number of siblings, having a private tutor, beliefs about own ranking, parental education, class size.

Table C1.8. Comparison of means in the *actual choices sample*

Educational Attainment	Baseline Major Choice				Beliefs on Monthly Earnings (GEL)				Beliefs on Unemployment Rate (%)			
	Control	Spillover	Treatment	F-test p-value	Control	Spillover	Treatment	F-test p-value	Control	Spillover	Treatment	F-test p-value
No Uni. Education	NA	NA	NA	NA	392	373	351	0.17	46.89	46.15	45.70	0.70
					(409.45)	(226.29)	(225.62)		(19.01)	(18.33)	(18.89)	
Exact and Natural Sc.	14.29%	15.76%	14.73%	0.85	964	959	1,333	0.09	29.70	30.49	30.83	0.68
	(35.05)	(36.49)	(35.48)		(602.45)	(713.12)	(643.19)		(16.76)	(17.35)	(18)	
Medical Sciences	15.22%	14.99%	15.68%	0.96	1,436	1,537	1,333	0.01	24.80	25.60	25.03	0.80
	(35.97)	(35.74)	(36.4)		(942.42)	(1044.31)	(778.07)		(16.61)	(17.03)	(16.06)	
Econ. and Business	28.57%	29.46%	28.98%	0.97	1,670	1,673	1,577	0.33	27.26	27.07	28.31	0.56
	(45.25)	(45.64)	(45.42)		(952.04)	(1127.48)	(733.42)		(17.46)	(17.02)	(17.59)	
Social Sciences	7.76%	9.04%	7.84%	0.77	1,237	1,267	1,113	0.02	31.11	30.07	29.74	0.58
	(26.8)	(28.72)	(26.91)		(784.89)	(884.21)	(733.42)		(18.03)	(18.23)	(17.51)	
Art and Humanities	13.98%	14.21%	15.91%	0.71	833	866	756	0.01	34.92	33.94	35.76	0.40
	(34.73)	(34.96)	(36.62)		(521.31)	(528.67)	(454.54)		(18.87)	(18.26)	(19.41)	
Law	20.19%	16.54%	16.86%	0.38	1,600	1,547	1,446	0.10	30.12	28.94	29.95	0.64
	(40.2)	(37.2)	(37.49)		(984.17)	(1008.42)	(912.47)		(18.56)	(18.2)	(18.21)	
Obs.	322	387	421		303	376	407		312	383	413	

Notes: Sample: 10th and 11th grade students who reported their actual major choices. Standard deviations are in parentheses beneath mean estimates in columns (1)-(3), (5)-(7) and (9)-(11); Columns (4), (8) and (12) report p-values for a F-test testing the null hypothesis that the means are equal for all three groups. Data are from the survey of tenth and eleventh grade students who reported their actual major choices.

Appendix C2

Appendix C2.1: College major fields (as seen by respondents)

1. Exact and Natural Sciences: Mathematics, Computer Science, Physics, Chemistry, Biology, Biochemistry, Geography, Geology, Ecology, Electrical and Mechanical Engineering, Transportation, Agriculture.

2. Medical Sciences: Medicine, Pharmacy, Dentistry, Public Health.

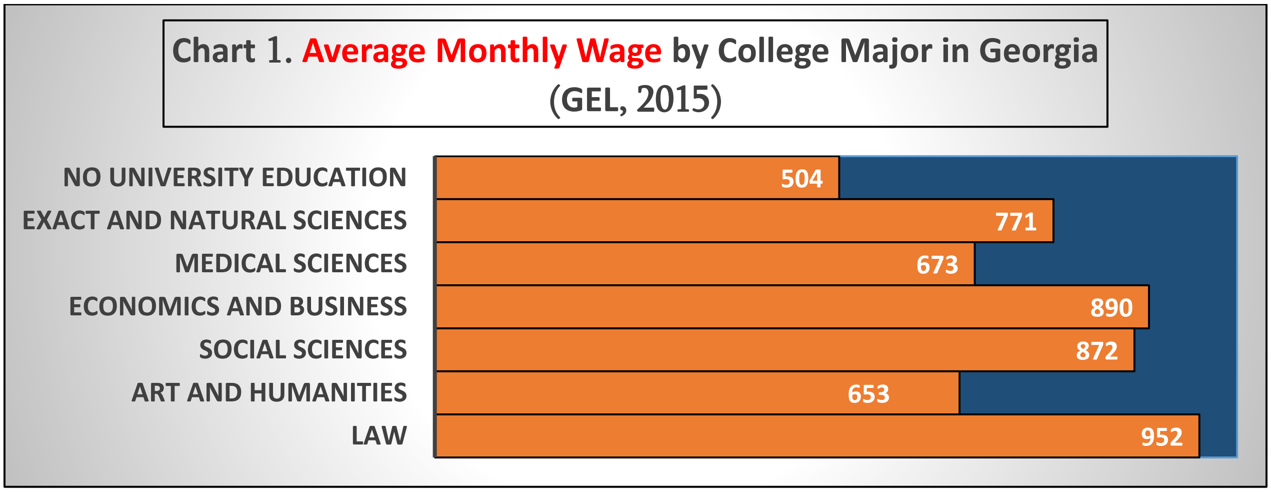
3. Economics and Business: Economics, Business Administration, Tourism, Management, Marketing, Accounting.

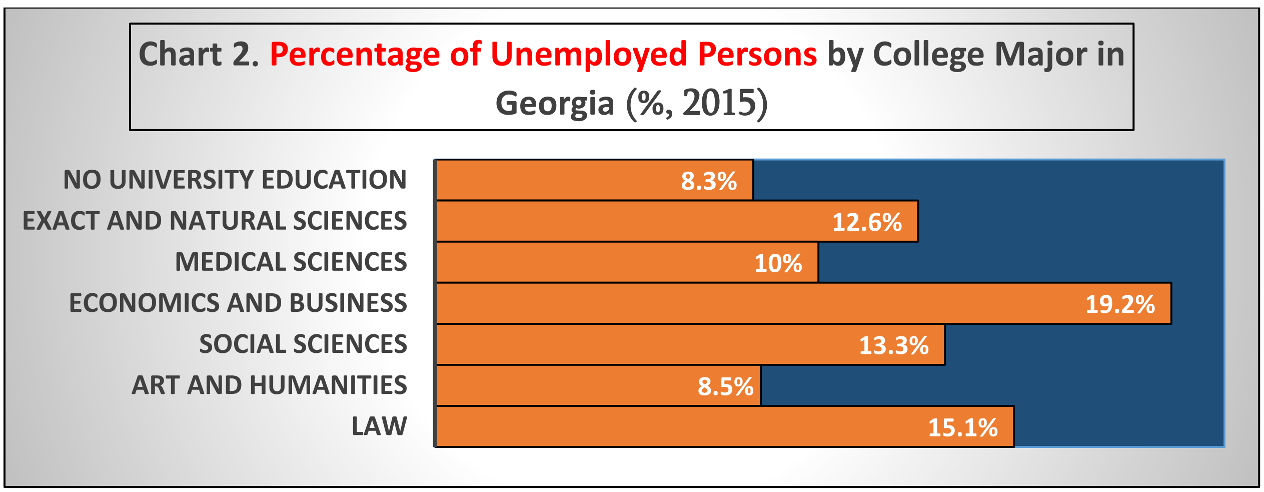
4. Social Sciences: Sociology, Politics, Journalism, Media and Communication, Political Studies, International Relations.

5. Art and Humanities: Philosophy, History, Archeology, Ethnology, Cultural Studies, Art History, Language Studies, Pedagogical Studies, Sports, Drama, Choreography.

6. Law: International Law, Public Law, Criminal Law, Civil Law.

Appendix C2.2: Information leaflet[1]





[1] All the information is based on data retrieved from the Georgian National Statistical Office (2015). This leaflet was translated from Georgian by the authors.

Information about the Charts on the Leaflet

Chart 1 Shows average monthly wages of full-time employed persons for each college major in Georgia

Chart 1 should be read as follows:

- In Georgia, full-time employed persons with ***no university degree***, on average, earn ***504 GEL per month***
- In Georgia, full-time employed persons with a ***university diploma in exact and natural sciences***, on average, earn ***771 GEL per month***
- In Georgia, full-time employed persons with ***a university diploma in medical sciences***, on average, earn ***673 GEL per month***
- In Georgia, full-time employed persons with a ***university diploma in economics and business administration***, on average, earn ***890 GEL per month***
- In Georgia, full-time employed persons with a ***university diploma in social sciences***, on average, earn ***872 GEL per month***
- In Georgia, full-time employed persons with a ***university diploma in art and humanities***, on average, earn ***654 GEL per month***
- In Georgia, full-time employed persons with a ***university diploma in law***, on average, earn ***952 GEL per month***

Chart 2 the percent of unemployed persons by university major in Georgia

An unemployed person is defined as a person aged 15 or older, who:

a) has not been employed during a given week

b) has actively sought employment in the prior four weeks

c) is available to start a job within next two weeks

Chart 2 should be read as follows**:**

- In Georgia, ***8.3%*** of persons with ***no university degree*** are unemployed
- In Georgia, ***12.6%*** of persons with a ***university diploma in exact and natural sciences*** are unemployed
- In Georgia, ***10%*** of persons with a ***university diploma in medical sciences*** are unemployed
- In Georgia, ***19.2%*** of persons with a ***university diploma in economics and business administration*** are unemployed
- In Georgia, ***13.3%*** of persons with a ***university diploma in social sciences*** are unemployed
- In Georgia, ***8.5%*** of persons with a ***university diploma in art and humanities*** are unemployed
- In Georgia, ***15.1%*** of persons with a ***university diploma in law*** are unemployed

Appendix C2.3: Survey Questionnaire[2]

<u>Round 1 (Baseline Intended College Major Choices)</u>

Please read each question and respond carefully. Depending on your response to Question 2, we will provide you with detailed information on:

- The university admission process for the college major *of your choice*
- The competitiveness of the college major *of your choice*
- Any international exchange programs available for the college major *of your choice*

There are four questions. Please respond to all the questions. If any question is unclear, please raise your hand.

1. Are you planning to apply to a university upon graduating from high school?

    ☐ Yes
    ☐ No

The following questions pertain to your college major choice. A detailed description of each major can be found in Appendix C2.1. From the list below, please select your top desired college major choice. Note that based on your college major choice, we will provide you with detailed information on the university admission process, competitiveness (number of applications vs offered places) and availability of exchange programs.

2. Please select your top desired college major from the list below. Please select only one major.

    ☐ Exact and Natural Sciences
    ☐ Medical Studies
    ☐ Economics and Business Administration
    ☐ Social Sciences
    ☐ Arts and Humanities
    ☐ Law

The following questions pertain to **your opinions** about earnings and unemployment for each major.

[2] Translated from Georgian by the authors.

3. In your opinion, among all individuals with a university diploma, what is the average amount that you believe these workers currently earn per month from full-time hired employment?

   <u>Example</u>: In Georgia, a person with a university diploma in medical studies earns, on average, X GEL per month from full-time hired employment.

College Major	**Average Monthly Salary from Full-time Hired Employment in Georgia (GEL)**
Exact and Natural Sciences	
Medical Studies	
Economics and Business Administration	
Social Sciences	
Arts and Humanities	
Law	

1. In your opinion, among all individuals with a university diploma, what is the percentage of unemployed individuals for each specialization?
   An unemployed person is defined as a person aged 15 or older, who:
      a) has not been employed during a given week
      b) has actively sought employment in the prior four weeks
      c) is available to start a job within next two weeks

   <u>Example</u>: In Georgia, X% of persons with a university diploma in arts and humanities is unemployed.

College Major	**Unemployment Rate (%)**
Exact and Natural Sciences	
Medical Studies	
Economics and Business Administration	
Social Sciences	
Arts and Humanities	
Law	

Post-Experimental Questionnaire

1. Do you have a laptop or personal computer at home?

   ☐ Yes
   ☐ No

2. How many siblings do you have?

   Number of Sister(s): ________ Number of Brother(s): ________

3. What college major does your father have?

   ____

4. What college major does your mother have?

   ____

5. Which district of Tbilisi do you live in?

   ____

6. Are you or your family considering hiring a tutor for your preparation for the Unified National Exams?

   ☐ Yes
   ☐ No

7. In this question, ranking is measured by a number from 1 to 100, with 1 indicating the highest rank and 100 indicating the lowest rank.

   On a ranking scale of 1-100, where do you think you would rank in terms of your scores from the Unified National Exams when compared to all individuals applying to university that year?

   ____

8. Imagine you just graduated from your desired major and you were working full time. What do you believe is the average amount in GEL that you would earn per month from full-time hired employment?

   Example: You believe that right after university graduation, you would earn X GEL from hired employment.

   ____

Round 2 (Revised Intended College Major Choices)

Please read each question and respond carefully. Depending on your responses, we will provide you with detailed information on:

- The student admission process at universities for the major
- Chances of being admitted for the major
- Exchange programs for the major
- Other relevant information

This questionnaire contains three questions. Please respond to all the questions. If any question is unclear, please raise your hand.

1. Are you planning to apply to a university upon graduating from high school?

   ☐ Yes
   ☐ No

The following questions pertain to your college major choice. A detailed description of each major can be found in Appendix C2.1. From the list below, please select/mark your top desired major. Note that based on your major choice, we will provide you with detailed information on the university admission process, the number of applicants and available places, availability of exchange programs, and other relevant information.

2. Have you discussed your future major choice with your parents over the last month?

   ☐ Yes
   ☐ No

3. Please select your top desired major from the list below. Please select only one major.

   ☐ Exact and Natural Sciences
   ☐ Medical Studies
   ☐ Economics and Business Administration
   ☐ Social Sciences
   ☐ Arts and Humanities
   ☐ Law

<u>Round 3 (Actual College Major Choices)</u>

The phone survey script

Hello Mr./Ms. *[Name]*. You participated in our survey on college major choices a year ago (two years ago for Grade 10s). Thank you for your participation. Would you have a few minutes to answer our questions?

1. Have you been admitted to a university?

   ☐ Yes
   ☐ No

2. What major are you going to study at university?

   ____

3. Is your current major choice different from your desired major choice[3]?

   ____

Thank you for your responses, your time is very much appreciated. We wish you good luck with your future studies!

[3] Note that desired college major choices are reported to the National Examination Centre before the exams and admission decision. Desired and current major choices may be different in case of insufficient exam scores.

Appendix C3

Determinants of College Major Choices (Supplementary Analysis)

Do students revise their majors toward majors associated with higher wages? Figure 3.3 displays the fraction of students who revised their actual major choices toward those associated with higher wages. The vertical bars represent the share of students for whom the real wage differences between the final and baseline specialization choices are positive. If higher wages were the driver for the college major changes, then one would expect that more students in the treatment and spillover groups would revise toward majors associated with higher wages. However, coefficient estimates in Table 3.6 show the specialization revision patterns across the final and baseline major choices are not explained by higher wages.[4] We also check whether the absolute or percentage differences in actual wages play a role – Appendix Tables C1.2 and C1.3 derive similar results. Next, we investigate the extent to which revisions are driven by differences in employment opportunities by major.

The revisions are not driven by the differences in the wages between the baseline and actual specialization choices. Therefore, changes in the college major choices cannot be explained by the wage differentials.

Do students revise their major choices toward majors associated with lower unemployment rates? Figure 3.4 displays the fraction of students who revised their actual major choices toward those associated with lower unemployment rates. The vertical bars represent the share of students for whom the real unemployment rate differences between the actual and baseline specialization choices are negative. If employment opportunities were the driver of the revisions in the majors, then more students in the treatment and spillover groups would revise their majors toward those associated with lower unemployment rates. Coefficient estimates in Table 3.6 suggest that more students in the treatment group revise toward majors associated with lower unemployment rate than in the control group, however the effect is insignificant. Do the absolute or percentage differences in unemployment rates explain the revisions? Appendix Tables C1.2 and C1.3 illustrate

[4] The coefficient estimates in column 2 is negative but insignificant at 5% level. Note that we do not find any significant effect of the actual wages on intended choice revisions. Thus, neither intended nor the actual major choice revisions are driven by differences in actual wages.

that neither percentage nor absolute differences in actual unemployment rates explain the changes in the specialization choices. Next, we investigate the extent to which revisions are driven by the differences in the expected earnings.

The revisions are not driven by the differences in the employment opportunities between the baseline and actual specialization choices. Therefore, changes in the specialization choices cannot be explained by the differences in unemployment rates.

Do students revise their major choices toward majors associated with higher expected earnings? Figure 3.5 displays the fraction of students who revised their actual major choices toward higher expected earnings. The vertical bars represent the share of students for whom the expected earning differences between the actual and baseline specialization choices are positive. We do not find any evidence of expected earnings explaining the change in the actual college major choices. Coefficient estimates in Table 3.6 derive similar results.[5] Thus, we conclude the expected earning differences between the two majors do not play a role. What are the other determinants, if actual wages and unemployment rates are not decisive for students when making their decisions? We explore the role of perceived actual wages and unemployment rates as a potential determinant. Next, we investigate the extent to which revisions are driven by the differences between perceived and actual wages.

The revisions are not driven by the differences in the expected earnings between the baseline and actual specialization choices. Therefore, changes in the college major choices cannot be explained by the expected earning differentials.

Do students revise their major choices toward majors associated with higher relative wages? Figure 3.6 displays the fraction of students who revised their actual major choices toward higher relative wages. Relative wage is defined as the difference between actual and perceived wages associated with the actual and baseline specialization choices, respectively. As an example, consider the students who revised their majors from economics and business (baseline) to exact and natural sciences (actual). Table 3.3 reports the actual and perceived wages of individuals with a degree in economics and business, 890 GEL and 1,696 GEL, respectively. In contrast, exact and natural sciences diploma holders actually earn 771 GEL, while students perceived that the wage

[5] Appendix Tables C1.2 and C1.3 show that the results remain the same in the alternative model specifications where the dependent variable is either the actual or percentage difference in the expected earnings.

was 940 GEL. Thus, students overestimated the cost of changing the specialization. In fact, one would only give up 119 GEL if choosing *exact and natural sciences* instead of *economics and business*. However, students in our sample perceived that the revision would be associated with a reduction in the wage of 756 GEL, much larger than the actual difference, 119 GEL.

Figure 3.6 shows the share of students whose revision behavior satisfies the following condition:

$$\Delta W_{Actual} - \Delta W_{Perceived} > 0,$$

where W stands for the average monthly wage and the differences between actual and perceived wages are defined as follows:

$$\Delta W_{Actual} = \frac{W_{Actual}\,(Actual\ Major) - W_{Actual}\,(Baseline\ Intended\ Major)}{W_{Actual}\,(Baseline\ Intended\ Major)}$$

$$\Delta W_{Perceived} = \frac{W_{Perceived}\,(Actual\ Major) - W_{Perceived}\,(Baseline\ Intended\ Major)}{W_{Perceived}\,(Baseline\ Intended\ Major)}$$

The following rationale explains the students' revision behavior - they learned that they did not have to sacrifice as much earnings by changing their specialization, as they perceived. If the differences between the actual and perceived earnings were the driver of the specialization choices, then one would expect that more students in the treatment and spillover groups would revise their majors toward higher relative wages compared to the students in the control group. Coefficient estimates in Table 3.6 suggest that this is not the case. Next, we investigate the extent to which revisions are driven by the differences between the perceived and actual unemployment rates.

The revisions are not driven by the differences between the perceived and actual wages across the baseline and actual major choices. Therefore, changes in the specialization choices cannot be explained by the differences between the perceived and actual wages.

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