You've Earned It: Estimating the Impact of Human Capital on Social Preferences

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Abstract

We combine data from a randomized evaluation and a laboratory experiment to measure the causal impact of human capital on respect for earned property rights, a component of social preferences with important implications for economic growth and development. We find that higher academic achievement reduces the willingness of young Kenyan women to appropriate others' labor income, and shifts players toward a 50-50 split norm in a modified dictator game. This study demonstrates that education may have long-run impacts on social preferences, norms and institutions beyond the human capital directly produced.

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1 Introduction

Social scientists have long sought to disentangle the relationship between formal education, cultural modernization, and economic development. In the African context, sociologists have argued that "Western" education is associated with the adoption of "modern" values including "independence from family and other traditional authority, belief in science and in man's ability to control his fate, and orientation toward the future" (Armer and Youtz 1971, p. 605). Inkeles (1969) constructs an index of individual modernity which aggregates independence from traditional sources of authority, openness to new experiences, belief in science and modern medicine, ambition, punctuality, and civic participation; he finds that educational attainment is the single most powerful predictor of a modern orientation in all six countries he studies.[1] More recently, Barro (1996) has shown that female education is the strongest long-term predictor of democracy. Many scholars have argued that these associations are driven by a casual link. For example, Mattes and Bratton (2007, p. 199) claim that education builds support for democratic institutions by "diffusing values of freedom, equality, and competition throughout the population," while Glaeser et al. (2004) argue that human capital gains are critical drivers of institutional change. However, whether schooling causes such changes in cultural values is an open question; it is also possible that those with an innately modern outlook choose to obtain more schooling, and the observed correlations result from sample selection. Thus, though researchers have identified a robust correlation between modern cultural values and industrialization (Inglehart and Baker 2000), the mechanisms through which such changes occur remain obscure.

In this paper, we provide evidence that academic achievement alters individual values, specifically social preferences governing the appropriation of others' income, as captured in an economic experiment. Our novel research design combines a randomized evaluation — specifically, the introduction of a scholarship program for girls in a random sample of Kenyan primary schools — with a lab experiment designed to measure respect for earned property rights. From a methodological perspective, ours is among the first studies to use

[1] See also Inkeles and Smith (1974). More generally, Easterlin (1981) argues that the introduction of mass primary education has preceded industrialization in most developed economies. Goldin and Katz (2008) trace out how the expansion of public education contributed to the economic and social transformation of U.S. society.

lab experimental methods to measure the impacts of a development intervention.[2] We argue that this setting provides cleaner identification of the link between education and social preferences than has previously been possible.

In 2001, the Dutch NGO ICS Africa introduced a scholarship competition for sixth grade girls (called the Girls Scholarship Program or GSP) in a random sample of primary schools in Busia District, in western Kenya; the program led to improvements of 0.2 to 0.3 standard deviations on standardized academic tests, relative to schools in the control group (Kremer et al. 2009). Our experimental subject pool comprises girls from the treatment and control schools in the scholarship program. The design allows us to identify the causal impact of academic achievement on social preferences using an instrumental variables approach, since assignment to a school in the scholarship program (treatment group) is unrelated to baseline characteristics such as cognitive ability and family background that might themselves affect social preferences.[3]

We measure the impact of academic achievement on social preferences in an experimental lab setting which allows us to turn off strategic considerations such as the fear of social sanctions. Economic experiments are a widely used tool for measuring cross-cultural differences in values, norms, and beliefs that are difficult to capture in survey data. In particular, dictator, ultimatum, and trust games have been conducted on every inhabited continent, with subject populations ranging from university students in the United States to hunter-gatherers in Tanzania (cf. Roth et al. 1991, Henrich et al. 2004).[4] Dictator games — in which one player (the "dictator") is provisionally allocated an amount of money, and decides how to divide it between *self* and another subject, *other* — measure the willingness to share in non-strategic settings, and have been used to measure the strength of egalitarian (or other) ideals underlying perceptions of what constitutes a "fair" distribution of income (cf. Forsythe et al. 1994, Cappelen et al. 2007, Barr et al. 2009).

[2]Barr et al. (2012) use public goods games to measure the impact of a school committee monitoring intervention in Uganda; while Fearon et al. (2009) use similar experiments to measure the impact of a post-conflict community development initiative in Liberia. Paluck and Green (2009) demonstrate that randomized experiments can be used to demonstrate the efficacy of policies explicitly intended to change cultural norms.

[3]Friedman et al. (2011) use a similar identification strategy to explore the impact of the GSP on political attitudes, knowledge, and behavior.

[4]See Henrich et al. (2010b) for an overview of the ways in which subjects in western university experimental labs are not representative of humanity in general.

We employ a variant of the dictator game designed to measure preferences governing the distribution of earned income — specifically, the willingness to appropriate *other*'s earnings. Hoffman et al. (1994) first used earned, rather than windfall, income in dictator games to generate an informal "property right"; they find that enhancing dictators' sense of entitlement via the earnings manipulation decreases generosity.[5] In contrast, our design increases the extent to which *other* has property rights over the budget: dictators in our experiment decide how to divide money that *other* was paid for completing a real effort task. Thus, our design intentionally separates the right to determine the final allocation — i.e. control rights, which Grossman and Hart (1986) define as property rights — from the "natural" but informal property rights proposed by Locke (1980[1690]), which result from generating something through one's own labor.[6] Our specific design measures how dictators treat those who have increased social surplus through their own effort.[7] The experiment was first proposed by Jakiela (2009), who reports that more educated Kenyan adults allocate significantly more to *other* (relative to the rest of the population) when deciding how to divide income earned by others, though not in other situations. The novel research design in the current paper, exploiting the random assignment of schools to the GSP treatment and control groups, allows us to determine whether this association is driven by the causal impacts of schooling on social preferences and beliefs about hard work.

We find that subjects drawn from the GSP treatment group have higher levels of academic performance (measured in terms of the primary school exit exam), and that they allocate significantly more to *other* in our modified dictator game. Point estimates suggest that a one standard deviation increase in academic test scores is associated with a 10 percentage point increase in the share of the budget allocated to *other*. Using data on subjects' expectations about the amount that dictators were likely to allocate to them, we show that our results are not driven by changes in beliefs: subjects drawn from the GSP

[5] Cherry (2001), Cherry et al. (2002), and List and Cherry (2008) conduct similar earnings treatments. Fahr and Irlenbusch (2000), Konow (2000), and Cappelen et al. (2007) also explore distributional preferences governing earned income.

[6] Building on Locke (1980[1690]), Gintis (2007) models "preinstitutional" property rights as the equilibrium result of the interaction between the endowment effect and possession. Following Fahr and Irlenbusch (2000), we refer to the entitlement effect generated by our design as an "earned property right."

[7] The design is quite similar to a trust game involving real effort rather than investment, except that receivers can only generate payoffs for themselves by "trusting" their labor income to the dictator.

treatment group do not expect that dictators will allocate them more. Hence, our findings can not be explained by changes in the beliefs of individuals holding identical (reciprocal) social preferences. We also report suggestive evidence from pilot experiments that girls in the GSP treatment group do not allocate more than control girls in a standard dictator game (involving unearned income). This suggests that academic success impacts the respect for earned property rights but not generalized altruism, a finding which is consistent with Jakiela (2009).

Our findings relate to recent evidence suggesting that the level of allocation to *other* observed in dictator games is strongly associated with the extent of market integration within a community (Henrich et al. 2004, Henrich et al. 2010a), though the underlying causal mechanism is not well understood. At the individual level, Almas et al. (2010) report that the tendency to reward others for hard work emerges during adolescence among Norwegian subjects: fifth graders participating in a dictator game preceded by a period of team production tended to favor egalitarian allocations, while older subjects were more inclined to base their allocation decisions on relative contributions to total output. Both Henrich et al. (2010a) and Almas et al. (2010) suggest that the fairness norms invoked in dictator games are not innate, but emerge over time through cognitive development and socialization. However, neither is able to identify a causal mechanism to explain how and why disparate cultural norms of fairness emerge where and when they do.

The project is also related to recent studies exploiting natural experiments to show how cultural values and norms evolve. Di Tella et al. (2007) demonstrate that the acquisition of formal land titles by squatters leads to the adoption of more market-oriented beliefs. Employing a methodology similar to ours, Fisman et al. (2009) combine a lab experiment with a natural experiment to show that random assignment of Yale law students to first year instructors trained in economics, rather than in law or humanities fields, leads to the adoption of distributional preferences which are both more selfish and more concerned with efficiency. In highlighting the extent to which life experiences shape individual preferences regarding property rights, our results are broadly consistent with both studies.

2 Research Design

2.1 Primary Education in Kenya

Since 1985, Kenya has had an educational system involving 8 years of primary school ("standards" 1 through 8) and 4 years of secondary school ("forms" 1 through 4). Admission to secondary school is contingent on the successful completion of a government exit exam, the Kenya Certificate of Primary Education (KCPE), at the end of standard 8. The KCPE is the equivalent of a primary school diploma, and the vast majority of students who complete standard 8 take the KCPE exam, whether or not they intend to continue on to secondary school.

Like many African countries, Kenya experienced large increases in educational attainment in the post-independence period. Between 1970 and the present, the adult literacy rate increased from 32 percent to 87 percent (UNDP 1993, 2013). Kenya instituted a policy of free primary education in 2003, and the gross primary enrollment ratio is now above 100 percent.[8] However, grade repetition is common, and more than a quarter of those who start primary school drop out before the end of standard 8 (UNDP 2013). Women have tended to lag behind men, particularly at higher levels of education: only 25 percent of Kenyan women over 25 completed secondary school, as compared with 52 percent of men (UNDP 2013). Since Kenyan children typically enter primary school at age 6 or 7 and frequently repeat grades, women are nearing marriageable age by the end of primary school; it is at this point that gender disparities in education begin to emerge.

Prior to the introduction of free primary education, parents of children in primary school had to pay school fees which averaged about 6.40 USD per year (Kremer et al. 2009). The revenue raised from school fees was used to pay for a range of educational inputs — for example, classroom maintenance and school supplies — which were not covered by the central government. These fees discouraged those not planning to attend secondary school from remaining in primary school and completing the KCPE exam.

[8] Prior to the introduction of free primary education, the gross primary enrollment rate was approximately 90 percent. See Lucas and Mbiti (2012) for an extended discussion of the abolition of school fees in Kenya.

2.2 The Girls' Scholarship Program (GSP)

The Girls' Scholarship Program (GSP) was an education initiative targeting adolescent girls who were enrolled in primary schools near Busia, Kenya, in 2000. The GSP was implemented by the Dutch NGO International Christian Support Africa (ICS) in 34 primary schools in Busia District. The aim of the program was twofold: to improve girls' academic performance by incentivizing effort, and to encourage girls to remain in school by defraying the costs (for those who won the scholarships). To that end, ICS organized a scholarship competition for girls enrolled in standard 6 in participating schools.

The program took advantage of the fact that most children in Kenyan primary schools take practice KCPE exams at the end of standards 4 through 7.[9] Like the KCPE, the practice exams are proctored by representatives of the District Education Offices (rather than the teachers themselves), and it is consequently very difficult to cheat. The GSP offered girls in program schools a performance incentive: in each year of the program, ICS awarded scholarships to all girls who scored in the top 15 percent of females in standard 6 in Busia District on the KCPE practice exam. For the two years after they won the competition, scholarship recipients were given an annual cash grant of approximately 12.80 USD (1000 Kenyan shillings) and had their school fees paid, for a total award amount of approximately 38 USD per winner. Thus, the total amount of the award package was large relative to the income of the typical Kenyan household, which averaged about 400 USD at the time of the intervention. Winners were also recognized at a public awards ceremony. ICS administered the competition in both 2001 and 2002, so two cohorts of girls received awards.

In order to assess the overall impact of the GSP, ICS conducted a randomized evaluation of the program. 69 primary schools in Busia District were randomly assigned to either the GSP treatment group or a control group which did not participate in the scholarship competition.[10] The program was announced in treatment schools in March of 2001, at which

[9] The exams are not required, and students must pay a fee of between 1 and 2 USD to participate in each practice exam.

[10] A parallel randomized experiment was simultaneously conducted in neighboring Teso district (Kremer et al. 2009), but since it is unclear whether the scholarship increased human capital in this district, in part due to program implementation difficulties there, follow-up surveys were only conducted in the Busia district. For that reason, we only have actual KCPE scores for Busia students, and we focus only on that

point school headmasters were asked to pass information about the GSP competition on to the parents of eligible girls.[11] To make sure that parents of children in GSP treatment schools were fully informed about the program, ICS also organized community meetings in September and October of 2001. A first cohort of program participants took practice KCPE exams in November of 2001, and scholarships were subsequently awarded to 110 girls. A second cohort of girls participated in the program the following year.

Kremer et al. (2009) discuss the impacts of the GSP intervention. In the year that they were eligible for the scholarship, girls in GSP treatment schools had practice exam scores that were 0.27 standard deviations higher than those in control schools. Though only girls scoring near the top of the distribution were eligible for scholarships, the GSP program led to test score improvements at all performance levels, and among boys (who were not eligible for scholarships). When program impacts are disaggregated by baseline test score (for the sub-sample of girls for whom baseline test scores are available), the results suggest that test scores increased by at least 0.19 standard deviations for the top three baseline test score quartiles, even though only 5 percent of girls in the next-to-lowest quartile of baseline test scores ended up winning a scholarship (Kremer et al. 2009). Kremer et al. (2009) also report that the program led to a 0.10 standard deviation increase in test scores among sixth grade boys in treatment schools, and to increases in teacher attendance, which may partially explain the apparent spillover effects.

2.3 Data Sources

We combine our experimental data with information from three additional sources. The first is administrative data on individual test scores in 2000 (prior to the intervention), 2001, and 2002. Because students have to pay a fee (approximately 1 to 2 USD) to take the KCPE practice exams, not all enrolled students participate. In 2001, for example, approximately 78 percent of girls in standard 6 in the control schools chose to take the practice exam. Test score data is available for the majority of students in GSP treatment

experiment in this paper.

[11] Only those girls who were enrolled in standards 5 and 6 in treatment schools in January 2001 were eligible for scholarships. This restriction was imposed to avoid creating incentives for girls to transfer from control to treatment schools.

and control schools.

Student surveys, which were administered in treatment and control schools in 2002, constitute a second source of data on our subjects. Because of financial constraints, only a limited amount of individual-level data was collected at the time of the intervention. Baseline data on individual characteristics (e.g. parents' names and education levels) was collected during school visits in early 2002, but only those students who were present in class on the day of the survey could be interviewed.

Finally, between 2005 and 2008, an extensive follow-up survey was administered to 1,862 women from both treatment and control schools — all girls in the GSP cohorts who could be located at the time of the follow-up survey. The effective tracking rate is 80 percent, and attrition from the survey does not differ substantially between the GSP treatment and control groups (Friedman et al. 2011). This follow-up survey provides information about educational attainment after the GSP competition, including self-reported KCPE scores for those who took the exam.[12]

2.4 Experimental Subjects

To estimate the impact of the GSP intervention on individual social preferences, it was necessary to recruit experimental subjects who were enrolled in standards 5 and 6 in the GSP treatment and control schools in 2001. This presented two challenges. First, eligible young women, many of whom had moved out of their family homes to marry or continue their schooling, had to be located and contacted. Second, they needed to be brought together to conduct our lab-in-the field experimental sessions.

Analysis of data from the GSP follow-up survey indicates that the program did not increase the probability of migrating out of Busia District (Friedman et al. 2011), so we felt that it was reasonable to focus on recruiting those individuals still residing there. Experimental sessions were conducted during the August break in the academic year so that girls who were in boarding school much of the time would be able to participate (while they were home visiting their families). Members of the research team met with local

[12] Administrative data on past KCPE scores is not publicly available from any centralized source. Because many girls took the KCPE exam in different years and might have changed schools, it was not feasible to collect hard copy records of KCPE scores for all the schools that GSP respondents might have attended.

officials throughout the district to compile a list of such potential participants. The list was organized by sublocation, the second most disaggregated level of local government in Kenya. We then identified clusters of sublocations which contained enough girls from the GSP treatment and control groups to warrant organizing an experimental session. Once experimental sessions were scheduled and target participants identified, the same local officials were tasked with delivering invitation letters to each of the girls explaining the project and inviting them to attend a specific experimental session.

We expected the GSP intervention to impact academic performance and other educational outcomes directly, and to influence preferences and values primarily through the education channel. It is therefore important to focus on a population for whom comparable education-related outcomes are available for the treatment and the control group. Because more than half of the control group was still in school at the time of the GSP follow-up survey (and estimates of the program's impact on educational attainment were consequently biased toward zero), we chose to focus on the KCPE score.[13] As discussed above, KCPE scores provide a measure of academic success for all those who complete primary school; moreover, the GSP follow-up survey does not suggest that girls in treatment schools were more likely to take the KCPE exam. Performance on the KCPE is a particularly salient measure of academic success, since it determines whether or not a student will be admitted to a government secondary school.[14] Because KCPE scores are such an important determinant of future academic success, it is not uncommon for Kenyan students to repeat standard 8 in order to retake the test. 14 of our 101 subjects report taking the KCPE exam twice. To avoid conflating academic performance with the likelihood of success, we focus on the first reported KCPE score. The majority of those in our sample (93 percent) took the KCPE between 2003 (the first year that a girl who was in standard 6 in 2001 could be eligible) and 2005.[15]

[13] In our sample, GSP treatment is associated with an 8.3 percentage point increase in the likelihood of being in school, but the effect is not significant. This estimated impact is very similar to the 7.9 percentage point effect reported in Friedman et al. (2011).

[14] Ozier (2010) reports that scoring above the mean on the KCPE increases the probability of completing secondary school by 20 percentage points. Test scores are arguably more relevant as an indicator of quality, rather than quantity, of education: Barro (2001) and Hanushek and Kimko (2000) both find that test scores on internationally comparable exams are more predictive of future income growth rates than years of schooling.

[15] Unfortunately, Kenyan secondary schools do not conduct regular standardized tests that could be used

The 101 young women in our sample (45 treatment vs. 56 control) were enrolled in 23 different schools in 2001, 10 treatment and 13 control. Thus, each session contained relatively small numbers of girls from the same primary school, though, since all subjects were from Busia District, they could easily be socially connected to girls who attended different primary schools. Subjects ranged in age from 17 to 23. 71 percent of them were still in school at the time of the experiment, while 12 of them were married. Subjects in the control group had completed an average of 8.3 years of schooling, while those in the treatment group had completed 8.6 years (though this difference is not significant).

Though they are not a random sample of girls in the GSP, our subjects are broadly representative of GSP Survey respondents who took the KCPE exam. Table 1 compares the two groups. Our subjects are similar to other GSP respondents who took the KCPE in terms of educational attainment, KCPE score, cognitive ability, English and Swahili vocabulary, household size, parents' education, and work experience. Our subjects are somewhat less likely to come from a GSP treatment school, though the difference is only marginally significant (p-value 0.056). They are also slightly (approximately 3 months) younger, but again this difference is only marginally significant (p-value 0.1). Thus, we expect that our findings would generalize to the population of GSP respondents who took the KCPE exam.

Table 2 compares the GSP treatment and control groups within our sample in terms of baseline (pre-GSP) characteristics. Those in the GSP treatment group are not significantly different from the control group in terms of age or parents' education (prior to the intervention). There is a small but insignificant different in baseline practice test scores (for those subjects who took the practice KCPE in 2000, prior to the GSP intervention). Given the randomized design and the absence of differences between the treatment and control groups at baseline, we believe it is reasonable to attribute differences in behavior within the experiment to the impact of the GSP program, and the gains in academic performance it generated, on individual social preferences.

It is, however, still possible that the process of locating young women and recruiting

to provide a more recent measure of academic achievement. Because those subjects attend secondary schools which vary in quality, grade point averages and class ranks would not be comparable.

them into our subject pool (described above) led to differences between our GSP treatment and control subjects that were not caused by the treatment. As discussed above, the Friedman et al. (2011) finding that random assignment to GSP treatment does not impact the probability of migrating out of the district partially addresses such concerns. In the Online Appendix, we explore the selection issue further by using data from the GSP follow-up survey to compare experimental subjects in the GSP treatment and control groups to non-participants in the GSP treatment and control groups who also completed the KCPE exam. Subjects drawn from the GSP treatment group look similar to other GSP treatment girls who took the KCPE across a wide range of outcomes. Comparing winners of the GSP scholarship to non-winners, we do not find a significant difference in the likelihood of participation in our experiments. We do find that GSP control girls who participate in our experiment differ from GSP control girls who took the KCPE but did not participate on 2 (of 11) dimensions: they are approximately six months younger and come from slightly wealthier households. To address any concerns about the potential impact of selection on age, we report specifications which include age as a control. Unfortunately, we cannot include household wealth at the time of the follow-up survey as a control, since it may have been directly affected by the GSP treatment; however, positive selection on wealth within the GSP control group would likely bias our estimated effects toward zero. Thus, though we cannot fully rule out the potential impact of differential selection, data from the GSP follow-up survey does not suggest major differences between our subjects and the rest of the sample in term of observable characteristics.

2.5 Experimental Design and Procedures

Our experiment is a modified dictator game designed to to measure respect for the "earned property rights" of others (Fahr and Irlenbusch 2000). As in all dictator games, one subject (the "dictator") divides a budget between *self* and an anonymous *other*, another subject attending the same experimental session (Kahneman et al. 1986, Forsythe et al. 1994, Camerer 2003). Our variant is a real effort dictator game in which each subject divides

money that was earned by *other*.[16]

Our study is motivated by previous evidence suggesting a link between educational attainment and social preferences, particularly respect for earned property rights. Jakiela (2009) conducts four different versions modified dictator game treatments in Kenyan villages. In her experiments, the dictator divides either her own or *other*'s earned or unearned income between *self* and *other*. She finds that villagers with more than a primary school education allocate more to *other* than less educated subjects in one of her four experimental treatment, the one in which subjects divide income earned by *other*. Thus, more educated subjects appear more inclined to respect the earned property rights others, but not more altruistic or generous overall. That result motivates the present study.

We replicate the experimental treatment in which Jakiela (2009) finds an association between education and allocation decisions: dictators divide money earned by *other* between *self* and *other*.[17] In our experiment, each subject was matched with an anonymous *other* who was seated in another room, and whose identity was not revealed during or after the experimental session. Subjects first learned about the structure of the experiment, and then about the nature of the real effort task (which determined earnings). We selected an activity which could be easily understood by all subjects, regardless of educational attainment, and which would allow players to increase their output by exerting greater effort up to some maximum feasible level: subjects earned money by clicking a handheld tally counter, and were paid based on the number of times they clicked within ten minutes.[18] Subjects were given a two-minute practice period during which they tried out the real effort

[16] Our design is identical to that used in Jakiela (2009), which was motivated by Ruffle (1998) and Greig (2006). Hoffman et al. (1994), Cherry (2001), Cherry et al. (2002), and List and Cherry (2008) conduct dictator games in which subjects divide their own earned income between *self* and *other*; they find that the amount allocated to *other* is lower when the dictator's endowment is earned. Bardsley (2008), List (2007), and Fisman et al. (2013) conduct modified dictator games which allow for both giving and taking.

[17] We also piloted the 3 other variants of the dictator game proposed in Jakiela (2009). However, we did not locate large enough numbers of potential participants to be able to carry out all 4 treatments. (Each session lasted approximately 3 hours, and each subject participated in only one treatment.) We chose to focus on the treatment described here because it is in that treatment that Jakiela (2009) finds an association between education and allocation decisions. In any potential analysis of the pilot data from the other 3 treatments, we face a weak instrument problem in the first stage regression because of the limited sample size.

[18] We opted for a non-cognitive task so that output would reveal minimal information about education or innate intelligence. The task was inspired by Ariely et al. (2009), but adapted to a non-computerized environment. Other non-cognitive tasks which have been used in experimental settings include stuffing envelopes (Konow 2000, Falk and Ichino 2006) and cracking walnuts (Fahr and Irlenbusch 2000).

task before they made their allocation decisions. After the practice period, subjects decided how they wished to divide *other*'s earnings between *self* and *other*. We used the strategy method: for each of the 20 possible earnings levels, subjects recorded the allocation that they wished to implement by circling the amount (presented as images of Kenyan currency) that they wished to allocate to *self*. We chose this pictorial approach to choice elicitation so that subjects who were relatively uncomfortable with entering numbers into tables could record their own allocation decisions. After individual decisions were recorded, subjects performed the real effort task for ten minutes, and were informed how much money they had earned (based on the piece rate and their level of production); they earned 30 Kenyan shillings (approximately \$0.375) for every 200 times they clicked the tally counter.[19] These activities took place in parallel in the two separate rooms. At the end of the experiment, one room was chosen at random, and the decisions of dictators in that room were combined with the earnings information about the matched subjects in the other room to determine final payoffs.[20]Complete experimental instructions, which were presented orally during the sessions, are included in the Online Appendix.

We conducted 4 experimental sessions in August of 2008, each of which was held at a different primary school in Busia District. August is a school vacation in Kenya, and empty primary school classrooms provide a sheltered location for conducting experiments. Primary schools are also easy for subjects to locate because they are well-known within the community. Because most schools in the area have one or two classrooms per grade level, it is also feasible to split subjects into separate rooms. Experimental sessions took approximately 3 hours. Final payouts averaged 1.80 USD (144 Kenyan shillings) plus a 0.25 USD (20 shilling) show-up fee.

[19]Interestingly, Jakiela (2009) finds no evidence that subjects exert less effort when they expect that another may appropriate a portion of their earnings.

[20]Thus, all subjects make allocation decisions which might determine final payoffs — this was necessary because of our small sample size. In contrast to Andreoni and Miller (2002) and Fisman et al. (2007), subjects in our experiment do not receive two sets of tokens (one based on their own decision and one based on the decision of another subject). Instead, each subject within a matched pair makes an allocation decision, and one of the two decisions is randomly chosen to determine payouts, as in Cappelen et al. (2007). The amount of money being allocated is determined by the effort level of the subject whose decision is not chosen to determine payouts.

3 Results

The main sample includes data from 101 subjects, each of whom made allocation decisions over all twenty potential budget sets. On average, subjects allocated 67.1 percent of the budgt to *self* and 32.9 percent to *other* (Table 3). Thus, our subjects allocate more to *other* than is typical in dictator games involving students (Camerer 2003), though not more than has been previously observed in African populations (Henrich et al. 2010a). The distribution has modes at 0 and 50 percent. 5 percent of subjects allocated the entire budget to *self*, while 13.7 percent split the budget evenly and an additional 14.9 percent allocated more than half to *other*. Subjects who had some secondary schooling allocated *other* slightly more than those who did not (33.6 versus 31.4 percent of the budget, p-value 0.0226, results not shown). More interestingly, there are clear differences between the GSP treatment and control groups in terms of behavior within the experiment. Subjects drawn from the GSP treatment group allocate *other* an average of 36.4 percent of the budget, as opposed to a mean allocation to *other* of 30.0 percent of the budget in the GSP control group (p-value < 0.001).[21] The two groups are equally likely to allocate the entire budget to *self*, but subjects drawn from the GSP treatment group are substantially more likely to allocate *self* and *other* exactly equal amounts (19.2 percent of subjects versus 9.3 percent, p-value < 0.001) or to allocate more than half the budget to *other* (16.8 percent versus 13.3 percent, p-value 0.031).

Our main analysis estimates the causal impact of academic performance on social preferences, as measured by the allocation to *other* within the dictator game, using the GSP treatment indicator as an instrument for the KCPE score (Table 4). The key outcome variable is the percent of the budget that the dictator allocates to *other*. We first report linear IV specifications (Panel A, Columns 1–3), then reduced form OLS specifications (Panel B, Columns 1–3), and the IV first stage (Panel C, Columns 1–3). The IV estimates indicate that a one standard deviation increase in a student's KCPE score causes a large

[21] Even after omitting 15 subjects in the GSP treatment group who won the scholarship, we still observe a significant relationship between random assignment to the GSP treatment group and the amount allocated to *other*. Subjects in the GSP treatment group who did not win the scholarship allocate *other* an average of 33.9 percent of the budget (p-value from a test of equality with the mean allocation to *other* made by subjects in the GSP control group < 0.001).

and statistically significant increase in the allocation to *other*. Without any regression controls, the coefficient on instrumented KCPE score is 10.6, and is significant at the 90 percent confidence level. After adding controls for individual age, ethnicity, and session-room fixed effects, the coefficient remains almost unchanged at to 10.3 and the confidence level increases to 95 percent (Table 4, Panel A, Columns 1–3).[22] Compared to an average allocation to *other* of 32.9 percent of the budget, this is a large effect. This corresponds to the approximately 6 percentage point average GSP treatment effect shown in the reduced form specifications (Panel B, Columns 1–3).

Panel C shows that the F-statistic in the first stage is between 5.3 and 6.3 depending on the controls, and that random assignment to the GSP program increases subsequent KCPE scores by an average of at least 0.6 standard deviations within our sample.[23] Though our first stage F-statistics are below the rule of thumb proposed in Staiger and Stock (1997), the coefficient of interest is median-unbiased in the just-identified case (Angrist and Pischke 2009); nonetheless, hypothesis tests may be incorrectly sized (Stock and Yogo 2002, Dufour 1997). Anderson and Rubin (1949) provides a statistic that produces confidence intervals of the correct size in the presence of weak instruments. These confidence regions are asymmetric and potentially disjoint or unbounded, but the AR statistic allows us to verify that our results are not dependent on inappropriately small Wald standard errors. With no controls or with age and ethnicity controls, the coefficient on the endogenous regressor KCPE score is marginally significant under the AR χ^2 test with p-values of 0.064 and 0.063, respectively, and with additional room fixed effects, it is highly significant with a p-value of 0.003. The 95 percent AR confidence intervals are, respectively, (-0.90,48.45), (-0.71,31.40), and (3.56,42.83). Although these barely include zero in the first two cases, overall the AR test merely shows that we can't reject even larger effects, as the asymmetric confidence intervals are skewed upwards compared to the standard confidence intervals.

[22] Age controls include both age in 2008 (normalized) and an indicator for being in the first GSP cohort. Studies by Fehr et al. (2008), Almas et al. (2010), Bekkers (2007), and Fowler (2006) suggest that age is an important predictor of altruistic behaviors. Ethnicity controls are indicators for being a member of a minority ethnic group (Teso or Luo) and for belonging to a minority subgroup of the locally dominant Luhya ethnic group.

[23] This GSP treatment effect on test scores is larger than the roughly 0.2 to 0.3 standard deviations effect reported in Friedman et al. (2011) for the full GSP Follow-up Survey sample. Sampling variation is a likely explanation for the discrepancy, given our limited subsample of 101 lab subjects.

This strongly suggests that our result is not a spurious consequence of a weak instrument.

Figure 1 presents our main result graphically via non-parametric, locally-weighted Fan regressions. The figure plots the average allocation to *other* in the GSP treatment and control groups as a function of budget size.[24] It is clear that, across the range of budget sizes, subjects drawn from the GSP treatment group allocate more to *other* than those drawn from the control group.

We further explore the impact of academic achievement on social preferences by estimated IV probit specifications where the outcome variable is an indicator for allocating *self* and *other* exactly equal amounts (Table 4, Panel A, columns 4-6). In all specifications, instrumented KCPE exam scores are positively and statistically significantly associated with a tendency to divide the budget evenly. Thus, academic achievement appears to shift subjects toward an exactly equal distribution of the budget. This pattern is consistent with the desire, documented in Charness and Rabin (2002), to avoid receiving a lower payoff than another subject.

4 Discussion

At this point, we have established the relationship between the GSP intervention and behavior in our experiment, and explored a one potential causal mechanism linking the scholarship program to respect for earned property rights: academic achievement as measured by KCPE exam scores. We now discuss the channels through which human capital might impact behavior in our experiment in more detail, and consider several alternative explanations of our empirical findings.

One possibility is that, as we have argued, human capital directly alters social preferences by increasing respect for earned property rights. In an educational environment where effort is rewarded and the benefits from effort are privately held, one might learn to embrace the values that lead to success in that environment. A related possibility is that success in school is a signal for success later in life, and after observing this signal, students choose self-serving moral codes: those who are capable of high productivity believe that

[24] Following Deaton (1997), we choose a reasonable bandwith by trial and error, since the figure is for illustrative purposes only.

it is important to reward high productivity. Either pathway might explain a causal impact of academic achievement on individual beliefs about what constitutes a fair allocation, particularly in settings where individual effort determines income.

An alternative explanation is that winning the scholarship contest impacted individual preferences via a channel other than academic achievement, for example, through a wealth effect. To explore this possibility, we estimated our main regression specifications omitting the 15 subjects who won the scholarship contest (results not shown). Though sample sizes, and consequently significance levels, are reduced somewhat, estimated coefficients are essentially unchanged.

Another possibility is that people choose allocations based on their beliefs about the types of individuals they are matched with in the experiment: those who believe that *other* is likely to be kind or altruistic may put more weight on the payoff to *other*, along the lines proposed in Levine (1998). Thus, individuals with different beliefs about the average level of altruism and respect for property rights in the population (or the experimental subject pool) might behave differently in our experiment even if their underlying preferences were the same. If GSP-induced improvements in test scores caused girls to attend higher quality secondary schools with smarter, kinder peers, academic achievement may be associated with increases in the amount allocated to *other* in our experiment because beliefs are different, even if social preferences (conditional on beliefs) are the same.

To explore the hypothesis that beliefs, rather than preferences, change with academic experience, we asked participants to report how much they thought *other* would allocate to them at four of the twenty possible budget sizes.[25] Table 5 reports OLS regressions of the average amount a subject believed her partner would allocate her on the GSP treatment indicator (Panel A) and the KCPE score (Panel B), both with and without controls. Neither treatment nor academic achievement is significantly associated with beliefs in any specification, and all estimated coefficients are quite small in magnitude. The point estimates suggest a negative relationship between KCPE scores and expectations, instead of

[25]Beliefs were elicited through survey questions and not in an incentive-compatible manner. However, the average belief reported in the survey is not significantly associated with the average amount a subject allocated to her partner. Moreover, beliefs are substantially higher, on average, than actual allocations, despite potential self- and social- image motivations to underestimate others' generosity. Thus we believe the beliefs data are reliable.

the positive relationship required if our results were explained by academic achievers reciprocating a higher perceived level of altruism among their peers. We are consequently able to rule out the possibility that academic achievement mainly impacts beliefs rather than social preferences.

Another alternative explanation for our main results is that the GSP treatment had a positive impact on generalized altruism rather than respect for earned property rights. Prior to conducting our main experiments, we conducted a pilot of a standard dictator game (in which dictators divided their own unearned income) with a small sample of 40 subjects, 19 from GSP treatment schools and 21 from control schools. In this small-scale pilot, girls in the GSP control group allocated *other* 19.0 percent of the budget, on average, while those in the treatment group allocated *other* an average of 16.6 percent of the budget (p-value 0.0229). Thus, the evidence suggests that, if anything, the GSP treatment is associated with lower levels of generalized altruism.

Finally, Table 6 shows that un-instrumented academic achievement on the KCPE exam is associated with an increase in the amount allocated to *other* in our main experimental treatment. However, the coefficient on KCPE score is substantially smaller than in the IV regressions reported earlier.[26] It is not surprising that the coefficients are different, since academic outcomes depend on factors such as parental influence, socioeconomic status, and innate individual personality traits which may also shape norms and preferences, as discussed in Malmendier and Nagel (2011).

The fact that the OLS coefficient is smaller suggests that some factors which explain better academic performance are associated with lower levels of respect for earned property rights, or possibly that the IV approach is helping to address attenuation bias caused by noise in the KCPE achievement test score. A further possibility that we cannot rule out is that the GSP experiment affects social preferences through channels other than the test score, with schooling attainment being the leading potential channel, and that the IV estimates are in part capturing effects through these other channels. While this possibility alters the interpretation of the KCPE coefficient estimates, the hypothesized

[26] A Hausman test rejects the equality of the IV and OLS coefficients with 90 percent confidence (p-value 0.065) when the full set of controls is included in the regressions, as in column 3.

schooling attainment channel is still consistent with the overall thrust of our argument that boosting human capital affects social preferences. Those readers who believe that schooling attainment — or some other outcome — is a major channel through which the scholarship program affects social preferences thus might prefer to focus on the reduced form results in Panel B of Table 4 rather than the IV results in Panel A. More generally, the GSP intervention may have changed the likelihood that a girl marries young, or expected lifetime wealth, or the level of social capital in treatment communities. Nonetheless, our reduced form results provide an estimate of the program on behavior in our experiment, and respect for earned property rights, regardless of the channel mediating these impacts.

5 Conclusion

We provide evidence that increases in human capital, as captured in academic achievement tests, alter individual values, generating greater respect for earned property rights. This finding demonstrates that formal education can have cultural impacts beyond the direct production of human capital, and may have social returns beyond whatever wage gains the human capital generates.

Though there is an extensive empirical literature exploring the labor market returns to education in less developed countries (cf. Duflo 2001), relatively few empirical studies have directly tested the claims of modernization theory — that formal education leads to changes in individual values — with convincing research designs. Such cultural change could benefit society in several ways. First, as individuals become more respectful of property rights and more permissive of earned wealth accumulation, the private returns to entrepreneurship may increase. This may be particularly important in rural villages in Africa, where strong egalitarian traditions often lead to the social sanctioning of households that accumulate wealth (Barr and Stein 2008, Platteau 2000). More speculatively, the expansion of educational opportunities may generate positive spillovers if changes in values eventually facilitate the emergence of market-oriented institutions (Glaeser et al. 2004, Bernard et al. 2010). At the same time, education may have impacts on individual values and beliefs other than those documented here; for example, academic success may change individual aspirations,

and these in turn may influence long-run outcomes (Ray 2006). Our work complements recent cross-cultural comparisons documenting the correlation between market integration and generosity within dictator games (Henrich et al. 2001, Henrich et al. 2010a), and contributes to the emerging literature documenting the causal mechanisms underlying changes in individual values (Di Tella et al. 2007, Fisman et al. 2009).

Our work is one of several recent studies which demonstrate that lab experiments can be combined with randomized controlled trials to measure the direct impact of programs on individual preferences and, more broadly, on social norms and cultural values. In response to recent calls for a greater focus on understanding why and how (rather than just whether) anti-poverty programs work, we demonstrate that progress in understanding the underlying mechanisms, which is so often the focus of lab experiments, can fit naturally together with the clean econometric identification generated by randomized trials.

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Table 1: **Summary Statistics: Subjects vs. Rest of GSP Sample**

Lab Experimental Subjects? $(S = 0, 1)$	$S = 0$	$S = 1$	Difference
N	1024	101	
GSP Treatment Group	0.546	0.446	0.100*
	(0.016)	(0.050)	(0.052)
Age (in 2008, based on date of birth)	20.153	19.901	0.252*
	(0.045)	(0.145)	(0.152)
First KCPE score	258.276	259.604	-1.328
	(1.392)	(4.430)	(4.643)
Highest grade completed (as of 2005)	8.602	8.426	0.176
	(0.028)	(0.127)	(0.130)
Ravens matrices score	20.727	21.538	-0.810
	(0.169)	(0.622)	(0.644)
English vocabulary score	9.939	10.089	-0.151
	(0.080)	(0.245)	(0.258)
Swahili vocabulary score	9.478	9.812	-0.334
	(0.081)	(0.254)	(0.267)
Respondent held job in last 12 months	0.119	0.129	-0.010
	(0.010)	(0.033)	(0.035)
Father's education	9.786	10.420	-0.634
	(0.133)	(0.395)	(0.417)
Mother's education	7.301	7.263	0.038
	(0.132)	(0.415)	(0.435)
Household size	6.951	6.812	0.139
	(0.088)	(0.283)	(0.297)
Household Assets (1000s of KSh)	27.727	30.095	-2.369
	(0.545)	(1.718)	(1.802)

Standard deviations in parentheses in columns 1 and 2, and standard errors in parentheses in column 3. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level. The first column includes data on the 1024 GSP follow-up survey respondents who took the KCPE exam but did not participate in our experimental sessions. The number of observations contributing to each number may differ from the pool sizes shown when particular variables are unavailable for some people.

Table 2: **Summary Statistics: GSP Treatment vs. Control**

GSP Treatment Group? $(T = 0, 1)$	Both	$T = 0$	$T = 1$	Difference
N	101	56	45	
Age	19.901	19.696	20.156	0.459
	(0.145)	(0.185)	(0.227)	(0.293)
Baseline father's education	11.631	11.469	11.788	0.319
	(0.404)	(0.596)	(0.555)	(0.814)
Baseline mother's education	9.574	9.733	9.419	-0.314
	(0.487)	(0.733)	(0.655)	(0.984)
Baseline practice KCPE score	0.077	-0.003	0.219	0.223
	(0.098)	(0.117)	(0.175)	(0.210)

Standard deviations in parentheses in columns 1, 2 and 3, and standard errors in parentheses in column 4. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level. The number of observations contributing to each number may differ from the subject pool sizes shown when particular variables are unavailable for some people. Data on father's education, mother's education, and baseline (2000) KCPE practice test score is available for (respectively) 65, 61, and 64 subjects.

Table 3: **The Impact of the Girls' Scholarship Program on Allocation Decisions**

GSP Treatment Group? $(T = 0, 1)$	Both	$T = 0$	$T = 1$	Difference
Share of budget allocated to *other*	32.865	30.029	36.394	6.365^{***}
	(0.462)	(0.606)	(0.695)	(0.922)
Allocated *other* zero	0.050	0.050	0.050	0.000
	(0.005)	(0.007)	(0.007)	(0.010)
Allocated *other* half of budget	0.137	0.093	0.192	0.099^{***}
	(0.008)	(0.009)	(0.013)	(0.016)
Allocated *other* more than half of budget	0.149	0.133	0.168	0.035^{**}
	(0.008)	(0.010)	(0.012)	(0.016)

Standard deviations in parentheses in columns 1, 2 and 3, and standard errors in parentheses in column 4. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level.

Table 4: **Instrumental Variable Results for KCPE Exam Scores**

	Dependent Variable: Allocation to Other			*Dependent Variable: Allocated Half to Other*		
	(1)	(2)	(3)	(4)	(5)	(6)
PANEL A: IV REGRESSION						
KCPE Score	10.060*	8.857*	9.937**	0.599***	0.594***	0.471**
	(5.634)	(4.709)	(4.360)	(0.201)	(0.186)	(0.214)
Budget	0.057	0.057	0.057	-0.007*	-0.008*	-0.009**
	(0.053)	(0.053)	(0.053)	(0.004)	(0.004)	(0.004)
Constant	32.092***	-233.354	-190.059	-0.82***	-16.576	-24.124**
	(1.713)	(191.051)	(175.376)	(0.157)	(10.899)	(10.035)
Observations	2020	2020	2020	2020	2020	2020
R^2	-0.106	0.013	0.084	.	.	.
PANEL B: REDUCED FORM						
GSP Treatment	6.365*	6.504*	7.469***	0.456***	0.496***	0.387**
	(3.420)	(3.493)	(2.223)	(0.163)	(0.155)	(0.177)
Budget	0.057	0.057	0.057	-0.009**	-0.009**	-0.009**
	(0.054)	(0.054)	(0.054)	(0.004)	(0.004)	(0.004)
Constant	29.137***	-81.442	-64.750	-1.188***	-7.421	-20.267**
	(2.417)	(186.918)	(173.601)	(0.119)	(8.594)	(9.109)
Observations	2020	2020	2020	2020	2020	2020
R^2	0.024	0.049	0.18	.	.	.
Pseudo R^2	.	.	.	0.029	0.039	0.082
PANEL C: FIRST STAGE						
GSP Treatment	0.633**	0.734**	0.752**	0.633**	0.734**	0.752**
	(0.272)	(0.293)	(0.297)	(0.272)	(0.293)	(0.297)
Constant	-0.294*	17.152	12.610	-0.294*	17.152	12.610
	(0.154)	(10.751)	(13.158)	(0.154)	(10.751)	(13.158)
First Stage F-stat	5.423	6.267	6.418	5.423	6.267	6.418
Observations	101	101	101	101	101	101
R^2	0.09	0.262	0.303	0.09	0.262	0.303
Age Controls	No	Yes	Yes	No	Yes	Yes
Ethnicity Controls	No	Yes	Yes	No	Yes	Yes
Classmate Control	No	Yes	Yes	No	No	Yes
Session-Rooms FEs	No	No	Yes	No	No	Yes

All errors are robust and clustered by school × GSP cohort (the unit of randomization in the GSP). *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level. IV regressions of the impact of the KCPE score on the percent of the budget allocated to *other* are estimated using GMM (Panel A, Columns 1 to 3); reduced form regressions of percent of budget allocated to *other* are estimated using OLS (Panel B, Columns 1 to 3). IV regressions of the impact of the KCPE score on the likelihood of allocating *other* exactly half use a conditional maximum-likelihood IV probit estimator (Panel A, Columns 4 to 6), while reduced form regressions of the indicator variable for allocating *other* exactly half are estimated via probit analysis (Panel B, Columns 4 to 6). The dependent variable in the first stage OLS regressions in Panel C is the KCPE score. The age controls are age, age squared, and GSP cohort. The ethnicity controls are indicators for being from the local minority Luhya or Teso ethnic groups. The classmate control is the number of girls in a session who are from the same primary school as the subject. KCPE scores are normalized to have a mean of zero and a standard deviation of one.

Table 5: **OLS Regressions of Expected Partner Share**

	Dep. Var.: *Allocation to* Other		
	(1)	(2)	(3)
PANEL A: IMPACTS OF GSP TREATMENT			
GSP Treatment	-0.485	0.402	-0.5
	(2.407)	(3.056)	(3.067)
Constant	47.318***	-67.345	-208.560
	(1.680)	(285.066)	(272.951)
Observations	100	100	100
R^2	0.0003	0.036	0.096
PANEL B: ASSOCIATION WITH KCPE SCORE			
KCPE Score	0.022	-0.662	-1.161
	(1.539)	(1.592)	(1.666)
Constant	47.100***	-53.209	-195.056
	(1.203)	(286.941)	(268.579)
Observations	100	100	100
R^2	0.00	0.038	0.103
Age Controls	No	Yes	Yes
Ethnicity Controls	No	Yes	Yes
Classmate Control	No	Yes	Yes
Rooms FEs	No	No	Yes

All specifications estimated using OLS and robust standard errors clustered by school × GSP cohort, the unit of randomization in the GSP. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level. The age controls are age, age squared, and GSP cohort. The ethnicity controls are indicators for being from the local minority Luhya or Teso ethnic groups. The classmate control is the number of girls in a session who are from the same primary school as the subject. KCPE scores are normalized to have a mean of zero and a standard deviation of one.

Table 6: **OLS Regressions of Allocation to *Other* on KCPE Scores**

	Dep. Var.: Allocation to Other		
	(1)	(2)	(3)
KCPE Score	2.956**	4.083***	3.223***
	(1.350)	(1.461)	(1.197)
Budget	0.057	0.057	0.057
	(0.054)	(0.054)	(0.054)
Constant	32.008***	-141.146	-123.923
	(1.601)	(175.282)	(173.911)
Observations	2020	2020	2020
R^2	0.023	0.063	0.175
Age Controls	No	Yes	Yes
Ethnicity Controls	No	Yes	Yes
Classmate Control	No	Yes	Yes
Rooms FEs	No	No	Yes

All specifications estimated using OLS and robust standard errors clustered by school × GSP cohort, the unit of randomization in the GSP. Coefficients significantly nonzero at .99 (***), .95 (**) and .90 (*) confidence levels. The age controls are age, age squared, and GSP cohort. The ethnicity controls are indicators for being from the local minority Luhya or Teso ethnic groups. The classmate control is the number of girls in a session who are from the same primary school as the subject. KCPE scores are normalized to have a mean of zero and a standard deviation of one.

Figure 1: **Fan regressions of Partner Share on Budget**