Parents' perceptions and children's education: Experimental evidence from Malawi*

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Abstract

Many models of human capital investment incorporate individual-level characteristics, like ability, that affect the returns to investment. The implication is that efficient investments depend on individual characteristics. However, the literature has paid relatively limited attention to the fact that it is perceived, not true, characteristics that determine investments. This paper uses data from a field experiment conducted in Malawi to assess whether parents have inaccurate perceptions about their children's academic abilities, and whether parents' inaccurate perceptions distort their investments in their children's education. I find that the divergence between parents' beliefs about their children's achievement and their children's true achievement is large, and that this creates a wedge between parents' intended and actual educational investments. Providing parents with information significantly impacts their investments, causing them to become more closely aligned with their children's achievement. Poorer, less-educated parents have less accurate perceptions about their children's academic abilities than richer, more-educated parents, and update their beliefs more in response to improved information. Inaccurate perceptions may thus exacerbate inequalities in educational outcomes between richer and poorer families.

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1 Introduction

Many models for optimal human capital investment contain individual-level characteristics, such as ability or achievement, which are assumed to affect the returns to investment (e.g., Becker, 1962). The implication is that efficient schooling investments depend on individual characteristics.

However, the literature has paid limited attention to the fact that it is parents' perceptions about their children's characteristics, not their children's true characteristics, that influence educational investments. If parents misperceive their children's individual-level characteristics, they may make inefficient decisions. To give a concrete example, consider a parent whose son has average performance in his verbal classes, but is far behind his class in math. Perhaps the parent could not afford tutoring for her son in all subjects, but could afford it for one subject. If she were aware of her son's poor math skills, she would know that investing in math tutoring would be valuable; without this information, she does not send her son to math tutoring, and his performance continues to lag. Since parents' investments represent a major determinant of educational outcomes (e.g., Houtenville and Conway (2008), Todd and Wolpin (2007)), these types of inefficiencies could have important, negative impacts on children's educational outcomes.

Inaccurate perceptions may be particularly problematic in developing countries because parents' education levels are low: in many countries in sub-Saharan Africa, less than 40% of parents are literate (UNESCO, 2007). Education may increase parents' capacity to judge their children's performance for themselves, or to comprehend external signals of their children's ability, like written report cards sent to them by their children's schools.

Several recent papers have shown that poor information about population-average characteristics like the returns to education (Nguyen, 2008; Jensen, 2010), and about school quality (Andrabi et al., 2009) can negatively affect educational outcomes. However, they do not explore whether inaccurate perceptions about individual level characteristics cause misallocations or inefficient investments. There has been some suggestive evidence linking misperceptions about ability with poor educational decisions (Chevalier et al., 2009; Connor et al., 2001; Stinebrickner and Stinebrickner, 2009), but, to my knowledge, no firm causal link has been demonstrated to date.[1]

This paper aims to fill this gap by analyzing a field experiment conducted in Malawi in

[1] Connor et al. (2001) conduct a qualitative analysis in England and Wales, showing that 13% of students cited uncertainties about their ability as the main reason for not going to university. Stinebrickner and Stinebrickner (2010) show that, in a sample of students at a U.S. college, a substantial proportion of dropouts are explained by students learning more about their own ability; presumably many of those students would have made different decisions if they had higher-quality information earlier on. Chevalier et al. (2009) find that beliefs about ability are correlated with educational outcomes or outcome expectations, even conditional on true ability.

2012. The field experiment targeted parents with children enrolled in grades 2-6 in primary school (mostly 8-16 year olds). I first measured the parents' beliefs about their children's current achievement levels. I then provided randomly selected parents with information about their children's true achievement (specifically, their average absolute achievement on tests administered in their school during the previous term, as well as their relative ranking within their class). Finally, I measured the effect of the intervention on parents' investments in education. The information delivered to parents during the intervention was very similar to the information that most developed and many developing countries already give to parents through report cards.[2]

The experimental results imply that inaccuracies in parents' perceptions about their children's academic abilities may have large, negative impacts on children's education in Malawi.[3] The first finding is that parents' perceptions of their children's recent achievement diverges substantially from children's true recent achievement: the average gap between the two is more than one standard deviation of the achievement distribution. Since achievement tests determine progression throughout school, and since inaccuracies in perceptions about recent achievement are also correlated with lack of knowledge about individual skills, this implies that parents may have inaccurate perceptions about the individual-level characteristics most relevant for educational investments. Parents also have inaccurate information about their children's performance relative to one another, with roughly one third of parents mistaken about which of their own children has higher achievement. Moreover, these inaccuracies may cause parents to misallocate their educational investments: while parents' investment choices depend clearly upon their beliefs, the relationship between their investments and their children's true achievement is much weaker.

The second finding is that, across a range of investments, providing parents with information caused them to reallocate their educational investments. To investigate whether

[2]In fact, the Malawi Ministry of Education requires that schools deliver absolute achievement information to parents. However, parents may still lack information because, first, the information is normally hand-delivered by the students and so often does not reach the parents, and second, parents often cannot understand the report cards.

[3]For the purposes of this paper, I use the concepts of "abilities" and "ability" to capture individual-level differences that affect the returns to investment. In my intervention, I proxy ability with measured achievement on school tests. Ideally, I would like to be able to separate the influences of "innate" ability from past effort and past investments, since they may have different implications for the returns to schooling investments, and thus parents' responses may depend on which aspect of "ability" is being measured. As has been extensively documented in the literature, however, it is difficult to measure "innate" aptitude, since most measures will combine many factors (e.g., genetic factors (Vinkhuyzen et al., 2009), early-life inputs (Paxson and Schady, 2005)), and separating all of these factors is not possible. Moreover, for my purposes, isolating "ability" from other factors is not crucial; rather, I am interested in how investments depend on any measure of individual-level returns. Although achievement may reflect school inputs more than some other measures one might use, it also has the advantage that, in this context, it is what parents anecdotally say is the most important factor determining the returns to different investments in their children's educations.

inaccurate perceptions prevent parents from choosing the right mix of complementary inputs to their children's education, I measured two investments where there is a relatively clear prediction for how the returns (and thus the efficient investment) vary with student achievement: willingness-to-pay (WTP) for remedial math and English textbooks, which should decrease with achievement because they are remedial (i.e., perceived substitutes with achievement), and choices among three, free subject-specific workbooks that are targeted for a child's performance level (remedial, average, and advanced). For both investments, I find that information shifted parents' investments, allowing them to do what they were trying (but failing) to do in the absence of information. For textbooks, parents who received information about their children's true performance increased their (relative) WTP for the textbook in the subject in which their child was doing relatively worse, which is what we would expect given the textbooks' remedial nature. For workbooks, information caused parents to shift their choices towards workbooks that corresponded more closely to their children's true achievement.

Inaccurate perceptions could also cause parents to misallocate their long-run educational investments. To gauge these effects, I conducted a lottery to pay for secondary school fees for one child in every 100 households in the sample (secondary school fees are one of the first high-cost investments that parents in Malawi make in their children's education). Parents in the sample received nine lottery tickets and chose how to allocate them across two of their children. I find that, at baseline, parents allocated more tickets to the child they believed was higher-achieving, but that, because many parents had mistaken beliefs about which of their own children had higher achievement, information caused parents to reallocate tickets towards the child that was truly higher-achieving.

Together, the first and second findings imply that parents' inaccurate perceptions about their children's academic abilities create a wedge between parents' intended investments (what they are trying to do) and their true investments (what they actually do). Whether this negatively impacts children's educational outcomes depends on what parents are maximizing and whether they know the correct production function. However, the fact that, for the above investments, we have relatively clear predictions for how the efficient investment depends on children's achievement (and parents' investments follow those predictions) implies that the negative impacts could be large.

I next investigate the dynamic implications. In particular, I investigate the hypothesis that inaccurate perceptions could serve to perpetuate educational inequalities across generations. If education enables parents to better judge their children's characteristics, then less-educated parents could have less accurate perceptions about their children than more-educated parents. This could cause them to make worse educational investments, and potentially prevent their children from attaining the same levels of human capital as children from more-educated parents.

Consistent with the hypothesis, I find that poorer and less-educated parents have less accurate perceptions about their children's academic performance than richer, more-educated parents. For example, the average gap between parents' beliefs and their children's true achievement is roughly 0.25 standard deviations, or 17 percentage points, lower in households where no parent has a secondary education compared with households where both parents have secondary education. Combined with the finding that information affects investments, these results imply that poor information could negatively impact the educational outcomes of children from poorer households more than the outcomes of children from richer households. It is still an open question, however, whether improving information could help to close these gaps. I find that information caused less-educated parents to update their beliefs more than more-educated parents, but the results are more mixed for whether the information also caused greater changes to the alignment of their investments with their children's achievement.

One potential concern with these results is whether the effects of information would persist beyond the controlled survey environment. To assess this, endline data on outcomes like expenditures and dropouts were collected for a subset of the sample one year after the information intervention. I find that information did affect later investments. For example, information increased the likelihood that a parent transferred their child to a different school, with parents at schools with low average achievement more likely to transfer their higher-achieving children (suggestive that they now thought it worth the cost of sending their child to a higher-achieving school) and parents at high-achievement schools more likely to transfer their lower-achieving children (suggestive that they had realized the school was not a good match).

As a second example, information affected dropouts: children of parents who had found out their children were doing well in school were less likely to drop out, while children of parents who had found out that their children were doing poorly were more likely to drop out. This suggests that the parents used the information to try to optimize their investments. Whether it led to a true increase in efficiency is an open question; however, the fact that the literature has generally suggested that education and ability are complements (Pitt et al., 1990; Aizer and Cunha, 2012) suggests it may have. It is important to note that these results also highlight that parents will use information in the way they see best, and so information is not a universal panacea for increasing education for everyone. When considering whether to increase information, policymakers need to trade off potential increases in efficiency with potential decreases in equity.

These results help advance our understanding of the causes of poor educational outcomes in developing countries. The literature has not yet answered the question of why, despite government measures across developing countries to improve access to education, large inequalities in educational attainment persist, both within developing countries and between less-developed and more-developed countries. For example, in Malawi, although primary schooling has been officially free since 1994 and the costs associated with enrollment are relatively low, the overall primary school completion rate is only 35%, and is over three times higher among students from the richest quintile of households than the poorest (World Bank, 2007). The literature has examined many factors (e.g., limited access to credit and school quality) to explain the poor outcomes, but none fully account for the patterns. This paper suggests that inaccurate perceptions about children's abilities may prevent some parents in developing countries from taking full advantage of educational opportunities, thereby stymying their children's educational outcomes.

This paper also contributes to several other strands of the literature. First, it contributes to the literature on how investments in education depend on returns. Several papers have examined whether individual investments in education respond to changes in the population-average returns to education (e.g., Abramitzky and Lavy, 2011; Freeman, 1976; Jensen, 2010; Kane, 1994; Nguyen, 2008; Oster and Steinberg, 2013), primarily finding that investments do respond to changes in returns. This paper builds on this work by showing that investments also respond to shocks to (perceived) individual-level returns. Second, it contributes to a large literature examining the role of market failures in education. Within that literature, it builds on a recent and growing stream examining the idea that it is perceptions, not reality, that drive educational investments (Banerjee et al., 2011). For example, Jensen (2010) shows that incorrect information about the overall returns to education affects investments, and that providing information can increase average educational outcomes. Finally, it contributes to a long literature that has examined how parents' investments depend on their children's ability (e.g., Behrman et al., 1994; Griliches, 1979). The relationship is difficult to estimate empirically, most notably because of reverse causality– since investments likely increase measured ability, one cannot distinguish whether high ability is a cause or effect of high investment. The recent literature has primarily used within-family comparisons and early-life ability measures, such as birthweight, to try to avoid the reverse causality concerns (e.g., Datar et al., 2010). However, this approach may not fully solve the identification problem, since it is difficult to find early-life measures that are not influenced by neonatal investments. This paper uses a within-person methodology that exploits the exogenous "shock" provided by the experiment to randomly selected parents' beliefs about their children's achievement (i.e., actual achievement relative to ex ante beliefs). By looking at how a parent's investments respond to finding out their children's achievement was lower or higher than expected, the analysis provides suggestive evidence on how each investment measured in the paper depends on children's achievement.

The remainder of the paper proceeds as follows. Section 2 describes the context and experimental design. Section 3 presents a simple conceptual framework. In section 4, I use the baseline data to examine whether parents have inaccurate perceptions and how that impacts their investments. Section 5 presents the results on the impact of information on shorter-run outcomes. Section 6 looks at longer-run outcomes. In Section 7, I conclude.

2 Context and Experimental Design

2.1 Context

Education System

Primary school in Malawi covers grades 1-8. Primary school has technically been free in Malawi since 1994, but it does involve other expenditures. Parents in the study sample spent an average of 1750 Malawi Kwacha (MWK) or roughly 10.6 USD annually per child per year in the study sample, which is an average of approximately 1% of annual household income per child.[4]) The largest required expenditure is for uniforms (an average of 580 Malawi Kwacha (MWK) or 3.51 USD per child per year in the study sample). Schools also charge informal required fees (an average of 380 MWK or 2.30 USD per child per year). The great majority of parents (89% of the baseline sample) also make supplemental investments in their children's education, expenditures like school supplies, supplemental tutoring offered by teachers or outside firms, and books. The average cost of these investments (not conditional on positive spending) is 790 MWK or 4.79 USD per child per year.

Dropouts are common in primary school: the nationwide primary school completion rate was only 35% in 2007 (World Bank). Although financial constraints undoubtedly play some role in why so many children fail to complete the primary cycle, in a recent World Bank report, the primary reason cited by pupils for dropping out was not financial, but instead "lack of interest", cited by 48% of dropouts. Lack of money trailed at a distant second (cited by 16%), followed by marriage/pregnancy (cited by 10%). Lack of interest may partially reflect poor performance: 40% of the parents in this study's sample who had had a child drop out during primary school before the baseline survey said that, when their child dropped out, the child no longer liked school because they were performing badly or because the material was too hard. Improving children's school performance and engagement

[4]Household income calculation excludes households where at least one parents' income was not reported (24% of households.)

early on could thus potentially improve attainment.

Secondary school, covering grades 9-12, is not free in Malawi, and is significantly more expensive than primary school. Annual secondary school fees for government schools range from 5,000 - 35,000 MWK per year (30 - 210 USD, over 4 times the median primary-school expenditures for children in my sample and 3-20% of household annual income). On top of that, parents must also purchase uniforms and supplementary supplies. As a result, anecdotally, many children do not attend secondary schooling as a result of the high fees.

Secondary schooling is also not open to all students. At the end of primary school, students take a standardized test, their performance on which is the sole determinant of their secondary school placement. The lowest performers fail the exam and are ineligible for any secondary school; the next tier pass but are not admitted to government schools (they can apply to private schools if they wish); the highest performers are admitted to government secondary schools.

School Report Cards

Schools are supposed to send report cards home to parents at the end of each term with children's achievement test results. The reports vary by school, but the majority have children's absolute scores on the achievement test and the absolute grades that those scores correspond to on the standard Malawian Ministry of Education grading scale of 1-4. (Appendix D contains two sample report cards used by schools in the study sample.) However, according to baseline survey data, 30% of parents in the study sample had not received any report cards from their child's school in the previous year. Since the reports are supposed to be hand-delivered by students, this could result from children losing the reports or choosing not to deliver them (parents of students who had performed badly within their classes are much more likely to report not receiving report cards). Schools or teachers may also sometimes fail to produce the reports, either because they run out of funds or did not want to put in the effort. Anecdotally, even if they receive the reports, many parents do not understand them, either because they cannot read or do not know how to interpret the information given.

2.2 Experimental Design

The basic idea of the experiment is to gauge parents' beliefs about their children's achievement, deliver true achievement information to randomly selected parents, and then measure the effects on parents' investments in education and student outcomes.

Sample

We worked with 39 schools in two districts in central Malawi (the Machinga and Balaka districts) for the study.[5] We started by conducting a sibling census during January - March of 2012, mapping out the sibling structures for all students enrolled in grades 2-6 at the schools. During March and April, 2012, we also gathered the term 2 achievement test data from the schools for all the students enrolled in grades 2-6. Test data was gathered for all "continuous assessments," or periodic exams administered during the term, and terminal exams, or exams administered at the end of the term. To create a single score (per subject) for use in the report cards and all of the analyses that follow, I use the Malawian Ministry of Education's grading guidelines to create weighted averages, where the weights are 40%/60% (grades 5-6), 60%/40% (grades 3-4), and 100%/0% (grade 2) for continuous assessments and terminal exams, respectively.[6] The average number of tests administered during the term to students in the final sample was 4.5 (the range was from from 1 to 7). Test questions are chosen by teachers from lists of standardized test questions contained in the standardized curriculum books given to all schools by the Malawi Ministry of Education.

Based on the achievement and sibling data, a sample of 3,464 households with at least two children enrolled in grades 2-6 with achievement test scores was drawn.[7]

Randomization

For households that had more than two children that met the sample inclusion criteria, I randomly selected two of the children for inclusion in the sample. Stratified based on proxies for parent education (specifically, school and principals' estimates of the maternal literacy rate in a family's village), and a measure of student achievement (specifically, the within-household between-sibling achievement gap), I randomly assigned half of the households to

[5]Schools were selected randomly from the universe of primary schools, oversampling schools with high and low expected levels of parent education to try to increase the heterogeneity in parent education within the sample.

[6]These weights are the weights that the Ministry of Education guidelines instruct teachers to use for calculation of end-of-term grades. If a class only offered continuous assessments (or terminal exams), the score used is 100% continuous assessments (or terminal exams). All continuous assessment exams were combined into an unweighted average to create the continuous assessment component of the score. If a student missed an exam, it was not included in their average: parents were informed of this and informed that it could lead to bias in their child's score if tests varied in difficulty and their child missed particularly easy or hard exams. This could differ from the method used by teachers, who sometimes will replace a child's score with a 0 if they missed the exam.

[7]I used multiple-children households as the sampling frame to be able to use parental allocations of lottery tickets across their children to study long-run investment allocations (described more below). Less than 3% of households in Malawi with children have only one child, so using multiple-child households does not have large costs in terms of external validity. The greater concern would be that households with tighter birth spacing would be over-represented in the sample since they are more likely to meet the sampling criterion. However, reassuringly, there are no within-sample gradients at baseline w.r.t. the between-sibling grade gap (e.g., no gradient in belief accuracy), either within or across parent SES categories.

a treatment group that received information about their children's recent achievement test results in school, and half to a control group, which did not receive information. Within the treatment group, half of households were assigned to a "detailed skills" treatment group which received more detailed information about students' performance (described more below).

Eligibility Interviews

Sample selection and treatment assignment were based on data gathered from students at school. Household eligibility was then verified by surveyors who conducted an eligibility questionnaire with the parents. Of the 3,464 sampled households, 21% of households were found to be ineligible during the parent interviews, leaving a sample of 2,716 eligible households.[8] Of the 2,716 sampled and eligible households, 97% (2,634 households) were found, available, and consented to participate in the baseline survey. Both eligibility and baseline survey completion are unrelated to treatment assignment.

Baseline Survey Visit and Information Intervention

All sampled households were visited by a surveyor. After conducting the eligibility questionnaire, the surveyor gathered baseline data, including baseline beliefs about both sampled children's achievement, and baseline education spending on each child.

During the elicitation of baseline beliefs, surveyors explained the grading scale used by Malawian schools to parents. As part of the explanation, they reviewed a sample report card with parents which had the same format as the report cards later delivered to the treatment group. This ensures that knowledge about the Malawian grading scale and whether the parent had seen a report card of the type used in the intervention was held constant across treatment and control groups.

For parents in the treatment group only, the surveyor then walked the parents through a report card that contained information about their own children's recent achievement test performance. The report card contained information about each student's absolute and relative performance on recent tests (See Appendix A for a sample report card), specifically, the child's absolute percent score, the absolute grade that that score corresponded to using the standard Malawian grading scale, and the child's "position rank" within their class-level distribution (equal to 100 minus their percentile rank, a statistic which is easier for parents in this environment to understand than percentiles given a long history of position rankings in schools). These statistics were included for the three subjects Malawian educators deem

[8]The most common cause of ineligibility was that both sampled children did not live in the same household. Eligibility for the initial sample was based on children's reports, and so ineligibility resulted from misreports by the children. There were also 18 households that were never successfully tracked; those are counted here as eligible.

most important (Math, English, and Chichewa, the local language), as well as for their "overall" performance (the average across those three subjects). The report card also told parents how many achievement tests were included in the averages displayed on the report card. Parents received the report card information for both sampled children in the family.

Surveyors walked each parent in the treatment group through every number on their children's actual report cards. The surveyors had received training on how to explain the information clearly to parents.

The numeric format for the report card was chosen based on a series of focus groups and qualitative interviews where local parents were shown a range of different formats, including ones with color coding or other visual aids. The primary criterion for selection was the ease with which illiterate parents could understand the information with the assistance of a surveyor.

Within the treatment group, the parents randomly assigned to the "detailed skills" treatment group also received an additional report card that discussed the child's performance on a series of 6 skills (2 math, 2 English, and 2 local language) chosen by local teachers as important skills for children in that grade to master (See Appendix B for sample). The grades displayed on the report card were assigned by the student's teachers. The format and grading scale for this report card were also chosen through qualitative interviews and focus groups. The point of this intervention was to see whether providing more details could help uneducated parents to become more engaged with their children's education.

I find that the detailed skills treatment had no effect on the outcomes measured (this was expected *ex ante*: the hypothesis was that the detailed skills treatment might affect later outcomes). I thus pool together the households from the detailed skills treatment with the other information treatment households in all of the results that follow.

2.3 Baseline Characteristics of Study Sample

Table 1 presents baseline sample characteristics and tests for balance across the treatment and control groups. 77% of respondents are female, and 92% of respondents were the primary decision maker about education in the household.[9] The level of education among parents is very low: the average years of education across parents in sample households is 4.7, and only 18 percent of households have one parent with at least secondary education.[10] 46% of respondents are farmers.

[9]The respondent identification protocol was to speak with the parent who was the primary decision maker if they were available. If the primary decision maker was unavailable, the surveyors spoke with the second parent if there was a second parent who was knowledgeable about educational decisions; if not, the surveyors returned later.

[10]Education levels among respondents is even lower: respondents have only 4.4 years of education on average, with only 11 percent with any secondary education or higher.

Household sizes are also large, with an average of 5 children per household. The children in the sample were in grades 2-6 and were 11.6 years old on average, primarily in the age range from 8 to 16 years old (the 5th and 95th percentiles of the age distribution). 51% of the children are female.

To test balance, I regressed each dependent variable on a dummy for being in the treatment group. The differences between the treatment and control groups are never large, and none are statistically significant at the 5% level except for students' baseline math achievement: students in the treatment group are performing a little worse than those in the control group at baseline (note that the different achievement measures are highly correlated, and that the overall achievement is just marginally not statistically significant). This is unlikely to confound the treatment effect estimates since the results mainly look at heterogeneity in treatment effects by child achievement. However, just to ensure this is not affecting the estimates, unless otherwise mentioned, I control for a student achievement measure in all of the regression results. Reassuringly, despite the lack of balance in performance, the accuracy of parents' perceptions (i.e., the absolute value of the gap between their beliefs and their children's scores) are balanced across treatment and control groups.

2.4 Measurement of Child Investment Outcomes

Remedial Textbooks and Level-Specific Workbooks

To analyze how inaccurate perceptions affect parents' ability to optimize the complementary inputs to their children's education, I measured parents' decisions about two investments for which we have fairly clear predictions about how the optimal investment depends on student performance: remedial textbooks and level-specific workbooks.

For the textbooks, I measured parents' willingness to pay (WTP) for subject-specific textbooks in Math and English. The textbooks are remedial (i.e., a perceived substitute with child achievement in this environment),[11] and so I can test whether textbook WTP decreases as parents find out their children are doing relatively better in a given subject. Parents' willingness to pay (WTP) for the textbooks was evaluated using a Becker-DeGroot-Marschak (BDM) methodology, which gives respondents an incentive to report their true WTP. Surveyors read parents a list of prices for the textbook (see Appendix C for a sample price list). For each price, the surveyor would ask the respondent whether she would commit to purchase the textbook at that price if that price was randomly chosen at the end of the survey. So, for example, the first question asked the respondent whether she would purchase the textbook if the randomly chosen price was 1,900 Malawi Kwacha (MWK), the textbook's

[11] Before the study began, I conducted a survey of teachers at schools in the sample and 100% of teachers surveyed thought that the textbooks were more useful in subjects in which children have lower achievement than in subjects in which children have higher achievement.

market price; the next question repeated the question for 1,700 MWK; the next for 1,500 MWK; etc. The procedure was repeated for two different textbooks, Math and English, for each child, and then one child, price, and textbook was randomly chosen at the end of the survey. If the parent's WTP for the chosen textbook was higher than or equal to the randomly chosen offer price, the parent would purchase the textbook.

Parents also make many non-monetary investments in their children that may depend on their children's achievement (e.g., asking a sibling to help a child with his homework). For credit-constrained parents, these non-monetary margins might be the primary adjustment margin. To capture this type of investment, I gave parents the choice between receiving 3 different subject-specific workbooks that were targeted for a child's specific achievement level: remedial, average, or advanced. Each parent was offered 4 workbooks (one in math and one in English for each of their two sampled children), and, for each workbook, chose which level they wanted to receive. I will examine how parents' choices correspond to their children's achievement levels. Although this choice is somewhat artificial, parents are continuously making decisions about educational inputs whose returns depend on performance. Analyzing their choice of workbook will provide us with a proxy for whether these other input choices are generally tailored correctly to their children's performance.

Secondary School Lottery

The workbooks and textbook investments will show us whether information enables parents to optimize their investments on the margin. To understand all the ways that information affects human capital development, we also want to know whether information can affect parents' larger investments, and especially their long-run choices of how far they want to send their children in school.[12]

To gauge this, since secondary schooling is one of the first high-cost, high-returns investments that parents make, I measured a short-run, real-stakes proxy for parents' secondary schooling investments. Specifically, I conducted a lottery to pay for four years of government secondary school fees for one child in every 100 households in the sample. Parents were given nine tickets for the lottery, and chose how they would allocate those tickets across their two sampled children.[13] There are two primary ways that student achievement might affect

[12] There are several reasons one might expect the answer to be different for parents' long-run education choices than for complementary inputs. When looking at complementary inputs, it is unlikely that parents would not try to maximize returns: conditional on spending a given amount on their child's education, it is unlikely that parents would choose to spend it in a less-effective way than a more-effective way. However, for long-run investments, there are other factors that might influence parents' investments besides returns maximization. For example, parents' long-run investments might be constrained by social norms (e.g., to educate the eldest son), and so information about returns could be inframarginal to the decision.

[13] There are two reasons that I used multiple tickets instead of one ticket: First, in a setting with inequality aversion, it increases my power to detect small shifts that would be inframarginal if there were just one ticket, and second, it allows me to also use this lottery to study inequality aversion (the subject of a different paper).

the expected return of a lottery ticket. First, perceived complementarity (substitutability) between performance and education would cause the perceived earnings return of a ticket to increase (decrease) with student performance. Second, the probability of admission to secondary school increases with achievement. At the end of primary school, students take a standardized achievement test, their performance on which is the sole determinant of their secondary school placement. The lowest performers fail the exam and are ineligible for any secondary school; the next tier pass but are not admitted to government school; the highest performers are admitted to government secondary schools. The lottery guidelines stated that it would pay for the full fees if the child were to be selected into government school, and 0 otherwise. The expected value of both the fees paid and the probability of attending secondary school (and thus receiving the secondary school earnings return) should therefore increase with a student's performance.

To test whether inaccurate perceptions cause parents to invest inefficiently in their children's secondary schooling, I will look at how their lottery ticket allocations depend on their children's relative performance, and whether information causes parents to shift their lottery tickets towards their higher-achieving children.

2.5 Data

I use data from two main sources in the analysis: survey data collected from parents, and administrative data gathered from schools.

Baseline Survey Data

The baseline survey was conducted between April and June of 2012. Baseline data gathered before the information intervention included modules gathering standard demographic information, income, and assets, as well as modules gathering parents' baseline spending on education, beliefs about the returns to education, and beliefs about their children's school performance. The survey also included the incentivized modules that are described above and are used to measure parents' educational investments (i.e., workbooks, textbooks, and lottery). These modules were administered after the information was delivered (in the treatment group) and so can be used to analyze the impact of the intervention.

Follow-up School Data Collection

At the end of term 3 of the 2011-2012 school year (July, 2012), we collected data from teachers' attendance books of students' attendance in the weeks following the baseline survey and end-of-year grades.[14] We were unable to collect this data from the full sample of schools,

[14] End-of-year grades are supposed to reflect performance throughout the year but, anecdotally, most teachers only take into account term 3 performance, so there could in theory still be impacts.

but the randomization was stratified by school so should be valid within the subsample. (There is no differential selection into having data by treatment status.)

Endline Survey

An endline survey measuring outcomes such as expenditures, dropouts, and other educational investments was conducted with a subset of the parents in the sample approximately one year after the baseline data was gathered (during June-July 2013).

3 Conceptual Framework

This section presents a simple model intended to motivate the empirical approach used in the paper. In the model, parents have a fixed budget for education and one child. The fixed budget, single-child model is used for expositional simplicity, and to capture the intuition that misperceptions may cause parents to choose an inefficient mix of investments even conditional on the level. The type of misallocation demonstrated in the model could also occur across children within the household. Moreover, misperceptions could also distort the level of parents' educational investments, a possibility which I discuss more during the empirical analysis.

Framework

A parent has a fixed budget for education to allocate between two inputs to her child's education. The efficient, returns-maximizing allocation can be found by solving the following maximization problem:

$$\max_{s^c, s^s} \; q(s^c, s^s, A) \text{ s.t. } s^c + s^s \leq y^{educ}$$

where q is the education production function; A is her child's achievement level; s^c, the first educational input, is a complement with achievement $\left(\frac{\partial^2 q}{\partial s^c \partial A} > 0\right)$; s^s, the second educational input, is a substitute with achievement $\left(\frac{\partial^2 q}{\partial s^s \partial A} < 0\right)$; and y^{educ} is her total budget for education. For example, s^s could be a remedial textbook which is more valuable for children who are not performing well in school, and s^c could be a candle to study alone at night, which is more useful for children who have a better understanding of the material.

Under standard assumptions (e.g., concave returns function), this problem will yield unique, returns-maximizing choice functions for educational inputs, $s^{c*}(A)$ and $s^{s*}(A)$, with the complement increasing in a child's achievement, $\frac{\partial s^{c*}}{\partial A} > 0$, and the substitute decreasing, $\frac{\partial s^{s*}}{\partial A} < 0$. For simplicity, we can parametrize these relationships as $s^{c*}(A) = \beta_0^c + \beta_1^c A$ and $s^{s*}(A) = \beta_0^s + \beta_1^s A$, with $\beta_1^c > 0, \beta_1^s < 0$.

Assume that the parent does not know her child's true achievement, A; instead, she has a belief about her child's achievement, PA, which may or may not be correct. Instead of choosing the returns-maximizing investments, $s^{c*}(A) = \beta_0^c + \beta_1^c A$ and $s^{s*}(A) = \beta_0^c + \beta_1^s A$, the parent instead chooses investments that depend on her beliefs,$s^{c*}(PA) = \beta_0^c + \beta_1^c PA$ and $s^{s*}(PA) = \beta_0^c + \beta_1^c PA$. As a result, she would earn lower returns than she could with perfect information: $q(s^{c*}(PA), s^{s*}(PA), A) \leq q(s^{c*}(A), s^{s*}(A), A)$.[15] This would reflect the fact that her investments would be less closely aligned with her child's true achievement, A.

Figure 1 plots this relationship graphically for simulated data, plotting parents' investments on the y-axis as a function of different measures of children's achievement on the x-axis. The left graph shows the complement, s^c; the right graph shows the substitute, s^s. The dashed line plots investments on the y-axis and parents' perceptions of their children's achievement, PA, on the x-axis. Since parents choose investments based on their perceptions, the lines will be steeply sloped with perceived achievement, with slopes of β_1^c and β_1^s for the complement and substitute graphs, respectively. (For notational simplicity, I will now refer to the slope of both lines as β_1.) These lines represent how parents *want* to invest. The solid line plots investments against true achievement, A (so the solid line has the same y-axis as the dashed line but a different x-axis). This line shows us how parents *actually* invest. If parents' perceptions were correct ($A = PA$), then the two lines would obviously be the same; if not, then the solid line will be flatter than the dashed line, with a slope of $\beta_1 \frac{Cov(PA,A)}{Var(A)}$.

Comparing the difference in the slopes of the two lines (i.e., $Abs.Val\{\beta_1(1 - \frac{Cov(PA,A)}{Var(A)})\}$), provides us with a way to gauge the inefficiency by measuring how far misperceptions push parents' investments from their desired investment path.The measure will range from 0 to β_1. A value of 0 would indicate that misperceptions are not affecting investments; for example, if there were no misperceptions (i.e., if $A = PA$) or if desired investments did not depend on achievement (i.e., if $\beta_1 = 0$), then the measure would have a value of 0. In contrast, if beliefs were completely independent of the truth, the difference would be maximized at β_1: parents' misperceptions would completely prevent them from optimizing their investments against their children's achievement.

[15] Note that this assumes that the parent is aware of the returns function, q. If the parent is unaware of the returns function, then we can interpret the above statement as holding for the parents' perceived returns function (e.g., that decreasing misperceptions will improve parents' *perceived* returns). Note that, since in general we do not know the true education production function, throughout the experimental results, we cannot evaluate whether the information improves the actual return on educational investment, only perceived returns. If the true returns function diverges from the perceived returns function, then, as in many other settings with multiple market or information failures, it is possible that improving student performance information could in fact interact with other misperceptions to move parents away from the individual optimum. This is one reason why it is useful to study investments where we have clear hypotheses about the true production function, like workbooks and textbooks.

Estimation

It is difficult to empirically estimate the difference between the gradient of investments on perceptions and the gradient of investments on true achievement because neither regression line will in general be causal. Assume parents' investments are determined by the model above plus a noise term (ε_i) and consider comparing the slope estimated from the following regression of investments on perceptions:

$$s_i = b_0^P + b_1^P PA_i + \varepsilon_i \tag{1}$$

with the slope estimated from the following regression of investments on true achievement:

$$s_i = b_0 + b_1 A_i + \varepsilon_i \tag{2}$$

The slope from regression 1 will be $\beta_1 + \frac{Cov(PA,\varepsilon)}{Var(PA)}$ while the slope from regression 2 will be $\beta_1 \frac{Cov(PA,A)}{Var(A)} + \frac{Cov(A,\varepsilon)}{Var(A)}$. Thus, comparing the slope will identify $\beta_1 \left(1 - \frac{Cov(PA,A)}{Var(A)}\right) + \left(\frac{Cov(PA,\varepsilon)}{Var(PA)} - \frac{Cov(A,\varepsilon)}{Var(A)}\right)$ and so will only give us a good measure of distortions due to misperceptions if the second term $\left(\frac{Cov(PA,\varepsilon)}{Var(PA)} - \frac{Cov(A,\varepsilon)}{Var(A)}\right)$ is equal to 0, that is, if unobserved determinants of investments are either uncorrelated or identically correlated with both A and PA.

However, now consider an intervention that changes parents' beliefs from PA to A. Estimating regression 2 with parents who have received the intervention (treatment parents) will now identify $\beta_1 + \frac{Cov(A,\varepsilon)}{Var(A)}$, whereas estimating it with parents who have not received the intervention (control parents) will, as stated above, identify $\beta_1 \frac{Cov(PA,A)}{Var(A)} + \frac{Cov(A,\varepsilon)}{Var(A)}$. Since the omitted variable terms are now identical between the two regressions, comparing the slope between treatment and control groups will now allow us to see whether misperceptions cause investments to diverge from parents' intentions; that is, to estimate $\beta_1 \left(1 - \frac{Cov(PA,A)}{Var(A)}\right)$.[16]

Extensions to the model

The above model assumes that parents' investments as a function of their perceptions, PA have the same slope, β_1, as their investments as a function of the truth, A. In reality, it is possible that the slopes could differ; for example, because of uncertainty, the slope of their investments as a function of perceptions (which I will denote by β_1^P) could be shallower than β_1 (i.e., $Abs\{\beta_1^P\} < Abs\{\beta_1\}$). As a result, comparing the slope between the two lines

[16]Note that this assumes that parents fully update their beliefs in response to the intervention. If they only partially update their beliefs, then the metric would be weighted downwards by the updating parameter (i.e., if updated beliefs were a weighted combination of A and PA with γ the weight on A, then the difference in slopes would uncover $\gamma\beta_1(1 - \frac{Cov(PA,A)}{Var(A)})$.

would now estimate $Abs.Val\{\beta_1(1 - \frac{\beta_1^P}{\beta_1}\frac{Cov(PA,A)}{Var(A)})\}$.[17] To assess whether $\beta_1^P = \beta_1$, one can test whether the information treatment effect on the slope of investments on achievement is equal and opposite to the information treatment effect on the slope of investments on perceptions.[18]

4 Baseline results: Parents have inaccurate perceptions that affect their investments

4.1 Gap between perceived and true achievement

I first examine whether parents have inaccurate perceptions by comparing each respondent's beliefs, elicited at the beginning of the baseline survey, about their child's achievement on the exams offered by their school during Term 2, with their child's true achievement on the same exams.[19] The left figure in Figure 2 shows kernel density plots of the parents' beliefs about their children's overall test scores (the solid line) and their child's true test scores (the dashed line). Scores are absolute scores expressed on a scale from 1 to 100.[20] Parents are overconfident: the mean of the believed achievement distribution is 16 points (or 0.9 of a standard deviation of the achievement distribution) higher than the mean of the true distribution. Beyond simple overconfidence, however, parents also have a range of misinformation about their children's achievement: the right figure plots a kernel density plot of each individual parent's beliefs relative to their child's true achievement. If parents

[17]Deciding whether this metric is more or less instructive than $Abs.Val\{\beta_1(1 - \frac{Cov(PA,A)}{Var(A)})\}$ will depend on the reasons why β_1 doffers from β_1^P.

[18]To see this, note that parent i with baseline perceptions PA_i and a child with true performance A_i will have a baseline investment of $s^P(PA)_i = \beta_0^P + \beta_1^P PA_i + \varepsilon_i$. After receiving information, her investments will become $s(A_i) = \beta_0 + \beta_1 A_i + \varepsilon_i$. As a result, the treatment effect as a function of A and PA can be expressed as $\tau(A_i, PA_i) = s(A_i) - s^P(PA_i) = \beta_0 + \beta_1 A_i + \varepsilon_i - (\beta_0^P + \beta_1^P PA_i + \varepsilon_i) = (\beta_0 - \beta_0^P) + \beta_1 A_i - \beta_1^P PA_i$. Thus, heterogeneity in the treatment effect by A will identify β_1 and heterogeneity by PA will identify $-\beta_1^P$, and so they should be equal and opposite if the investment gradient does not differ between perceptions and the truth.

[19]Beliefs were elicited using visual aids: parents pointed to the score on a visual scale. Elicited beliefs are binned at 5-point increments; results are robust to binning the true achievement scores at 5-point increments to make the comparisons.

[20]I focus on absolute performance information for two reasons. First, parents appeared to respond more to absolute than to relative performance (e.g., if one simultaneously analyzes how a parent responded to the shock to their absolute and to their relative beliefs, parents on average responded more to the shock to their absolute beliefs). Second, there was an implementation issue with the relative achievement information delivered to the first 595 treatment households. All of the absolute performance information they received was correct, but they received two pieces of incorrect relative performance information: for one child, in the space for true overall relative performance, their chichewa relative performance was listed (which has a correlation of 0.83 with the true overall), and for the other child, in the space for math relative performance, their english relative performance was listed (correlation of 0.55 with the true math). The results are robust to dropping the 595 treatment households (and corresponding controls) that received the incorrect relative performance information, and to using either absolute or relative performance for the analyses.

were simply overconfident by, say, 5-10 points, the plot would have all of its density between 5 and 10; rather, the density is spread widely. 21% of parents are under-confident.

The magnitude of the gap between perceived and true achievement is large: Table 1 shows that the average magnitude of the gap (i.e., the average absolute value of beliefs relative to the truth) is 21 points, or 1.2 standard deviations of the performance distribution. Parents also misperceive their children's within-subject performance, with average misperception levels of 22, 24, and 26 points for English, Chichewa, and Math (respectively), and their their children's between-subject (math relative to english) performance. There are also inaccuracies about the children's relative performance (child 1 relative to child 2), with the average parent's perceptions about the achievement gap between her children deviating from the true gap by an average of 18 score points (1.1 std. dev.), and 31% of parents wrong about which of their children has higher scores.

Misunderstanding the difficulty of the grading scale does not seem to drive inaccurate beliefs, as Appendix Figure A.1 shows similar patterns using children's relative performance (i.e., the child's percentile rank within his or her class) as well.

4.2 Baseline investments against beliefs: How parents are trying to invest

Parents' inaccurate perceptions about their children's academic abilities would only cause inefficiencies if parents' investments depend on their children's achievement (i.e., if $\beta_1 \neq 0$ in the notation of the conceptual framework). Building from the simple framework presented in Section 3, Figure 3 presents suggestive evidence on this by comparing the slope of investments on believed performance with the slope of investments on true performance. The data are from the control group only so that we can evaluate how investments are distributed in the absence of information.

The dashed lines plot local linear regression lines of parents' investment decisions on the y-axis against the parents' beliefs about their children's absolute achievement on the x-axis.

Workbooks (Complements)

Panel (a) shows the graphs for Math and English workbook choices: here, the y-axis represents the parents' choice between beginner/average/advanced workbooks, with the 3 choices parametrized as -1/0/1 for simplicity (so parents who chose a beginner workbook are coded with -1, average with 0, etc.)[21] As expected since workbook difficulty is a perceived complement with achievement, the dashed line slopes steeply upwards, showing that parents choose

[21] The relationship is robust to other parametrizations; e.g., indicators for choosing the beginner workbook, an indicator for choosing the advanced workbook. Recall that all workbooks are free; this choice shows us how parents tailor their non-monetary investments.

more difficult workbooks when they believe their children are performing better. The relationships are highly statistically significant: linear regression coefficients of workbook choice on parent beliefs (listed in the figure) yield t-stats of 33 and 43 for math and English.[22]

Textbooks (Substitutes)

Panel (b) shows the relationship for the investment that is a substitute with achievement: WTP for remedial textbooks. I analyze between-subject WTP (i.e., math WTP - English WTP) because it has more clear predictions for behavior than within-subject WTP: between-subject WTP holds constant parents' other investments in education, allowing us to isolate whether they spend relatively more in the subject in which they think their child is underperforming.[23] The y-axis shows the log of WTP for the remedial math textbook minus the log of WTP for the remedial English textbook.[24] The x-axis shows the parents' beliefs about the child's performance in math relative to English. As we would predict given the fact that the textbook is a perceived substitute with achievement (i.e., remedial), parents are willing to pay more for the book in the subject that their child is more behind in, and so the line slopes steeply downwards, with a t-stat of 15 in a linear regression.

Secondary School Lottery

Before analyzing how secondary school lottery ticket allocations depend upon beliefs about performance, I give a brief overview of the lottery data. Appendix Figure A.2 shows a histogram of how parents split their tickets between their children (i.e., the number of tickets given to the child with more tickets - the number of tickets given to the child with fewer tickets). Consistent with a high degree of inequality aversion, over 75% of parents split their nine tickets as evenly as possible. This means that, in most cases, I will be analyzing parents' choices of which child to give their ninth ticket to. This would be analogous to parents' real-world decisions when they have to make a choice between their children, for example, if they

[22] Each regression contains two observations per parent – one for each sampled sibling – and so standard errors for the linear regressions are clustered at the household level.

[23] To see that the within-subject predictions are less clear, consider a parent who has received negative information about their child's math achievement. Because the math textbook is remedial, holding all else constant, the parent's WTP for the math textbook should increase. However, all else is not held constant: the negative shock to math performance is correlated with a negative shock to overall performance, which means that, say, the parent might increase their estimate of the chances that their child will drop out of school in the next year, thereby decreasing the value of the textbook. The net prediction is ambiguous. In contrast, consider a parent who received negative information about how well their child was performing in math relative to English. In this case, comparing their math WTP with their English WTP would hold constant the parent's estimated chances of child dropout, and give the unambiguous prediction that math WTP should fall relative to English WTP.

[24] Logs are used to improve precision, but results are robust to using levels. Only 6% of WTP observations have values of 0; for these, I replace with the log of 10% of the lowest value of the price list, but, since there are so few 0's, the results are nearly the same regardless of whether I replace 0's with the log of 50% of the lowest value, the log of 10% of the lowest value, or drop the 0's from the regressions.

can only afford to send one child to secondary school and so are forced to choose between them. (The desire to look at forced-choosing drove the decision to offer an odd number of lottery tickets.) Note that the use of a 9-ticket lottery does not sacrifice any power relative to if I had just offered parents one ticket, but does allow me to look at inequality aversion (which I am exploring in a separate paper). In the following analyses, I use the number of tickets given to a given child as the dependent variable; the results are robust to using an indicator for whether the parent gave more tickets to a given child instead.

Panel (c) of Figure 3 shows how parents' ticket allocations in the control group depend on children's achievement. The dashed line shows the local linear regression line where the dependent variable is the number of secondary school lottery tickets given by the parent to the child they perceived was higher-achieving minus the number given to the child they perceived was lower-achieving, and the independent variable is the perceived performance gap between the perceived-higher-achieving child and perceived-lower-achieving child. The line slopes upwards, and remains above 0 throughout: across the treatment group, parents are giving more tickets to the child they think is higher-performing. Appendix Table 1 provides some suggestive evidence that this is not just because beliefs are correlated with other factors by showing that the predictive power of perceived achievement is robust to controlling for child gender or age. The slope is steepest near 0, which could reflect that parents care primarily about the rank order of their children (which makes sense given that this is a linear decision) and/or that parents' decisions are constrained by inequality aversion.

4.3 Baseline investments against true achievement: How parents actually invest

To provide suggestive evidence for whether inaccurate perceptions distort investments, I now compare how parents' baseline investments depend upon their children's true achievement with how their investments depend upon their beliefs. So, I compare the slope of the dashed lines in Figure 3 with the slope of the solid lines, which plot parents' investments against their children's true achievement. For the workbooks and textbooks, the solid lines thus have the same y-axis as the dashed lines, but a different x-axis. If parents had fully accurate information, the causal investment function would have the same slope on beliefs as it does on the true score; if parents' beliefs were random, the line on the true score would be flat, indicating that inaccurate perceptions would fully prevent parents from tailoring their investments to their children's performance level. The results show that the gradient of investments on true achievement is closer to flat than to the gradient on beliefs. For the textbooks and workbooks, although the solid lines all have non-zero slopes (statistically significant at the 5% level for the linear regressions), the coefficient magnitude and t-statistics for the slopes of the solid lines are much smaller than those for the dashed lines (only 20-33% of the magnitude for both coefficients and t-statistics).

For the lottery, the dependent variable of the solid line is now the number of secondary school lottery tickets given by the parent to the higher-achieving relative to the lower-achieving child, and the independent variable is the true performance gap between the higher-achieving child and lower-achieving child. Again, the solid line is more flat than the dotted line, and everywhere below it, with the difference between lines statistically significant everywhere except for near zero and large positive values on the x-axis, where density is low and where there might be a ceiling problem given inequality aversion constraints. The divergence between the lines suggests that parents are trying to give more tickets to their higher-performing child, but are prevented from doing so since they do not always know who their higher-performing child is.

The finding that investments have a steep gradient with beliefs but a much flatter gradient with the truth implies that parents try to tailor their investments to their children's achievement level, but are prevented from doing so by their own inaccurate perceptions of their children's achievement levels. However, this evidence is suggestive, not causal: both parents' beliefs and children's scores could be correlated with other factors that determine parents' investment decisions. That is, an alternate interpretation of the difference in the slopes between the perceived (dashed) and true (solid) lines is that beliefs and scores simply have different correlations with the unobserved determinants of investments. For example, parents who have a preference for a given subject might be overconfident about their children's achievement in that subject and might also invest more (difficulty or money) in that subject. This could produce results like we see for the complements (workbooks) but not the substitute (textbooks) investments. The omitted variable story to reconcile both the substitute and complements graphs would need to be more subtle, but is obviously possible.[25,26] As described in section 3, we can use the information experiment results to distinguish whether the difference is causal.[27]

[25]Indeed, more suggestive evidence for a causal interpretation comes from the fact that, consistent with the causal predictions laid out in Section 3, the slope of the true line for both the substitutes and the complements is an attenuated version of the slope of the beliefs line; there is no reason to expect that more general endogeneity stories would take this specific form.

[26]For the lottery, parents may not be trying to give more tickets to the higher-performing child and failing as a result of poor information, but rather could be giving more tickets to the child they like better, and liking a child better could be correlated with being overconfident about the child's ability.

[27]One variant of the omitted variable story would be that some other factor other than children's recent achievement (e.g., their underlying ability) underlies parents' decisions and is more highly correlated with beliefs than true achievement. Again, if that were the case, then we should not see investments respond to the information treatment.

4.4 Heterogeneity in belief accuracy by parent education

As outlined in the introduction, I am interested in testing the hypothesis that inaccurate perceptions are one of the factors that prevent the children of poorer, less-educated parents from attaining the same level of human capital as children of richer, more-educated parents. The first part of this hypothesis is that less-educated parents have less accurate beliefs, which I investigate in this section. The power of the analysis is limited by the limited heterogeneity in parent background within the sample, but I can still test whether the within-sample gradient is consistent with the hypothesis.

Table 2 regresses parents' belief accuracy (the absolute value of the gap between believed and true performance) on a measure of parents' education, specifically, an indicator for having completed any secondary education or higher, averaged across parents in the household.[28] Columns (1) through (11) show the results for overall, math, English, and Chichewa performance. The first column for each subject shows the raw gap: the coefficients indicate that less-educated parents have less accurate beliefs than more-educated parents, and are all statistically significant at the 1% (overall, math, and Chichewa) or 5% (English) levels. The second column for each subject adds controls for the characteristics of the parent we talked to within the household (whether they are female and whether they were the primary education decision maker) and for the child (gender and standard fixed effects); the gap remains largely unchanged. Finally, the third column adds school fixed effects, which attenuate the coefficients a little, but, except for English, the effects are still statistically significant at the 1% level. This suggests that it is not just that less-educated parents attend schools that give worse information, but that the gaps exist even within schools.

Appendix Table A.3 shows that the heterogeneity is not due to the particular measures used, but is robust across different measures of parent education and child achievement.

Since the children of less-educated parents have lower achievement than the children of more-educated parents, one potential explanation for the heterogeneity in belief accuracy could be if beliefs are less accurate at lower achievement levels. Note that this could still create a poverty trap, but is a distinct mechanism from the mechanism that less-educated parents are worse at judging children's performance in general. A first piece of suggestive

[28] The average across parents in the household is used for two reasons: First, the data (presented in Appendix Table A.2) provide suggestive evidence that there are both information sharing and information dilution between parents: Col. (1) and col. (2) shows that, for both mothers and fathers, both parents' education matter; if anything, the respondent's spouse's education matters more, although we cannot reject equal effects. Col. (3) shows that the respondent's own education matters more for one-parent households, which is consistent with information dilution, although there are obviously many other differences between one- and two-parent households. As a result, Col. (4) (and the specifications in the main tables) use the average across parents in the household. Second, consistent with the regression results, focus group discussions held before the project began also indicated that parents share information so the respondent's own education would be less informative than a household-level measure.

evidence that this does not explain the full gap comes from Table 2, columns (13)-(16), which show heterogeneity in belief accuracy about *relative* performance, either math relative to English or one sibling relative to the other. If lower performance fully explained the heterogeneity, then we would not expect heterogeneity in these relative measures, but instead there is statistically significant (albeit smaller) heterogeneity by parent education.

A second piece of evidence that the heterogeneity in belief accuracy does not only reflect achievement heterogeneity comes from running the following regression:

$$A_{ij}^B = b_0 + b_1 A_{ij} + b_2 Educ_i + b_3 A_{ij} \times Educ_i + \varepsilon_{ij} \tag{3}$$

where A_{ij}^B represents parent i's beliefs about their child j's achievement, A_{ij} represents the child's true achievement, and $Educ_i$ represents the education of parent i. If heterogeneity in belief accuracy were due purely to heterogeneity in A_{ij}, we would find $b_3 = 0$. In contrast, if more-educated parents have more accurate beliefs even conditional on performance, then we should find that $b_3 > 0$, that is, that true achievement is more predictive of the beliefs of more-educated parents than less-educated parents, conditional on a child's achievement level. Table 3 shows the results of the regression: across different specifications, there is statistically significant evidence that $b_3 > 0$, suggesting that less-educated parents' less accurate beliefs do not just result from their children's lower achievement. We also see that $b_2 < 0$, i.e., that more educated parents are better at perceiving when their children are at the bottom of the achievement distribution.[29]

5 Impact of information

5.1 Effect of information on beliefs

The information experiment only has power to affect investments if it changes parents' beliefs. To provide some evidence on this, at the very end of the baseline survey, after the treatment

[29] Appendix Table A.4 provides further suggestive evidence that the heterogeneity in belief accuracy by parent education reflects heterogeneity in their ability to assess a child's performance. The table shows how perception accuracy about overall (cols. (1)-(2)), math (cols. (3)-(4)), English (cols. (5)-(6)), and Chichewa (cols. (7)-(8)) achievement change as students progress through school. All specifications have household fixed effects to control for selection of parents as children progress. For Math performance (col. (3)), parents' belief accuracy decreases as students age, which probably results from the material getting more difficult, making it harder for parents to judge performance on their own. However, col. (4) shows that the pattern is less pronounced for more-educated parents. This is consistent with a role for parent judgement, and for less-educated parents having a harder time judging their children's performance as the material becomes more difficult. Note that the performance gap in math does not follow a similar pattern and so does not seem to explain the finding: the children of less-educated parents actually catch up to the children of more-educated parents in math as they progress through schools. For English and Chichewa, we do not see the same pattern, as it may be easier for parents to judge their children's language performance as their children improve and can speak and translate.

group had received information, surveyors asked respondents how well they thought that their children would perform on an achievement exam if they took it that same day (the day of the survey).[30]

Figure 4 compares the absolute value of the difference between each child's term 2 achievement and her parent's beliefs, using either the parent's beginning-of-survey beliefs (dark grey bars) or the parent's end-of-survey beliefs (light grey bars). (Note that the beginning-of-survey beliefs were elicited directly about the term 2 achievement whereas the end-of-survey beliefs were not.) The left two bars are for the control group, the right for the treatment. The first point is that we have balance: the p-value for the difference between the size of baseline inaccuracy of perceptions (the heights of the dark grey bars) between treatment and control is 0.83. Second, in the control group, the end-of-survey beliefs are, if anything, farther from children's true achievement, although the difference is small (just 0.9 points relative to a base of 20.4). In contrast, in the treatment group, the information appears to have affected beliefs, since the end-of-survey beliefs are 7.6 points closer to children's true achievement than beginning-of-survey beliefs were, a difference which is statistically significantly different (with a p-value$<$ .01) from both the control group's end-of-survey beliefs gap and from the treatment group's beginning-of-survey beliefs gap. Note that respondents do not fully update their beliefs, potentially because they are Bayesian updaters who had prior information about their children's underlying abilities.

5.2 Information treatment effects

Graphical evidence

I now examine whether the change in beliefs caused by the information treatment affected parents' investments in textbooks and workbooks. Figure 5 shows local linear regression plots of parents' investments on the y-axis against their children's true achievement on the x-axis. The solid line represents the control group (so is the same line from Figure 4); the dashed line represents the treatment group.

If the differences in Figure 4 between the control group's investments plotted against truth vs. beliefs represent the causal impact of inaccurate beliefs, then the treatment group's investments plotted against true achievement should be much steeper than the control group's

[30] Although re-asking about term 2 exams would have given a more direct measure of whether parents understood the information that they had been given, I did not do that for two reasons: (1) Survey pretesting results indicated that, for parents in the treatment group, asking about term 2 exams again seemed too much like we were testing them on what they had learned, and, for parents in the control group, they found it strange that we were exactly repeating a previous question; and (2) To understand how much parents updated their underlying beliefs of their children's academic abilities, it is more interesting to have a proxy for their children's academic abilities (which, if they are bayesian updaters, would be some weighted combination of their initial beliefs and the new information), not their static assessment of how their child performed on the term 2 exams (which, since it is knowable, should just be the new information).

investments on true achievement, and instead should look more like the control group's investments plotted on beliefs (dashed line in Figure 4): in the notation of the conceptual framework, both will have slope β_1.[31] In contrast, if the differences in Figure 4 were a non-causal result of correlations with omitted factors differing between truth and beliefs, then the treatment line should look the same as the control line.

Consistent with a causal interpretation, Figure 5 shows that the information treatment clearly affected the treatment groups' investments, causing them to look much more similar to the control groups' investments plotted on beliefs (i.e., more similar to the dashed lines from Figure 4).

Regression Evidence: Textbooks and Workbooks

I now perform a formal test of whether the information treatment changed the slope of the investment functions by running the following regression:

$$s_{ij} = c_0 + c_1 A_{ij} * Treat_i + c_2 A_{ij} + c_3 Treat_i + c_4' X_{ij} + \varepsilon_{ij} \tag{4}$$

where i indexes households, j indexes siblings, s is the investment, A is the relevant achievement metric (e.g., math for math workbooks, math - English achievement for between-subject textbook WTP), $Treat_i$ is an indicator for being assigned to the treatment group, and X_{ij} is a vector of control variables.[32] Since each household has multiple observations (one for each sibling $j \in \{1, 2\}$), standard errors are clustered at the household level.

The prediction is that the information treatment makes the slope steeper, so that $c_1 > 0$ for complements (the workbooks), and that $c_1 < 0$ for substitutes (textbook WTP). (Note that c_1 will provide a measure of the $\beta_1(1 - \frac{Cov(PA,A)}{Var(PA)})$ metric from Section 3 which allows us to assess the extent to which perceptions inaccuracy distorts investments.)

Table 4 presents the regression results: Panels A and B use math and English workbook choice as the dependent variables, and Panel C uses the log of WTP for the math remedial textbook minus the log of WTP for the English remedial textbook. Column (1) shows the base specification: consistent with the graphical evidence and the predictions of the model, c_1 is positive for the workbooks and negative for the textbooks. All 3 coefficients are highly

[31] Note that this assumes that parents fully update: if they do not fully update, then the investment gradient in the treatment group would represent a weighted average of the control group's investment gradient on truth and the control group's investment gradient on beliefs, with the weight on beliefs equal to the updating parameter weight placed on the new information.

[32] Results are robust to excluding the controls. Control variables include school fixed effects, parent education, the between-child score gap, and parents' education level. Note that this includes all variables underlying the stratification but not the stratum fixed effects themselves– since the intent was never to control for stratum FE, some of the stratum are very small, and so 20% of observations are lost because they have no variation in treat within their stratum if we control for stratum FE. The results are, however, robust to controlling for stratum FE.

statistically significant (p< .001). Moreover, the magnitude of the effects is large: comparing the coefficients on $Treat \times Score$ (slope in the treatment group) with the coefficients on $Score$ (slope in the control group) shows that the treatment increased the slope of the lines by 200%, 170%, and 430% for math workbooks, english workbooks, and textbook WTP, respectively. That is, parents' investments in the treatment group were 3, 2.7, and 5.3 times as responsive in the treatment group (relative to the control group) to a given change in child achievement.

Regression Evidence: Secondary School Lottery

Since the number of lottery tickets was constrained at the household level, the regression analysis obviously must include a household fixed effect. I thus run the following regressions:

$$Tix_{ij} = c_0 + c_1 Treat_i \times 1\{HigherPerformingSib\}_{ij} + c_2 1\{HigherPerformingSib\}_{ij} + \tau_i + \varepsilon_{ij} \tag{5}$$

$$\begin{aligned} Tix_{ij} = & c_0 + c_1 Treat_i \times 1\{HigherPerformingSib\}_{ij} + c_2 1\{HigherPerformingSib\}_{ij} + \\ & c_3 Treat_i \times A_{ij} + c_4 A_{ij} + \tau_i + \varepsilon_{ij} \end{aligned} \tag{6}$$

where Tix_{ij} represents the number of tickets given to sibling j in household i, $1\{HigherPerformingSib\}_{ij}$ is an indicator that sibling j is the higher-achieving sibling in his or her house, A_{ij} is child j's achievement, and τ_i is a household fixed effect. Equation 5 allows us to see whether the treatment caused parents to shift tickets towards their higher-achieving child: the prediction is that $c_1 > 0$. Equation 6 tests for whether the size of the effect depends on the performance gap between the children: the prediction is that $c_1 > 0$ and/or $c_3 > 0$, depending on whether parents primarily care about rank order or the performance gap.

Panel D of Table 4 shows the regression results. Column (1) shows the regression of Equation 5. The information treatment caused parents to allocate an average of 0.98 more tickets to their higher-scoring sibling compared (t-stat=7.5), a significant magnitude given that parents in the control group allocated, on average, .53 more tickets to their higher-scoring children. Column (2) tests for whether the information also changes the slope of the line (Equation 6), and finds that there is no statistically significant heterogeneity: parents seem to primarily use the rank order information, which is a reasonable way to make a decision in an all-or-nothing investment.[33]

[33] If parents were deciding solely based on returns and achievement were the only factor determining returns, then the rational decision would only depend on the rank order between their children. Obviously, parents are not only considering returns given the high level of demonstrated inequality aversion, but since 75% of parents are splitting their tickets as evenly as possible, one might think of them as fully constrained by inequality aversion to the 5/4 split, in which case the decision collapses to an all-or-nothing decision depending solely on rank order.

Regression Evidence: Robustness

One potential concern with the previous analyses is that performance is obviously not randomly assigned. Thus, if there is heterogeneity in the effect of information based on some other factor correlated with performance, then it could also cause the observed results. It is reassuring that the direction of the effects fits exactly the predictions of the framework in Section 3, combined with the baseline investments analysis from Section 4. In addition, columns (2) through (6) of panels A-C and (3) - (6) of panel D provide suggestive evidence that omitted variables do not drive the result by showing that the results are robust to controlling for household fixed effects (panels A-C only – all specifications already include household fixed effects in panel D) and to controlling for interactions of individual-level control variables with treatment.[34,35]

Additional treatment effects analysis: Lottery

One reason that parents might split their tickets so evenly between their children at baseline is that they are unsure of which child would be the better investment; another reason is that they are averse to investing unequally in their children. I provide some evidence on this question by regressing the absolute value of the gap between the tickets given to one sibling vs the other on $Treat_i$ (Results not shown). Since we already saw that the information intervention affected beliefs, if uncertainty were the primary driver, we would expect parents in the treatment group to split their tickets less equally than parents in the control group. I find that the treatment only increased the gap by 0.14 tickets on average, with the p-value for the difference only 0.17. This is equivalent to 73.5% of parents splitting their tickets as evenly as possible in the treatment group relative to 75.3% in the control group, so the differences are not large. Thus, although uncertainty may play some role in the equal outcomes, it does not seem to be a primary factor: inequality aversion likely plays a large role.

It is also natural to wonder how information will affect distribution of tickets along other dimensions that may be correlated with performance and/or perceptions. Given the

[34] Note that, if all we wanted to do was identify whether information caused investments to be more closely aligned with performance, then assuming that the treatment is exogenous to the error term (i.e., random assignment) is sufficient. To give a concrete example of the potential omitted variable concern: a respondent's initial beliefs uncertainty could cause them to respond to improved information by increasing their investments in their children's education, absent any change in the point estimate of their beliefs, since decreasing uncertainty should increase investments. If uncertainty is correlated with children's true performance, then the change in slope could pick up heterogeneity based on uncertainty, not based on the information itself. However, if this were the case, then controlling for other factors correlated with uncertainty interacted with treatment should attenuate the treatment effects, which is not what we see. Thus, this type of heterogeneity does not seem to be driving the effects.

[35] Appendix Table A.5 also provides suggestive evidence that the gradient of the investment function on beliefs is the same as the gradient of the investment function on true achievement: using the test described in section 3, we cannot reject that heterogeneity in the treatment effect by parents' baseline beliefs is the same as heterogeneity by true achievement.

widespread prevalence of underinvestment in girls' education, one might hypothesize that parents underestimate their daughters and that information could in fact help increase investment in girls' education. This is not what I find. If anything, parents in the treatment group allocated fewer tickets to their girls, although the difference (.25 tickets) is not statistically significant (p-value=0.21).[36]

5.3 Heterogeneity by parent education: Updating

Table 5 looks at whether less-educated parents change their beliefs more than more-educated parents in response to the information treatment. Column (1) replicates the finding (shown previously in Figure 4) that the information treatment affected beliefs by regressing the absolute value of the difference between a respondent's beginning-of-survey beliefs and end-of-survey beliefs about overall achievement on a dummy for treatment. (Recall that beginning-of-survey beliefs were about term 2 achievement whereas the end-of-survey beliefs were about a slightly different metric – how the child would perform on an achievement test taken that day – but that the difference can still proxy for the change in beliefs.) Column (2) shows that there is heterogeneity in the treatment effect by parent education: less-educated parents have a larger treatment effect on updating than more-educated parents, with every additional year of education decreasing the amount by which beliefs change by 0.37 score points (statistically significant at the 1% level). This means that going from no-education to completing primary school decreases the treatment effect on updating by roughly 3 score points, which, at 54% of the mean level of updating in the control group, is a sizable difference. Column (3) shows that the result is not due to child achievement levels differing by parent education, as it is robust to controlling for the interaction of treatment with child achievement. Part of the reason that less-educated parents update more is that they have less accurate baseline perceptions, but column (4) shows that that is not the full story. The specification controls for the interaction between treatment and baseline belief accuracy (i.e., the absolute value of the gap between initial beliefs and the truth); even in this specification, the heterogeneity by parent education remains sizable (just decreasing from 0.32 to 0.29) and is still significant at the 1% level. This is consistent with less-educated parents having more uncertain baseline beliefs and so having a larger bayesian updating parameter.[37]

[36] This could partially reflect the fact that parents in fact overestimate their girls relative to their boys, as girls are performing worse on average in school (roughly 2 points lower achievement) but parents believe their girls are performing almost as well as their boys (beliefs only 0.45 worse on average).

[37] Note that if less-educated parents simply expected that their children's achievement would change more from term to term, this should not vary across the treatment and control groups.

5.4 Heterogeneity by parent education: Treatment effects on investments

I now examine whether heterogeneity in belief accuracy and updating translates into heterogeneous effects of the information treatment on investment behavior. Whether it should theoretically translate is not obvious: the size of the treatment effect depends on many other factors (e.g., credit constraints for the textbooks, heterogeneity in beliefs about the complementarity of an investment and achievement) which could also differ by parent education. The workbooks should hold these other factors the most constant since there are no credit constraints (it is a free choice) and since beliefs about how optimal choice changes with ability should be relatively uniform, but even there there could be heterogeneity.

Figure 6 shows the treatment effect graphs for workbooks (panel (a)), textbooks (panel (b)), and the secondary school lottery (panel (c)), split between households with no parents with secondary education (left column) and households with any parent with secondary education (right column). Results are robust to using different education measures.

Workbooks

Starting with the math workbooks, there appear to be two differences between the graphs. First, the control (solid) line is flatter for the parents with no secondary education: they have less accurate beliefs at baseline about what workbook would match their children's baseline achievement. Second, the treatment (dashed) line is steeper for the parents with no secondary education: this is consistent with them updating their beliefs more. The patterns for English workbooks look similar, but less pronounced (consistent with the heterogeneity in belief accuracy being smaller in English).

Table 6 tests for heterogeneity in a regression framework. I find that less-educated parents' workbook decisions respond more to information than more-educated parents' decisions do. Columns (1) and (4) replicate the standard result without heterogeneity for comparison, for math and English workbooks respectively. Columns (2) and (5) show heterogeneity by parent education (specifically: by average years of education of parents in the household), and Columns (3) and (6) show heterogeneity in slopes for the treatment group only. For math (Col. (2)), the baseline (i.e., control group) slope is steeper for parents with more education (see the coefficient on $ScoreXParentYrsofEduc$, significant at the 5% level). As a result, more-educated parents change their investments less in response to information, with the treatment effect on the slope decreasing by roughly 6% (-.0012/.019) for each additional year of education, significant at the 1% level (Col (3), coefficient on $ScoreXTreatXParentYrsofEduc$). For English, the patterns are similar: less-educated parents have larger treatment effects, significant at the 5% level.

Textbooks

Turning to the textbooks in Table 6, the point estimate indicates that the investment gradient of less-educated parents changed more than those of more-educated parents (coefficient on $Score \times Treat \times ParentYrsofEduc$), but precision is low and the difference is not statistically significant. This only modest heterogeneity could partially reflect credit constraints, which could cause the investments of less-educated parents to respond less than those of more educated parents. There is statistically significant heterogeneity in the shift in the intercept: across all parents, the treatment increased relative WTP for math relative to English (this is because parents are in general more overconfident about math than English), but significantly less so for more-educated parents, who are less over-confident about math than less-educated parents (both absolutely and relative to English.)

Lottery

The lottery graphs (panel (c) of Figure 6) again look relatively similar for less-educated and more-educated parents. Table 6 shows that the treatment effects are smaller for more-educated parents, but that the heterogeneity is not statistically significant. Since there was small but significant heterogeneity by parent education in belief accuracy about between-sibling performance, the lack of heterogeneity in treatment effects could result either from a lack of statistical power, or heterogeneity in other dimensions (e.g., inequality aversion, the weight placed by more-educated vs. less-educated parents on their children's performance).

6 Results: Longer-run Outcomes

The above results demonstrate that information can affect parents' investments in education. One open question, however, is whether the effects of information will persist over time. To get at that, I turn to the results of the endline survey conducted with a subset of the parents roughly one year after the baseline survey, as well as data collected from schools in the following year.[38] The advantage of these data is that they allow us to gauge the persistence and the external validity of the earlier results (i.e., whether they also affected parents' investments made outside of the controlled survey setting). However, these results are noisier and harder to cleanly interpret than than those presented above since they reflect more other factors, including the reaction of the children to the information, the resulting responses of the parents to the children, etc..

6.1 Persistence of beliefs

I first check whether parents appear to remember the information one year after receiving it. If they completely forgot it, then the treatment effects would likely not have persisted

[38] For budget reasons, it was infeasible to conduct the endline survey with the full sample.

over time. In the end line survey, we elicited parents' beliefs about their children's current achievement. Appendix Table A.6 verifies that the endline beliefs of treatment parents correspond more tightly with their children's past achievement than the endline beliefs of control parents by regressing parents' beliefs at endline on their children's true past achievement (from term 2 of 2011-2012), $Treat$, and an interaction of past achievement with $Treat$. For beliefs about overall, math, and english achievement, one can see that the beliefs of parents in the treatment group are shifted downwards (coefficient on $Treat$ is negative) and more steeply related to past achievement (coefficient on $Treat \times Score$ is positive), although the relationship is much stronger and only statistically significant for overall. The overall information may have been more salient to parents.[39]

A second observation about the beliefs data is that beliefs change significantly over time: for parents in the control group, the correlation between their baseline beliefs and beliefs elicited in the endline survey is only 0.24. Although this may partially reflect changes in performance, it likely also reflects that beliefs can be transitory and uncertain (e.g., beliefs changed for the control group even within the course of the baseline survey.) As a result, it becomes difficult to conduct the analysis from section 4 and use the baseline beliefs to generate predictions for how parents should respond to the information. As shown in Appendix Table A.7, in the control group, current investments are generally only weakly related to baseline beliefs, but they are somewhat more strongly related to the parents' end line beliefs, which is suggestive that a lack of persistence in beliefs explains the weak correlations with baseline beliefs (this is obviously also consistent with reverse causality). So, in the next section, I turn directly to the information treatment effects without first analyzing the predictions from the control group. Note that this does not mean that parents in the control group were not guided by their beliefs when making their investment decisions, but rather that I do not have good enough measurement of parents' contemporaneous beliefs while making a given decision to generate meaningful predictions.

6.2 Information Treatment Effects: Endline Data

Figure 7 shows graphs for how information affected the primary longer-run investments measured. Unfortunately, due to smaller sample sizes and noisier outcome data than for the outcomes examined earlier, the lines are never statistically significantly different at a given point and so I remove the confidence intervals for ease of interpretation. The findings in the graphs and regressions are consistent.

Table 7 tests for how information affected investments measured after the initial baseline survey and whether information changed the slope of the investment function. Col-

[39]For chichewa, the relationship in fact goes the wrong direction; this could reflect that parents in the treatment group actually thought they helped their children to improve significantly in chichewa.

umn (1) tests for an average treatment effect; columns (2) and (3) present coefficients on $Treat$ and $Treat \times A$ from estimation of regression 4, and columns (4) and (5) present an alternative specification where the outcome variable is regressed on $Treat$ and $Treat \times 1\{AboveMedianA\}$ (where $1\{AboveMedianA\}$ is an indicator for having above-median achievement) for ease of interpretation. All regressions use the child's overall achievement on the term 2 2011-2012 achievement exams (the same measure used for earlier regressions) as the measure of A.

On the extensive margin (dropouts), there was no statistically significant increase in dropouts in the treatment group. However, high-achieving students in the treatment group were less likely to have dropped out of school, while low-achieving students were more likely, which is what we would predict given that most parents believe that education and achievement are complements. The change in the gradient of the investment line is significant at the 1% level. In terms of magnitudes, dropout fell by 1.5 percentage points for students who were above-median achievement and increased by 2.2 percentage points for students who were below (cols (4) and (5)). These are large effects relative to the control group mean (2%).[40,41]

On the intensive margin, first, there was an average effect on transfers (Panel E): parents in the treatment group transferred their children to a different school 3 percentage points more in the treatment group than control (significant at the 1% level), which could reflect an uncertainty effect. The fact that there is no significant heterogeneity by child achievement (col. (3)) may reflect the fact that the predictions vary by school type: at schools with low average achievement, finding out a child is doing well might make it worth the transport or monetary costs of changing him to a better school, so transfers would increase with achievement. In contrast, at high-quality schools, finding out a child is doing poorly might be indicative of a poor match, and so the prediction would go the opposite direction. And indeed, consistent with these predictions, if one looks at heterogeneity in the treatment effects by school quality (proxied by school-average achievement), transfers increase with child achievement at low-quality schools and decrease with child achievement at high-quality schools (results shown in Appendix Table A.9 and Figure 8a).

[40] These estimates define dropout as being no longer enrolled. If we include students who the parent did not expect to enroll in school the following year, the treatment effect coefficients are very similar in magnitude, but the control group mean is roughly twice as high (4%).

[41] Since parents were on average overconfident, the positive gradient shift would imply that overall dropouts should fall. There are two potential reasons why we do not see a statistically significant effect here: First, there could be an "uncertainty effect" (parents invest more due to decreased uncertainty) that would counteract the negative average shock received by parents; and second, dropouts did increase in the treatment group but we do not have the statistical power to detect it. Unfortunately, we do not have the precision here to distinguish between these hypotheses: I can neither reject a relatively large increase in dropouts overall, or that the uncertainty effect is large (which I assess by looking for a treatment group for households whose baseline beliefs were very close to the truth).

There was no significant effect on overall expenditures on education (either an average effect or an effect on the gradient of investments), attendance, or chores.[42]

There is suggestive evidence that the information affected parents' non-monetary investments, with a positive treatment effect of 0.065 standard deviations on an "index" measure (the average standardized effect across all investments measured, where all are normalized to have positive indicate an increase in investment), significant at the 1% level.[43] Surprisingly, however, there is no heterogeneity in this overall effect by child performance. There are several potential explanations for this. First, it could represent an uncertainty effect or an effect on parents' engagement with/empowerment about their children's education. Second, it could represent a Hawthorne effect, although both the treatment and control parents were aware that the study team was conducting an education study with them and their schools. Finally, there could be heterogeneity in some of the investments, but I could lack the statistical power to detect it, and/or the index measure mixes complements with substitutes.[44]

Discussion of the results

The dropout results highlight that information is not a panacea to increase educational attainment for all: although there was no average effect, the intervention did decrease enrollment among some populations of students. A full analysis of the welfare implications of these adjustments would rely on knowing both the production function and exactly what was crowded out or in. Unfortunately, the endline survey was too short to gather detailed information on all household expenditures and schooling for all children, so I cannot evaluate whether the changes to dropouts crowded in, say, extra schooling for other siblings in the household vs. expenditures for the parents.[45] What I can say is that the literature has generally found that ability and education investments are complements (e.g., Pitt et al. (1990) and Aizer and Cunha (2012)), and so the findings are consistent with increased efficiency.

[42] Appendix Table A.8 has the detailed breakdowns of all indices.

[43] The non-monetary parent investments index includes instructing the child to work on their homework, helping the child with their homework, asking others to help the child with homework, giving the child a light source to study at night, monitoring the child's exercise books, sending the child to school with food or water, pushing the child to attend school regularly.

[44] Appendix Table A.8 has the detailed treatment effects for each item in the index. For example, light sources to study might be more useful for children who are doing well in school, and indeed the heterogeneity goes that way but is not significant. In contrast, asking someone else to help a child with their homework is likely a substitute with achievement, and there we see that the effect is larger for households with below-median achievement children than for those with above-median achievement (7.1%, statistically significant at the 5% level for below-median vs. 3.1%, which is not statistically significantly different from 0 for above-median), although we cannot reject equality.

[45] I do have data on the schooling of one sibling in the household and can rule out that changes to the school enrollment of children who were induced to stay in school or drop out were fully offset by changes to the enrollment of that sibling, but households have 5 children on average so a different siblings' education could have been affected.

6.3 Heterogeneity in the Regression Results by Parent Education

Columns (1) - (4) of Table 8 examine whether there is heterogeneity in the observed effects by parent education. Surprisingly given there were no significant effects in the full sample for expenditures, there is significant heterogeneity by parent education: less-educated parents in the treatment group increased their expenditures on their lower-achieving children relative to their higher-achieving children (cols (1) and (2)). But, the more educated parents become, the more they instead begin to spend more on their higher-achieving children relative to their lower-achieving children, until (based on linear extrapolation), the gradient changes direction at roughly 5 years of education and parents begin investing more in their higher-achieving than lower-achieving children. (Note that this does not just result from the linear specification, as the conclusion is similar when one estimates the relationship less parametrically within different education bins.)[46] A similar pattern holds for attendance: for less-educated parents, attendance increases more for low-achieving children, whereas for more-educated parents, attendance increases more for higher-achieving children. Figure shows the attendance and expenditure heterogeneity results graphically.

The heterogeneity by parent education in the treatment effects on attendance and expenditures do not seem to be driven by heterogeneity in baseline expenditures or in perceptions about the complementarity between ability and schooling, since controlling for the interaction of those variables with $Treat$ and with $Treat \times Score$ does not attenuate the heterogeneity by parent education. There could, however, be heterogeneity by parent education in their returns based on their place on the production function. For less-educated parents, who might not expect that their children could attend secondary school, the (perceived) returns to gaining further skills could be somewhat concave (e.g., high returns to gaining initial skills like reading and low returns after), which could be why they compensate more. In contrast, for more-educated parents who might think their child has some chance but not a guarantee of admission to secondary school, the (perceived) returns might be convex (i.e., high return for pushing the child over the hump into secondary school admissions), causing them to reinforce.

Thus, these results suggest that, relative to the outcomes examined in section 4.4, there might be greater heterogeneity in the production function faced by – and thus the efficient returns-maximizing action – parents of different education levels. In that sense, it does not make sense to talk about which type of parents respond "more"; rather, they respond differently.

[46]These results are robust to trimming the outliers, e.g., top-coding the data at the 99th percentile.

7 Conclusion

This paper tests whether parents' inaccurate perceptions about their children's academic abilities impact their investments in their children's education. I find that there are large discrepancies between parents' beliefs about their children's recent achievement and their children's true recent achievement. At baseline, parents try to tailor their investments to their children's achievement levels, but their inaccurate beliefs prevent them from doing so. Providing parents with information significantly impacts their investments, allowing them to invest in the way that they were trying to without information.

I also find significant heterogeneity in belief accuracy and treatment effects by parent education. Less-educated parents in the sample have less accurate information about children's recent achievement, and update their beliefs more in response to improved information. Some of their investments also respond more to improved information. Taken together, the findings support the hypothesis that inefficiencies caused by misperceptions could provide one channel through which educational inequalities persist across generations despite expansions in educational access.

One open question for future research is how the negative impacts of parents' misperceptions can best be mitigated. The findings here suggest that a one-time infusion of information can help to change short- and medium-run beliefs, but it is an open question whether other interventions, such as more sustained, repeated information interventions, would be more effective in changing behavior in the long-run. It is also an open question whether information would be more effective if interacted with other interventions. For example, the textbook results provided suggestive evidence that credit constraints may have inhibited less-educated parents from taking full advantage of the information treatment. Future research can further explore whether information needs to be combined with other resources to more significantly impact children's education in developing countries.

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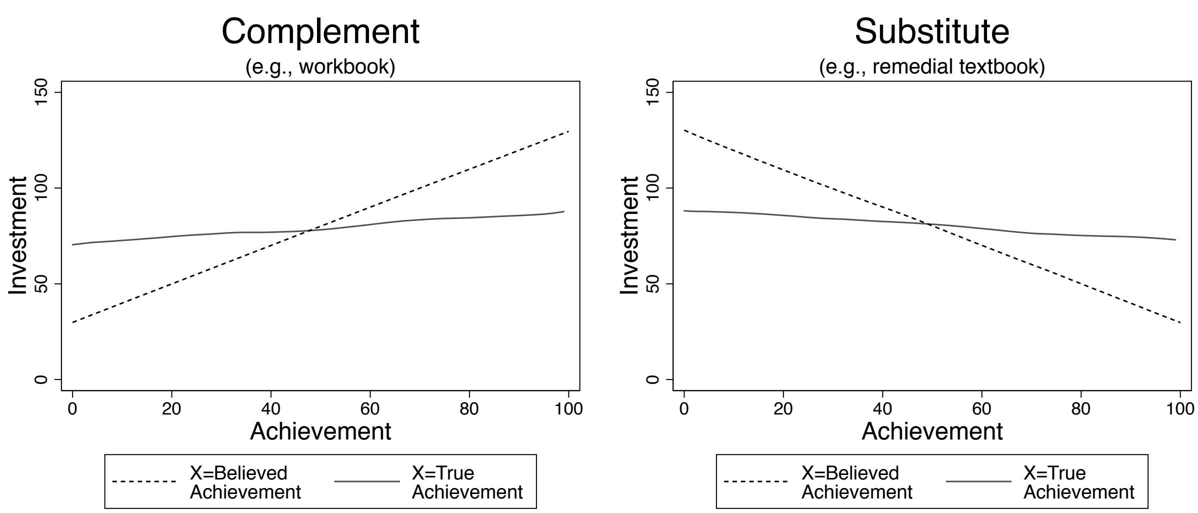
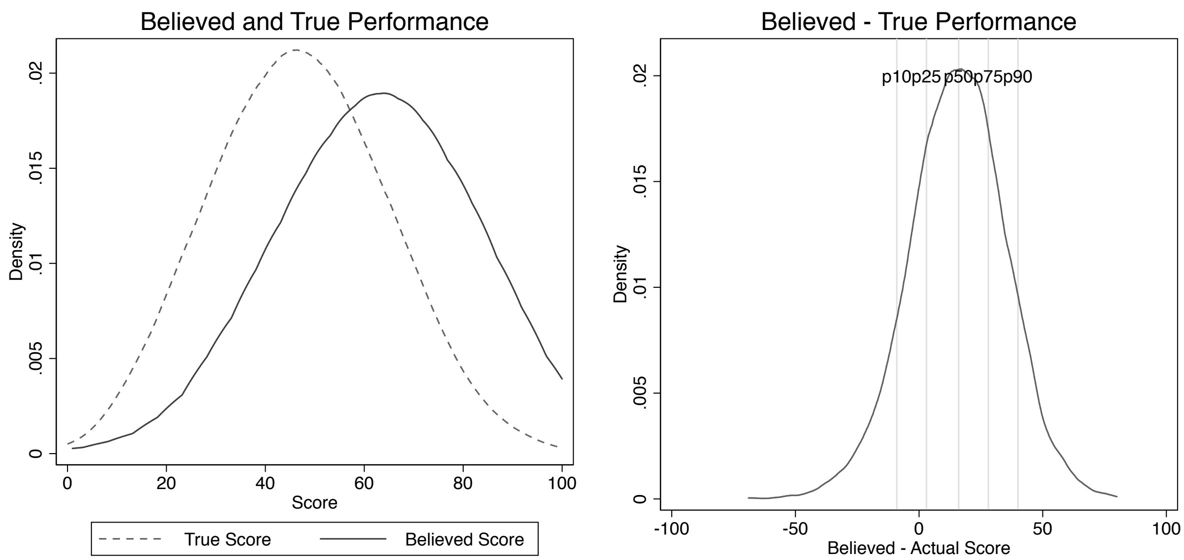
Figure 1: Conceptual framework

Inaccurate perceptions could cause investment gradient on truth to be more flat than on beliefs



Notes: Illustrative graph, not based on real data.

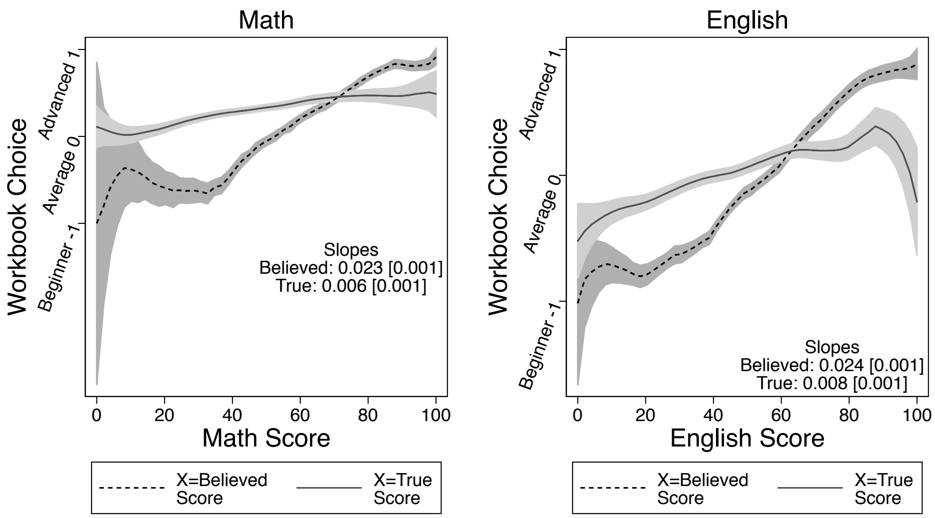
Figure 2: Parents have misperceptions about their children's achievement



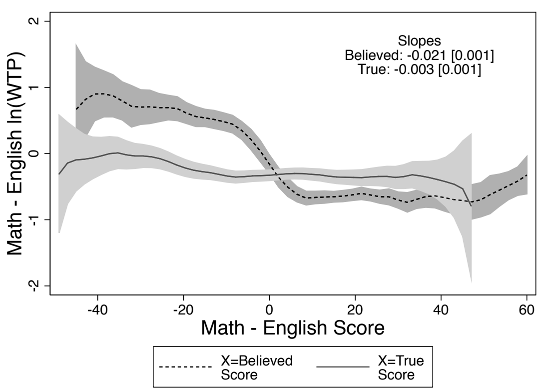
Notes: Data source is baseline data (full sample). The left graph shows kernel density plots comparing the distribution of parents' beliefs about their children's Term 2 2011-2012 achievement test performance, elicited at the beginning of the baseline survey, with the distribution of their children's true Term 2 achievement test performance. The right graph shows a kernel density plot of the distribution, across parents, of each parent's beliefs about their child's test scores relative to their child's true test scores. The lines represent the percentiles of the distribution.

Figure 3: Consistent with an inefficiency, the investment gradient on true achievement is more flat than on believed achievement

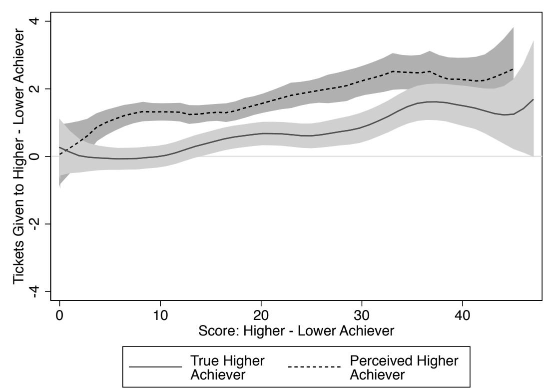
(a) **Workbooks (Complements)**



(b) **Textbook WTP (Substitute)**

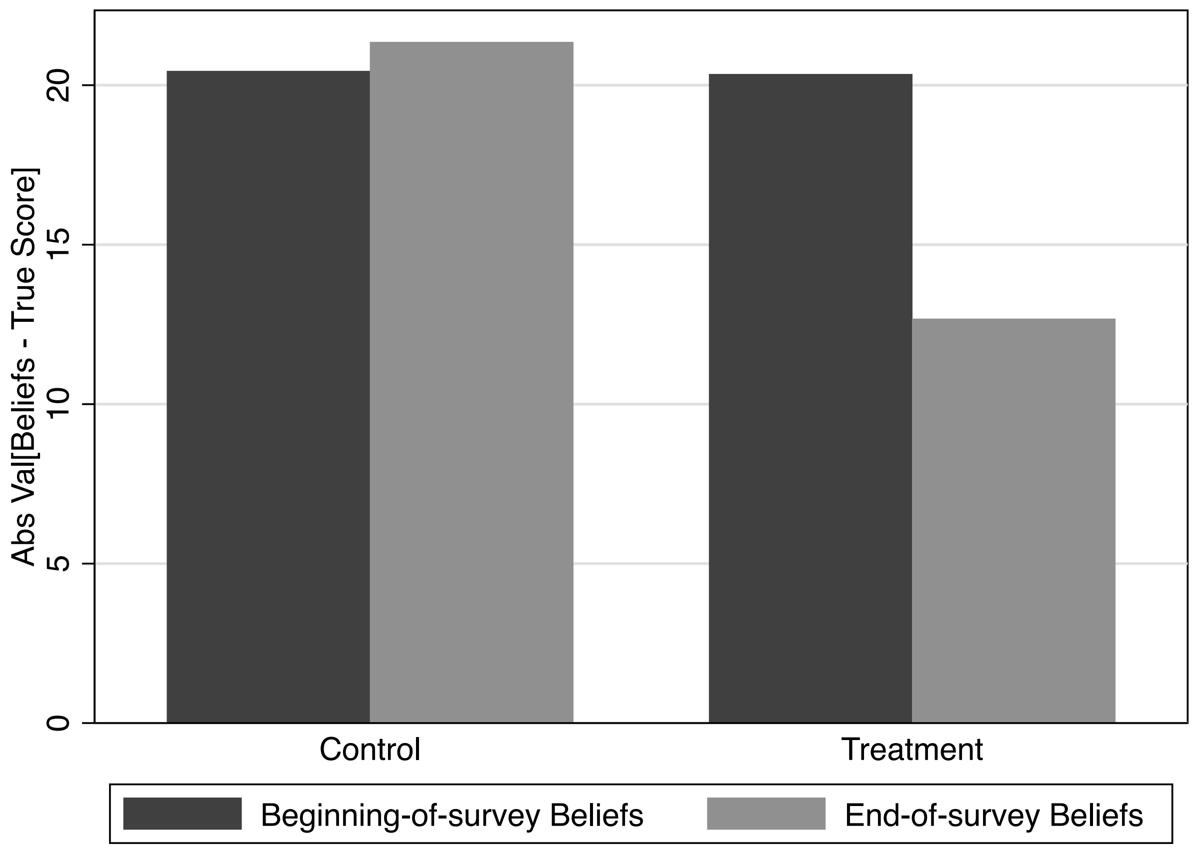


(c) **Secondary School Lottery**



Notes: Data source is baseline data. All lines are local linear regression lines with investments as the dependent variable and either true (solid line) or perceived (dashed line) achievement as the x-axis. Beliefs were elicited from parents at the beginning of the baseline survey. For the workbook graphs (panel (a)), the dependent variable is the parent's choice among 3 level-specific workbooks which are parametrized as -1 (beginner), 0 (average) and 1 (advanced). For textbook WTP (panel (b)), the dependent variable is the difference in the parent's log WTP for a remedial math textbook relative to a remedial English textbook. For the secondary school lottery, the dependent variable is tickets given to the higher relative to the lower achiever (so, for the dashed line, the dependent variable is the number of secondary school lottery tickets given by the parent to the child they perceived to be higher-achieving relative to the number given to the child that was perceived to be lower-achieving child and the x-axis it the perceived gap between the perceived-higher-achiever and the perceived-lower-achiever. For the solid line, the dependent variable is the number of secondary school lottery tickets given by the parent to the higher-achieving relative to the lower-achieving child and the true achievement gap (higher - lower achiever) is the independent variable). The grey areas represent 95% confidence intervals.

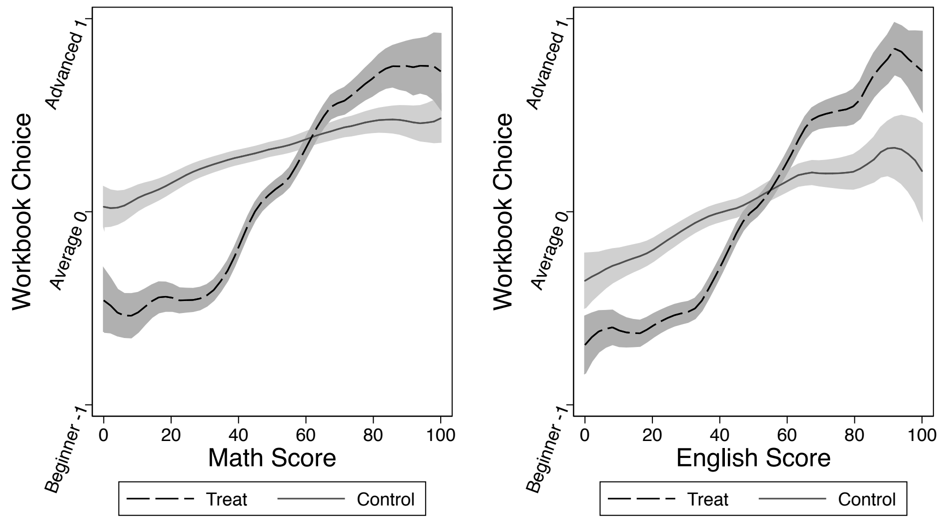
Figure 4: Information shifts parents' beliefs towards their children's true achievement



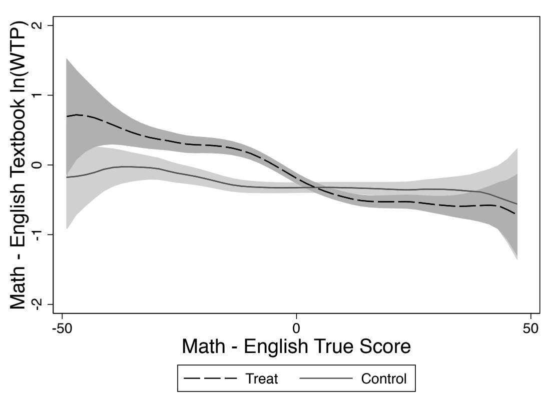
Notes: Data source is baseline survey data. The dark gray bars show the absolute value of the difference between children's true term 2 2011-2012 achievement test scores and their parents' "beginning of survey" beliefs about those scores, which were elicited at the beginning of the baseline survey (before the information treatment). The light gray bars show the absolute value of the difference between children's true term 2 achievement test scores and their parents' beliefs about their children's hypothetical scores if they took an achievement test on the day of the baseline survey, which were elicited at the end of the baseline survey (after the information treatment). The p-value for equality between the treatment and control groups for the height of the dark grey bars is .825 (i.e., there is balance) while the p-value for equality between the treatment and control groups for the height of the light grey bars is ¡.01, as is the p-value for the difference between the heights of the dark and light gray bars for the treatment group.

Figure 5: Treatment effects: Information increases the gradient of investments on true achievement

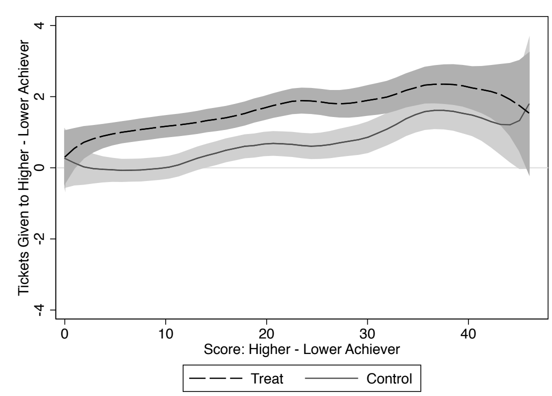
(a) **Workbooks (Complements)**



(b) **Textbook WTP (Substitute)**



(c) **Secondary School Lottery**

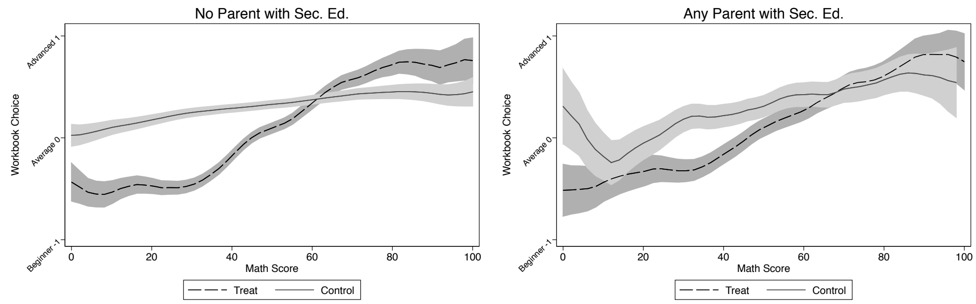


Notes: Data source is baseline survey data. All lines are local linear regression lines with investments as the dependent variable and true achievement as the x-axis. For the workbook graphs, the dependent variable is the parent's choice among 3 level-specific workbooks which are parametrized as -1 (beginner), 0 (average) and 1 (advanced). For textbooks, the dependent variable is the difference in the parent's log WTP for a remedial math textbook relative to a remedial English textbook. For the secondary school lottery, the dependent variable is tickets given to the higher relative to the lower achiever. The grey areas represent 95% confidence intervals.

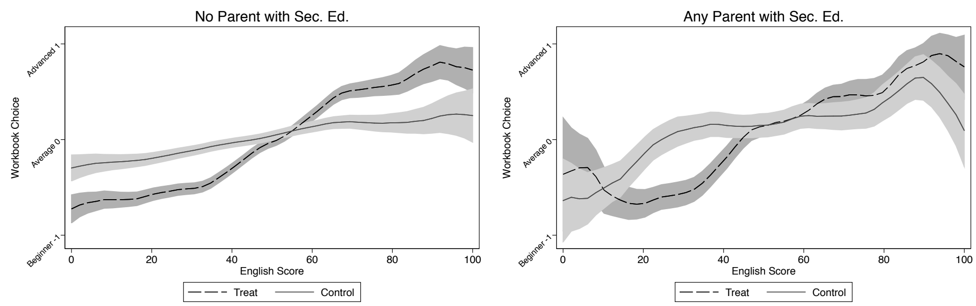
Figure 6: Heterogeneity in treatment effects by parent education (Textbooks, workbooks, secondary school lottery)

(a) **Workbooks (Complements)**

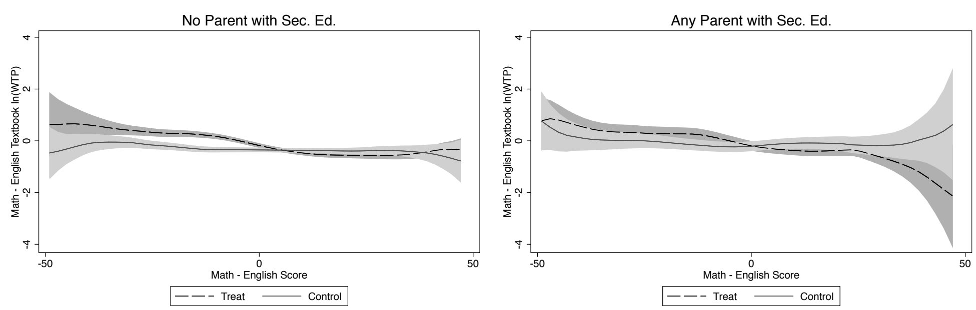
Math Workbooks



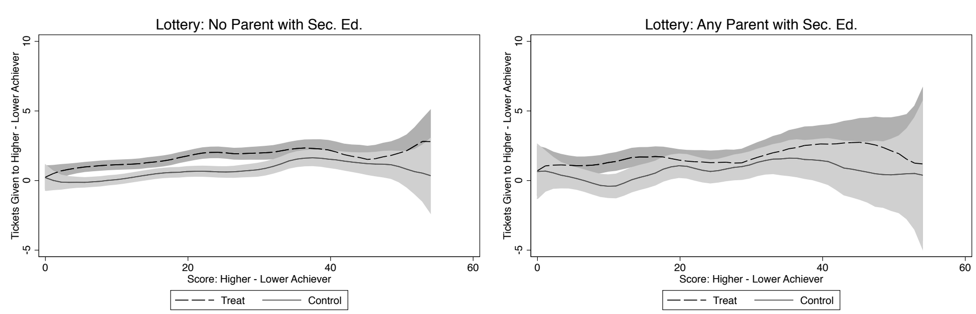
English Workbooks



(b) **Textbook WTP (Substitute)**

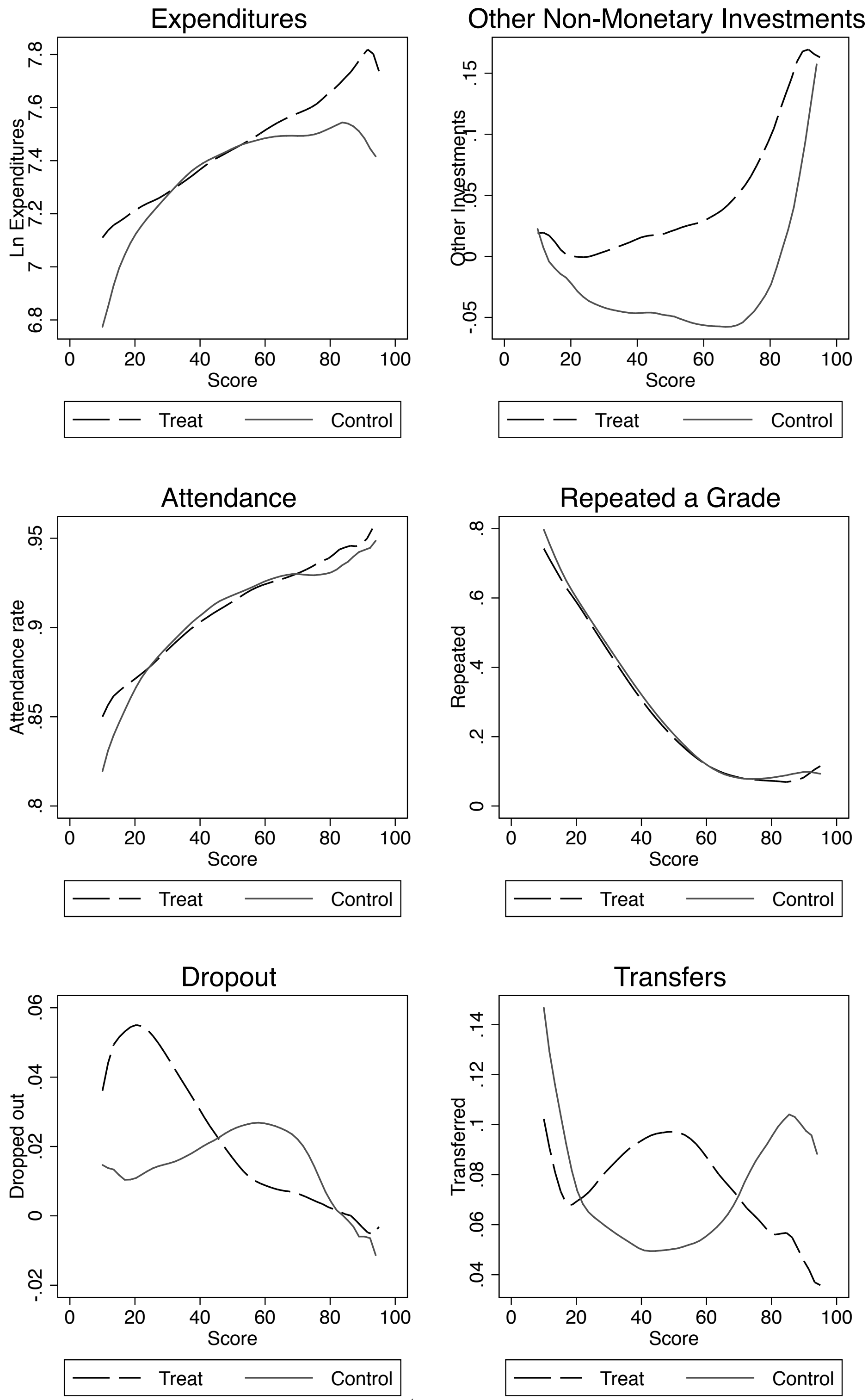


(c) **Secondary School Lottery**



Notes: The figure contains the same figures displayed in Fig. 5 (See notes for Fig. 5 for more detailed description) but estimated separately for households with no parents with at least secondary education (left column) and households where at least one parent has secondary education or higher (right column).

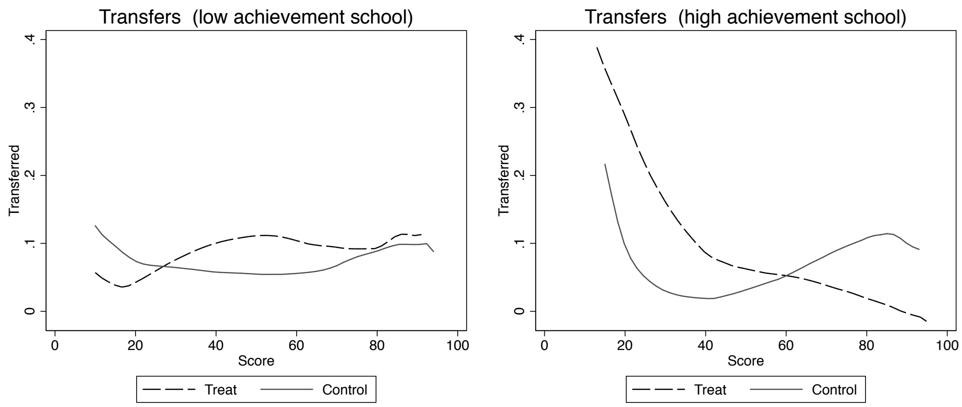
Figure 7: Treatment effects: Longer-run outcomes



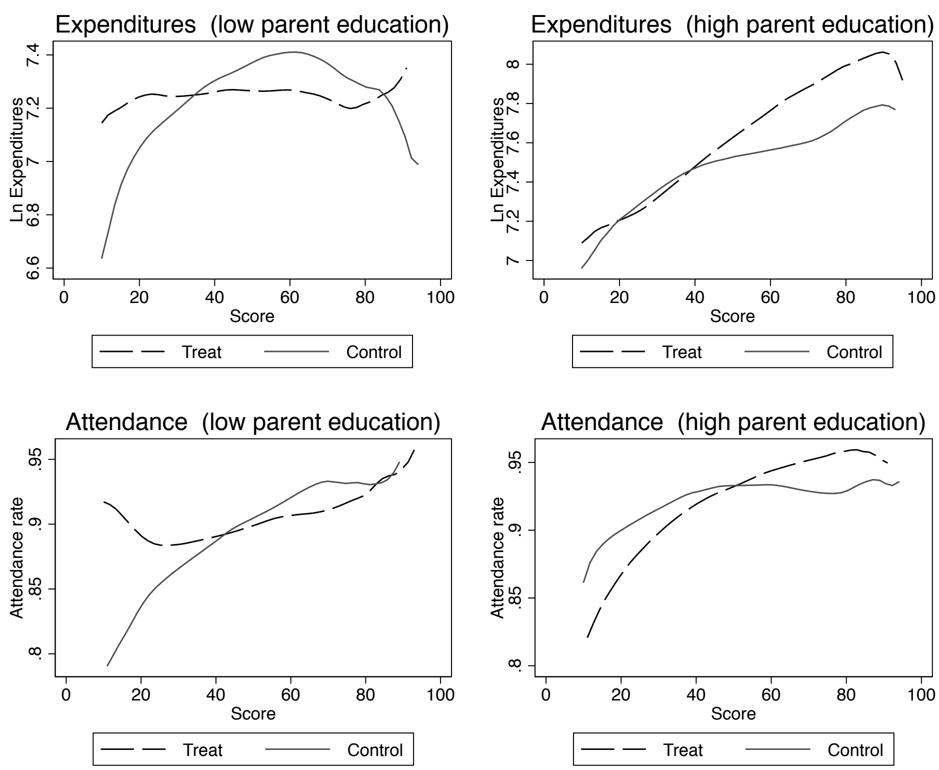
Notes: Data sources are endline survey data (expenditures, other non-monetary investments, homework help, asked for homework help, dropout, and transfers) and data collected from schools (attendance rate, grade repetition). All lines are local linear regression lines with investments as the dependent variable and true baseline achievement as the x-axis.

Figure 8: Heterogeneity in longer-run treatment effects (Selected outcomes)

(a) **Heterogeneity by school average achievement**



(b) **Heterogeneity by parent education**



Notes: The figure contains the same figures displayed in Fig. 7 (See notes for Fig. 5 for more detailed description) but estimated separately for different subsamples. In panel (a), the results are estimated separately for schools in the top quartile of overall student achievement (right graph) and schools not in the top quartile (left graph). In panel (b), the results are estimated separately for households where parents have above-median average years of education (right column) and below-median average years of education (left column).

Table 1. Baseline Summary Statistics

	Full Sample		Control	Treat	Treat - Control		
	Mean	SD	Mean	Mean	Mean	Std. Error	P-val T=C
Respondent Background							
Female	0.77	0.42	0.77	0.76	-0.01	0.02	0.37
Primary education decisionmaker	0.92	0.27	0.91	0.92	0.01	0.01	0.31
Age	40.8	11.0	40.7	41.0	0.3	0.4	0.47
Education (years)	4.4	3.6	4.4	4.5	0.0	0.1	0.78
Respondent has secondary education +	0.11	0.31	0.11	0.11	0.01	0.01	0.62
Parent can read or write Chichewa	0.67	0.47	0.67	0.68	0.01	0.02	0.67
Respondent is farmer	0.46	0.50	0.47	0.46	-0.01	0.02	0.70
Respondent's weekly income	2,126	4,744	2,051	2,203	197	194	0.31
Household Background							
Number of chidren[a]	5.1	1.7	5.2	5.1	-0.1	0.1	0.47
One-parent household	0.19	0.39	0.19	0.20	0.01	0.02	0.47
Parents' average education (years)	4.7	3.3	4.7	4.6	0.0	0.1	0.74
Any parent has secondary education+	0.18	0.38	0.17	0.19	0.02	0.01	0.24
Student Information							
Child's grade level	3.7	1.4	3.7	3.7	0.0	0.0	0.94
Child's age	11.6	2.7	11.7	11.6	-0.1	0.1	0.21
Child is female	0.51	0.50	0.52	0.50	-0.02	0.01	0.25
Baseline attendance	0.91	0.13	0.92	0.91	0.00	0.00	0.72
Annual per-child education expenditures	1,742	2,791	1,712	1,772	58	83	0.48
Fees paid to schools	381	1,128	384	378	-7	24	0.78
Uniform expense	576	1,019	548	603	50	36	0.17
School supplies, books, tutoring, and other supplementary[b]	785	1,819	780	790	14	62	0.82
Any supplementary expenditures on child	0.90	0.30	0.90	0.89	-0.01	0.01	0.49
Achievement Scores							
Overall score	47	17	47	46	-0.7	0.5	0.11
Math score	45	20	45	44	-1.1	0.5	0.04
English score	44	20	44	44	-0.6	0.5	0.29
Chichewa score	51	23	52	51	-0.6	0.6	0.34
(Math-English) Score	1	20	1	1	-0.5	0.5	0.30
Respondent's Beliefs about Child's Achievement Scores							
Believed Overall Score	62	17	63	62	-0.8	0.5	0.11
Believed Math Score	65	19	65	64	-0.9	0.6	0.09
Believed English Score	55	21	56	55	-0.7	0.6	0.25
Believed Chichewa Score	67	19	67	67	-0.1	0.6	0.87
Beliefs about (Math-English) Score	9	21	10	9	-0.2	0.6	0.71
Respondent's Misperceptions about Child's Achievement							
Abs Val{Believed-True Overall Score}	20	14	20	20	-0.1	0.4	0.77
Abs Val{Believed-True Math Score}	26	18	26	26	-0.1	0.5	0.85
Abs Val{Believed-True English Score}	21	16	22	21	-0.6	0.5	0.23
Abs Val{Believed-True Chichewa Score}	24	18	24	24	0.2	0.5	0.73
Abs Val{Believed-True (Math-English) Score}	22	17	22	22	-0.4	0.5	0.39
Abs Val{Believed-True Overall Score (Child 1-2)}	19	15	19	18	-0.4	0.6	0.55
Beliefs about Returns to Education							
Returns to education (secondary school/primary earnings)	3.22	3.79	3.28	3.16	-0.11	0.15	0.47
Believes education and achievement complementary	0.91	0.29	0.90	0.91	0.00	0.01	0.68
Sample Sizes							
Sample Size- Households	2,634		1,327	1,307			
Sample Size- Children	5,268		2,654	2,614			

Notes: Data Source is baseline survey. Standard errors for the t-test of equality (T=C) clustered at the household level.
a. Counted as a child if either of the primary caregivers for the reference child is a parent of the child.
b. Includes exercise books and pencils, textbooks and supplementary reading books, backpacks, and tutoring expenses.
c. Respondent said that they thought the earnings of a more able child would increase "more" or "much more" than the earnings of a less ab getting a secondary education

Table 2. Less educated parents have less accurate beliefs

	Dependent Variable= Abs. Val. (True - Believed Performance)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Performance Measure:	Overall	Overall	Overall	Math	Math	Math	English	English	English
Secondary Education +	-3.552***	-3.530***	-2.200***	-4.870***	-4.957***	-4.864***	-1.877**	-1.874**	-1.119
	[0.720]	[0.720]	[0.751]	[0.867]	[0.867]	[0.921]	[0.836]	[0.832]	[0.870]
Observations	5,019	5,019	5,019	5,021	5,021	5,021	5,021	5,021	5,021
R-squared	0.005	0.007	0.047	0.006	0.020	0.050	0.001	0.005	0.028
Dep Var Mean	20.41			25.82			21.42		
	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Performance Measure:	Chichewa	Chichewa	Chichewa	Math - Engl	Math - Engl	Math - Engl	Child 2 - 1	Child 2 - 1	Child 2 - 1
Secondary Education +	-4.464***	-4.206***	-2.484***	-1.472*	-1.461*	-2.090**	-1.915*	-1.768*	-1.242
	[0.868]	[0.864]	[0.935]	[0.830]	[0.834]	[0.922]	[1.000]	[0.996]	[1.087]
Observations	5,021	5,021	5,021	5,021	5,021	5,021	5,017	5,017	5,017
R-squared	0.005	0.024	0.059	0.001	0.005	0.027	0.001	0.007	0.028
Dep Var Mean	23.84			22.12			18.68		
Col. Specification Details									
Child and Parent Controls		✓	✓		✓	✓		✓	✓
School FE			✓			✓			✓

Notes. Robust standard errors in brackets. Standard errors clustered at the household level. Child and parent controls include a control for child gender, grade FE, parent gender, and whether the parent is the primary education decisionmaker. "Secondary Education +" measures the average across parents in the household of an indicator for whether they obtained a secondary education.
*** p<0.01, ** p<0.05, * p<0.1

Table 3. Relationship between beliefs and true achievement: Heterogeneity by parent education

	Dependent Variable = Respondent's Beliefs about Child's Score			
	(1)	(2)	(3)	(4)
Subject	Overall	Math	English	Chichewa
True Score	0.305***	0.189***	0.276***	0.234***
	[0.0142]	[0.0146]	[0.0171]	[0.0131]
True Score * (Secondary Education +)	0.158***	0.205***	0.123**	0.116***
	[0.0431]	[0.0426]	[0.0498]	[0.0432]
Secondary Education +	-7.405***	-12.59***	-0.794	-5.558**
	[2.369]	[2.188]	[2.616]	[2.821]
Observations	5,019	5,021	5,021	5,021
R-squared	0.122	0.060	0.090	0.086

Notes. Robust standard errors in brackets. Standard errors clustered at the household level. Include controls for child gender and grade FE. "Secondary Education +" measures the average across parents in the household of an indicator for whether they obtained a secondary education.
*** p<0.01, ** p<0.05, * p<0.1

Table 4. Information treatment effects (Textbooks, workbooks, and secondary school fee lottery)

	(1)	(2)	(3)	(4)	(5)
Panel A. Dependent Var: Math Workbook Choice					
Treat x Math Score	0.013***	0.012***	0.012***	0.011***	0.011***
	[0.00093]	[0.0017]	[0.0017]	[0.0017]	[0.0017]
Math Score	0.0065***	0.0081***	0.0081***	0.0084***	0.0084***
	[0.00065]	[0.0012]	[0.0012]	[0.0012]	[0.0012]
Treat	-0.91***				
	[0.049]				
HH FE		✓	✓	✓	✓
Observations	5,239	5,239	5,239	5,239	5,239
R-squared	0.217	0.695	0.695	0.696	0.696
Panel B. Dependent Var: English Workbook Choice					
Treat x English Score	0.013***	0.013***	0.013***	0.013***	0.013***
	[0.00096]	[0.0017]	[0.0017]	[0.0017]	[0.0017]
English Score	0.0076***	0.0089***	0.0089***	0.0086***	0.0085***
	[0.00073]	[0.0012]	[0.0012]	[0.0012]	[0.0012]
Treat	-0.68***				
	[0.048]				
HH FE		✓	✓	✓	✓
Observations	5,239	5,239	5,239	5,239	5,239
R-squared	0.204	0.710	0.710	0.714	0.715
Panel C. Dependent Var: ln(Math Textbook WTP) - ln(English Textbook WTP)					
Treat x Math - English Score	-0.013***	-0.013***	-0.013***	-0.014***	-0.014***
	[0.0022]	[0.0038]	[0.0038]	[0.0039]	[0.0039]
Math - English Score	-0.0030*	-0.0016	-0.0015	-0.00048	-0.00041
	[0.0016]	[0.0025]	[0.0025]	[0.0028]	[0.0028]
Treat	0.15***				
	[0.041]				
HH FE		✓	✓	✓	✓
Observations	5,183	5,183	5,183	5,183	5,183
R-squared	0.033	0.601	0.601	0.602	0.602
Panel D. Dependent Var: Lottery tickets received					
Treat * (Higher-scoring Sibling)	0.98***	0.98***	0.98***	0.94***	0.95***
	[0.13]	[0.22]	[0.22]	[0.21]	[0.22]
Treat * (Overall Score)		0.0017	0.0017	0.0052	0.0036
		[0.0090]	[0.0090]	[0.0088]	[0.0091]
Higher-scoring Sibling	0.53***	-0.16	-0.17	-0.16	-0.19
	[0.091]	[0.16]	[0.15]	[0.15]	[0.16]
Overall score		0.034***	0.034***	0.031***	0.033***
		[0.0064]	[0.0064]	[0.0063]	[0.0064]
HH FE	✓	✓	✓	✓	✓
Observations	5,258	5,258	5,258	5,258	5,080
R-squared	0.105	0.125	0.129	0.161	0.175
Column Includes Controls for:					
Treat * Female			✓	✓	✓
Treat * Grade level				✓	✓
Treat * Educ. Expenditures					✓

Notes: Each observation is a child. Standard errors clustered at the household level. All regressions control for school FE, parents' education, level, and the between-child score gap. All regressions control for the main effects of any variables interacted with Treat. Workbook choices are -1 for beginner, 0 for average, 1 for advanced. The results can be interpreted as follows: Take for example Panel A., column (1). The coefficient on true score (here: True Math Score, .0065) is the slope of the line in the control group: if a child's score increased by one point, the expected chance that their parent chose the next level of workbook increases by .65% higher. The coefficient on Treat x Score represents the change in slope for the treatment group; the coefficient of .013 means the treatment increased the slope by roughly 200% (.013/.0065).
*** p<0.01, ** p<0.05, * p<0.1

Table 5. Less-educated parents update their beliefs more than more-educated parents

	Dependent Variable: Abs Val (End-of-Survey - Beginning-of-Survey Beliefs)			
	(1)	(2)	(3)	(4)
Treat * (Parent Years of Education)		-0.37***	-0.32***	-0.29***
		[0.11]	[0.11]	[0.10]
Treat	8.29***	10.01***	14.59***	-3.30***
	[0.36]	[0.64]	[1.11]	[1.26]
Parent Years of Education		-0.02	-0.02	-0.02
		[0.06]	[0.06]	[0.06]
Treat * (Overall Score)			-0.10***	0.07***
			[0.02]	[0.02]
Overall score			0.00	0.02
			[0.01]	[0.01]
Treat * Abs Val{Believed-True Overall Score}				0.48***
				[0.03]
Abs Val{Believed-True Overall Score}				0.03
				[0.02]
Observations	4,984	4,951	4,951	4,951
R-squared	0.126	0.133	0.143	0.305
Dep. Var Mean in Treat	13.72			
Dep Var Mean in Control	5.456			

Notes: Standard errors clustered at the household level. Beginning-of-survey beliefs measure beliefs elicited before the information intervention about Term 2 2011-2012 achievement (the same metric delivered to parents. End-of-survey beliefs measure beliefs elicited after the information intervention about the child's achievement if they were to take an achivement test that day. Parent years of education is average across parents in the household.
*** p<0.01, ** p<0.05, * p<0.1

Table 6. Treatment effect heterogeneity by parent education

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Math Workbook			English Workbook			Textbook WTP			Lottery		
Dependent Variable	*100 for Advanced,0 for Average, -100 for Beginner*			*100 for Advanced,0 for Average, -100 for Beginner*			*ln(Textbook WTP) for Math - English*			*Tickets given to higher - lower achiever*		
Sample	*All*	*All*	*Treat Only*	*All*	*All*	*Treat Only*	*All*	*All*	*Treat Only*	*All*	*All*	*Treat Only*
Score * Treat	0.013***	0.019***		0.012***	0.016***		-0.013***	-0.017***		0.0017	0.0033	
	[0.00093]	[0.0016]		[0.00096]	[0.0017]		[0.0022]	[0.0037]		[0.0096]	[0.017]	
Score * Treat* Parent Yrs of Educ		-0.0012***			-0.00066**			0.00073			-0.00031	
		[0.00027]			[0.00029]			[0.00059]			[0.0029]	
Score	0.0065***	0.0029**	0.021***	0.0077***	0.0061***	0.022***	-0.0030*	0.000055	-0.017***	0.034***	0.033***	0.038***
	[0.00066]	[0.0011]	[0.0012]	[0.00073]	[0.0013]	[0.0011]	[0.0016]	[0.0026]	[0.0027]	[0.0072]	[0.012]	[0.013]
Score * Parent Yrs of Educ		0.00078***	-0.00041**		0.00032	-0.00032*		-0.00058	0.00014		0.00023	-0.000063
		[0.00020]	[0.00019]		[0.00022]	[0.00019]		[0.00038]	[0.00045]		[0.0020]	[0.0020]
Treat	-0.91***	-1.22***		-0.68***	-0.79***		0.15***	0.30***		0.97***	1.05***	
	[0.049]	[0.086]		[0.048]	[0.086]		[0.041]	[0.071]		[0.22]	[0.40]	
Treat * Parent Yrs of Educ		0.065***			0.023			-0.032***			-0.017	
		[0.015]			[0.015]			[0.012]			[0.070]	
Parent Yrs of Educ	0.0022	-0.039***	0.023**	0.0083**	-0.0029	0.019*	0.0079	0.024***	-0.0083	-0.00075	0.0067	-0.012
	[0.0034]	[0.011]	[0.010]	[0.0035]	[0.012]	[0.0097]	[0.0062]	[0.0084]	[0.0089]	[0.020]	[0.051]	[0.048]
Score Used	*Math*	*Math*	*Math*	*English*	*English*	*English*	*Math - Engl*	*Math - Engl*	*Math - Engl*	*Higher - Lower Child*	*Higher - Lower Child*	*Higher - Lower Child*
Observations	5,203	5,203	2,588	5,203	5,203	2,588	5,183	5,183	2,575	2,611	2,611	1,299
	0.216	0.220	0.292	0.206	0.207	0.325	0.033	0.035	0.059	0.045	0.045	0.027
P-val: Treat * Perf * Educ=0		5.0e-06			0.022			0.22			0.91	

Robust standard errors in brackets. Standard errors clustered at household level. Each observation is a child (cols (1)-(9)) or a household (cols (10)-(12)). *** p<0.01, ** p<0.05, * p<0.1

Table 7. Information treatment effects: Longer-run outcomes

Variable	A. Ave. treatment effect: Coeff est. (Std. Error) for Treat	B. Heterogeneity in treatment effects by performance - linear spec: Coeff est. (Std. error) for Treat	B. Heterogeneity in treatment effects by performance - linear spec: Coeff est. (Std. error) for TreatXScore	C. Heterogeneity in treatment effects by performance - nonparametric spec: Coeff est. (Std. Error) for Treat	C. Heterogeneity in treatment effects by performance - nonparametric spec: Coeff est. (Std. Error) for TreatXAbove-Median Score	Control group mean	N
A. Dropout and Transfer (from endline survey data)							
Dropout	0.004	0.055	-0.001	0.022	-0.037	0.021	1,786
	[0.0071]	[0.0211]***	[0.0004]***	[0.0115]*	[0.0145]***		
Transfer	0.030	0.023	0.000	0.022	0.017	0.057	1,781
	[0.0141]**	[0.0364]	[0.0007]	[0.0193]	[0.0245]		
B. Investments (from endline survey data)							
Total education expenditures	104.45	119.70	-0.33	100.54	4.18	2,362.06	1,729
	[164.3198]	[291.4968]	[6.8414]	[177.5604]	[229.9965]		
ln(Total education expenditures)	0.001	0.093	-0.002	0.014	-0.030	7.389	1,709
	[0.0488]	[0.1144]	[0.0022]	[0.0611]	[0.0738]		
Avg. standardized effect across other non-monetary investments[a,b]	0.065	0.070	0.000	0.057	0.015	-0.01	1,720
	[0.0260]***	[0.0570]	[0.0011]	[0.0319]*	[0.0392]		
Avg. standardized effect across other chores[c]	0.058	0.010	0.001	0.034	0.049	0.00	1,681
	[0.0413]	[0.1042]	[0.0022]	[0.0500]	[0.0687]		
C. Attendance and grades (from endline data collected from schools)							
Attendance rate in weeks following baseline survey	-0.002	-0.008	0.000	-0.002	-0.002	0.911	1,827
	[0.0078]	[0.0261]	[0.0005]	[0.0124]	[0.0153]		
End-of-year grade	-0.016	0.122	-0.003	0.030	-0.095	1.970	1,241
	[0.0357]	[0.0907]	[0.0019]	[0.0465]	[0.0696]		

Notes: Data sources are endline survey and endline data collected from schools. Each observation is a child. Standard errors clustered at the household level. All regressions control for child gender, child baseline achievement, grade fixed effects, school fixed effects, and the baseline value of the dependent variable, if available (not available for dropouts, transfers, pushing children to attend school). *** p<0.01, ** p<0.05, * p<0.1

a. All variables were standardized and normalized so that an increase in investments/monitoring was positive

b. Average across the following investments: instructing the child to work on their homework, helping the child with their homework, asking others to help the child with homework, giving the child a light source to study at night, monitoring the child's exercise books, sending the child to school with food or water, pushing the child to attend school regularly.

c. Average across 2 chores measures: hours of chores and # times fetched water

Table 8. Treatment effect heterogeneity by parent education: Longer-run outcomes

Independent Variables	(1) Treat	(2) Treat X Score	(3) Treat X Parent Yrs. Educ.	(4) Treat X Score X Parent Yrs. Educ.	(5) Control group mean: Below-median parent educ	(6) Control group mean: Above-median parent educ
Dependent Variables						
A. Dropout and Transfer (from endline survey data)						
Dropout	0.068	-0.002	-0.003	0.000	0.033	0.005
	[0.0387]*	[0.0007]**	[0.0053]	[0.0001]		
Transfer	-0.080	0.002	0.024	-0.001	0.056	0.059
	[0.0648]	[0.0013]*	[0.0134]*	[0.0003]*		
B. Investments (from endline survey data)						
Total education expenditures	1,027.051	-31.044	-185.693	6.219	2,089.172	2,652.512
	[562.9661]*	[14.2060]**	[125.0146]	[3.4713]*		
ln(Total education expenditures)	0.369	-0.009	-0.056	0.001	7.293	7.489
	[0.2030]*	[0.0039]**	[0.0366]	[0.0007]**		
Avg. standardized effect across other non-monetary investments[a,b]	0.057	0.000	0.004	0.000	-0.125	0.042
	[0.0955]	[0.0018]	[0.0201]	[0.0004]		
Avg. standardized effect across other chores[c]	0.091	-0.001	-0.017	0.000	-0.040	0.000
	[0.1537]	[0.0030]	[0.0293]	[0.0006]		
C. Attendance and grades (from endline data collected from schools)						
Attendance rate in weeks following baseline survey	0.080	-0.002	-0.018	0.000	0.894	0.927
	[0.0443]*	[0.0008]*	[0.0082]**	[0.0001]**		
End-of-year grade	-0.001	0.000	0.030	-0.001	1.940	1.989
	[0.1608]	[0.0035]	[0.0310]	[0.0007]		

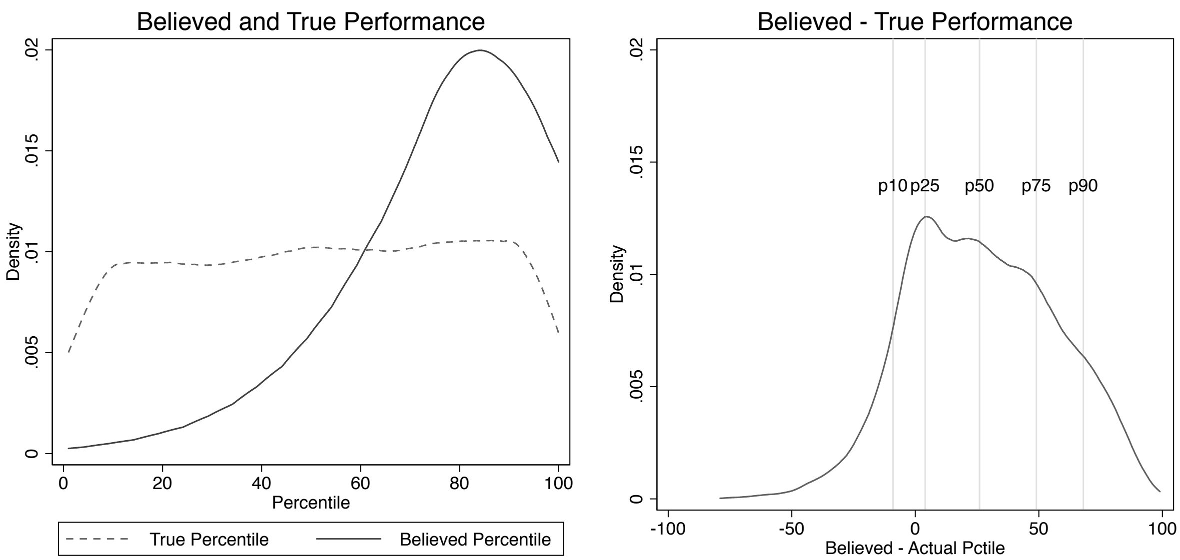
Notes: Data sources are endline survey and endline data collected from schools. Each observation is a child. Standard errors clustered at the household level. All regressions control for child gender, child baseline achievement, grade fixed effects, school fixed effects, and the baseline value of the dependent variable, if available (not available for dropouts, transfers, pushing children to attend school). Parent Yrs. Educ. is average years of education across parents in the household. *** p<0.01, ** p<0.05, * p<0.1

a. All variables were standardized and normalized so that an increase in investments/monitoring was positive

b. Average across the following investments: instructing the child to work on their homework, helping the child with their homework, asking others to help the child with homework, giving the child a light source to study at night, monitoring the child's exercise books, sending the child to school with food or water, pushing the child to attend school regularly.

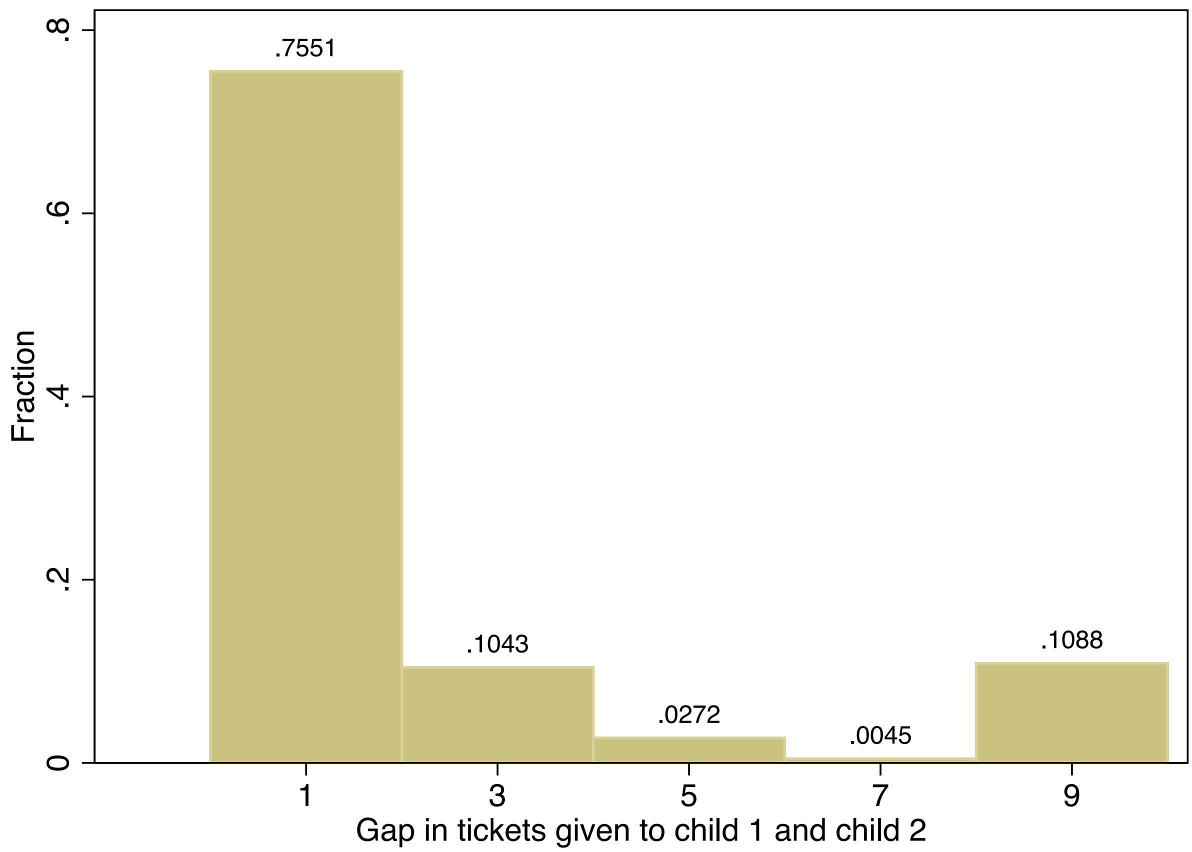
c. Average across 2 chores measures: hours of chores and # times fetched water√

Figure A.1: Misperceptions about Children's Relative Achievement



Notes: Data source is baseline data (full sample). The left graph shows kernel density plots comparing the distribution of parents' beliefs about their children's Term 2 2011-2012 relative achievement test performance (i.e., within-class percentile rank), elicited at the beginning of the baseline survey, with the distribution of their children's true Term 2 relative achievement test performance. The right graph shows a kernel density plot of the distribution, across parents, of each parent's beliefs about their child's relative test performance relative to their child's true relative test performance. The lines represent the percentiles of the distribution.

Figure A.2: Lottery Ticket Allocations



Notes: Data source is baseline data (full sample). Histogram shows how the parents split their lottery tickets between their children and, specifically, the number of tickets given to the child who received more tickets relative to the number of tickets given to the child who received fewer tickets. The total number of tickets was 9.

Appendix Table 1. Correlation between lottery tickets and child characteristics (Control group only)

	Dependent Variable = Tickets given to child		
	(1)	(2)	(3)
Believed higher-scoring sibling	0.95***	0.89***	0.94***
	[0.15]	[0.15]	[0.15]
Believed score	0.033***	0.029***	0.028***
	[0.0071]	[0.0071]	[0.0073]
Grade level		0.23***	0.21***
		[0.044]	[0.045]
Female			-0.20
			[0.12]
Household Fixed Effects	✓	✓	✓
Observations	2,640	2,640	2,550
R-squared	0.219	0.235	0.248

Notes. Robust standard errors in brackets. Sample is control group only. Each observation is a child.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table 2. Both parents' educations affect the accuracy of parents' beliefs

	Dependent Variable= Abs. Val. (True - Believed Score)			
	(1)	(2)	(3)	(4)
Sample	Mothers from 2-parent Households	Fathers from 2-parent Households	1-parent Households	All
Respondent has at least secondary education	-1.369	-0.0305	-4.757***	
	[0.888]	[1.316]	[1.421]	
Spouse has at least secondary education	-2.381***	-2.991		
	[0.856]	[1.823]		
Avg. Number of parents with at least secondary education				-3.510***
				[0.705]
Observations	2,902	1,190	998	5,220
R-squared	0.006	0.003	0.009	0.005
P-val: parent 1=parent2	0.490	0.281		

Robust standard errors in brackets. Standard errors clustered at the household level.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table 3. Effect of parents' education on the accuracy of beliefs robust to different measures parent education and child achievement

		Coefficient Estimate for:							
		Respondent's				Parent Average			
Dependent Variables	Full Sample Mean [SD]	Years of Educ	Above-Median Educ	At least Secondary Educ	Parent is literate	Years of Educ	Above-Median Educ	At least Secondary Educ	Parent is literate
A. SCORES									
Abs Val{Believed-True Overall Score}	20.39	-0.18	-0.83	-2.55	-1.08	-0.2	-0.49	-3.53	-1
	[14.460]	[0.060]***	[0.450]*	[0.680]***	[0.490]**	[0.070]***	[0.450]	[0.720]***	[0.590]*
Abs Val{Believed-True Math Score}	25.75	-0.25	-0.82	-4.06	-0.82	-0.27	-0.73	-4.95	-1.19
	[18.000]	[0.070]***	[0.540]	[0.790]***	[0.580]	[0.080]***	[0.540]	[0.870]***	[0.710]*
Abs Val{Believed-True English Score}	21.35	-0.11	-0.78	-1.33	-0.9	-0.1	-0.33	-1.87	-0.85
	[16.440]	[0.070]	[0.490]	[0.770]*	[0.550]	[0.070]	[0.490]	[0.830]**	[0.650]
Abs Val{Believed-True Chichewa Score}	23.81	-0.25	-1.46	-2.92	-0.72	-0.3	-1.51	-4.2	-0.52
	[17.540]	[0.080]***	[0.540]***	[0.850]***	[0.570]	[0.080]***	[0.530]***	[0.860]***	[0.670]
Abs Val{Believed-True (Math-English) Score}	22.08	-0.06	-0.13	-1.74	0.72	-0.04	-0.48	-1.46	0.57
	[17.400]	[0.070]	[0.530]	[0.750]**	[0.580]	[0.080]	[0.530]	[0.830]*	[0.700]
Abs Val{Believed-True Overall Score (Child 1-2)}	18.67	-0.2	-1.94	-0.54	-1.31	-0.24	-1.57	-1.77	-1.72
	[15.130]	[0.080]***	[0.600]***	[0.940]	[0.670]*	[0.090]***	[0.600]***	[1.000]*	[0.810]**
Wrong about which child higher scoring	0.31	-0.01	-0.06	-0.03	-0.02	-0.01	-0.05	-0.05	-0.02
	[0.460]	[0.000]	[0.020]***	[0.030]	[0.020]	[0.000]	[0.020]***	[0.030]	[0.020]
B. PERCENTILES									
Abs Val[Believed-True Overall Percentile]	32.16	-0.35	-1.99	-4.9	-2.65	-0.4	-1.61	-5.87	-2.78
	[24.030]	[0.100]***	[0.700]***	[1.110]***	[0.750]***	[0.100]***	[0.700]**	[1.160]***	[0.930]***
Abs Val[Believed-True Math Percentile]	33.34	-0.37	-1.93	-5.82	-2.67	-0.41	-1.88	-6.86	-2.85
	[25.000]	[0.100]***	[0.730]***	[1.110]***	[0.800]***	[0.110]***	[0.730]***	[1.190]***	[0.990]***
Abs Val[Believed-True English Percentile]	30.58	-0.23	-1.51	-2.38	-2.15	-0.29	-1.18	-3.35	-2.49
	[23.350]	[0.100]**	[0.690]**	[1.140]**	[0.730]***	[0.100]***	[0.680]*	[1.220]***	[0.920]***
Abs Val[Believed-True Chichewa Percentile]	33.77	-0.25	-1.01	-3.93	-1.41	-0.29	-0.95	-5.03	-1.51
	[24.720]	[0.100]***	[0.730]	[1.140]***	[0.770]*	[0.110]***	[0.720]	[1.220]***	[0.940]
Abs Val[Believed-True (Math-English) Percentile]	25.66	-0.31	-2.37	-2.06	-1.16	-0.29	-2.18	-2.2	-1.43
	[21.560]	[0.090]***	[0.640]***	[1.000]**	[0.700]	[0.100]***	[0.640]***	[1.080]**	[0.850]*
Abs Val{Believed-True Overall Pctile (Child 1-2)}	32.55	-0.45	-3.53	-3.77	-3.44	-0.47	-2.19	-4.95	-3.23
	[22.740]	[0.120]***	[0.900]***	[1.350]***	[0.990]***	[0.130]***	[0.900]***	[1.440]***	[1.200]***
Wrong about which child higher percentile	0.34	-0.01	-0.06	-0.05	-0.04	-0.01	-0.05	-0.08	-0.03
	[0.470]	[0.000]	[0.020]***	[0.030]	[0.020]**	[0.000]	[0.020]***	[0.030]***	[0.020]
Sample Size	5268	5230	5230	5230	5242	5230	5230	5230	5242

Notes: Each observation is a child. Standard errors clustered at the household level. Regressions control for child's gender, grade, parent gender.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table 4. Heterogeneity in belief accuracy as children progress through primary school

	Dependent Variable=Abs Val{True - Believed Performance}							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Overall	Overall	Math	Math	English	English	Chichewa	Chichewa
Child grade	0.754***	0.862***	0.785***	0.965***	0.273	0.395*	0.0534	0.118
	[0.168]	[0.183]	[0.216]	[0.234]	[0.217]	[0.236]	[0.207]	[0.225]
Secondary education * Child grade		-0.818		-1.535**		-0.898		-0.492
		[0.595]		[0.756]		[0.765]		[0.713]
Observations	5,052	5,019	5,054	5,021	5,054	5,021	5,054	5,021
R-squared	0.682	0.681	0.666	0.668	0.592	0.593	0.690	0.689
HH FE	✓	✓	✓	✓	✓	✓	✓	✓

Standard errors in brackets. Standard Errors clustered at household level. Secondary Education is an indicator for having any education of secondary level or higher, averaged across both parents. Regression includes controls for child gender and whether the child was the first or second child discussed in the survey. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table 5. Heterogeneity in treatment effects by achievement and beliefs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Math Workbook		English Workbook		Textbook WTP		Lottery	
Dependent Variable	*100 for Advanced,0 for Average, -100 for Beginner*		*100 for Advanced,0 for Average, -100 for Beginner*		*ln(Textbook WTP) for Math - English*		*Tix given to higher - lower achiever*	
Treat x True Score	0.013***	0.016***	0.013***	0.017***	-0.013***	-0.015***	0.0017	0.015
	[0.00093]	[0.00090]	[0.00096]	[0.00088]	[0.0022]	[0.0021]	[0.0096]	[0.0093]
Treat x Believed Score		-0.015***		-0.015***		0.011***		-0.035***
		[0.0010]		[0.00086]		[0.0021]		[0.0067]
Treat	-0.91***	-0.054	-0.68***	-0.0011	0.15***	0.046	0.97***	0.99***
	[0.049]	[0.068]	[0.048]	[0.052]	[0.041]	[0.044]	[0.22]	[0.21]
True Score	0.0065***	0.0016***	0.0076***	0.0013**	-0.0030*	-0.000012	0.034***	0.011
	[0.00065]	[0.00057]	[0.00073]	[0.00058]	[0.0016]	[0.0015]	[0.0072]	[0.0067]
Believed Score		0.022***		0.023***		-0.021***		0.070***
		[0.00072]		[0.00060]		[0.0014]		[0.0045]
Observations	5,239	5,233	5,239	5,233	5,183	5,177	2,611	2,606
R-squared	0.217	0.374	0.204	0.405	0.033	0.097	0.045	0.146
p-val: treatXperf+treatXbeliefs=0		0.26		0.22		0.14		0.041

Robust standard errors in brackets. Standard errors clustered at the household level.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table 6. Persistence of information's effect on beliefs

Subject	(1) Overall	(2) Math	(3) English	(4) Chichewa
Score * Treat	0.283**	0.0224	0.0391	-0.114**
	[0.139]	[0.0493]	[0.0534]	[0.0452]
Treat	-12.44**	-5.544**	-4.018	2.309
	[5.272]	[2.517]	[2.621]	[2.671]
Score	0.244***	0.188***	0.177***	0.238***
	[0.0379]	[0.0365]	[0.0402]	[0.0345]
Observations	1626	1627	1627	1627
R-squared	0.046	0.053	0.043	0.046

Notes. Robust standard errors in brackets. Standard errors clustered at the household level. Include controls for child gender and grade FE.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table 7. Correlations between beliefs and endline survey outcomes (Control group only)

Variable	Baseline beliefs	Avg. baseline/ endline	Endline Beliefs	Control Mean	N
A. Dropout and transfers (from endline survey data)					
Child dropped out	0.000	0.000	0.000	0.021	776
	[0.0003]	[0.0001]	[0.0001]		
Child transferred	0.000	0.000	0.000	0.057	773
	[0.0005]	[0.0005]	[0.0004]		
B. Education Expenditures (from endline survey data)					
Total education expenditures	-3.780	5.976	10.283	2,315.109	752
	[4.5511]	[6.0179]	[5.1353]**		
ln(Total education expenditures)	0.000	0.003	0.005	7.386	747
	[0.0020]	[0.0025]	[0.0019]**		
Expenditures on school fees	-0.176	1.113	1.408	426.742	752
	[0.8125]	[0.9889]	[0.8382]*		
Supplementary education expenditures	-3.355	5.339	8.983	1,859.378	752
	[4.3026]	[5.7152]	[4.6801]*		
Books and school supplies	0.009	2.356	2.507	600.313	752
	[1.1183]	[1.4387]	[1.1777]**		
Uniforms	-3.084	-1.465	1.748	779.309	752
	[2.1790]	[2.6944]	[2.0432]		
Backpacks	0.473	1.103	1.164	174.438	752
	[0.8377]	[0.9655]	[0.8583]		
Tutoring	-0.705	4.779	4.761	268.617	752
	[2.8962]	[3.6585]	[2.5179]*		
C. Non-monetary investments (from endline survey data)					
Helped child with homework	0.002	0.002	0.000	0.376	744
	[0.0011]*	[0.0014]	[0.0011]		
Asked someone to help child with homework	0.000	-0.002	-0.002	0.245	748
	[0.0010]	[0.0012]	[0.0010]*		
# times gave child light source to study at night over last 4 weeks	0.020	0.071	0.062	2.624	734
	[0.0159]	[0.0209]***	[0.0148]***		
# times child went to school without food or water in last 4 weeks	-0.004	-0.005	0.000	10.674	733
	[0.0210]	[0.0260]	[0.0201]		
Has to push child to attend school regularly	-0.003	-0.004	-0.002	0.343	729
	[0.0011]**	[0.0015]***	[0.0012]		
# times monitored child's exercise books in last 4 weeks	-0.011	0.016	0.028	8.499	734
	[0.0187]	[0.0228]	[0.0189]		
# times instructed child to work on homework in last 4 weeks	-0.015	0.001	0.015	1.982	734
	[0.0090]*	[0.0112]	[0.0109]		
Average standardized effect across other investments[b]	0.000	0.002	0.002	-0.043	752
	[0.0010]	[0.0013]	[0.0010]		
D. Chores (from endline survey data)					
Hours of chores given to child over last 4 weeks	0.087	0.120	0.058	23.805	732
	[0.0468]*	[0.0676]*	[0.0549]		
# times child fetched water in last 4 weeks	-0.008	0.003	0.012	4.672	734
	[0.0176]	[0.0225]	[0.0175]		
Average standardized effect across chores[c]	0.001	0.003	0.002	-0.022	734
	[0.0017]	[0.0023]	[0.0018]		
E. Attendance and grades (from data collected from schools)					
Attendance rate in weeks following baseline survey	0.001	0.001	0.000	0.911	916
	[0.0004]***	[0.0006]	[0.0004]		
Repeated a grade	-0.007	-0.008	-0.002	0.275	2,193
	[0.0006]***	[0.0013]***	[0.0010]**		
End of year grade	0.017	0.020	0.010	1.970	637
	[0.0022]***	[0.0050]***	[0.0043]**		

Notes: Sample is control group only. Each observation is a child. Standard errors clustered at the household level. All regressions control for child gender, child baseline achievement, grade fixed effects, school fixed effects, and the baseline value of the dependent variable, if available (not available for dropouts, transfers, pushing children to attend school). Indicators for whether child has end of year grade or repeat data are 1 if yes, 0 if no, an data collection did not happen at that child's school. *** p<0.01, ** p<0.05, * p<0.1

a. All variables were standardized and normalized so that an increase in investments/monitoring was positive

b. Average across 4 investments: purchased supplementary books, enrolled child in tutoring, helped child with homework, and asked someone else to help child with homework

Appendix Table 8. Effect of Information on Endline Outcomes (Detailed data)

	A. Ave. treatment effect	B.Heterogeneity in treatment effects by performance - linear spec		C.Heterogeneity in treatment effects by performance - nonparametric spec			
Variable	Coeff est. (Std. Error) for Treat	Coeff est. (Std. error) for Treat	Coeff est. (Std. error) for TreatXScore	Coeff est. (Std. Error) for Treat	Coeff est. (Std. Error) for TreatXAbove-Median Score	Control group mean	N
A. Dropout and transfers (from endline survey data)							
Child dropped out	0.004	0.055	-0.001	0.022	-0.037	0.021	1,786
	[0.0071]	[0.0211]***	[0.0004]***	[0.0115]*	[0.0145]***		
Child transferred	0.030	0.023	0.000	0.022	0.017	0.057	1,781
	[0.0141]**	[0.0364]	[0.0007]	[0.0193]	[0.0245]		
B. Education Expenditures (from endline survey data)							
Total education expenditures	104.446	119.697	-0.326	100.538	4.181	2,362.056	1,729
	[164.3198]	[291.4968]	[6.8414]	[177.5604]	[229.9965]		
ln(Total education expenditures)	0.001	0.093	-0.002	0.014	-0.030	7.389	1,709
	[0.0488]	[0.1144]	[0.0022]	[0.0611]	[0.0738]		
Expenditures on school fees	-11.262	125.673	-2.924	-3.618	-16.155	452.527	1,729
	[30.8161]	[63.2576]**	[1.6116]*	[27.1752]	[47.6499]		
Supplementary education expenditures	101.938	-67.747	3.623	79.804	43.814	1,902.915	1,729
	[157.2436]	[275.8275]	[6.4398]	[172.7078]	[217.8835]		
Books and school supplies	60.201	105.098	-0.959	85.900	-52.653	617.639	1,729
	[57.4169]	[94.0822]	[1.8028]	[59.9890]	[63.2710]		
Uniforms	34.341	-93.891	2.737	-25.075	118.634	806.402	1,729
	[70.2289]	[138.7450]	[2.5003]	[87.7873]	[100.2063]		
Backpacks	39.774	7.431	0.691	43.197	-6.654	178.607	1,729
	[27.0808]	[50.9720]	[1.0991]	[31.5332]	[36.1590]		
Tutoring	-36.405	0.771	-0.794	-1.680	-71.155	300.267	1,729
	[88.8218]	[158.3696]	[4.3080]	[83.1294]	[151.0584]		
C. Non-monetary investments (from endline survey data)							
Helped child with homework	-0.030	-0.034	0.000	-0.046	0.033	0.374	1,699
	[0.0283]	[0.0627]	[0.0012]	[0.0337]	[0.0423]		
Asked someone to help child with homework	0.055	0.100	-0.001	0.071	-0.033	0.243	1,710
	[0.0269]**	[0.0638]	[0.0013]	[0.0328]**	[0.0424]		
# times gave child light source to study at night over last 4 weeks	0.425	0.207	0.005	0.274	0.316	2.610	1,674
	[0.4019]	[0.8888]	[0.0182]	[0.4809]	[0.6163]		
# times child went to school without food or water in last 4 weeks	-1.461	-2.374	0.019	-1.778	0.654	10.676	1,677
	[0.5431]***	[1.1978]**	[0.0224]	[0.6710]***	[0.7717]		
Has to push child to attend school regularly	0.067	0.028	0.001	0.059	0.017	0.341	1,666
	[0.0256]***	[0.0622]	[0.0012]	[0.0340]*	[0.0413]		
# times monitored child's exercise books in last 4 weeks	-1.132	-1.352	0.005	-1.120	0.002	8.458	1,681
	[0.4862]**	[1.1325]	[0.0217]	[0.6125]*	[0.7345]		
# times instructed child to work on homework in last 4 weeks	0.559	0.819	-0.006	0.447	0.224	1.972	1,669
	[0.2489]**	[0.4720]*	[0.0090]	[0.2897]	[0.3448]		
Average standardized effect across other investments[b]	0.063	0.067	0.000	0.056	0.015	-0.046	1,720
	[0.0257]***	[0.0562]	[0.0011]	[0.0315]*	[0.0387]		
D. Chores (from endline survey data)							
Hours of chores given to child over last 4 weeks	1.905	0.546	0.029	1.430	1.008	23.814	1,676
	[1.3251]	[2.9363]	[0.0664]	[1.3845]	[2.1819]		
# times child fetched water in last 4 weeks	0.273	0.155	0.003	0.061	0.420	4.656	1,671
	[0.3702]	[1.0159]	[0.0195]	[0.5117]	[0.6188]		
Average standardized effect across chores[c]	0.050	0.010	0.001	0.028	0.045	-0.023	1,681
	[0.0370]	[0.0952]	[0.0020]	[0.0458]	[0.0617]		
E. Attendance and grades (from data collected from schools)							
Attendance rate in weeks following baseline survey	-0.002	-0.008	0.000	-0.002	-0.002	0.911	1,827
	[0.0078]	[0.0261]	[0.0005]	[0.0124]	[0.0153]		
End of year grade	-0.016	0.122	-0.003	0.030	-0.095	1.970	1,241
	[0.0357]	[0.0907]	[0.0019]	[0.0465]	[0.0696]		

Notes: Data sources are endline survey and endline data collected from schools. Each observation is a child. Standard errors clustered at the household level. All regressions control for child gender, child baseline achievement, grade fixed effects, school fixed effects, and the baseline value of the dependent variable, if available (not available for dropouts, transfers, pushing children to attend school). *** p<0.01, ** p<0.05, * p<0.1

a. All variables were standardized and normalized so that an increase in investments/monitoring was positive

b. Average across the following investments: instructing the child to work on their homework, helping the child with their homework, asking others to help the child with homework, giving the child a light source to study at night, monitoring the child's exercise books, sending the child to school with food or water, pushing the child to attend school regularly.

c. Average across 2 chores measures: hours of chores and # times fetched water

Appendix Table 9. Transfer Results: Heterogeneity by School Type

	(1)	(2)	(3)
Dependent Variable	Transferred	Transferred	Transferred
Treat	0.030**	0.023	-0.018
	[0.014]	[0.036]	[0.040]
Treat X High-achievement school			0.20**
			[0.098]
Treat X Score		0.00015	0.0012
		[0.00071]	[0.00083]
Treat X Score X High-achievement school			-0.0042**
			[0.0017]
Observations	1,781	1,781	1,781
R-squared	0.038	0.038	0.043
Dep Var Mean (Control group)	0.057		
P-val:TreatXscore=0		0.83	0.16
P-val:TreatXscore + TreatXscoreXhigh quality=0			0.041

Robust standard errors in brackets. Standard errors clustered at household level.
High-achievement schools are in the top quartile of average student achievement scores.
*** p<0.01, ** p<0.05, * p<0.1

A Sample Information Intervention Report Card

Report Card

Name: NDEMA LONGWE **Standard: 2**

	Score	**Grade**	**Position**
Maths:	75/100	3	10/100
English:	33/100	1	71/100
Chichewa:	67/100	3	38/100
Overall:	58/100	2	52/100

Number of Exams Administered in Class: 3

Grades
1 = Needs support
2 = Average
3 = Good
4 = Excellent

B Sample Detailed Skills Report Card

Skills Report Card for Ndema Longwe (Standard 3)

	Ndema's Grades	Grades of other children in Ndema's class: NO	A LITTLE	YES
English		NO	A LITTLE	YES
1. Can Ndema read simple words like class and house?	Yes	****	**	*
2. Can Ndema copy and complete simple sentences?	A little	**	****	*
Maths		NO	A LITTLE	YES
1. Can Ndema add 3-digit numbers?	Yes	*	***	***
2. Can Ndema multiple 3-digit numbers?	No	***	***	*
Chichewa		NO	A LITTLE	YES
1. Kuwelenga ndi kulemba?	A little	****	**	*
2. Kutchula mau moyenelela?	No	***	***	*

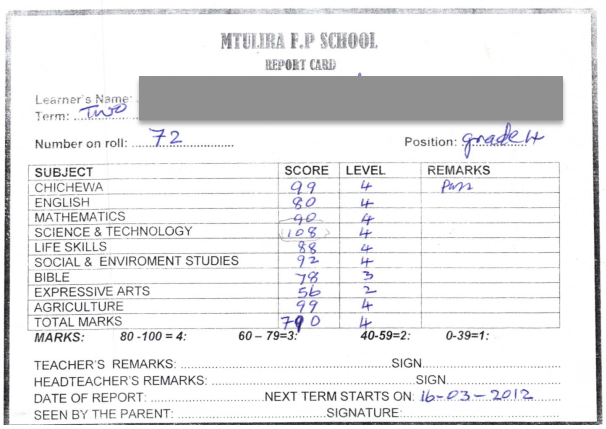
Number of kids in class is 70

Each star represents 10 kids (* = 10)

C Sample Price List

Surveyor: For each row, say: "At the end of the interview, if the randomly selected textbook is the <u>math</u> book for [*NAME*] and the randomly selected price is [*PRICE*] MWK, will you purchase the book?"				
a)	**1900**MWK	☐ 1. YES	*or*	☐ 2. NO
b)	**1700**MWK	☐ 1. YES	*or*	☐ 2. NO
c)	**1500**MWK	☐ 1. YES	*or*	☐ 2. NO
d)	**1300** MWK	☐ 1. YES	*or*	☐ 2. NO
e)	**1100** MWK	☐ 1. YES	*or*	☐ 2. NO
f)	**900**MWK	☐ 1. YES	*or*	☐ 2. NO
g)	**700**MWK	☐ 1. YES	*or*	☐ 2. NO
h)	**500**MWK	☐ 1. YES	*or*	☐ 2. NO
i)	**300**MWK	☐ 1. YES	*or*	☐ 2. NO

D Sample Baseline Report Cards



MTULIRA F.P SCHOOL

REPORT CARD

Learner's Name: [redacted]

Term: Two

Number on roll: 72 Position: grade 4

SUBJECT	SCORE	LEVEL	REMARKS
CHICHEWA	99	4	Pass
ENGLISH	80	4	
MATHEMATICS	90	4	
SCIENCE & TECHNOLOGY	108	4	
LIFE SKILLS	88	4	
SOCIAL & ENVIROMENT STUDIES	92	4	
BIBLE	78	3	
EXPRESSIVE ARTS	56	2	
AGRICULTURE	99	4	
TOTAL MARKS	790	4	

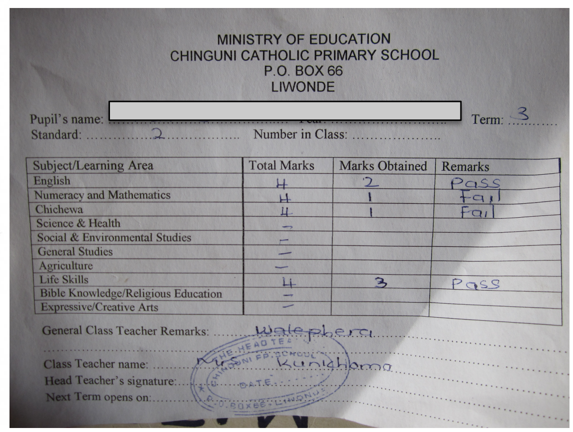
MARKS: 80 -100 = 4: 60 – 79=3: 40-59=2: 0-39=1:

TEACHER'S REMARKS: .... SIGN................

HEADTEACHER'S REMARKS: .... SIGN................

DATE OF REPORT: .... NEXT TERM STARTS ON: 16-03-2012

SEEN BY THE PARENT: .... SIGNATURE:................



MINISTRY OF EDUCATION
CHINGUNI CATHOLIC PRIMARY SCHOOL
P.O. BOX 66
LIWONDE

Pupil's name: [redacted] Term: 3

Standard: 2 Number in Class: ....

Subject/Learning Area	Total Marks	Marks Obtained	Remarks
English	4	2	Pass
Numeracy and Mathematics	4	1	Fail
Chichewa	4	1	Fail
Science & Health	–		
Social & Environmental Studies	–		
General Studies	–		
Agriculture	–		
Life Skills	4	3	Pass
Bible Knowledge/Religious Education	–		
Expressive/Creative Arts	–		

General Class Teacher Remarks: Walephera

Class Teacher name: Mrs Kunkhoma

Head Teacher's signature: ....

Next Term opens on: ....

Sample report cards delivered by schools in the study sample to parents.