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 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' desired investments (how they want to invest) and their actual investments (how they invest in reality). 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 between richer and poorer families.

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1 Introduction

Many models for optimal human capital investment incorporate 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 relatively 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 have inaccurate beliefs about their children's individual-level characteristics, they may make inefficient decisions. To give a concrete example, consider a parent whose son is doing well 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). Lack of education may limit 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. Although cross-country data on parent belief accuracy is unavailable, Figure 1 uses pre-existing data from two countries (India and the U.S.) to show that, within-country, less-educated parents have less accurate beliefs about their children's academic abilities than more-educated parents.

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, there has been no firm causal link establishing that inaccurate beliefs cause people to make different decisions than they would with accurate beliefs.

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

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, including in Malawi. However, the information in the official report cards is often hard for parents to interpret, or does not make it to parents since it is hand-delivered by students. The intervention presented information in a format that was more clear for parents, and surveyors walked parents through it to ensure understanding.

The experimental results suggest that inaccuracies in parents' perceptions about their children's academic abilities may have large, negative impacts on children's education in Malawi.[1] 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. 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 children has higher achievement. Since achievement tests determine progression through school, and since inaccuracies in perceptions about recent achievement also correlate with inaccurate beliefs about child educational competencies (e.g., whether the child can do 2-digit addition), beliefs about achievement are likely relevant for a broad range of educational investments. Indeed, the baseline (control group) investment data provides suggestive evidence that these inaccuracies lead to misallocations, since parents' investments depend strongly upon their beliefs, but the relationship with true achievement is much weaker.

The second finding is that, across a range of investments, providing parents with information causes them to reallocate their educational investments. We begin with investments for which we have clear predictions for how the returns (and thus the efficient investments)

[1]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. The best measure would be the one that best proxies for cross-person heterogeneity in the returns to investment. One could argue that, for this purpose, "innate" ability would have been better than achievement, which reflects both innate ability and past inputs. As has been extensively documented in the literature, however, it is difficult to measure "innate" ability, so any measure will combine many factors (e.g., genetic factors (Vinkhuyzen et al., 2009), early-life inputs (Paxson and Schady, 2005)). Although achievement may reflect past inputs more than some other measures one might use, it also has two clear advantages: first, it determines progression through school and entrance to higher levels of education, thereby almost surely affecting the returns to investment, and second, it is what parents themselves identify in qualitative interviews as the best proxy for heterogeneity in the returns to investment.

vary with student achievement; these investments provide strong evidence for whether inaccurate perceptions can lead to inefficiencies. First, to investigate whether parents choose the right mix of supplementary inputs to their children's education, the experiment measured 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), as well as choices among three, free subject-specific workbooks that are targeted for a child's performance level (remedial, average, and advanced).[2] For both investments, I find that information affected investments by allowing parents 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 consistent with increased efficiency since the textbooks are remedial. For workbooks, information caused parents to shift their choices towards workbooks that corresponded more closely to their children's true achievement level.

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. Although there is no definitive evidence on whether schooling is a complement with ability/achievement, the existing literature generally suggests that it is (Pitt et al., 1990; Aizer and Cunha, 2012), and indeed that is what most parents in the sample believe. I find that, at baseline, parents allocate more tickets to the child they believe is higher-achieving, but that many parents have mistaken beliefs about which of their children has higher achievement, and so information causes parents to reallocate tickets towards the child that is 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' desired investments (how they want to invest) and their actual investments (how they invest in reality). Whether this negatively impacts children's educational outcomes depends on parents' objectives and whether they know the correct production function. However, the fact that, for the above

[2]Although both of these investments get at the same theoretical idea, there are several reasons to use both. One advantage of studying textbooks is that they help with external validity since textbooks are a more common purchase. WTP is also more granular than workbook choice and so could have higher statistical power. However, since textbook purchases require a cash outlay (unlike the workbooks, which are a choice between free items), the treatment effects for textbooks could be dampened by liquidity constraints. This makes it difficult to use textbooks to examine heterogeneity based on parent SES, since heterogeneous treatment effects would be confounded by heterogeneity in liquidity constraints. These were some of the original design justifications for looking at both investments simultaneously.

investments, we have relatively clear predictions for how the efficient investment depends on achievement (and parents' investments follow those predictions) implies that the negative impacts could be large.

I next investigate the implications by investigating the hypothesis that inaccurate perceptions play a role in perpetuating 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 prevent their children from attaining the same levels of human capital and earnings as children from more-educated parents.

Consistent with the hypothesis, I find that poorer and less-educated parents have less accurate beliefs 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 (17 percent of the mean) 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. I also find that information causes less-educated parents to update their beliefs more than more-educated parents, and find some evidence that information caused a greater change to their investment decisions.

The previous outcomes were all measured near the time of the intervention, and so one potential concern with these results is whether the effects of information 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 intervention. I find that information did affect later investments. For example, information increased transfers of children between schools, causing parents at schools with low average achievement to be more likely to transfer out their higher-achieving children (suggestive that they now thought the higher-achieving school was worth the potential costs) and parents at high-achievement schools to be more likely to transfer out their lower-achieving children (suggestive that they realized the school was not a good match).

As a second example, information affected dropouts: children whose parents had found out that their children were doing well in school were less likely to drop out, while children whose parents 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 (cited above) has generally suggested that education and ability are complements 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 may 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 from taking full advantage of educational opportunities, thereby stymying their children's educational outcomes.

This paper also contributes to several strands of the literature. First, it contributes to a recent and growing literature suggesting that inaccurate information and beliefs affect educational decisions. This literature has primarily focused on the role of information about *population-average* parameters, such as the returns to education, or information about school quality and the school system (Jensen, 2010; Nguyen, 2008; Dinkelman and Martínez A, 2014; Andrabi et al., 2009). For example, Jensen (2010) shows that providing households with the information on the average earnings differences between more and less-educated people increases educational attainment. There is a small segment of the literature focused on the role of inaccurate beliefs about *individual-level* parameters, with papers that strongly suggest a role, but, to my knowledge, none of the previous literature tests whether inaccurate beliefs hinder investments by testing whether changing beliefs allows parents to better target their investments to their children's ability level (Chevalier et al., 2009; Connor et al., 2001; Stinebrickner and Stinebrickner, 2009; Filippin and Paccagnella, 2012; Bergman, 2014).[3]

[3] 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. Filippin and Paccagnella (2012) provide observational evidence that academic choices are more closely aligned with ability in the Netherlands, where there is early aptitude testing, than in Italy,

Second, it contributes to a related literature, begun in psychology, documenting inaccuracies in beliefs about ability. Most of the literature focuses on beliefs about *own* ability (see Dunning, Heath, and Suls, 2004, for a review).[4] Third, it contributes to the literature documenting the positive influence of parents' education on children's educational outcomes (Rosenzweig and Wolpin, 1994; Oreopoulos et al., 2006; Andrabi et al., 2012; Banerji et al., 2013) by highlighting one channel for effects (parents' beliefs).

Finally, it contributes to a long literature examining 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), but 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.[5] 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 her children's achievement was lower or higher than expected, the analysis provides evidence on how investments depend on 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 examines heterogeneity by parent education. Section 7 looks at longer-run outcomes. In Section 8, I conclude.

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where there is not. Kaufmann (ming) and Attanasio and Kaufmann (2014) examine the role of individual-level expectations about the returns to schooling in influencing schooling decisions, but the focus is not on determining whether beliefs are accurate. Andrabi et al. (2009) delivers individual-level information but it is bundled with school-level information so the intent is not to separately identify its effects. Bergman (2014) provides academic information to parents in a developed country context (Los Angeles in the U.S.), but the emphasis is more on monitoring and agency issues than targeting investments to a child's ability level.

[4]Hess (1997) and Miller et al. (1991) in the psychology literature are exceptions that present evidence of a weak correlation between parent perceptions and their childs true performance. The literature in economics is primarily focused on overconfidence (e.g., Cesarini et al. (2009) and Camerer and Lovallo (1999)).

[5]Some papers also use within-twin-pair comparisons (e.g., Rosenzweig and Zhang (2009)), but the external validity beyond twins is unknown.

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, or roughly 1% of annual household income. The largest required expenditure is for uniforms (580 MWK or 3.51 USD per child per year). Schools also charge informal required fees (380 MWK or 2.30 USD per child per year). Eighty nine percent of parents make supplemental investments in their children's education, like school supplies, supplemental tutoring, and books, with average (unconditional) spending of 790 MWK or 4.79 USD per child per year.

Dropouts are common in primary school: the nationwide primary school completion rate was 35% in 2007 (World Bank). In a recent World Bank report, the primary reason cited by pupils for dropping out was "lack of interest," cited by 48% of dropouts. 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 said that, when their child dropped out, the child no longer liked school because they were performing badly. Poor performance early-on may thus be a barrier to long-run 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 - 10,000 MWK per year (30 - 60 USD, over 4 times the median primary-school expenditures in the study sample). Parents must additionally purchase uniforms and supplementary supplies. Anecdotally, many children do not attend secondary schooling as a result of the high fees. Secondary schooling is not open to all students, with admissions governed by a high-stakes achievement test administered at the end of primary school.

School Report Cards

Schools are supposed to send report cards to parents each term with children's achievement test results. The reports vary by school, but the majority have children's absolute achievement test scores and the absolute grades 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 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 less likely to have received report cards). 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

The study worked with 39 schools in two districts in central Malawi (the Machinga and Balaka districts).[6] 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. Multiple-sibling households were used as the sampling frame because, as described below, we wanted to use inter-sibling tradeoffs to understand parents' long-run investment allocations.[7] 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.[8] Test data were 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.[9] On average, students in the sample took 4.5 tests during the term. 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.

[6] 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.

[7] Using multiple-child households does not have much cost in terms of external validity since fewer than 3% of households in Malawi with children have only one child. The greater potential concern is that households with tighter birth spacing would be over-represented in the sample, but, reassuringly birth spacing is uncorrelated with belief accuracy in the sample, both within and across parent SES categories.

[8] We also gathered Term 1 data on average test scores, which is matchable to the term 2 data for a subset of the sample.

[9] If a class only offered continuous assessments (or terminal exams), the score used is 100% continuous assessments (or terminal exams). All continuous assessments were combined into an unweighted average. 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.

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.

Randomization

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 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). For households that had more than two children that met the sample inclusion criteria, two children were randomly selected for the sample.

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.[10] 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, including a review of a sample report card which had the same format as the report cards later delivered to the treatment group. This held constant across the treatment and control groups both knowledge about the Malawian grading scale and whether the parent had seen a report card of the type used in the intervention.

The surveyor then walked parents (treatment group only) through a report card describing their own children's recent achievement. The report card contained each student's absolute and relative performance on recent tests (See Appendix A for a sample report

[10] 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 but untracked.

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 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 report card. 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. 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 specific skills (2 math, 2 English, and 2 Chichewa) 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.

2.3 Baseline Characteristics of Study Sample

Table 1 presents baseline sample characteristics and tests for balance across the treatment and control groups. Among respondents, 77% are female, and 92% are the primary decision maker about education in the household.[11] The level of education among parents is very low: the average years of education across parents in sample households is 4.7. Almost half the respondents are farmers. Households are 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), with 51% of the

[11] 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.

children female.

To test balance, I regress 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. This is unlikely to confound the treatment effect estimates since the results mainly look at heterogeneity in treatment effects by child achievement. However, 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, the accuracy of parents' perceptions (i.e., the absolute value of the gap between their beliefs and their children's scores) is balanced across treatment and control groups.

2.4 Measurement of Child Investment Outcomes

The experiment measured two main types of investments. First, we measured several "lab-in-the-field" style investments for which we have clear predictions for the *perceived* "right choice" (i.e., whether the investments are perceived complements or substitutes with ability/achievement): WTP for remedial textbooks, choices among level-specific workbooks, and allocation of lottery tickets. The clear predictions allow us to easily test whether information affected investments. The textbooks and workbooks also have the advantage that we have clear predictions for the *actual* right choice / production functions (e.g., the advanced workbooks were designed specifically to be better for high-achieving children than the beginner workbooks), thus enabling us to make a stronger case that parents' misallocations actually lower true returns, not just perceived returns. (For the lottery, we must rely on estimates from the literature on complementarities in other contexts, so may be less sure).These investments are also useful because the production function likely is relatively similar for poor and rich households, allowing us to more easily test for heterogeneous responses to information. For budgetary reasons, these investments were all measured the same day as the information treatment.

Second, we measured longer-run investments (such as expenditures, transfers, etc.), for which we did not have clear predictions *ex ante* for the perceived complementarities. These investments allow us to test for the persistence and relevance of the other effects. The experiment also provides us with a method for inferring the (perceived) complementarity based on parents' responses.

Remedial Textbooks and Level-Specific Workbooks

The textbooks and workbooks were chosen because (a) we have clear predictions for the "right choice", and (b) they would provide evidence about whether parents correctly optimize the supplementary inputs to their children's education. For the textbooks, the survey team 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),[12] and so the clear prediction to test is that textbook WTP will decrease 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. (See Appendix C for a sample price list and description of how the BDM mechanism was implemented).

Parents also make many non-monetary investments that may depend on 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, surveyors 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 tailored correctly to their children's performance.

Secondary School Lottery

In addition to measuring supplementary investments, it is also useful to measure parents' larger investments. Since secondary schooling is one of the first high-cost, high-returns investments that parents make, the survey team measured a short-run, real-stakes proxy for parents' secondary schooling investments. Specifically, we conducted a lottery to pay for four years of government secondary school fees for one child in every 100 households in the sample; four years of fees were worth roughly 120 - 240 USD at the time of the experiment depending on what tier of secondary school the child was admitted to. Parents were given nine tickets for the lottery and chose how they would allocate the tickets across their two sampled children.[13] There are two primary ways that student achievement might affect the expected return of a lottery ticket. First, perceived complementarity between performance

[12]Before the study began, we surveyed 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.

[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).

and education would cause the perceived earnings return of a ticket to increase with student performance. Most parents in this setting believe ability and schooling are complements. 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 (25% of test-takers in 2010); 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 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 achievement.

Later investments and outcomes

To test for the persistence of effects, I also analyze the effects on expenditures, dropouts, transfers between schools, repetition, non-monetary investments, etc., in the year following the survey as end line outcomes

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 collected before the information intervention included modules with standard demographic, income, and assets questions, as well as modules gathering parents' baseline spending on education, beliefs about the returns to education, and beliefs about their children's achievement. The uncertainty of parents' beliefs was also elicited by asking parents to distribute 10 tokens across bins representing different achievement amounts, thus tracing out the PDF of their beliefs. 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. To assess how much parents updated their beliefs, at the very end of the survey (after the treatment 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. (Note that this is *not* the same measure that the treatment group received information about.)[14]

[14] 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, asking about that would not have shown us how much

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.[15] We were unable to collect this data from the full sample of schools, 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 approximately one year after the baseline data was gathered (June-July 2013). For budgetary reasons, only a subset of the sample was surveyed.

3 Conceptual Framework

This section presents a simple model 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 illustrate how inaccurate beliefs can cause parents to choose an inefficient mix of educational inputs even conditional on the level of spending. This same model could also be used to describe misallocations across children within the household.[16] Similar ideas go through when the education budget is flexible.

The model

A parent has a fixed budget for education to allocate between two inputs to her child's education. Her returns-maximizing allocation solves 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 measuring the long-run output of the investment (e.g., earnings); A is the child's achievement level; s^c, the first input, is a complement with achievement $\left(\frac{\partial^2 q}{\partial s^c \partial A} > 0\right)$; s^s, the second input, is a substitute with achievement $\left(\frac{\partial^2 q}{\partial s^s \partial A} < 0\right)$; output is increasing in both inputs $\left(\frac{\partial q}{\partial s^c} > 0, \frac{\partial q}{\partial s^s} > 0\right)$; and y^{educ} is the total budget for education. For example, s^s could be a remedial textbook which is more valuable for children

parents updated their underlying beliefs about their children's academic abilities, since parents should have fully updated about that measure even if they did not update their underlying beliefs about their children's full set of abilities – beliefs which had been developed over multiple years and likely have tighter priors.

[15] 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.

[16] To see this, simply relabel the two inputs as "spending on child 1" and "spending on child2" and take A to be the gap in achievement between child 1 and child 2.

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. Note that achievement is used here as a (potentially noisy) measure of a child's ability level – it is an input, not an output, to the static production function.[17] I assume in this framework that parents knows the true production function, q.[18] I discuss the implications if parents do not know the true function later in the paper.

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 $\beta_1^c > 0, \beta_1^s < 0$. (To simplify notation, I will now refer to both investments as $s^*(A)$ and both slopes as β_1.)

In the model, parents do not know their children's true achievement, A; instead, they have a belief about their children's achievement, $\tilde{A}$. Instead of choosing the returns-maximizing investments, $s^*(A) = \beta_0 + \beta_1 A$, each parent instead chooses investments that depend on her beliefs, $s^*(\tilde{A}) = \beta_0 + \beta_1 \tilde{A}$. As a result, if her belief is inaccurate, she would earn inefficiently low returns: $q(s^*(\tilde{A})|A) \leq q(s^*(A)|A)$.

Note that I use the term "inaccurate beliefs" to characterize any beliefs that diverge from true achievement: $Abs.Val.(\tilde{A}_i - A_i) > 0$. Inaccurate beliefs could either be unbiased ($E(\tilde{A}) = E(A)$) or biased ($E(\tilde{A}) \neq E(A)$). With unbiased, inaccurate beliefs, if investments are linear, the level of inputs that parents choose is on average (across all parents) correct ($Abs.Val.(E(s^*(\tilde{A})) - E(s^*(A))) = 0$), but each individual parent would not choose the correct inputs to meet their child's needs ($Abs.Val.(s^*(\tilde{A}_i) - s^*(A_i)) \geq 0$), and so returns would be sub-optimally low. In contrast, with biased beliefs, both the average levels of the inputs and the returns would be distorted. Note that this does not imply lower returns than in the unbiased case; what matters for distortions is the mean absolute value of the gap between each individual's beliefs and the truth $E(Abs.Val.(\tilde{A} - A))$, not the gap between the population distributions of beliefs and the truth $Abs.Val.(E(\tilde{A}) - E(A))$.

[17] In a dynamic model, one could think of achievement as a state variable that would determine the current returns to investment based on innate ability and previous investments.

[18] In general, revealed preference tests only teach us about perceived returns, not true returns; as such, by observing parents' behavior in this setting, we can test whether increasing beliefs accuracy improves output as measured by the parents' *perceived* production function, or whether a given input is a *perceived* complement or substitute with achievement. This is one reason it is useful to study investments where we have clear hypotheses about the true production function, like workbooks and textbooks, and can test whether parents' perceived production functions conform to our ex ante expectations.

Testing for distortions

Inaccurate beliefs decrease returns by distorting investments away from parents' desired investment functions, where I use the terms "desired investment function" to represent how parents *want* to invest (that is, the $s^*(.)$ function which maps achievement to input choices to correctly solve parents' optimization function) and the term "actual investment functions" to represent how parents *actually* invest (that is, the true (average) mapping from children's achievement to parents' input choices ($s(A)$)). Gauging whether there are divergences between the two functions can thus help us evaluate whether inaccurate beliefs cause inefficiently low returns even if we do not have long-run returns measures.[19]

Figure 2 demonstrates how to do this. To determine the desired investment function, one can look at how investments depend on perceived achievement (the dotted lines), since that shows us how parents want to invest. These lines should be steeply sloped with slope β_1. To determine the actual investment function, one can see how investments depend on true achievement (the solid lines: the line has the same y-axis as the dotted line but a different x-axis).

Prediction 1. *Distortions induced by inaccurate beliefs will cause the slope of the actual investment function (i.e., the gradient of investments on true achievement) to be flatter than the slope of the desired investment function (i.e., the gradient of investments on perceived achievement), with the difference in slopes being* $\beta_1(1 - \frac{cov(\tilde{A},A)}{var(A)})$.

The logic is similar to a measurement error setup with attenuation bias.[20] To see this, express beliefs as a regression function of true achievement, $\tilde{A} = \gamma_0 + \gamma_1 A + \nu$, with ν uncorrelated by definition with A and $\gamma_1 = \frac{cov(\tilde{A},A)}{var(A)}$ by the standard OLS formula. Investments can then be expressed as follows:

$$\begin{aligned} s &= \beta_0 + \beta_1 \tilde{A} \\ &= (\beta_0 + \beta_1\gamma_0) + \beta_1\gamma_1 A + \beta_1 \nu \end{aligned}$$

Thus, the slope of the actual investment function ($\beta_1\gamma_1$) should be attenuated if $\gamma_1 < 1$. This is the case in the sample here ($\gamma_1 = .3$) and should be the case for most processes by which we might expect beliefs to form (e.g., Bayesian updating where the prior is the true distribution and the parent receives signals of true ability). More generally, it will be true whenever beliefs and the truth are imperfectly correlated and the distribution of beliefs has

[19]Indeed, since the intervention was small, just a one-time infusion of knowledge, we might not get long-run impacts even if there are inefficiencies since a more sustained intervention could be needed.

[20]Note that one difference from the standard measurement error setup is that, there, only the econometrician has the mis-measured regressor and so the bias is only in the estimated regression line, whereas here, the actor herself is using the mis-measured regressor, and so the bias affects the true investment function.

the same or lower variance than the distribution of true achievement.[21] The intuition behind the attenuated slope is that investments do not respond as much to true achievement as they would optimally, causing inefficiently low returns.

Estimation

It is difficult to empirically estimate the difference between the gradients of investments on perceptions and 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 due to heterogeneous tastes (ε_i) and consider comparing the slopes estimated from the following two regressions:

$$s_i = b_0^P + b_1^P \tilde{A}_i + \varepsilon_i \quad (1)$$

$$s_i = b_0^A + b_1^A A_i + u_i \quad (2)$$

The slope from regression 1 will be $\beta_1 + \frac{cov(\tilde{A},\varepsilon)}{var(\tilde{A})}$ while the slope from regression 2 will be $\beta_1\gamma_1 + \frac{cov(A,u)}{var(A)} = \beta_1\gamma_1 + \frac{cov(A,\varepsilon)}{var(A)}$ (since, from above, we can see that $u_i = \varepsilon_i + \beta_1\nu_i$, and $cov(\nu_i, A_i) = 0$). Thus, the difference in slopes will be $\beta_1 (1 - \gamma_1) + \left(\frac{cov(\tilde{A},\varepsilon)}{var(\tilde{A})} - \frac{cov(A,\varepsilon)}{var(A)}\right)$ and so will only give us a good measure of distortions due to inaccurate beliefs (i.e., $\beta_1 (1 - \gamma_1)$) if the second term $\left(\frac{cov(\tilde{A},\varepsilon)}{var(\tilde{A})} - \frac{cov(A,\varepsilon)}{var(A)}\right)$ is equal to 0, that is, if unobserved determinants of investments are either uncorrelated or identically correlated with both A and $\tilde{A}$.

However, now consider an intervention that changes parents' beliefs from $\tilde{A}$ to A, and thus shifts them from investing based on perceptions, $s^*(\tilde{A}) = \beta_0 + \beta_1\tilde{A}$, to investing based on the truth, $s^*(A) = \beta_0 + \beta_1 A$. Estimating regression 2 with parents who have not received the intervention (control parents) will still identify $\beta_1\gamma_1 + \frac{cov(A,\varepsilon)}{var(A)}$, whereas estimating it with parents who have received the intervention (treatment parents) will now identify $\beta_1 + \frac{cov(A,\varepsilon)}{var(A)}$. Since the omitted variable terms are now identical between the two estimates, comparing the slope between treatment and control groups will now allow us to see whether inaccurate beliefs distort parents' investments at baseline; that is, to estimate $\beta_1 (1 - \gamma_1)$.[22] This leads

[21] To see this, one can rewrite $\frac{cov(\tilde{A},A)}{var(A)} = corr(\tilde{A}, A)\frac{std(\tilde{A})}{std(A)}$. Other processes for which $\gamma < 1$ are if the beliefs distribution is the same as the achievement distribution, or if parents were more over-confident for lower-ability children. Beliefs processes with $\gamma_1 \geq 1$ include (1) simple over (or under) confidence where beliefs are just shifted from the truth by a constant, i.e,. $\tilde{A} = A + a$ for some constant a, and (2) classical measurement error, in which $\tilde{A} = A + \varepsilon$ with ε mean-0 white noise. Note that, under these processes, beliefs would not be contained within the 1-100 range. Misallocations would still be problematic, but we would have to use other tests to detect them: this test is enabled by the fact that, at the top (or bottom) end of the achievement distribution, beliefs can be inaccurate in only one direction, and so $E(Abs.Val.(\tilde{A}_i - A_i))$ becomes $Abs.Val.(E(\tilde{A}_i) - E(A_i))$.

[22] 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 $\tilde{A}$ with λ the weight on A, then the difference

to the following testable prediction:

Prediction 2. *If inaccurate beliefs distort parents' baseline investments, then the gradient of the investment functions against true achievement in the treatment group will be steeper than in the control group, with the difference in slope equal to* $\beta_1(1 - \frac{cov(\tilde{A},A)}{var(A)})$ *(i.e.,* $\beta_1(1-\gamma_1)$*).*

The intuition is that information allows parents to correct their baseline mistakes and invest along their desired investment function.

This paper aims to understand not just whether inaccurate beliefs distort educational investments in aggregate, but also whether heterogeneity in belief accuracy by parent SES can help explain why we see heterogeneity in education outcomes by parent SES. This framework indicates that parents whose returns have been more dampened through this channel should have more distorted investment functions at baseline.

Prediction 3. *The gradient of the investment function will change more in response to the information treatment for groups with less accurate baseline beliefs than groups with more accurate baseline beliefs.*

Note that this assumes that the desired investment function (β) does not vary by group.

This analysis is focused on heterogeneity in treatment effects (i.e., changes to the slope of the investment functions), not average treatment effects (ATE's), since ATE's will understate the level of distortions (e.g., if beliefs are unbiased but inaccurate). However, if parents are overconfident (under confident), it would also cause non-zero ATE's: average investments in complements would increase (decrease) and in substitutes would decrease (increase). (Note that I use the term "overconfidence" to represent upwardly-biased biased beliefs, as opposed to precisely-biased beliefs.)[23]

Extending the model: Uncertainty

The above model assumes that the slope of parents' desired investment function does not change when they receive information. In reality, the slopes could differ because of uncertainty: specifically, if baseline beliefs are more uncertain than post-intervention beliefs, then the slope of the desired investment function for beliefs (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, looking at how the information treatment affected the slope of the actual investment line would now estimate $Abs.Val\{\beta_1 - \beta_1^P\gamma_1\}$. Besides overconfidence, uncertainty is another reason we could get ATE's from the information experiment (i.e., if $E(\beta_0^P + \beta_1^P A) \neq E(\beta_0 + \beta_1 A)$). To assess whether $\beta_1^P = \beta_1$, one can test whether the information treatment effect on the slope of

in slopes would uncover $\lambda\beta_1(1-\gamma_1)$.

[23] Both definitions are used in the literature, with Bénabou and Tirole (2002) an example of overoptimism.

investments on achievement is equal and opposite to the information treatment effect on the slope of investments on perceptions.[24] Another testable implication of uncertainty is that people who had correct beliefs at baseline (i.e., for whom $A = \tilde{A}$) will still change their investments in response to information: they will shift from investing $\beta_0^P + \beta_1^P A + \varepsilon$ to investing $\beta_0 + \beta_1 A + \varepsilon$. Once we move beyond the model and allow the total budget for education to be flexible as well, another hypothesis to test is that uncertainty causes people who have accurate beliefs at baseline to spend more in response to information, if uncertainty leads to underinvestment (i.e., $\beta_0^P + \beta_1^P A < \beta_0 + \beta_1 A$).

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.[25] The left figure in Figure 3 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.[26] 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

[24] To see this, note that parent i with baseline perceptions $\tilde{A}_i$ and a child with true performance A_i will have a baseline investment of $s^P(\tilde{A})_i = \beta_0^P + \beta_1^P \tilde{A}_i + \varepsilon_i$. 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 $\tilde{A}$ can be expressed as $\tau(A_i, \tilde{A}_i) = s(A_i) - s^P(\tilde{A}_i) = (\beta_0 - \beta_0^P) + \beta_1 A_i - \beta_1^P \tilde{A}_i$. Thus, the heterogeneity in the treatment effect by A will identify β_1 and heterogeneity by $\tilde{A}$ will identify $-\beta_1^P$. (This assumes both β_1 and β_1^P are constant across the sample.) Note that this test works even with incomplete updating, in which case the heterogeneity by A identifies $\beta_1 \lambda$ (where λ is the updating parameter) and the heterogeneity by $\tilde{A}$ identifies $-\beta_1 \lambda + (\beta_1 - \beta_1^P)$.

[25] Beliefs were elicited by asking parents to point to the score on a visual scale. Elicited beliefs are binned at 5-point increments; results are robust to binning true achievement scores at 5-point increments as well.

[26] 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.

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 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: on average, 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 for overall achievement, with similar magnitudes for the gaps within specific subjects (Table 1). This corresponds to correlations of 0.3 between believed and true achievement for overall achievement, and 0.2-0.3 within subjects. Obviously the test measure itself has some noise so, to put this in perspective, the correlation between the first and second tests taken by the students within the terms were much higher: 0.8 for overall achievement, and 0.6 - 0.7 within subjects. The measures delivered were then the averages across 4.5 tests per student (on average), so should have even higher reliability. I also have data for a subset of the sample on their achievement in a previous term, which has correlations with current-term achievement of 0.6 for overall and 0.5 within subjects. The higher correlation between different achievement measures than between achievement and beliefs suggests that the disconnect between beliefs and achievement does not just reflect noise in achievement. Parents also have inaccurate beliefs about between-subject (math relative to English) achievement, as well as inter-sibling relative achievement (child 1 relative to child 2), with beliefs about the inter-sibling achievement gap 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., percentile rank within a class).

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

Motivated by the conceptual framework in Section 3 (Prediction 1), Figure 4 presents suggestive evidence on whether parents' inaccurate beliefs cause distortions by comparing the slope of investments on believed performance with the slope of investments on true performance. Recall that the prediction is that, if there are distortions, the slope of investments on actual achievement will be flatter than the slope on perceived achievement. 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 locally linear regression lines of investments against believed achievement.

Workbooks (Complements) and Textbooks (Substitutes)

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.).[27] As expected since workbook difficulty is a perceived complement with achievement, the dashed line slopes steeply upwards, showing that parents choose more difficult workbooks when they believe their children are performing better.

Panel (b) shows the relationship for WTP for remedial textbooks. I analyze between-subject WTP (i.e., math WTP - English WTP, equivalent to running with child fixed effects (FE's)) 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.[28] The y-axis shows the log of WTP for the remedial math textbook minus the log of WTP for the remedial English textbook.[29] The x-axis shows the parents' beliefs about the child's performance in math relative to English. Because the textbooks are remedial (substitute with achievement), the prediction is that parents are willing to pay more for the book in the subject their child is more behind on, which is what we see since the line slopes steeply downwards.

The relationships between perceived achievement and investments are highly statistically significant: linear regression coefficients of workbook choice on parent beliefs yield t-stats of 33, 43, and 15 for math workbooks, English workbooks, and textbooks, respectively.

Secondary School Lottery

Before looking at the lottery investment functions, I give a brief overview of the lottery data. Appendix Figure A.2 shows a histogram of how parents split their tickets between

[27]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.

[28]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.

[29]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.

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, the analysis is examining which child parents choose to give their ninth ticket to. This is analogous to parents' real-world decisions when they have to make a choice between their children, for example, if they 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.)

Panel (c) of Figure 4 shows how parents' ticket allocations in the control group depend on children's achievement. For the dashed line, 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.[30] 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' decisions are constrained by an aversion to inequality between their children (see Appendix Figure A.2). Note that the fact that the oldest children in the sample are still at least 2.5 years away from secondary school and that admissions are not guaranteed may help explain why performance is a more powerful predictor than age.

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. Thus, 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. 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 achievement gap between the higher-achieving and lower-

[30]The results are robust to using an indicator for whether the parent gave more tickets to a given child as the dependent variable.

achieving child.

The results show that the gradient of investments on true achievement is flatter than 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 solid line is flatter 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. Parents appear to be 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 suggests that parents try to tailor their investments to their children's achievement, but their inaccurate beliefs prevent them from doing this as much as they would like. 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.[31] Although it is reassuring that the slopes conform to the causal predictions laid out in the Conceptual Framework, which omitted variables would not be expected to do in general, firmer evidence is needed; the information experiment results can help establish whether the difference is causal.

5 Impact of information

In this section, motivated by the Conceptual Framework, we use the information treatment to establish whether parents' inaccurate beliefs negatively impact the allocations of their investments by testing whether information changed the gradient of the investment function. We pool all treatment households together for the analysis.[32]

[31] 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 all the findings would need to be subtle, but is obviously possible. One variant of the omitted variable concern is if a factor other than children's achievement underlies parents' decisions and is more highly correlated with beliefs than true achievement. Under any of these stories, we should not see investments respond to the information treatment.

[32] Recall that half of the parents in the treatment group were part of a "detailed skills" group that, in addition to receiving average achievement information, also also received information about their children's specific skills areas (e.g., can they add vs. can they multiply). For the allocations analyzed here, we don't have predictions that parents would respond differentially based on their child's specific skills (e.g., there's no ex ante reason to expect that parents would want to pay more for a math textbook if their child is doing worse in addition than subtraction), and so pool the detailed skills treatment with the other information treatment. We return to look at the detailed skills treatment in Section 7.2, since the hypothesis was that the detailed skills treatment could improve later outcomes by increasing parents' overall engagement.

5.1 Effect of information on beliefs

The information experiment only has power to affect investments if it changes parents' beliefs. Figure 5 compares the absolute value of the difference between each child's term 2 achievement and her parent's beliefs, using either the parent's beliefs elicited at the beginning of the baseline survey about her children's recent past achievement (dark grey bars) or the beliefs elicited at the end of the baseline survey (after the treatment group had received information) about how well the child would do on an achievement test if he/she took it that day (light grey bars). (Note that, for reasons described in Section 2.5, the beginning-of-survey and end-of-survey beliefs questions asked for beliefs about different things.) In the control group, the beginning-of-survey and end-of-survey beliefs are similarly far from children's true achievement. In contrast, in the treatment group, the information affected beliefs, since the end-of-survey beliefs are 7.6 points closer to children's true achievement than beginning-of-survey beliefs are, a difference which is statistically significantly different (p-value$<$ .01) from both the control group's end-of-survey gap and the treatment group's beginning-of-survey 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. (This does not mean that respondents did not understand the information delivered since end-of-survey beliefs were elicited about a different measure than the information that was delivered.)

5.2 Information treatment effects

Graphical evidence

I now use the information experiment to test whether the differences in Figure 4 between the investment gradient on the truth and beliefs represent the causal impact of inaccurate beliefs. Figure 6 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.

Per Prediction 2 from the conceptual framework, if the differences in Figure 4 between the control group's investment gradient on true achievement and investment gradient on beliefs represent the causal impact of inaccurate beliefs, then the treatment group's investment gradient on true achievement should be much steeper than the control group's, and be more similar to the control group's gradient on beliefs (dashed line in Figure 4). 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 6 shows that the information treatment clearly affected the treatment groups' investments, causing them to look much more similar to the desired investment functions (i.e., the control groups' investments on beliefs 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 (Prediction 2) by running the following regression:

$$s_{ij} = c_0 + c_1 A_{ij} * Treat_i + c_2 A_{ij} + c_3 Treat_i + c_4' X_{ij} + \varepsilon_{ij} \quad (3)$$

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.[33] 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 $\beta_1(1 - \frac{cov(\tilde{A},A)}{var(\tilde{A})})$ metric from Section 3 which allows us to assess how much inaccurate beliefs distort investments.)

Table 2 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 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 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.

These investments were chosen specifically to allow us to see whether inaccurate beliefs cause misallocations; the levels of the investments – and thus the ATE's– are not particularly interesting in and of themselves (e.g., the average amount that parents paid for math relative to english textbooks is not that interesting). However, for completeness, Col (6) shows the

[33] Results are robust to excluding the controls. Control variables include school FE's, 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 (e.g., in the plan written before the experiment was conducted) was never to control for stratum FE's, some of the stratum are very small, and so 20% of observations would be lost because there is no variation in treat within their stratum. The results are, however, robust to controlling for stratum FE.

ATE's.[34]

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:

$$Tix_{ij} = c_0 + c_1 Treat_i \times 1\{HigherPerformingSib\}_{ij} + c_2 1\{HigherPerformingSib\}_{ij} + \tau_i + \varepsilon_{ij} \quad (4)$$

$$\begin{aligned} Tix_{ij} = & c_0 + c_1 Treat_i \times 1\{HigherPerformingSib\}_{ij} + c_2 1\{HigherPerformingSib\}_{ij} + \\ & c_3 Treat_i \times A_{ij} + c_4 A_{ij} + \tau_i + \varepsilon_{ij} \end{aligned} \quad (5)$$

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 4 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 5 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 2 shows the regression results. Column (1) shows the regression of Equation 4. The information treatment caused parents to allocate an average of 0.98 more tickets to their higher-scoring sibling (t-stat=7.5), a significant magnitude relative to the control group mean of 0.53. Column (2) tests for whether the information also changes the slope of the line (Equation 5), 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.[35]

[34]As described in the Conceptual Framework, ATE's can result from overconfidence (negative/positive ATE's for complements/substitutes) and uncertainty (positive ATE's for both). Here uncertainty leading to underinvestment is likely minor since parents were forced to make choices, and so, unsurprisingly, the ATE's reflect the shift in the distribution of beliefs (parents were overconfident about math, English, and math relative to English at baseline).

[35]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 a change in the slope. 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 (5) 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.[36]

Uncertainty

In addition to changing the mean of individual parents' beliefs, the information treatment likely also decreased the uncertainty of parents' beliefs, which could also affect their investments if the slope of the desired investment function depends on uncertainty ($\beta_1^P \neq \beta_1$). In Appendix Table A.8 (Col (4), Panel A), I test for uncertainty effects by testing for treatment effects among parents whose initial beliefs were close to their child's true achievement. For English workbooks and textbooks, baseline uncertainty does not seem to play a big role. For math workbooks, parents who have their beliefs confirmed are more likely to chose the lower-difficulty workbook; this is driven entirely by parents with low-achieving children, so likely indicates that they were not sure enough to choose the beginning workbook without confirmation. We also see uncertainty effects for the lottery: treatment parents who received confirmation that their initial beliefs were essentially correct allocated 0.55 more tickets to the child that was higher performing.[37] Note that, for the lottery uncertainty effects, an alternative explanation is that the treatment increased the salience of achievement.

[36]Note that, if all we wanted to do was identify whether information caused investments to be more closely aligned with performance, then random assignment of the treatment is sufficient for identification. 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.

[37]See also Appendix Table A.6 which uses the second test described in section 3, with consistent results: for workbooks and textbooks, we cannot reject that heterogeneity in the treatment effect by parents' baseline beliefs is the same as heterogeneity by true achievement, but we can for the lottery.

Additional analysis: Lottery

One reason that parents might split their tickets so evenly between their children 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 $Treat_i$. Since information presumably decreased uncertainty, 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. Thus, although uncertainty may play some role in the equal allocations, it does not seem to be a primary factor: inequality aversion likely plays a large role.

It is also natural to wonder how information affects the distribution of tickets along other dimensions that may be correlated with performance and/or perceptions. Given the 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 the results show. If anything, parents in the treatment group allocate fewer tickets to their girls, although the difference (.25 tickets) is not statistically significant (p-value=0.21).[38]

6 Heterogeneity by parent education

The previous section suggested that inaccurate beliefs distort parents' investments. I next examine whether this contributes to a poverty trap. The hypothesis is that less-educated parents have less accurate beliefs than more-educated parents, and that this is one factor that prevents their children from attaining the same level of human capital as the children of richer, more-educated parents. The power of the analysis is limited by the limited heterogeneity in parent background within the sample, but the data still allow a test of whether the within-sample gradient is consistent with the hypothesis.

6.1 Belief accuracy

The first part of this hypothesis is that less-educated parents have less accurate beliefs. Table 3 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 com-

[38]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).

pleted any secondary education or higher, averaged across parents in the household.[39] The raw gaps (first column for each subject) show that less-educated parents have less accurate beliefs than more-educated parents. The findings are robust to child and parent controls (e.g., gender: second column for each perf measure), and even exist within schools (third column), suggesting that less-educated parents do not simply attend schools that give worse information. 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. However, although controlling for child achievement (col. (4) of Table 3) does attenuate the gap, statistically significant gaps still remain. Note that, since achievement is not exogenous, controlling for it will likely bias downward the effect of parent education from the true relationship, since less-educated parents' less accurate beliefs could have contributed to their child's low current achievement.[40] The gaps are also robust to controlling for parents' overconfidence (not shown).

Less-educated parents also have more uncertain beliefs (Appendix Table A.5, cols (4) - (6)), and are more overconfident (Table A.5, cols (1)-(3)), although their overconfidence is an almost mechanical effect of their children having lower achievement; controlling for achievement, less educated parents are, if anything, less confident, which is similar to what

[39]The average across parents in the household is used for two reasons: First, 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. Second, 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.

[40]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.

Filippin and Paccagnella (2012) find for students.

6.2 Updating

Table 4 looks at whether less-educated parents change their beliefs more than more-educated parents in response to the information treatment. Column (1) regresses 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, and its interaction with parent education. (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.) There is significant heterogeneity: less-educated parents have a larger treatment effect 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, or 54% of the mean level of updating in the control group. Part of the reason that less-educated parents update more is that they have less accurate baseline beliefs, but column (2) shows that that is not the full story, since 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), as well as the interaction between treatment and baseline achievement; even in this specification, the heterogeneity by parent education remains sizable and 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.

Cols. (3) and (4) look at whether the treatment also shifted the mean of beliefs more for less-educated parents; the coefficients are consistent with less-educated parents shifting their beliefs more negatively, but the differences are not statistically significant.

6.3 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. The analysis is complicated by the fact that the desired investment function (β_1 from the conceptual framework) could also vary by parent background. This is most plausible for the workbooks, for which it is difficult to see why more educated parents should have a different mapping between achievement and a free choice. This is less likely for textbooks and the lottery; for example, for textbooks, credit constraints could change how much parents can spend and thus how much their spending responds to achievement.

Figure 7 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 and Textbooks

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 are worse at targeting their investments at baseline. 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 5 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), (4), and (7) show heterogeneity by parent education for workbooks and textbooks (specifically: by average years of education of parents in the household), and Columns (2), (5), and (8) show heterogeneity in slopes for the treatment group only. For math (Col. (1)), 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 (1), coefficient on $ScoreXTreatXParentYrsofEduc$). For English, the patterns are similar: less-educated parents have larger treatment effects, significant at the 5% level.

For textbooks, 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. The modest heterogeneity could reflect credit constraints.

Lottery

The lottery graphs (panel (c) of Figure 4) again look relatively similar for less-educated and more-educated parents. Table 5 shows that the treatment effects are smaller for more-educated parents, but not statistically significantly so. Since there was small but significant heterogeneity by parent education in belief accuracy about between-sibling performance, the absence of significant heterogeneity in treatment effects could result either from a lack of statistical power, or heterogeneity in the desired investment functions (caused, for example, by heterogeneity in inequality aversion, or in the weight placed by achievement relative to other factors).

7 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 examine 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 months. 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 other factors, including the reaction of the children to the information, the resulting responses of the parents to the children, etc.

7.1 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 over time. In the end line survey, we elicited parents' beliefs about their children's current achievement. Appendix Table A.7 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 (i.e., what we delivered to the treatment group in the intervention), $Treat$, 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 more closely aligned to past achievement (the coefficient on $Treat$ is negative and on $Treat \times Score$ is positive), although the relationship is much stronger and only statistically significant for overall achievement, which may have been more salient to parents.[41]

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. This likely reflects a combination of changes in achievement, and the fact that beliefs can be transitory and uncertain. For the experimental outcomes analyzed in Section 4, I started by looking at how investments in the control group depended on parents' baseline beliefs to develop predictions for the desired investment function. That analysis was enabled by the fact that those investment decisions were made at a single point in time, directly after the beliefs were elicited, and so we knew exactly what beliefs were guiding parents' decisions. Here, it is more difficult because the outcomes

[41] 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.

(e.g., expenditures, attendance) were determined continuously over the period between the baseline and endline surveys, a period over which beliefs were changing, and so the relevant beliefs would be some average belief measure across the period, which we do not have, and which baseline beliefs do not seem to proxy well for. As shown in Appendix Table A.8, in the control group, current investments are only weakly related to baseline beliefs, but are somewhat more strongly related to the parents' end line beliefs. Since end line beliefs were obviously measured after the outcomes were determined, the potential for reverse causality (i.e., did parents spend more on their child because they thought their child was high-performing or vice verse?) makes these correlations harder to interpret. So, in the next section, I turn directly to the information treatment effects without first analyzing the predictions from the baseline beliefs data. Note that this is not problematic for looking at the information treatment effects, it just means we don't have *ex ante* predictions to check the findings against.

7.2 Information Treatment Effects: Endline Data

Relative to the investments examined earlier, the level of these investments is more interesting (i.e., we care if the intervention changed overall spending levels). So, while we are still primarily interested in testing for misallocations (treatment effects on the slope of the investment functions), we also want to understand whether there are ATE's.

Figure 8 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. Table 6 shows the regression results. Columns (1) and (2) present coefficients on $Treat$ and $Treat \times A$ from estimation of regression 3; for ease of interpretation, columns (3) and (4) 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). 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. Finally, Column (5) tests for an average treatment effect.

On the extensive margin (dropouts), information changed the gradient of the investment function: 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 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 (3) and (4)). These are large effects relative to the control group mean (2%). However, there was no statistically significant increase in dropouts in the treatment group: since parents were overconfident at baseline, the fact that we do not see a corresponding increase in dropouts could reflect that their uncertainty also decreased, thereby increasing their investments, or be due to lack of statistical precision.

On the intensive margin, I first look at transfers. It is perhaps surprising that, although there is no significant change in the gradient of the investment function (Cols (1) - (4)), there is an ATE: 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). This may reflect the fact that the desired investment function varies 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 be positively sloped 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 investment function would have the opposite slope. And, indeed, that is exactly what we see once we condition on school-quality, proxied by school-average achievement (Apppendix Table A.11 and Figure 9a): information causes the gradient of the investment function to become more positive at low-quality schools and more negative at high-quality schools.

There is 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. Third, statistical power to detect the change in the gradient could be too low. Finally, the index could mix complements with substitutes.[44]

[42] Appendix Table A.9 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.9 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-

Uncertainty

As mentioned, one reason that we might not detect positive average treatment effects for dropouts even though dropouts appear to increase with student achievement and parents were initially overconfident could be uncertainty effects. Column (4) of Appendix Table 8 tests for uncertainty effects by testing for changes in behavior by parents whose baseline beliefs were close to the truth; there is not strong evidence for uncertainty effects, but power is limited.[45]

Detailed skills treatment results

Appendix Table 8 (cols. (2) and (3)) test for whether the detailed skills treatment increased parent engagement and investments. There are no statistically significant impacts on any outcomes, but unfortunately power appears to be limited.

7.3 Heterogeneity in the Regression Results by Parent Education

Columns (1) - (4) of Table 7 examine whether there is heterogeneity in the observed effects by parent education. For expenditures, we see significant heterogeneity: less-educated parents in the treatment group increased their expenditures and attendance 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.

Thus, these results suggest that there might be important heterogeneity in the production function faced by parents of different education levels: in the language of the Conceptual Framework, the desired investment functions (β) appear to differ by parent education. For less-educated parents, who might not expect that their children could attend secondary school, the investment function could have a more negative slope because parents might

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]We also tried looking at heterogeneity in the ATE's by parents' baseline uncertainty or by how many exams the child's teacher used, but unfortunately the power for both tests is limited and so we do not find conclusive results.

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

think there is a high return to attaining basic skills like reading, but not after that. In contrast, for more-educated parents who might think their child has some chance but not a guarantee of admission to secondary school, the investment function could be more positively sloped since parents think there is a high return for pushing the child over the hump into secondary school admissions.

As a result of the heterogeneity in the desired investment functions, the heterogeneity by parent education in how much the investment functions' slopes changed after the information treatment no longer shows us whether the investments of more- or less-educated parents are more distorted at baseline. Rather, to see which group's investments are more distorted by inaccurate beliefs at baseline, one should refer to the the results in section 6.1, which used outcomes (especially the workbooks) for which the desired investment functions are likely more homogeneous across SES levels.

Average treatment effects

Since less-educated parents are on average more overconfident than more-educated parents, one might be concerned that less-educated parents might invest less in complements as a result of the information treatment. However, Table 7 (cols (5) and (6)) show that there is no statistically significant heterogeneity by parent education in average treatment effects for any investments examined. This could be because of uncertainty effects, or because of the heterogeneity discussed above in the desired investment functions.

8 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. There is also some evidence that their investments respond more to improved information.

Taken together, the findings suggest that one reason we may see poverty persist across generations even in the face of expanding access to education is inaccurate beliefs: poorer households, which on average are less-educated, have less accurate beliefs about their children, and this causes them to invest inefficiently in their children's education, thereby hindering the future earnings of their households. This could serve to perpetuate inequalities within countries, as well as provide one channel to explain why human capital levels across countries do not converge.

It is perhaps surprising that baseline information is poor if the returns to knowledge are high and the information exists. However, parents may over-estimate their own knowledge, or (perceived) information acquisition costs may be high, both of which have been suggested in the U.S. in Bergman (2014) (and, indeed, qualitative interviews in the study area reveal that less-educated parents are intimidated to talk to their children's teachers). This is consistent with other findings in the literature that information constraints matter for human capital investment (Jensen, 2010; Dinkelman and Martínez A, 2014).

In general, an intervention that corrects one market imperfection can move us farther from the optimum if there are multiple market failures. For example, the finding that information allows parents to invest along their desired investment functions only implies an improvement in efficiency if parents know the true returns functions, an assumption which seems very plausible for the more straightforward investments examined (e.g., remedial textbooks, workbooks) but may be less so for others. One particular concern in this setting is that information could cause investments to decrease for some students. I do not find evidence of average decreases in investments here, but the allocation effects do imply decreases in certain investments for certain types of students (e.g., dropouts increased for lower-achieving students). If there are no other market failures, then this would still be making their households better off (i.e., allowing them to move their investments to higher-returns opportunities), but there may be other reasons we think households underinvest in education for those students, such as agency issues within the household, externalities, or underestimation of returns. In that case, we need to think carefully about whether we would want to scale up this type of policy. A first-best solution would be to deliver the information and use other policies to more directly correct the other market failures, since withholding information about achievement would have efficiency costs of the types highlighted in the conceptual framework and throughout the paper. However, if other complementary policies are not feasible, keeping information low could potentially be a second-best solution.

Another concern for scale-up would be if beliefs enter directly into the utility function, an assumption which is sometimes made in the behavioral economics literature (e.g., Köszegi (2006), Bénabou and Tirole (2002), and Weinberg (2009)) and can imply that some overconfidence is optimal. If beliefs just have consumption value, then increasing information could improve household's earnings or wealth but with a cost for average utility. If beliefs also have motivational value, as highlighted in Bénabou and Tirole (2002), then increasing information could also have earnings or wealth costs, although we might expect this channel to be less relevant when talking about parents' confidence, not own confidence.

If policymakers did want to scale up the policy, one open question for future research is how to best do that. 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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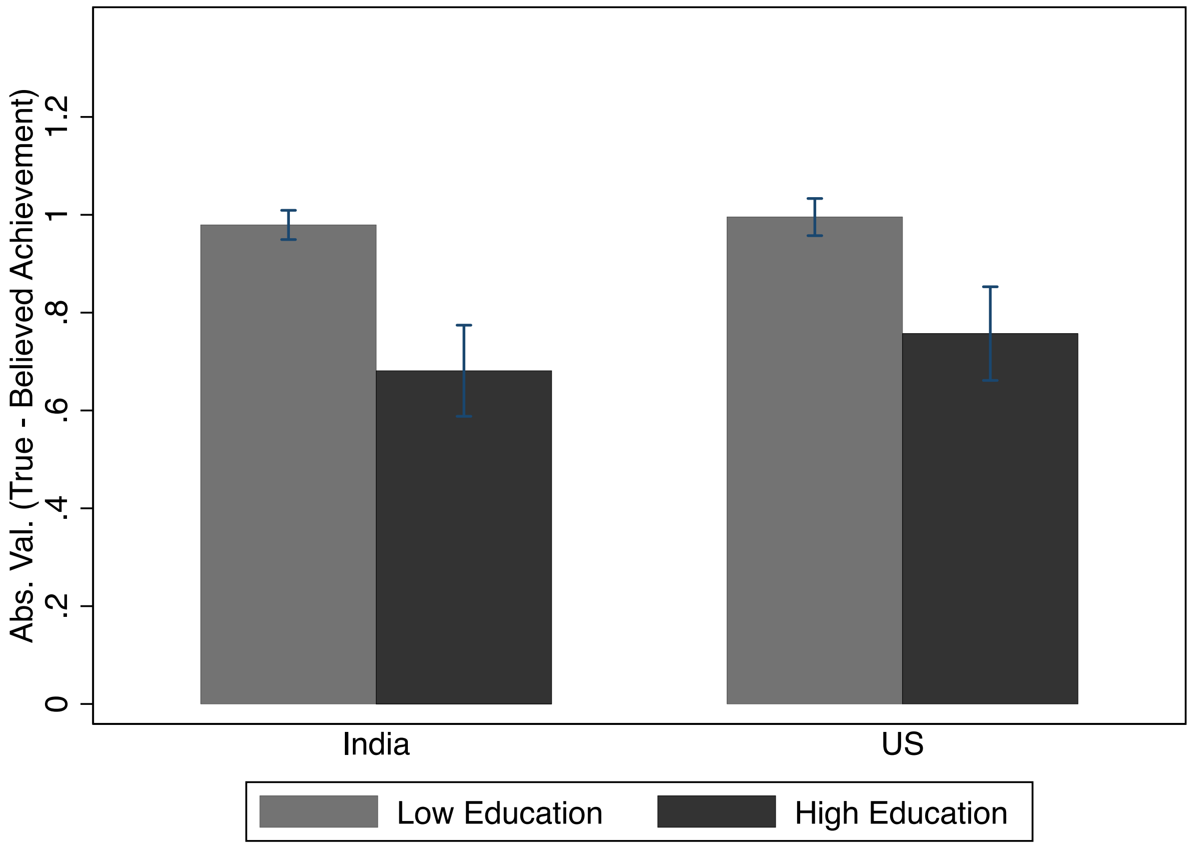
Figure 1: Motivating evidence

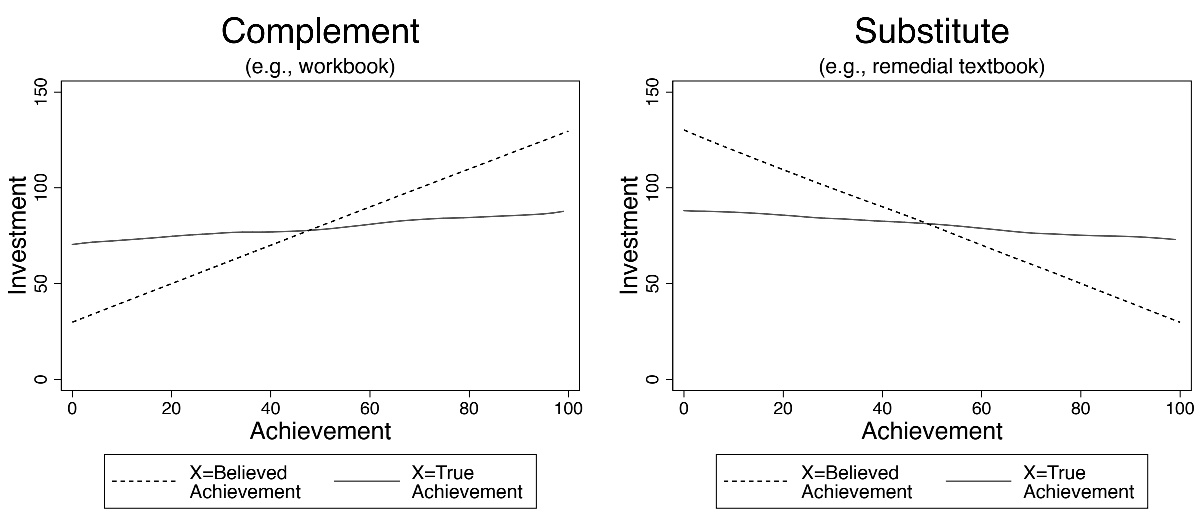
Across different contexts, less-educated parents have less accurate perceptions about their children's academic abilities



Notes: Bar height represents the absolute value of the gap between parents' perceptions about their child's performance on a test and their child's true performance on a test. Units normalized so that the low-education group has a value of 1. High-education taken as the top decile of education, corresponds to college in the U.S. and secondary school in India; results robust to other measures of education (above-median, top-quartile, etc.). India data source is Banerjee et al. (2010), where the academic ability measure is the average performance on the math and reading assessments designed by Pratham to assess learning outcomes. U.S. data source is the Beginning School Study (Alexander and Entwisle, 2006), a longitudinal study of children's academic and social development conducted with families of children enrolled in Baltimore City Public Schools. The academic ability measure is student performance (grades) on their school report cards.

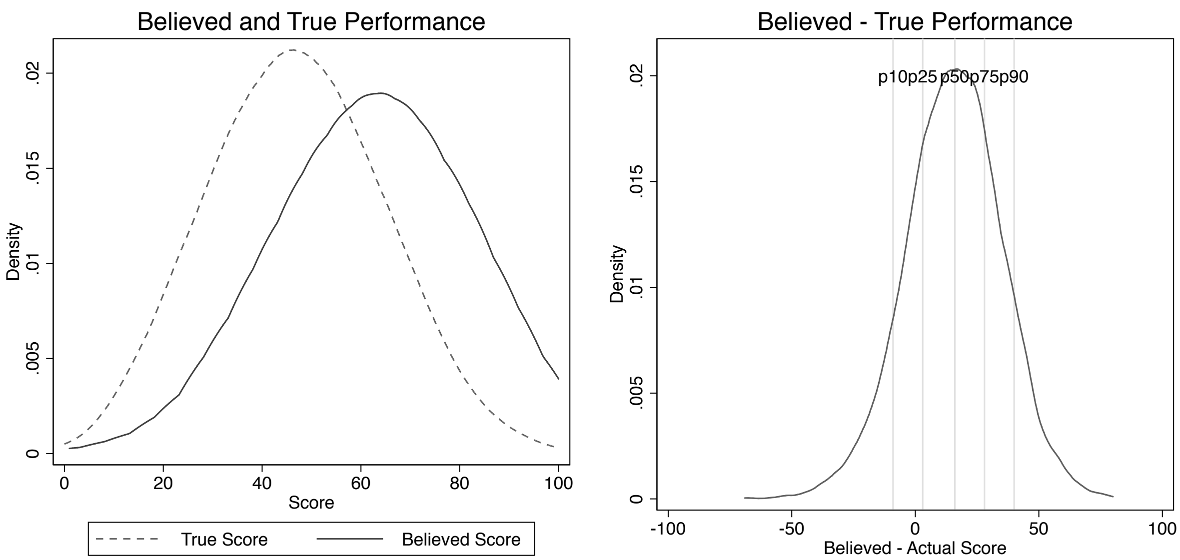
Figure 2: Conceptual framework

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



Notes: Illustrative graph, not based on real data.

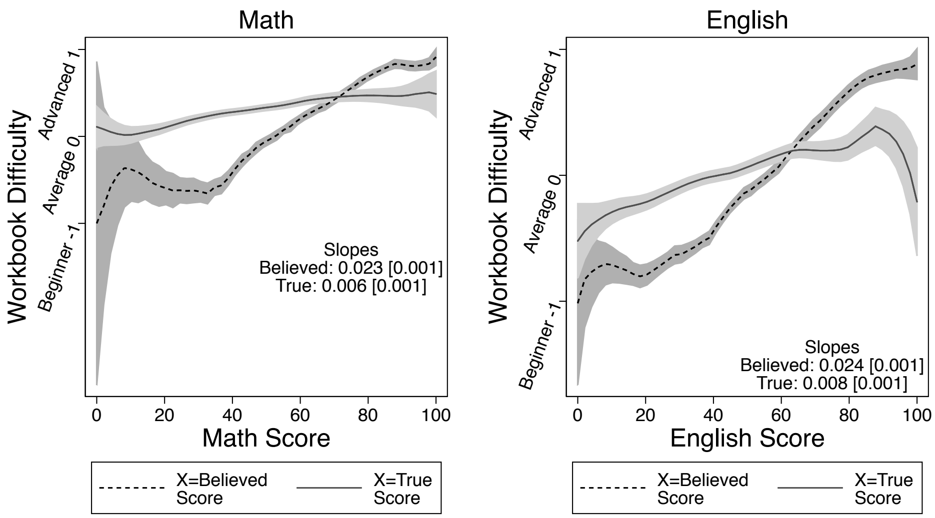
Figure 3: Parents have inaccurate perceptions about their children's achievement



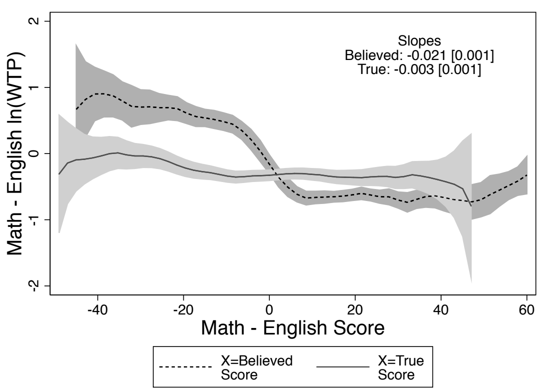
Notes: Data source is baseline data (full sample). The left graph shows kernel density plots comparing the distribution of parents' beliefs about their children's Term 2 2011-2012 achievement test performance, elicited at the beginning of the baseline survey, with the distribution of their children's 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 4: Consistent with a distortion, the investment gradient on true achievement is flatter than on believed achievement

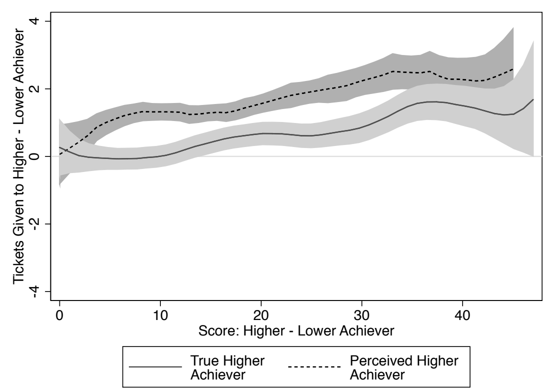
(a) **Workbooks (Complements)**



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

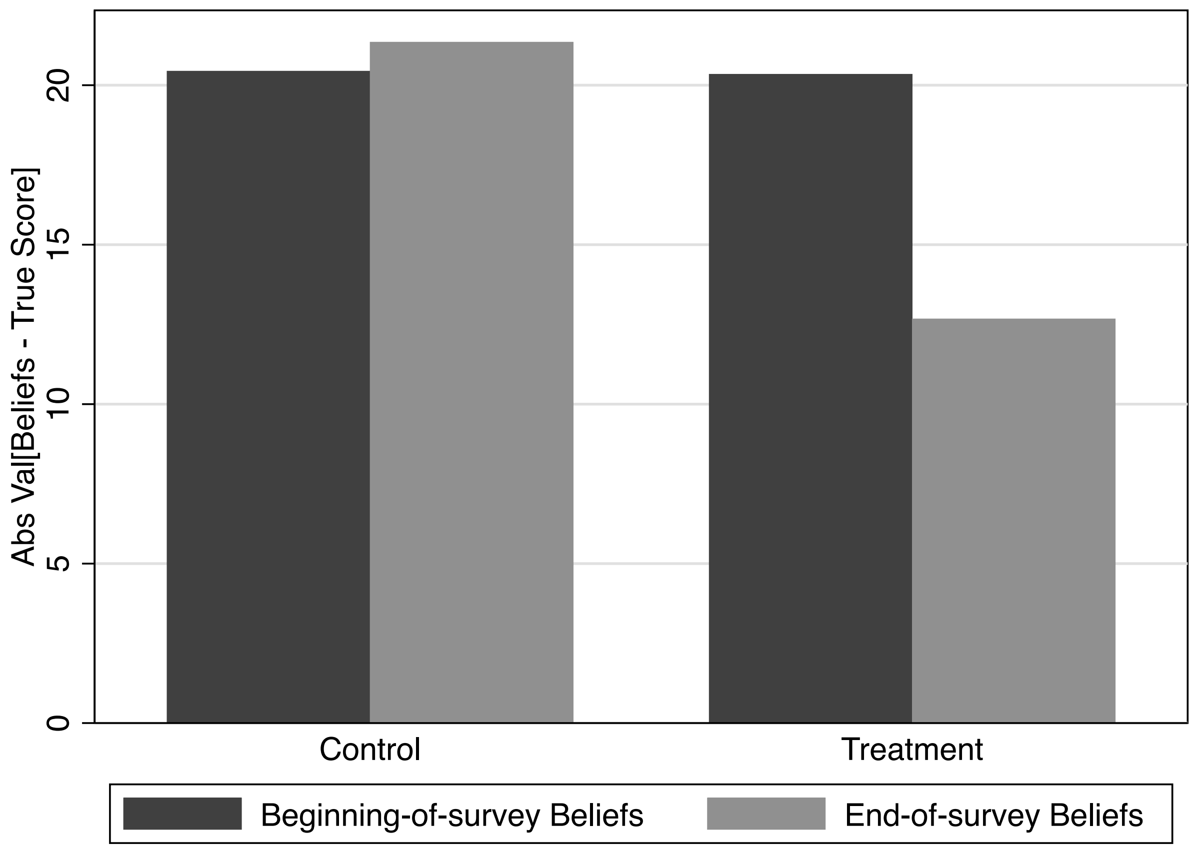


(c) **Secondary School Lottery**



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.

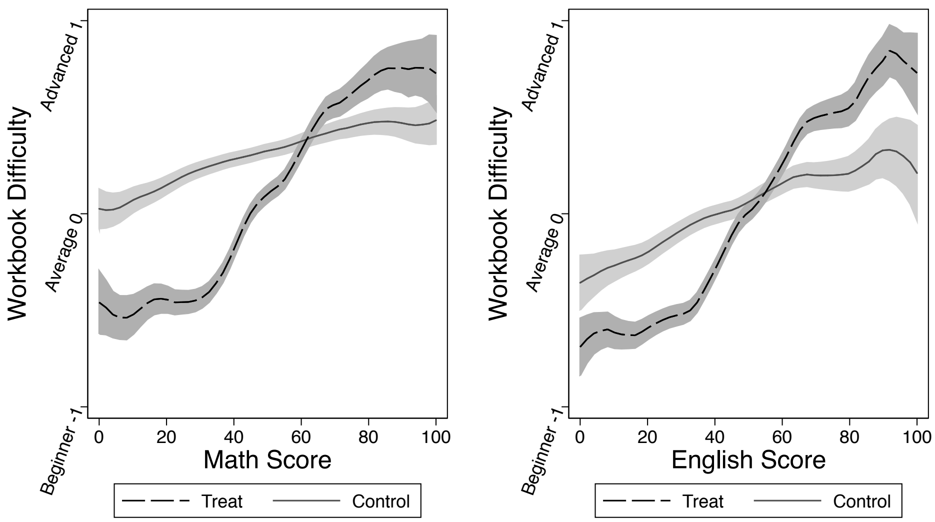
Figure 5: Information shifts parents' beliefs towards their children's true achievement



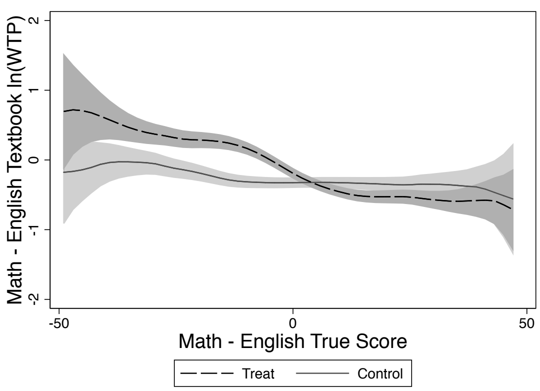
Notes: Data source is baseline survey data. The dark gray bars show the absolute value of the difference between children's true term 2 2011-2012 achievement test scores and their parents' "beginning of survey" beliefs about those scores, which were elicited at the beginning of the baseline survey (before 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 6: Treatment effects: Information increases the gradient of investments on true achievement

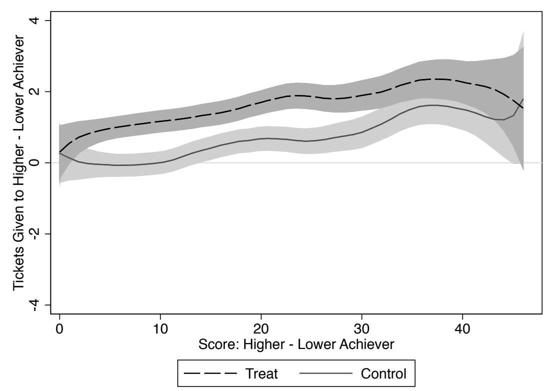
(a) **Workbooks (Complements)**



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



(c) **Secondary School Lottery**



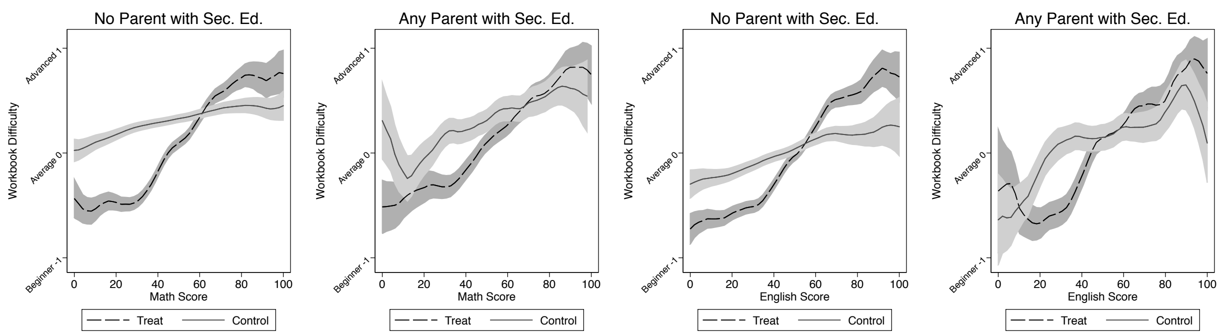
Notes: Data source is baseline survey data. All lines are local linear regression lines with investments as the dependent variable and true achievement as the x-axis. For the workbook graphs, the dependent variable is the parent's choice among 3 level-specific workbooks which are parametrized as -1 (beginner), 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 7: Heterogeneity in treatment effects by parent education (Textbooks, workbooks, secondary school lottery)

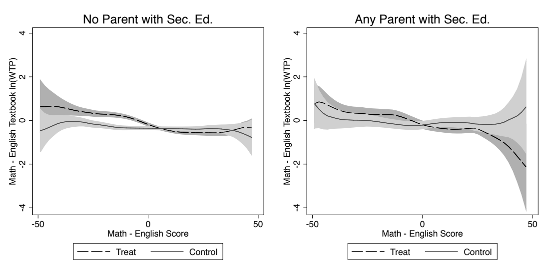
(a) **Workbooks (Complements)**

Math Workbooks

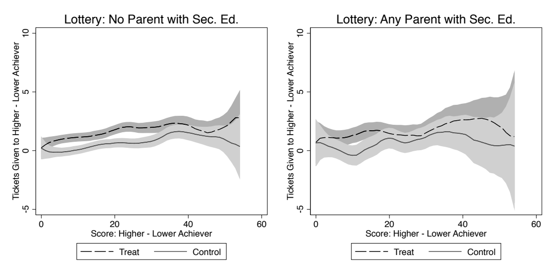
English Workbooks



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

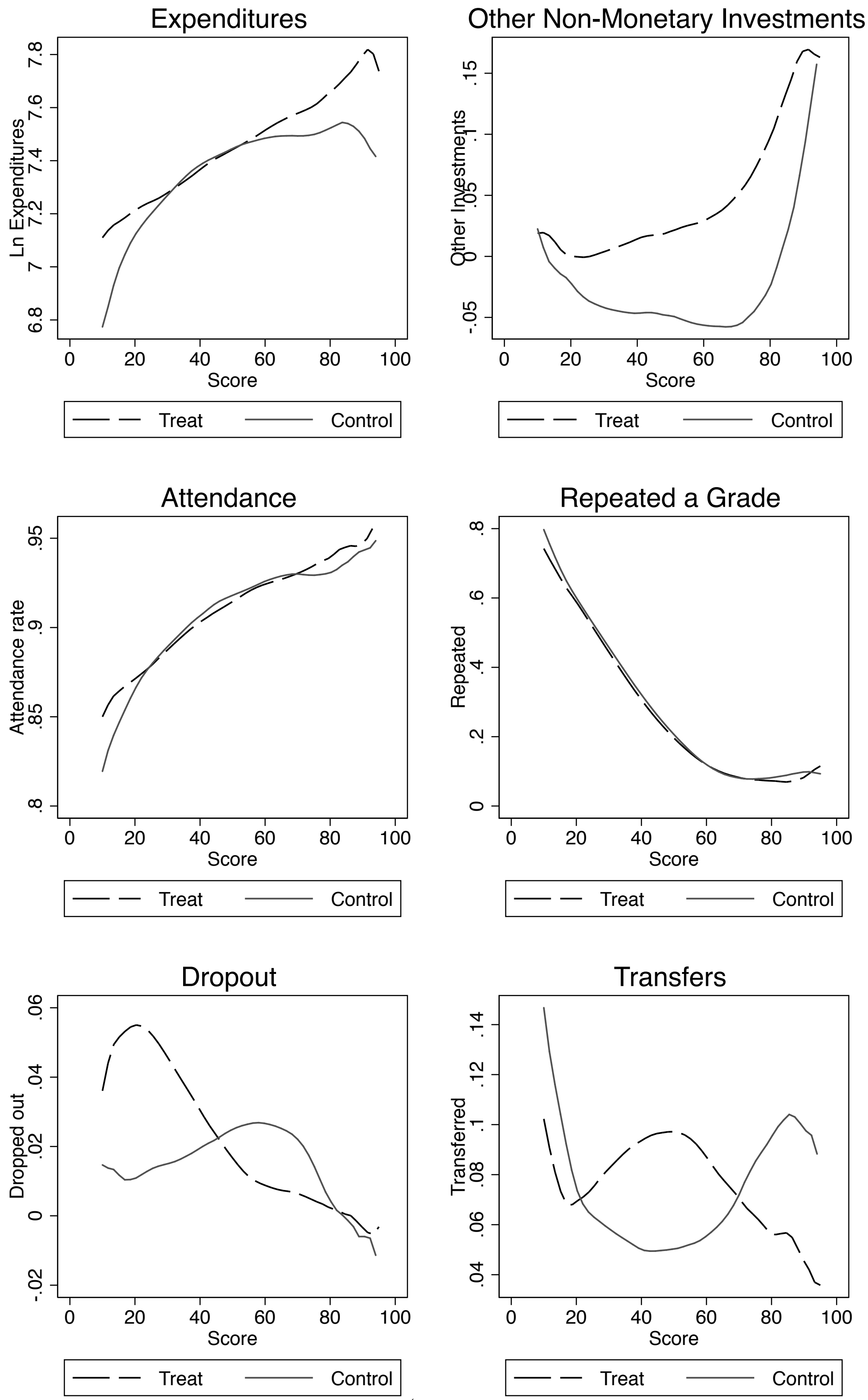


(c) **Secondary School Lottery**



Notes: 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.

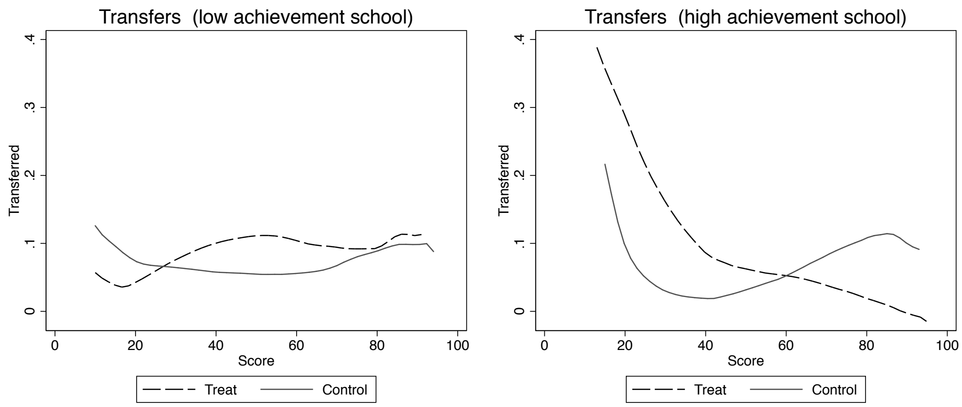
Figure 8: Treatment effects: Longer-run outcomes



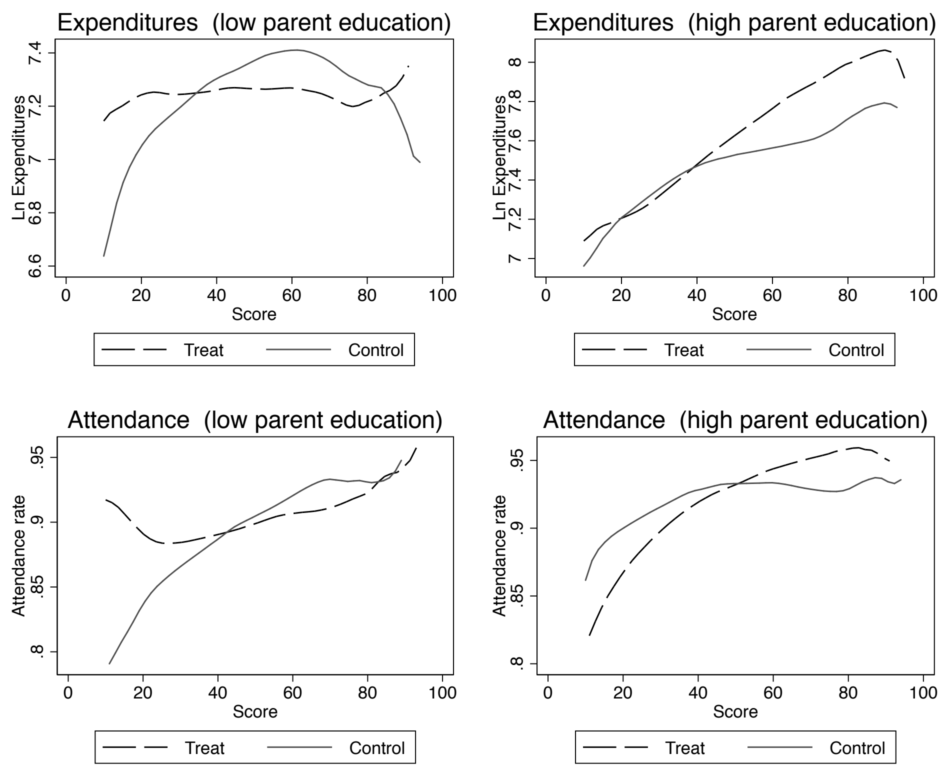
Notes: Data sources are endline survey data (expenditures, other non-monetary investments, homework help, asked for homework help, dropout, and transfers) and data collected from schools (attendance rate, grade repetition). All lines are local linear regression lines with investments as the dependent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

Figure 9: 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. 8 (See notes for Fig. 6 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 decision maker	0.92	0.27	0.91	0.92	0.01	0.01	0.31
Age	40.8	11.0	40.6	41.0	0.32	0.44	0.47
Education (years)	4.44	3.57	4.42	4.45	0.04	0.13	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	0.5	0.47	0.46	-0.01	0.02	0.7
Respondent's weekly income	2,126	4,744	2,051	2,203	197	194	0.31
Household Background							
Number of chidren[a]	5.13	1.74	5.16	5.1	-0.05	0.07	0.47
One-parent household	0.19	0.39	0.19	0.2	0.01	0.02	0.47
Parents' average education (years)	4.66	3.25	4.68	4.64	-0.04	0.12	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	3.72	1.37	3.72	3.72	0	0.04	0.94
Child's age	11.6	2.68	11.7	11.6	-0.1	0.08	0.21
Child is female	0.51	0.5	0.52	0.5	-0.02	0.01	0.25
Baseline attendance	0.91	0.13	0.92	0.91	0	0	0.72
Annual per-child education expenditures	1,742	2,791	1,712	1,772	58.0	83.0	0.48
Fees paid to schools	381	1,128	384	378	-6.84	23.9	0.78
Uniform expense	576	1,019	548	603	49.9	36.1	0.17
School supplies, books, tutoring, etc.	785	1,819	780	790	14.3	62.3	0.82
Any supplementary expenditures on child	0.9	0.3	0.9	0.89	-0.01	0.01	0.49
Achievement Scores							
Overall score	46.8	17.5	47.1	46.4	-0.74	0.46	0.11
Math score	44.9	20.2	45.4	44.4	-1.08	0.54	0.04
English score	44.2	20.1	44.5	43.9	-0.56	0.53	0.29
Chichewa score	51.3	22.6	51.5	51.0	-0.57	0.59	0.34
(Math − English) Score	0.71	19.5	0.93	0.5	-0.53	0.51	0.3
Respondent's Beliefs about Child's Achievement Scores							
Believed Overall Score	62.4	16.5	62.7	62.0	-0.78	0.48	0.11
Believed Math Score	64.7	19.0	65.2	64.3	-0.94	0.55	0.09
Believed English Score	55.3	20.9	55.6	54.9	-0.71	0.62	0.25
Believed Chichewa Score	66.8	19.4	66.8	66.7	-0.1	0.6	0.87
Beliefs about (Math − English) Score	9.48	21.5	9.59	9.37	-0.23	0.63	0.71
Respondent's Misperception about Child's Achievement							
Abs Val [Believed − True Overall Score]	20.4	14.5	20.4	20.3	-0.12	0.43	0.77
Abs Val [Believed − True Math Score]	25.8	18.0	25.8	25.7	-0.1	0.52	0.85
Abs Val [Believed − True English Score]	21.4	16.4	21.6	21.1	-0.57	0.48	0.23
Abs Val [Believed − True Chichewa Score]	23.8	17.5	23.7	23.9	0.18	0.51	0.73
Abs Val [Believed − True (Math-English) Score]	22.1	17.4	22.3	21.9	-0.44	0.51	0.39
Abs Val [Believed − True Overall Score (Child1-2)]	18.7	15.1	18.9	18.5	-0.35	0.59	0.55
Beliefs about Returns to Education							
Returns to educ. (sec. school/prim. earnings)	3.22	3.79	3.28	3.16	-0.11	0.15	0.47
Believes educ. and achievement complementary	0.91	0.29	0.9	0.91	0	0.01	0.68
Sample Sizes							
Sample Size–HHs	2,634		1,327	1,307			
Sample Size–Kids	5,268		2,654	2,614			

Notes. Data Source is baseline survey. Standard errors for the t-test of equality.

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 able child from getting a secondary education.

Table 2: Information treatment effects (Textbooks, workbooks, and sec. school fee lottery)

	Treatment effect on slope					A.T.E.
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Dependent Var: Math Workbook Choice						
Treat × Math Score	0.013***	-0.0035***	0.012***	0.011***	0.011***	
	[0.00093]	[0.00040]	[0.0017]	[0.0017]	[0.0017]	
Treat	-0.91***					-0.31***
	[0.049]					[0.020]
Math Score	0.0065***	0.015***	0.0081***	0.0084***	0.0084***	0.013***
	[0.00065]	[0.00056]	[0.0012]	[0.0012]	[0.0012]	[0.00051]
Household FE		✓	✓	✓	✓	
Observations	5,239	5,239	5,239	5,239	5,239	5,239
R-squared	0.217	0.154	0.695	0.696	0.696	0.184
Panel B. Dependent Var: English Workbook Choice						
Treat × English Score	0.013***	-0.00028	0.013***	0.013***	0.013***	
	[0.00096]	[0.00043]	[0.0017]	[0.0017]	[0.0017]	
Treat	-0.68***					-0.13***
	[0.048]					[0.021]
English Score	0.0076***	0.014***	0.0089***	0.0086***	0.0085***	0.014***
	[0.00073]	[0.00058]	[0.0012]	[0.0012]	[0.0012]	[0.00052]
Household FE		✓	✓	✓	✓	
Observations	5,239	5,239	5,239	5,239	5,239	5,239
R-squared	0.204	0.170	0.710	0.714	0.715	0.177
Panel C. Dependent Var: ln(Math Textbook WTP) - ln(English Textbook WTP)						
Treat × (Math − English Score)	-0.013***	-0.013***	-0.013***	-0.014***	-0.014***	
	[0.0022]	[0.0022]	[0.0038]	[0.0039]	[0.0039]	
Treat	0.15***					0.14***
	[0.041]					[0.041]
Math − English Score	-0.0030*	-0.0032**	-0.0015	-0.00048	-0.00041	-0.0099***
	[0.0016]	[0.0016]	[0.0025]	[0.0028]	[0.0028]	[0.0011]
Household FE		✓	✓	✓	✓	
Observations	5,183	5,183	5,183	5,183	5,183	5,183
R-squared	0.033	0.030	0.601	0.602	0.602	0.024
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]	
Household 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. Regressions control for school FE, parents' education, level, and the between-child score gap, and the main effect of any variables interacted with Treat. Workbook choices are -1 for beginner, 0 for average, 1 for advanced. The treatment effect on slope results can be interpreted as follows: Take for example Panel A., column (1). The coefficient on Math Score is the slope of the line in the control group: if a child's score increases by one point, the chance that her parent chooses the next level of workbook increases by .65%. The coefficient on Treat x Score represents treatment effect on the slope; the coefficient of .013 means the treatment increased the slope by 200% (.013/.0065). A.T.E. stands for Avg. Treatment Effect. *** p<0.01, ** p<0.05, * p<0.1

Table 3: Less educated parents have less accurate beliefs

	Dependent Variable= Abs Val [True − Believed Performance]							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Performance Measure:	Overall	Overall	Overall	Overall	Math	Math	Math	Math
Secondary Education +	-3.552***	-3.530***	-2.200***	-1.427**	-4.870***	-4.957***	-4.864***	-3.981***
	[0.720]	[0.720]	[0.751]	[0.639]	[0.867]	[0.867]	[0.921]	[0.821]
Observations	5,019	5,019	5,019	5,019	5,021	5,021	5,021	5,021
Dep var mean	20.41				25.82			
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Performance Measure:	English	English	English	English	Chichewa	Chichewa	Chichewa	Chichewa
Secondary Education +	-1.877**	-1.874**	-1.119	-0.521	-4.464***	-4.206***	-2.484***	-1.518**
	[0.836]	[0.832]	[0.870]	[0.800]	[0.868]	[0.864]	[0.935]	[0.765]
Observations	5,021	5,021	5,021	5,021	5,021	5,021	5,021	5,021
Dep var mean	21.42				23.84			
	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)
Performance Measure:	Math−Eng	Math−Eng	Math−Eng	Math−Eng	Child2−1	Child2−1	Child2−1	Child2−1
Secondary Education +	-1.472*	-1.461*	-2.090**	-2.799***	-1.945*	-1.790*	-1.190	-1.785*
	[0.830]	[0.834]	[0.922]	[0.836]	[0.998]	[1.002]	[1.095]	[1.000]
Observations	5,021	5,021	5,021	5,021	2,514	2,514	2,514	2,514
Dep var mean	22.12				18.73			
Col. Specification Details								
Child and Parent Controls		✓	✓	✓		✓	✓	✓
School FE			✓				✓	
Performance Control				✓				✓

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 4: Less-educated parents update their beliefs more than more-educated (but the mean of their beliefs does not shift significantly more)

Dependent Variable:	Abs Val [Beliefs from end − beg. of survey]		Beliefs from end − beg. of survey	
	(1)	(2)	(3)	(4)
Treat × (Parent yrs. education)	-0.37***	-0.29***	0.19	-0.0033
	[0.11]	[0.098]	[0.14]	[0.13]
Treat	10.0***	-3.30***	-7.53***	-12.8***
	[0.64]	[1.26]	[0.84]	[2.05]
Parent yrs. education	-0.021	-0.019	0.015	0.0085
	[0.061]	[0.061]	[0.072]	[0.072]
Treat × Score Control		✓		✓
Treat × | Beliefs − Truth | Control		✓		✓
Observations	4,951	4,951	4,951	4,951
R-squared	0.133	0.305	0.053	0.233
Dep Var Mean in Treat	13.72		-5.91	
Dep Var Mean in Control	5.456		0.722	

Notes: Standard errors clustered at the household level. Beginning-of-survey 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 5: Treatment effect heterogeneity by parent education

	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		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Sample:	All	Treat Only	All	All	Treat Only	All	All	Treat Only	All	All	Treat Only	All
Treat × Score	0.019***			0.016***			-0.017***			0.0033		
	[0.0016]			[0.0017]			[0.0037]			[0.017]		
Treat × Score × (Parent yrs of educ.)	-0.0012***			-0.00066**			0.00073			-0.00041		
	[0.00027]			[0.00029]			[0.00059]			[0.0029]		
Score	0.0029**	0.021***		0.0061***	0.022***		0.000055	-0.017***		0.033***	0.038***	
	[0.0011]	[0.0012]		[0.0013]	[0.0011]		[0.0026]	[0.0027]		[0.012]	[0.013]	
Score × (Parent yrs of educ.)	0.00078***	-0.00041**		0.00032	-0.00032*		-0.00058	0.00014		0.00047	0.000064	
	[0.00020]	[0.00019]		[0.00022]	[0.00019]		[0.00038]	[0.00045]		[0.0020]	[0.0020]	
Treat	-1.22***		-0.35***	-0.79***		-0.12***	0.30***		0.27***	1.05***		1.12***
	[0.086]		[0.036]	[0.086]		[0.038]	[0.071]		[0.071]	[0.40]		[0.23]
Treat × (Parent yrs of educ.)	0.065***		0.0093	0.023		-0.0013	-0.032***		-0.027**	-0.015		-0.023
	[0.015]		[0.0061]	[0.015]		[0.0065]	[0.012]		[0.012]	[0.069]		[0.040]
Parent yrs of educ.	-0.039***	0.023**	-0.0022	-0.0029	0.019*	0.0097*	0.024***	-0.0083	0.023***	-0.00044	-0.016	0.0090
	[0.011]	[0.010]	[0.0047]	[0.012]	[0.0097]	[0.0053]	[0.0084]	[0.0089]	[0.0084]	[0.050]	[0.047]	[0.029]
Score Used	Math	Math	Math	English	English	English	Math − English	Math − English	Math − English	Higher − Lower Child	Higher − Lower Child	Higher − Lower Child
Observations	5,203	2,588	5,203	5,203	2,588	5,203	5,183	2,575	5,183	2,611	1,299	2,611
R-squared	0.220	0.292	0.184	0.207	0.325	0.179	0.035	0.059	0.014	0.047	0.028	0.047
p-val: Treat × Perf × Educ=0	5.0e-06			0.022			0.222			0.888		

Notes. Robust standard errors in brackets. Standard errors clustered at household level. The first column for each outcome variable shows the heterogeneity by parent education in the information treatment effect on the gradient of the investment function. The second column shows the heterogeneity by parent education in the gradient of the investment function in the treatment group. The third column shows the heterogeneity in the average effect of the treatment. Each observation is a child (cols (1)-(9)) or a household (cols (10)-(12)).
*** p<0.01, ** p<0.05, * p<0.1

Table 6: Information treatment effects: Longer-run outcomes

	Coefficient estimate (standard error) for:						
	A. Heterogeneity in treatment effects by performance − linear spec		B. Heterogeneity in treatment effects by performance − nonparametric spec		C. Ave. treatment effect		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Independent Variable:	Treat	Treat× Score	Treat	Treat× Above-Median Score	Treat	Control group mean	N
Dependent Variables							
Panel A. Dropout and transfer (from endline survey data)							
Dropout	0.055	-0.0011	0.022	-0.037	0.004	0.021	1,786
	[0.021]***	[0.0004]***	[0.012]*	[0.015]***	[0.007]		
Transfer	0.023	0.0002	0.022	0.017	0.03	0.057	1,781
	[0.036]	[0.0007]	[0.019]	[0.025]	[0.014]**		
Panel B. Investments (from endline survey data)							
Total educ. expenditures	119.70	-0.325	100.54	4.181	104.45	2,362.06	1,729
	[291.50]	[6.841]	[177.56]	[230.00]	[164.32]		
ln(Total educ. expenditures)	0.093	-0.0019	0.014	-0.03	0.0013	7.389	1,709
	[0.114]	[0.002]	[0.061]	[0.074]	[0.049]		
Avg. std. effect across other non-monetary investments[a,b]	0.07	-0.0001	0.057	0.015	0.065	-0.012	1,720
	[0.057]	[0.0011]	[0.032]*	[0.039]	[0.026]***		
Avg. std. effect across other chores[c]	0.01	0.001	0.034	0.049	0.058	-0.0009	1,681
	[0.104]	[0.002]	[0.05]	[0.069]	[0.041]		
Panel C. Attendance and grades (from data collected from schools)							
Attendance rate following baseline survey	-0.008	0.0001	-0.0015	-0.0017	-0.002	0.911	1,827
	[0.026]	[0.0005]	[0.012]	[0.015]	[0.008]		
End-of-year grade	0.122	-0.003	0.03	-0.095	-0.016	1.97	1,241
	[0.091]	[0.0019]	[0.047]	[0.07]	[0.036]		

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 7: Treatment effect heterogeneity by parent education: Longer-run outcomes

	Heterogeneity by parent education in:							
	A. Treatment effect heterogeneity by performance − linear spec				B. Ave. treatment effect			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Independent Variable:	Treat	Treat × Score	Treat × Parent Yrs. Educ.	Treat × Score × Parent Yrs. Educ.	Treat	Treat × Parent Yrs. Educ.	Control group mean: Below-median parent educ.	Control group mean: Above-median parent educ.
Dependent Variables								
Panel A. Dropout and transfer (from endline survey data)								
Dropout	0.068 [0.039]*	-0.0015 [0.0007]**	-0.003 [0.005]	0.0001 [0.0001]	0.002 [0.013]	0.0007 [0.0016]	0.033	0.005
Transfer	-0.08 [0.065]	0.002 [0.0013]*	0.024 [0.013]*	-0.0005 [0.0003]*	0.02 [0.025]	0.002 [0.005]	0.056	0.059
Panel B. Investments (from endline survey data)								
Total educ. expenditures	1,027 [562.97]*	-31.04 [14.21]**	-185.69 [125.02]	6.219 [3.471]*	-448.76 [347.71]	122.38 [91.91]	2,089	2,653
ln(Total educ. expenditures)	0.369 [0.203]*	-0.009 [0.004]**	-0.056 [0.037]	0.0014 [0.0007]**	-0.049 [0.097]	0.012 [0.018]	7.293	7.489
Avg. std. effect across other non-monetary investments[a,b]	0.06 [0.097]	-0.0003 [0.0018]	0.004 [0.02]	0 [0.0004]	0.047 [0.045]	0.003 [0.009]	-0.091	0.075
Avg. std. effect across other chores[c]	0.124 [0.17]	-0.0017 [0.003]	-0.024 [0.033]	0.0006 [0.0006]	0.037 [0.073]	0.005 [0.014]	-0.021	0.026
Panel C. Attendance and grades (from data collected from schools)								
Attendance rate following baseline survey	0.08 [0.044]*	-0.0015 [0.0008]*	-0.018 [0.008]**	0.0003 [0.0001]**	0.011 [0.014]	-0.003 [0.003]	0.894	0.927
End-of-year grade	-0.0013 [0.161]	0.0003 [0.004]	0.03 [0.031]	-0.0008 [0.0007]	0.008 [0.059]	-0.005 [0.012]	1.94	1.99

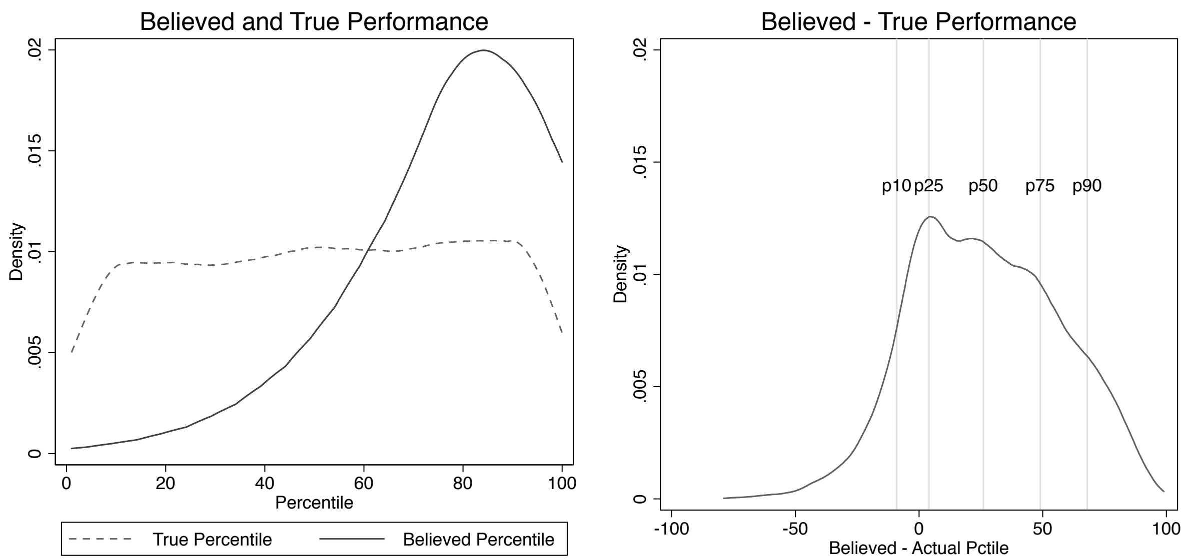
Notes. Data sources are endline survey and endline data collected from schools. Each observation is a child. Standard errors clustered at the household level. All regressions control for child gender, child baseline achievement, grade fixed effects, school fixed effects, and the baseline value of the 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.

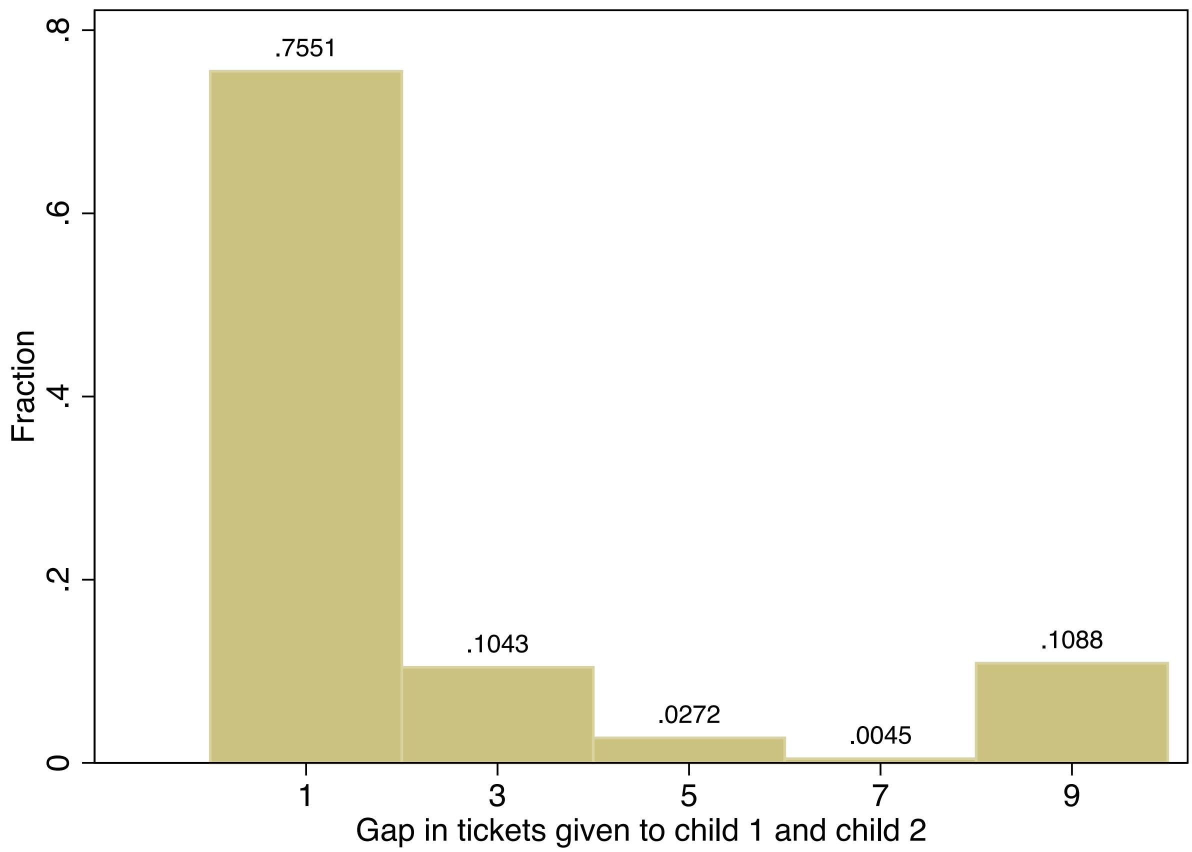
c. Average across 2 chores measures: hours of chores and # times fetched water.

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

	Dependent Variable = Tickets given to child		
	(1)	(2)	(3)
Believed higher-scoring sibling	0.92***	0.86***	0.92***
	[0.15]	[0.15]	[0.15]
Believed score	0.034***	0.030***	0.029***
	[0.0071]	[0.0071]	[0.0073]
Grade level		0.23***	0.21***
		[0.044]	[0.045]
Female			-0.20
			[0.12]
Household FE	✓	✓	✓
Observations	2,640	2,640	2,550
R-squared	0.217	0.234	0.247

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 Performance]			
	(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	-0.031	-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: parent1=parent2	0.490	0.281		

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

Appendix Table 3: Less-educated parents have less accurate beliefs: Finding robust to different measures education and achievement

		Coefficient estimate for:							
		Respondent's				Parent Average			
	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
Dependent Variables									
Panel A. Scores									
Abs Val [Believed − True Overall Score]	20.39	-0.18	-0.831	-2.549	-1.082	-0.197	-0.488	-3.526	-0.996
	[14.47]	[0.063]***	[0.451]*	[0.684]***	[0.491]**	[0.066]***	[0.45]	[0.72]***	[0.593]*
Abs Val [Believed − True Math Score]	25.75	-0.246	-0.816	-4.056	-0.818	-0.274	-0.733	-4.954	-1.192
	[18]	[0.073]***	[0.538]	[0.787]***	[0.582]	[0.079]***	[0.537]	[0.867]***	[0.713]*
Abs Val [Believed − True English Score]	21.35	-0.106	-0.781	-1.328	-0.898	-0.105	-0.326	-1.869	-0.846
	[16.44]	[0.071]	[0.494]	[0.767]*	[0.548]	[0.074]	[0.493]	[0.832]**	[0.649]
Abs Val [Believed − True Chichewa Score]	23.81	-0.248	-1.458	-2.921	-0.725	-0.299	-1.513	-4.204	-0.517
	[17.54]	[0.075]***	[0.537]***	[0.848]***	[0.571]	[0.077]***	[0.534]***	[0.864]***	[0.669]
Abs Val [Believed − True (Math-English) Score]	22.08	-0.061	-0.134	-1.735	0.723	-0.035	-0.482	-1.461	0.574
	[17.4]	[0.072]	[0.526]	[0.754]**	[0.58]	[0.078]	[0.525]	[0.834]*	[0.698]
Abs Val [Believed − True Overall Score (Child1-2)]	18.67	-0.204	-1.944	-0.544	-1.307	-0.244	-1.568	-1.768	-1.717
	[15.13]	[0.084]***	[0.6]***	[0.936]	[0.67]*	[0.091]***	[0.599]***	[0.996]*	[0.814]**
Wrong about which child higher scoring	0.311	-0.007	-0.057	-0.026	-0.016	-0.008	-0.048	-0.054	-0.022
	[0.463]	[0.003]***	[0.018]***	[0.03]	[0.02]	[0.003]***	[0.018]***	[0.032]*	[0.024]
Panel B. Percentiles									
Abs Val [Believed − True Overall Percentile]	32.16	-0.355	-1.99	-4.9	-2.654	-0.396	-1.61	-5.873	-2.78
	[24.03]	[0.098]***	[0.704]***	[1.113]***	[0.749]***	[0.105]***	[0.701]**	[1.155]***	[0.933]***
Abs Val [Believed − True Math Percentile]	33.34	-0.372	-1.928	-5.82	-2.671	-0.413	-1.885	-6.861	-2.848
	[25]	[0.101]***	[0.73]***	[1.109]***	[0.801]***	[0.11]***	[0.726]***	[1.187]***	[0.992]***
Abs Val [Believed − True English Percentile]	30.58	-0.233	-1.514	-2.377	-2.147	-0.292	-1.176	-3.354	-2.493
	[23.35]	[0.097]***	[0.687]**	[1.139]**	[0.73]***	[0.105]***	[0.682]*	[1.221]***	[0.92]***
Abs Val [Believed − True Chichewa Percentile]	33.78	-0.251	-1.014	-3.926	-1.411	-0.293	-0.952	-5.032	-1.512
	[24.72]	[0.101]***	[0.728]	[1.143]***	[0.775]*	[0.109]***	[0.724]	[1.217]***	[0.943]
Abs Val [Believed − True (Math-English) Percentile]	25.66	-0.314	-2.373	-2.059	-1.156	-0.287	-2.183	-2.2	-1.428
	[21.56]	[0.09]***	[0.638]***	[1.002]**	[0.701]*	[0.096]***	[0.637]***	[1.078]**	[0.848]*
Abs Val [Believed − True Overall Percentile (Child1-2)]	32.55	-0.448	-3.525	-3.766	-3.44	-0.473	-2.185	-4.948	-3.234
	[22.74]	[0.125]***	[0.904]***	[1.353]***	[0.99]***	[0.133]***	[0.902]***	[1.444]***	[1.2]***
Wrong about which child higher percentile	0.339	-0.007	-0.064	-0.051	-0.036	-0.008	-0.051	-0.075	-0.027
	[0.473]	[0.003]***	[0.019]***	[0.03]*	[0.02]*	[0.003]***	[0.019]***	[0.032]**	[0.025]
Sample size	5,268	5,230	5,230	5,230	5,230	5,230	5,230	5,230	5,242

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 school

	Dependent Variable= Abs Val [True − Believed Performance]							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Performance Measure:	Overall	Overall	Math	Math	English	English	Chichewa	Chichewa
Child grade	0.754***	1.346***	0.785***	1.548***	0.273	0.634	0.053	0.387
	[0.168]	[0.300]	[0.216]	[0.382]	[0.217]	[0.386]	[0.207]	[0.362]
Yrs. of education × Child grade		-0.124**		-0.163**		-0.074		-0.070
		[0.053]		[0.067]		[0.068]		[0.063]
Household FE	✓	✓	✓	✓	✓	✓	✓	✓
Observations	5,052	5,019	5,054	5,021	5,054	5,021	5,054	5,021
R-squared	0.682	0.682	0.666	0.668	0.592	0.593	0.690	0.689

Notes. Standard errors in brackets. Standard Errors clustered at household level. Years of education is 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 by parent education in overconfidence, uncertainty, and achievement

	Overconfidence			**Uncertainty**			**Achievement**	
Dependent Variable:	Believed - True Score			Std. Dev. of Beliefs about Score			Score	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Secondary Education +	-3.243***	-3.325***	0.596	-4.037***	-3.976***	-3.936***	5.711***	5.842***
	[0.948]	[0.977]	[0.764]	[0.495]	[0.507]	[0.496]	[0.872]	[0.894]
Child and parent controls		✓	✓		✓	✓		✓
Score control			✓			✓		
Observations	5,220	5,019	5,220	5,206	5,007	5,206	5,230	5,029
R-squared	0.002	0.005	0.367	0.013	0.015	0.014	0.009	0.012
Dep. Var. Mean	15.63			7.70			46.72	

Notes. Standard errors in brackets. Standard Errors clustered at household level.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table 6: Heterogeneity in treatment effects by achievement and beliefs

	Math Workbook	English Workbook	Textbook WTP	Lottery	
Dependent Variable:	100 for Advanced, 0 for Average, -100 for Beginner		ln(Textbook WTP) for Math−English	Tickets	
	(1)	(2)	(3)	(4)	(5)
Treat × True score	0.016***	0.017***	-0.015***	0.048***	
	[0.00086]	[0.00086]	[0.0020]	[0.0061]	
Treat × Believed score	-0.015***	-0.015***	0.011***	-0.034***	
	[0.00094]	[0.00082]	[0.0018]	[0.0067]	
True score	0.0016***	0.0013**	-0.000012	0.0019	
	[0.00057]	[0.00059]	[0.0015]	[0.0041]	
Believed score	0.022***	0.023***	-0.021***	0.070***	
	[0.00065]	[0.00055]	[0.0012]	[0.0045]	
Treat	-0.054	-0.0011	0.046		
	[0.064]	[0.048]	[0.041]		
Treat × Higher-scoring Sibling					1.27***
					[0.14]
Treat × Believed Higher-scoring Sib					-0.83***
					[0.14]
Higher-scoring Sibling					-0.016
					[0.091]
Believed Higher-scoring Sibling					1.55***
					[0.091]
Household FE					✓
Observations	5,233	5,233	5,177	5,214	5,212
R-squared	0.374	0.405	0.097	0.209	0.212
P-val: (Treat × True) + (Treat × Beliefs)=0	0.231	0.197	0.098	0.024	
P-val: (Treat × High-Perf Sib) + (Treat × Bel'v'd High-Perf Sib)=0					0.002

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

Appendix Table 7: Persistence of information's effect on beliefs

	(1)	(2)	(3)	(4)
Subject:	Overall	Math	English	Chichewa
Treat × Score	0.283**	0.022	0.039	-0.114**
	[0.139]	[0.049]	[0.053]	[0.045]
Treat	-12.440**	-5.544**	-4.018	2.309
	[5.272]	[2.517]	[2.621]	[2.671]
Score	0.244***	0.188***	0.177***	0.238***
	[0.038]	[0.036]	[0.040]	[0.035]
Observations	1,626	1,627	1,627	1,627
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 8: Detailed skills treatment and uncertainty effects

	Coefficient estimate (std. error) for:				
	A. Ave. Treatment effects	B. Detailed skills treatment effects		C. Uncertainty: Beliefs within 10 pts of truth)	
Independent Variable:	Treat	Treat	Detailed Skills Treatment	Treat	N
Dependent Variables					
Panel A. Experimental outcomes					
Math worbook	-0.313 [0.021]***	-0.304 [0.026]***	-0.017 [0.03]	-0.066 [0.036]*	5,239
English workbook	-0.125 [0.021]***	-0.112 [0.026]***	-0.026 [0.029]	0.023 [0.032]	5,239
ln(WTP for Math - English textbook)	0.14 [0.041]***	0.213 [0.049]***	-0.143 [0.057]	0.07 [0.072]	5,219
Lottery tix given: higher - lower performer	0.984 [0.128]***	0.949 [0.154]***	0.069 [0.178]	0.478 [0.192]***	2,629
Panel B. Longer-run data: Dropout and transfer					
Dropout	0.004 [0.007]	-0.004 [0.008]	0.016 [0.01]	0.004 [0.009]	1,786
Transfer	0.03 [0.014]**	0.045 [0.018]***	-0.029 [0.02]	0.029 [0.024]	1,781
Panel C. Longer-run data: Investments					
Total educ. expenditures	104.45 [164.32]	2.632 [188.74]	192.26 [260.81]	378.10 [274.06]	1,729
ln(Total educ. expenditures)	0.0013 [0.049]	0.031 [0.06]	-0.056 [0.071]	0.07 [0.086]	1,709
Avg. std. effect: non-monetary investments[a,b]	0.065 [0.026]***	0.045 [0.032]	0.037 [0.035]	0.075 [0.04]*	1,720
Avg. std. effect: other chores[c]	0.058 [0.041]	0.037 [0.051]	0.04 [0.059]	0.051 [0.07]	1,681
Panel D. Longer-run data: Attendance and grades					
Attendance rate	-0.002 [0.008]	-0.006 [0.009]	0.007 [0.011]	0.01 [0.013]	1,827
End-of-year grade	-0.016 [0.036]	-0.026 [0.047]	0.02 [0.052]	0.019 [0.068]	1,241

Notes. Data sources are baseline survey, endline survey and endline data collected from schools. 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 - c. See notes for Tbl **??**.

Appendix Table 9: Correlations between beliefs and endline survey outcomes (Control group only)

Independent Variable:	Baseline beliefs	Avg. base-line/endline	Endline beliefs	Control group mean	N
Dependent Variables					
Panel A. Dropout and transfers (from endline survey data)					
Dropout	0.0002	-0.0001	-0.0001	0.021	776
	[0.0003]	[0.0001]	[0.0001]		
Transfer	-0.0004	0.0002	-0.0002	0.057	773
	[0.0005]	[0.0005]	[0.0004]		
Panel B. Education Expenditures (from endline survey data)					
Total educ. expenditures	-3.78	5.976	10.28	2,315	752
	[4.551]	[6.018]	[5.135]**		
ln(Total educ. expenditures)	-0.0002	0.003	0.005	7.386	747
	[0.002]	[0.003]	[0.0019]**		
Expenditures on school fees	-0.176	1.113	1.408	426.74	752
	[0.813]	[0.989]	[0.838]*		
Supplementary educ. expenditures	-3.355	5.339	8.983	1,859	752
	[4.303]	[5.715]	[4.68]*		
Books and school supplies	0.009	2.356	2.507	600.31	752
	[1.118]	[1.439]	[1.178]**		
Uniforms	-3.084	-1.465	1.748	779.31	752
	[2.179]	[2.694]	[2.043]		
Backpacks	0.473	1.103	1.164	174.44	752
	[0.838]	[0.966]	[0.858]		
Tutoring	-0.705	4.779	4.761	268.62	752
	[2.896]	[3.659]	[2.518]*		
Panel C. Non-monetary investments (from endline survey data)					
Helped child with homework	0.002	0.002	0.0004	0.376	744
	[0.0011]*	[0.0014]	[0.0011]		
Asked someone to help child with homework	0.0002	-0.0016	-0.0019	0.245	748
	[0.001]	[0.0012]	[0.001]*		
# times gave child light source to study at night over last 4 weeks	0.02	0.071	0.062	2.624	734
	[0.016]	[0.021]***	[0.015]***		
# times child went to school without food or water in last 4 weeks	-0.004	-0.004	-0.0001	10.67	733
	[0.021]	[0.026]	[0.02]		
Has to push child to attend school regularly	-0.003	-0.003	-0.0015	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
	[0.019]	[0.023]	[0.019]		
# times instructed child to work on homework in last 4 weeks	-0.015	0.0014	0.015	1.982	734
	[0.009]*	[0.011]	[0.011]		
Ave. std. effect across other investments[b]	0.0001	0.0014	0.0015	-0.009	752
	[0.001]	[0.0013]	[0.001]		
Panel D. Chores (from endline survey data)					
Hours of chores given to child over last 4 weeks	0.087	0.12	0.058	23.81	732
	[0.047]*	[0.068]*	[0.055]		
# times child fetched water in last 4 weeks	-0.008	0.003	0.012	4.672	734
	[0.018]	[0.023]	[0.018]		
Ave. std. effect across chores[c]	0.0011	0.003	0.002	0	734
	[0.0019]	[0.003]	[0.0019]		
Panel E. Attendance and grades (from data collected from schools)					
Attendance rate following baseline survey	0.0012	0.0007	0.0002	0.911	916
	[0.0004]***	[0.0006]	[0.0004]		
Repeated a grade	-0.006	-0.008	-0.002	0.275	2,193
	[0.0006]***	[0.0013]***	[0.001]**		
End-of-year grade	0.017	0.02	0.01	1.97	637
	[0.002]***	[0.005]***	[0.004]**		

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, and missing if 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.

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

Appendix Table 10: Effect of Information on Endline Outcomes (Detailed data)

	A. Heterogeneity in treatment effects by performance – linear spec		B. Heterogeneity in treatment effects by performance – nonparametric spec		C. Avg. treatment effect		
Independent Variable:	Treat	Treat	Treat× Score	Treat	Treat× Above-Median Score	Control group mean	N
Dependent Variables							
Panel A. Dropout and transfers (from endline survey data)							
Dropout	0.055 [0.021]***	-0.0011 [0.0004]***	0.022 [0.012]*	-0.037 [0.015]***	0.004 [0.007]	0.021	1,786
Transfer	0.023 [0.036]	0.0002 [0.0007]	0.022 [0.019]	0.017 [0.025]	0.03 [0.014]**	0.057	1,781
Panel B. Education Expenditures (from endline survey data)							
Total educ. expenditures	119.70 [291.50]	-0.325 [6.841]	100.54 [177.56]	4.181 [230.00]	104.45 [164.32]	2,362.06	1,729
ln(Total educ. expenditures)	0.093 [0.114]	-0.0019 [0.002]	0.014 [0.061]	-0.03 [0.074]	0.0013 [0.049]	7.389	1,709
Expenditures on school fees	125.67 [63.26]**	-2.924 [1.612]*	-3.618 [27.18]	-16.15 [47.65]	-11.26 [30.82]	452.53	1,729
Supplementary educ. expenditures	-67.75 [275.83]	3.623 [6.44]	79.80 [172.71]	43.81 [217.88]	101.94 [157.24]	1,902.92	1,729
Books and school supplies	105.10 [94.08]	-0.959 [1.803]	85.90 [59.99]	-52.65 [63.27]	60.20 [57.42]	617.64	1,729
Uniforms	-93.89 [138.75]	2.737 [2.5]	-25.07 [87.79]	118.63 [100.21]	34.34 [70.23]	806.40	1,729
Backpacks	7.431 [50.97]	0.691 [1.099]	43.20 [31.53]	-6.653 [36.16]	39.77 [27.08]	178.61	1,729
Tutoring	0.771 [158.37]	-0.794 [4.308]	-1.68 [83.13]	-71.15 [151.06]	-36.40 [88.82]	300.27	1,729
Panel C. Non-monetary investments (from endline survey data)							
Helped child with homework	-0.034 [0.063]	0.0001 [0.0012]	-0.046 [0.034]	0.033 [0.042]	-0.03 [0.028]	0.374	1,699
Asked someone to help child with homework	0.1 [0.064]	-0.001 [0.0013]	0.07 [0.033]**	-0.033 [0.042]	0.055 [0.027]**	0.243	1,710
# times gave child light source to study at night over last 4 weeks	0.207 [0.889]	0.005 [0.018]	0.274 [0.481]	0.316 [0.616]	0.425 [0.402]	2.61	1,674
# times child went to school without food or water in last 4 weeks	-2.374 [1.198]**	0.019 [0.022]	-1.778 [0.671]***	0.654 [0.772]	-1.461 [0.543]***	10.68	1,677
Has to push child to attend school regularly	0.028 [0.062]	0.0008 [0.0012]	0.059 [0.034]*	0.017 [0.041]	0.067 [0.026]***	0.341	1,666
# times monitored child's exercise books in last 4 weeks	-1.352 [1.133]	0.005 [0.022]	-1.12 [0.613]*	0.002 [0.735]	-1.132 [0.486]**	8.458	1,681
# times instructed child to work on homework in last 4 weeks	0.819 [0.472]*	-0.005 [0.009]	0.447 [0.29]	0.224 [0.345]	0.559 [0.249]**	1.972	1,669
Ave. std. effect across other investments[b]	0.07 [0.057]	-0.0001 [0.0011]	0.057 [0.032]*	0.015 [0.039]	0.065 [0.026]***	-0.012	1,720
Panel D. Chores (from endline survey data)							
Hours of chores given to child over last 4 weeks	0.546 [2.936]	0.029 [0.066]	1.43 [1.385]	1.008 [2.182]	1.905 [1.325]	23.81	1,676
# times child fetched water in last 4 weeks	0.155 [1.016]	0.003 [0.02]	0.061 [0.512]	0.42 [0.619]	0.273 [0.37]	4.656	1,671
Ave. std. effect across chores[c]	0.01 [0.104]	0.001 [0.002]	0.034 [0.05]	0.049 [0.069]	0.058 [0.041]	-0.0009	1,681
Panel E. Attendance and grades (from data collected from schools)							
Attendance rate following baseline survey	-0.008 [0.026]	0.0001 [0.0005]	-0.0015 [0.012]	-0.0017 [0.015]	-0.002 [0.008]	0.911	1,827
End-of-year grade	0.122 [0.091]	-0.003 [0.0019]	0.03 [0.047]	-0.095 [0.07]	-0.016 [0.036]	1.97	1,241

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.

Appendix Table 11: Transfer results: Heterogeneity by school type

	Dependent Variable = Transferred		
	(1)	(2)	(3)
Treat	0.030**	0.023	-0.018
	[0.014]	[0.036]	[0.040]
Treat × High-achievement school			0.20**
			[0.098]
Treat × Score		0.00015	0.0012
		[0.00071]	[0.00083]
Treat × Score × 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 in Control	0.06		
p-val: (Treat × Score)=0			
p-val: (Treat × Score) + (Treat × Score × High ach.)=0			0.041

Notes: Standard errors clustered at the 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 BDM Methodology

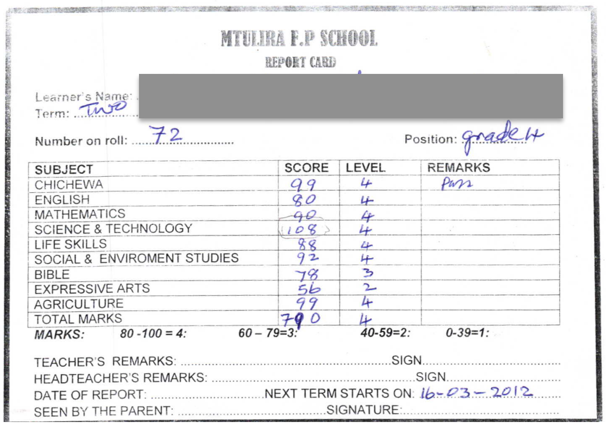
Sample price list

Surveyor: For each row, say: "At the end of the interview, if the randomly selected textbook is the **math** 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

Description of methodology

Surveyors began by reading a description of how the BDM methodology would work and doing a short demo. Extensive pretesting was conducted to ensure that all parents would understand this introductions. Surveyors then read parents a list of prices for the textbook. 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 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.

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	

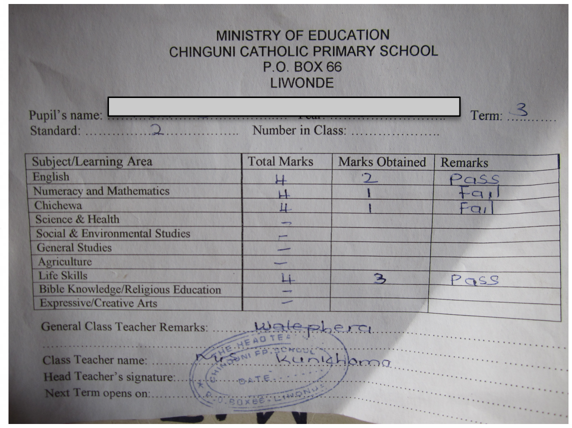
MARKS: 80 -100 = 4: 60 – 79=3: 40-59=2: 0-39=1:

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: Mr Kunkhoma

Head Teacher's signature: ....

Next Term opens on: ....

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