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' intended and actual educational investments. Providing parents with information significantly impacts their investments, causing them to become more closely aligned with their children's achievement. Poorer, less-educated parents have less accurate perceptions about their children's academic abilities than richer, more-educated parents, and update their beliefs more in response to improved information. Inaccurate perceptions may thus exacerbate inequalities in educational outcomes between richer and poorer families.

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1 Introduction

Many models for optimal human capital investment 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 has average performance in his verbal classes, but is far behind his class in math. Perhaps the parent could not afford tutoring for her son in all subjects, but could afford it for one subject. If she were aware of her son's poor math skills, she would know that investing in math tutoring would be valuable; without this information, she does not send her son to math tutoring, and his performance continues to lag. Since parents' investments represent a major determinant of educational outcomes (e.g., Houtenville and Conway (2008), Todd and Wolpin (2007)), these types of inefficiencies could have important, negative impacts on children's educational outcomes.

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

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

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

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

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

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

The second finding is that, across a range of investments, providing parents with in-

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

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

formation causes them to reallocate their educational investments. To investigate whether inaccurate perceptions prevent parents from choosing the right mix of complementary inputs to their children's education, the experiment examines two investments where there is a relatively clear prediction for how the returns (and thus the efficient investment) vary with student achievement: willingness-to-pay (WTP) for remedial math and English textbooks, which should decrease with achievement because they are remedial (i.e., perceived substitutes with achievement), and choices among three, free subject-specific workbooks that are targeted for a child's performance level (remedial, average, and advanced).[4] 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. I find that, at baseline, parents allocated more tickets to the child they believed was higher-achieving, but that, because many parents had mistaken beliefs about which of their own children had higher achievement, information caused parents to reallocate tickets towards the child that was truly higher-achieving.

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

[4]Although both of these investments get at the same theoretical idea, there are several reasons to use both. The primary advantage of using 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 (vs. the workbooks which are only a choice between free items), the treatment effects for textbooks could be damped 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. It could also have limited statistical power. These were the original design justifications for looking at both investments simultaneously.

children's achievement (and parents' investments follow those predictions) implies that the negative impacts could be large.

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

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

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

As a second example, information affected dropouts: children whose parents had found out that their children were doing well in school were less likely to drop out, while children whose parents who had found out that their children were doing poorly were more likely to drop out. This suggests that the parents used the information to try to optimize their investments. Whether it led to a true increase in efficiency is an open question; however, the

fact that the literature has generally suggested that education and ability are complements (Pitt et al., 1990; Aizer and Cunha, 2012) suggests it may have. It is important to note that these results also highlight that parents will use information in the way they see best, and so information is not a universal panacea for increasing education for everyone. When considering whether to increase information, policymakers need to trade off potential increases in efficiency with potential decreases in equity.

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

This paper also contributes to several other strands of the literature. First, it contributes to the literature on how investments in education depend on returns. Several papers have examined whether individual investments in education respond to changes in the population-average returns to education (e.g., Abramitzky and Lavy, 2011; Freeman, 1976; Jensen, 2010; Kane, 1994; Nguyen, 2008; Oster and Steinberg, 2013), primarily finding that investments do respond to changes in returns. This paper builds on this work by showing that investments also respond to shocks to (perceived) individual-level returns. Second, it contributes to a large literature examining the role of market failures in education. Within that literature, it builds on a recent and growing stream examining the idea that it is perceptions, not reality, that drive educational investments (Banerjee et al., 2011). For example, Jensen (2010) shows that providing households with the information on the earnings differences between more and less educated people increases average educational outcomes, suggesting that there may be inaccurate information at baseline. Finally, it contributes to a long literature that has examined how parents' investments depend on their children's ability (e.g., Behrman et al., 1994; Griliches, 1979). The relationship is difficult to estimate empirically, most notably because of reverse causality– since investments likely increase measured ability, one cannot distinguish whether high ability is a cause or effect of high investment. The recent

literature has primarily used within-family comparisons and early-life ability measures, such as birthweight, to try to avoid the reverse causality concerns (e.g., Datar et al., 2010). However, this approach may not fully solve the identification problem, since it is difficult to find early-life measures that are not influenced by neonatal investments. This paper uses a within-person methodology that exploits the exogenous "shock" provided by the experiment to randomly selected parents' beliefs about their children's achievement (i.e., actual achievement relative to ex ante beliefs). By looking at how a parent's investments respond to finding out their children's achievement was lower or higher than expected, the analysis provides suggestive evidence on how each investment measured in the paper depends on children's achievement.

The remainder of the paper proceeds as follows. Section 2 describes the context and experimental design. Section 3 presents a simple conceptual framework. In section 4, I use the baseline data to examine whether parents have inaccurate perceptions and how that impacts their investments. Section 5 presents the results on the impact of information on shorter-run outcomes. Section 6 examines heterogeneity by parent education. Section 7 looks at longer-run outcomes. In Section 8, I conclude.

2 Context and Experimental Design

2.1 Context

Education System

Primary school in Malawi covers grades 1-8. Primary school has technically been free in Malawi since 1994, but it does involve other expenditures. Parents in the study sample spent an average of 1750 Malawi Kwacha (MWK) or roughly 10.6 USD annually per child per year in the study sample, 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 only 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 - 35,000 MWK per year (30 - 210 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).[5] We started by conducting a sibling census during January - March of 2012, mapping out the sibling structures for all students enrolled in grades 2-6 at the schools. During March and April, 2012, we also gathered the term 2 achievement test data from the schools for all the students enrolled in grades 2-6. Test data 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

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

report cards and all of the analyses that follow, I use the Malawian Ministry of Education's grading guidelines to create weighted averages, where the weights are 40%/60% (grades 5-6), 60%/40% (grades 3-4), and 100%/0% (grade 2) for continuous assessments and terminal exams, respectively.[6] On average, students in the sample took 4.5 tests over the course of 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.

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

Randomization

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.[8] Of the 2,716 sampled and eligible households, 97% (2,634 households) were found,

[6]If a class only offered continuous assessments (or terminal exams), the score used is 100% continuous assessments (or terminal exams). All continuous assessmentss 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.

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

[8]The most common cause of ineligibility was that both sampled children did not live in the same household. Eligibility for the initial sample was based on children's reports, and so ineligibility resulted from misreports

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.

For parents in the treatment group only, the surveyor then walked the parents through a report card that contained information about their own children's recent achievement test performance. The report card contained information about each student's absolute and relative performance on recent tests (See Appendix A for a sample report card), specifically, the child's absolute percent score, the absolute grade that that score corresponded to using the standard Malawian grading scale, and the child's "position rank" within their class-level distribution (equal to 100 minus their percentile rank, a statistic which is easier for parents in this environment to understand than percentiles given a long history of position rankings in schools). These statistics were included for the three subjects Malawian educators deem most important (Math, English, and Chichewa, the local language), as well as for their "overall" performance (the average across those three subjects). The report card also told parents how many achievement tests were included in the averages displayed on the report card. Parents received the report card information for both sampled children in the family.

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

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

Within the treatment group, the parents randomly assigned to the "detailed skills" treatment group also received an additional report card that discussed the child's performance on a series of 6 skills (2 math, 2 English, and 2 local language) chosen by local teachers

by the children. There were also 18 households that were never successfully tracked; those are counted here as eligible.

as important skills for children in that grade to master (See Appendix B for sample). The grades displayed on the report card were assigned by the student's teachers. The format and grading scale for this report card were also chosen through qualitative interviews and focus groups. The point of this intervention was to see whether providing more details could help uneducated parents to become more engaged with their children's education.

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

2.3 Baseline Characteristics of Study Sample

Table 1 presents baseline sample characteristics and tests for balance across the treatment and control groups. 77% of respondents are female, and 92% of respondents were the primary decision maker about education in the household.[9] The level of education among parents is very low: the average years of education across parents in sample households is 4.7.46% of 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). 51% of the children are 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, just to ensure this is not affecting the estimates, unless otherwise mentioned, I control for a student achievement measure in all of the regression results. Reassuringly, despite the lack of balance in performance, the accuracy of parents' perceptions (i.e., the absolute value of the gap between their beliefs and their children's scores) are balanced across treatment and control groups.

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

2.4 Measurement of Child Investment Outcomes

Remedial Textbooks and Level-Specific Workbooks

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

For the textbooks, parents' willingness to pay (WTP) for subject-specific textbooks in Math and English was measured. The textbooks are remedial (i.e., a perceived substitute with child achievement in this environment),[10] and so the prediction 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 generally tailored correctly to their children's performance.

Secondary School Lottery

The workbooks and textbook investments will show us whether information enables parents to optimize their investments on the margin. To understand all the ways that information affects human capital development, we also want to know whether information can affect parents' larger investments.

To gauge this, since secondary schooling is one of the first high-cost, high-returns investments that parents make, I measured a short-run, real-stakes proxy for parents' secondary

[10]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.

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. Parents were given nine tickets for the lottery and chose how they would allocate those tickets across their two sampled children.[11] There are two primary ways that student achievement might affect the expected return of a lottery ticket. First, perceived complementarity (substitutability) between performance and education would cause the perceived earnings return of a ticket to increase (decrease) with student performance. 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; 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.

2.5 Data

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

Baseline Survey Data

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

[11] 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).

this is *not* the same measure that the treatment group received information about.)[12]

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.[13] 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 with a subset of the parents in the sample approximately one year after the baseline data was gathered (during June-July 2013).

3 Conceptual Framework

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

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 returns to investment (e.g., earnings); A is the child's achievement level; s^c, the first educational input, is a complement with achievement $\left(\frac{\partial^2 q}{\partial s^c \partial A} > 0\right)$; s^s, the second educational input, is a substitute with

[12] 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 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.

[13] 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.

achievement $\left(\frac{\partial^2 q}{\partial s^s \partial A} < 0\right)$; and y^{educ} is the total budget for education. 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.[14] For example, s^s could be a remedial textbook which is more valuable for children who are not performing well in school, and s^c could be a candle to study alone at night, which is more useful for children who have a better understanding of the material.

Under standard assumptions (e.g., concave returns function), this problem will yield unique, returns-maximizing choice functions for educational inputs, $s^{c*}(A)$ and $s^{s*}(A)$, with the complement increasing in a child's achievement, $\frac{\partial s^{c*}}{\partial A} > 0$, and the substitute decreasing, $\frac{\partial s^{s*}}{\partial A} < 0$. For simplicity, we can parametrize these relationships as $s^{c*}(A) = \beta_0^c + \beta_1^c A$ and $s^{s*}(A) = \beta_0^s + \beta_1^s A$, with $\beta_1^c > 0, \beta_1^s < 0$. (To simplify notation, I will now refer to both investments as $s^*(A)$ and both slopes as β_1.)

Assume that the parent does not know her child's true achievement, A; instead, she has a belief about her child's achievement, $\tilde{A}$. Instead of choosing the returns-maximizing investments, $s^*(A) = \beta_0 + \beta_1 A$, the 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 incorrect, she would earn inefficiently low returns: $q(s^*(\tilde{A})|A) \leq q(s^*(A)|A)$. Note that this assumes that the parent knows the true production function, q.[15]

Testing for distortions

Inaccurate beliefs decrease returns by distorting investments away from parents' desired investment functions. Gauging whether there are distortions in the investment functions can thus help us evaluate whether inaccurate beliefs cause inefficiently low returns even if we do not have long-run returns measures. Figure 1 demonstrates how to do this. To determine the optimal (intended) investment function, one can look at how investments depend on perceived achievement (the dotted lines). These lines should be steeply sloped with slope β_1. These lines represent how parents *want* to invest. To determine the actual investment

[14] 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.

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

function, one would 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). These lines shows us how parents *actually* invest.

Prediction 1. *If inaccurate beliefs cause distortions in parents' investments, then the investment gradient on true achievement will be more flat than the investment gradient on perceived achievement, with the difference in slopes being* $\beta_1(1 - \frac{cov(\tilde{A},A)}{var(A)})$.

To see this, note that this is analogous to the standard measurement error setup,[16] and so the investment relationship can be written as follows:

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

where $\nu = \beta_1(\tilde{A} - A)$ and thus $cov(A, \nu) \neq 0$. Using the standard omitted variable bias formula, the slope of the actual investment function line will be $\beta_1 \frac{cov(\tilde{A},A)}{var(A)}$. Although the magnitude will obviously depend on the exact relationship between $\tilde{A}$ and A, under most reasonable processes for belief formation, this would result in an attenuated slope ($|\beta_1 \frac{cov(\tilde{A},A)}{var(A)}| \in \{0, |\beta_1|\}$).[17]

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 (ε_i) and consider comparing the slopes estimated from the following two regressions:

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

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

[16] 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 themself is using the mis-measured regressor, and so the bias affects the true investment function.

[17] One can see this by rewriting $\frac{cov(\tilde{A},A)}{var(A)} = corr(\tilde{A}, A)\frac{std(\tilde{A})}{std(A)}$. $corr(\tilde{A}, A)$ is bounded above by 1 and should be non-negative. $\frac{std(\tilde{A})}{std(A)}$ measures the relative concentration of the distributions; it could theoretically be more than 1, but, under most reasonable processes by which one might expect beliefs to form (e.g., if beliefs were concentrated towards the median, or if parents were more over-confident for lower-ability children), we would expect it to be less than one. In the sample, the ratio is 0.94.

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 $\beta_1 \frac{cov(\tilde{A},A)}{var(A)} + \frac{cov(A,\varepsilon)}{var(A)}$. Thus, the difference in slopes will be $\beta_1 \left(1 - \frac{cov(\tilde{A},A)}{var(A)}\right) + \left(\frac{cov(\tilde{A},\varepsilon)}{var(\tilde{A})} - \frac{cov(A,\varepsilon)}{var(A)}\right)$ and so will only give us a good measure of distortions due to inaccurate beliefs 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. Estimating regression 2 with parents who have received the intervention (treatment parents) will now identify $\beta_1 + \frac{cov(A,\varepsilon)}{var(A)}$, whereas estimating it with parents who have not received the intervention (control parents) will identify $\beta_1 \frac{cov(\tilde{A},A)}{var(A)} + \frac{cov(A,\varepsilon)}{var(A)}$. 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 $\beta_1 \left(1 - \frac{cov(\tilde{A},A)}{var(A)}\right)$.[18] This leads to the following testable prediction:

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

The intuition is that information allows parents to correct their baseline mistakes and invest along their intended 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 slope 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 optimal investment function (β) does not vary by group.

Extensions to the model

The above model assumes that parents' investments as a function of their perceptions, $\tilde{A}$ have the same slope, β_1, as their investments as a function of the truth, A. In reality, it

[18] 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 γ the weight on A, then the difference in slopes would uncover $\gamma\beta_1(1 - \frac{cov(\tilde{A},A)}{var(A)})$.

is possible that the slopes could differ; for example, because of uncertainty, the slope of investments as a function of perceptions (which I will denote by β_1^P) could be shallower than β_1 (i.e., $Abs\{\beta_1^P\} < Abs\{\beta_1\}$). As a result, comparing the slope between the two lines would now estimate $Abs.Val\{\beta_1(1 - \frac{\beta_1^P}{\beta_1}\frac{cov(\tilde{A},A)}{var(A)})\}$.To assess whether $\beta_1^P = \beta_1$, one can test whether the information treatment effect on the slope of investments on achievement is equal and opposite to the information treatment effect on the slope of investments on perceptions.[19]

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.[20] The left figure in Figure 2 shows kernel density plots of the parents' beliefs about their children's overall test scores (the solid line) and their child's true test scores (the dashed line). Scores are absolute scores expressed on a scale from 1 to 100.[21] Parents are overconfident: the mean of the believed achievement distribution is 16 points (or 0.9 of a standard deviation of the achievement distribution) higher than the mean of the true distribution. Beyond simple overconfidence, however, parents also have a range of misinformation about their children's achievement: the right figure plots a kernel density plot of each individual parent's beliefs relative to their child's true achievement. If parents were simply overconfident by, say, 5-10 points, the plot would have all of its density between

[19]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 $\tau(A_i, \tilde{A}_i) = s(A_i) - s^P(\tilde{A}_i) = \beta_0 + \beta_1 A_i + \varepsilon_i - (\beta_0^P + \beta_1^P\tilde{A}_i + \varepsilon_i) = (\beta_0 - \beta_0^P) + \beta_1 A_i - \beta_1^P\tilde{A}_i$. Thus, heterogeneity in the treatment effect by A will identify β_1 and heterogeneity by $\tilde{A}$ will identify $-\beta_1^P$.

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

[21]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.

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 performance, with similar magnitudes for the gaps within specific subjects (Table 1). This corresponds to correlations of 0.34 between believed and true achievement for overall, and 0.23-0.29 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: 0.80 for overall (average), and 0.57 - 0.66 within subjects. The measures delivered were then the averages across 4.5 tests per student (on average), so should have even higher reliability. This suggests that the disconnect between beliefs and true test performance is not simply the result of noise. 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 3 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. 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.)[22] As expected since workbook difficulty is a perceived comple-

[22]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.

ment 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) because it has more clear predictions for behavior than within-subject WTP: between-subject WTP holds constant parents' other investments in education, allowing us to isolate whether they spend relatively more in the subject in which they think their child is underperforming.[23] The y-axis shows the log of WTP for the remedial math textbook minus the log of WTP for the remedial English textbook.[24] The x-axis shows the parents' beliefs about the child's performance in math relative to English. 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 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.[25]

Secondary School Lottery

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

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

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

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

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 3 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.[26] 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 that parents' decisions are constrained by inequality aversion. Note that the facts 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. So, I compare the slope of the dashed lines in Figure 3 with the slope of the solid lines, which plot parents' investments against their children's true achievement. For the workbooks and textbooks, the solid lines thus have the same y-axis as the dashed lines, but a different x-axis. 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-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 more flat than the dotted line, and everywhere below it, with the difference between lines statistically significant everywhere except for near zero and large

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

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.[27] Although it is reassuring that the slopes conform to the causal predictions laid out in Section 3, which omitted variables would not be expected to do in general, firmer evidence is needed. As described in section 3, we can use the information experiment results to distinguish whether the difference is causal.

5 Impact of information

5.1 Effect of information on beliefs

The information experiment only has power to affect investments if it changes parents' beliefs. Figure 4 compares the absolute value of the difference between each child's term 2 achievement and her parent's beliefs, using either the parent's 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). In the control group, the beginning-of-survey and end-of-survey beliefs are similarly far from children's true achievement (just 0.9 points difference relative to a base of 20.4). 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.

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

(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 5 shows local linear regression plots of parents' investments on the y-axis against their children's true achievement on the x-axis. The solid line represents the control group (so is the same line from Figure 4); the dashed line represents the treatment group.

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

Regression Evidence: Textbooks and Workbooks

I now perform a formal test of whether the information treatment changed the slope of the investment functions (Prediction 2) by running the following regression:

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

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.[28] Since each household has multiple observations (one for

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

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 the extent to which perceptions inaccuracy distorts 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.

Regression Evidence: Secondary School Lottery

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

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

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

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 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

robust to controlling for stratum FE.

to their higher-scoring sibling compared (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.[29]

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 (6) of panels A-C and (3) - (6) of panel D provide suggestive evidence that omitted variables do not drive the result by showing that the results are robust to controlling for household fixed effects (panels A-C only – all specifications already include household fixed effects in panel D) and to controlling for interactions of individual-level control variables with treatment.[30,31]

Additional treatment effects analysis: Lottery

One reason that parents might split their tickets so evenly between their children at baseline is that they are unsure of which child would be the better investment; another reason is that they are averse to investing unequally in their children. I provide some evidence on this question by regressing the absolute value of the gap between the tickets given to one sibling vs the other on $Treat_i$ (Results not shown). Since we already saw that the information

[29]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.

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

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

intervention affected beliefs, if uncertainty were the primary driver, we would expect parents in the treatment group to split their tickets less equally than parents in the control group. I find that the treatment only increased the gap by 0.14 tickets on average, with the p-value for the difference only 0.17. This is equivalent to 73.5% of parents splitting their tickets as evenly as possible in the treatment group relative to 75.3% in the control group, so the differences are not large. Thus, although uncertainty may play some role in the equal outcomes, it does not seem to be a primary factor: inequality aversion likely plays a large role.

It is also natural to wonder how information will affect distribution of tickets along other dimensions that may be correlated with performance and/or perceptions. Given the 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).[32]

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 completed any secondary education or higher, averaged across parents in the household.[33] The

[32]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).

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

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), 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. Table 4 shows that this type of heterogeneity does not explain the heterogeneity by parent education by estimating following regression:

$$\tilde{A}_{ij} = b_0 + b_1 A_{ij} + b_2 Educ_i + b_3 A_{ij} \times Educ_i + \varepsilon_{ij} \tag{6}$$

where $\tilde{A}_{ij}$ represents parent i's beliefs about child j's achievement, A_{ij} represents the child's true achievement, and $Educ_i$ represents the education of parent i. Table 4 shows that $b_3 > 0$, that is, that true achievement is more predictive of the beliefs of more-educated parents than less-educated parents.[34]

6.2 Updating

Table 5 looks at whether less-educated parents change their beliefs more than more-educated parents in response to the information treatment. Column (1) replicates the finding (shown previously in Figure 4) that the information treatment affected beliefs by regressing the absolute value of the difference between a respondent's beginning-of-survey beliefs and end-of-survey beliefs about overall achievement on a dummy for treatment. (Recall that beginning-of-survey beliefs were about term 2 achievement whereas the end-of-survey beliefs were about

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.

[34]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.

a slightly different metric – how the child would perform on an achievement test taken that day – but that the difference can still proxy for the change in beliefs.) Column (2) shows that there is heterogeneity in the treatment effect by parent education: less-educated parents have a larger treatment effect on updating than more-educated parents, with every additional year of education decreasing the amount by which beliefs change by 0.37 score points (statistically significant at the 1% level). This means that going from no-education to completing primary school decreases the treatment effect on updating by roughly 3 score points, which, at 54% of the mean level of updating in the control group, is a sizable difference. Column (3) shows that the result is not due to child achievement levels differing by parent education, as it is robust to controlling for the interaction of treatment with child achievement. Part of the reason that less-educated parents update more is that they have less accurate baseline perceptions, but column (4) shows that that is not the full story. The specification controls for the interaction between treatment and baseline belief accuracy (i.e., the absolute value of the gap between initial beliefs and the truth); even in this specification, the heterogeneity by parent education remains sizable (just decreasing from 0.32 to 0.29) and is still significant at the 1% level. This is consistent with less-educated parents having more uncertain baseline beliefs and so having a larger bayesian updating parameter.[35]

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 efficient investment function (β_1 from the conceptual framework) could also vary by parent background. This is likely to be the case for textbooks, for which credit constraints could change how much parents spend and respond. The workbooks should hold these other factors the most constant since there are no credit constraints (it is a free choice) and since beliefs about how optimal choice changes with ability should be relatively uniform, but even there there could be heterogeneity.

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

Workbooks 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

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

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

Turning to the textbooks in Table 6, the point estimate indicates that the investment gradient of less-educated parents changed more than those of more-educated parents (coefficient on $Score \times Treat \times ParentYrsofEduc$), but precision is low and the difference is not statistically significant. The modest heterogeneity could reflect credit constraints dampening the optimal investment function of poorer parents.

Lottery

The lottery graphs (panel (c) of Figure 6) again look relatively similar for less-educated and more-educated parents. Table 6 shows that the treatment effects are smaller for more-educated parents, but 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 intended 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 year.[36] 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.6 verifies that the endline beliefs of treatment parents correspond more tightly with their children's past achievement than the endline beliefs of control parents by regressing parents' beliefs at endline on their children's true past achievement (from term 2 of 2011-2012), $Treat$, and an interaction of past achievement with $Treat$. For beliefs about overall, math, and english achievement, one can see that the beliefs of parents in the treatment group are shifted downwards (coefficient on $Treat$ is negative) and more steeply related to past achievement (coefficient on $Treat \times Score$ is positive), although the relationship is much stronger and only statistically significant for overall. The overall information may have been more salient to parents.[37]

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

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

[37]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.

in the control group were not guided by their beliefs when making their investment decisions, but rather that I do not have good enough measurement of parents' contemporaneous beliefs while making a given decision to generate meaningful predictions.

7.2 Information Treatment Effects: Endline Data

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

Table 7 tests for how information affected investments measured after the initial baseline survey and whether information changed the slope of the investment function. Column (1) tests for an average treatment effect; columns (2) and (3) present coefficients on $Treat$ and $Treat \times A$ from estimation of regression 3, and columns (4) and (5) present an alternative specification where the outcome variable is regressed on $Treat$ and $Treat \times 1\{AboveMedianA\}$ (where $1\{AboveMedianA\}$ is an indicator for having above-median achievement) for ease of interpretation. All regressions use the child's overall achievement on the term 2 2011-2012 achievement exams (the same measure used for earlier regressions) as the measure of A.

On the extensive margin (dropouts), there was no statistically significant increase in dropouts in the treatment group. However, high-achieving students in the treatment group were less likely to have dropped out of school, while low-achieving students were more likely, which is what we would predict given that most parents believe that education and achievement are complements. The change in the gradient of the investment line is significant at the 1% level. In terms of magnitudes, dropout fell by 1.5 percentage points for students who were above-median achievement and increased by 2.2 percentage points for students who were below (cols (4) and (5)). These are large effects relative to the control group mean (2%).[38,39]

On the intensive margin, first, there was an average effect on transfers (Panel E): parents

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

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

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

There was no significant effect on overall expenditures on education (either an average effect or an effect on the gradient of investments), attendance, or chores.[40]

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.[41] Surprisingly, however, there is no heterogeneity in this overall effect by child performance. There are several potential explanations for this. First, it could represent an uncertainty effect or an effect on parents' engagement with/empowerment about their children's education. Second, it could represent a Hawthorne effect, although both the treatment and control parents were aware that the study team was conducting an education study with them and their schools. Finally, there could be heterogeneity in some of the investments, but I could lack the statistical power to detect it, and/or the index measure mixes complements with substitutes.[42]

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

[41] 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.

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

Discussion of the results

The dropout results highlight that information is not a panacea to increase educational attainment for all: although there was no average effect, the intervention did decrease enrollment among some populations of students. A full analysis of the welfare implications of these adjustments would rely on knowing both the production function and exactly what was crowded out or in. Unfortunately, the endline survey was too short to gather detailed information on all household expenditures and schooling for all children, so I cannot evaluate whether the changes to dropouts crowded in, say, extra schooling for other siblings in the household vs. expenditures for the parents.[43] What I can say is that the literature has generally found that ability and education investments are complements (e.g., Pitt et al. (1990) and Aizer and Cunha (2012)), and so the findings are consistent with increased efficiency.

7.3 Heterogeneity in the Regression Results by Parent Education

Columns (1) - (4) of Table 8 examine whether there is heterogeneity in the observed effects by parent education. Surprisingly given there were no significant effects in the full sample for expenditures, there is significant heterogeneity by parent education: less-educated parents in the treatment group increased their expenditures on their lower-achieving children relative to their higher-achieving children (cols (1) and (2)). But, the more educated parents become, the more they instead begin to spend more on their higher-achieving children relative to their lower-achieving children, until (based on linear extrapolation), the gradient changes direction at roughly 5 years of education and parents begin investing more in their higher-achieving than lower-achieving children. (Note that this does not just result from the linear specification, as the conclusion is similar when one estimates the relationship less parametrically within different education bins.)[44] A similar pattern holds for attendance: for less-educated parents, attendance increases more for low-achieving children, whereas for more-educated parents, attendance increases more for higher-achieving children. Figure shows the attendance and expenditure heterogeneity results graphically.

The heterogeneity by parent education in the treatment effects on attendance and expenditures do not seem to be driven by heterogeneity in baseline expenditures or in perceptions about the complementarity between ability and schooling, since controlling for the interaction of those variables with $Treat$ and with $Treat \times Score$ does not attenuate the heterogeneity by parent education. There could, however, be heterogeneity by parent education in their returns based on their place on the production function. For less-educated parents, who

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

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

might not expect that their children could attend secondary school, the (perceived) returns to gaining further skills could be somewhat concave (e.g., high returns to gaining initial skills like reading and low returns after), which could be why they compensate more. In contrast, for more-educated parents who might think their child has some chance but not a guarantee of admission to secondary school, the (perceived) returns might be convex (i.e., high return for pushing the child over the hump into secondary school admissions), causing them to reinforce.

Thus, these results suggest that, relative to the outcomes examined in section 6.1, there might be greater heterogeneity in the production function faced by – and thus the efficient returns-maximizing action – parents of different education levels. As a result, the heterogeneity examined in section 6.1 in beliefs, updating, and straightforward-investments may provide a better proxy of whose investments are more distorted by inaccurate beliefs than heterogeneity in these longer-run outcomes.

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. Some of their investments also respond more to improved information. Taken together, the findings support the hypothesis that inefficiencies caused by misperceptions could provide one channel through which educational inequalities persist across generations despite expansions in educational access.

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

parents from taking full advantage of the information treatment. Future research can further explore whether information needs to be combined with other resources to more significantly impact children's education in developing countries.

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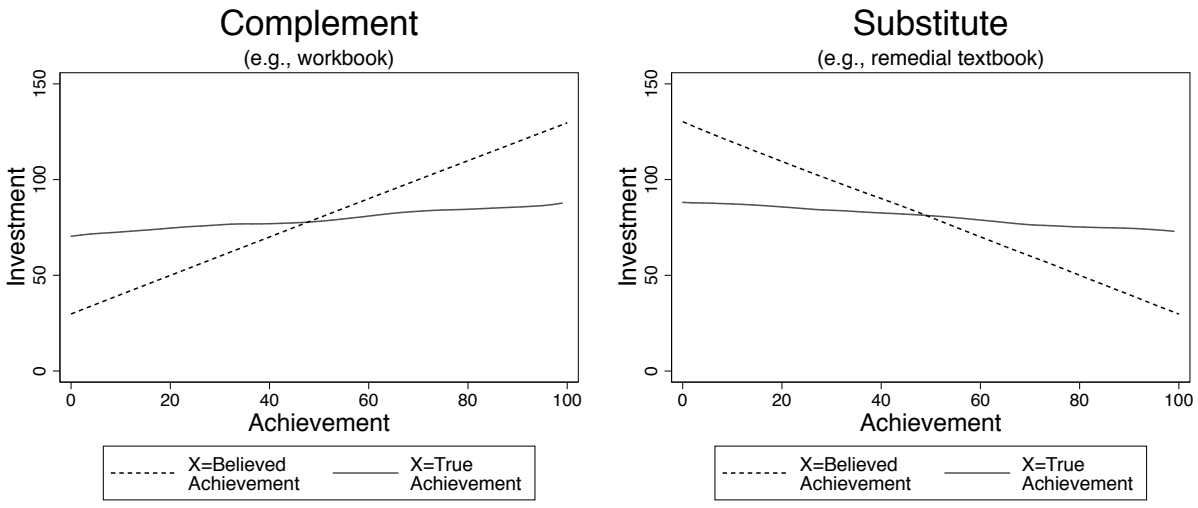
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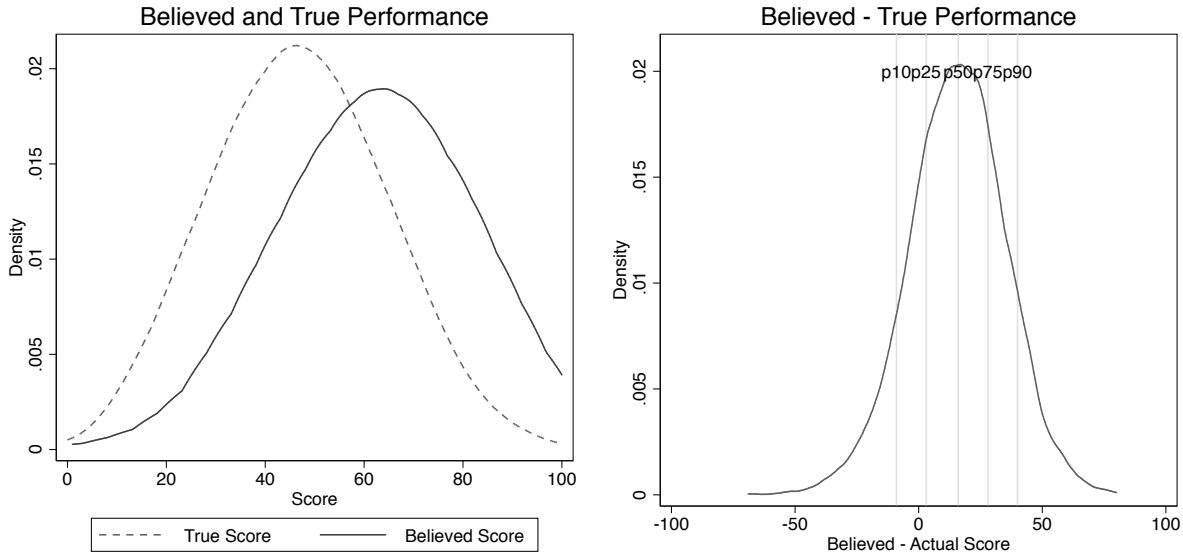
Figure 1: Conceptual framework

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



Notes: Illustrative graph, not based on real data.

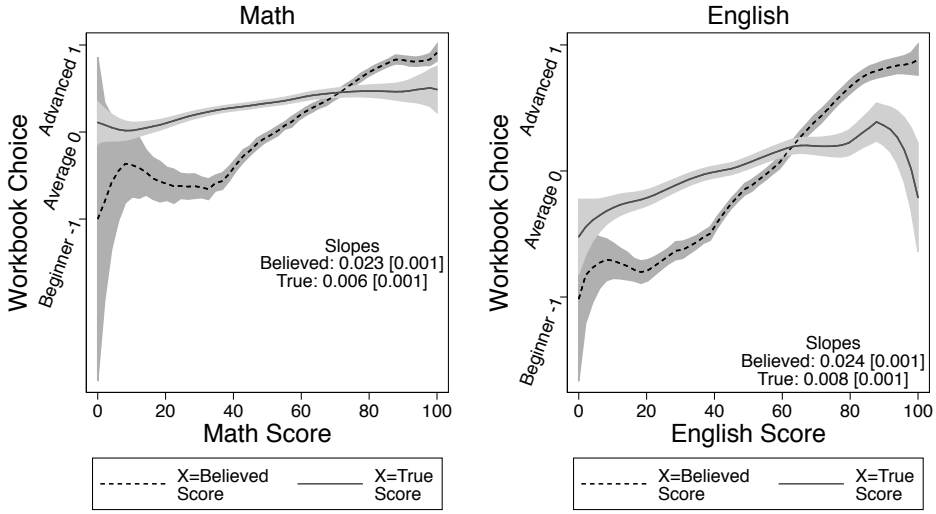
Figure 2: Parents have misperceptions about their children's achievement



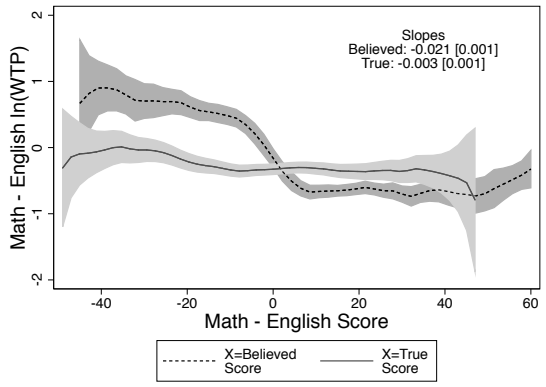
Notes: Data source is baseline data (full sample). The left graph shows kernel density plots comparing the distribution of parents' beliefs about their children's Term 2 2011-2012 achievement test performance, elicited at the beginning of the baseline survey, with the distribution of their children's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

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

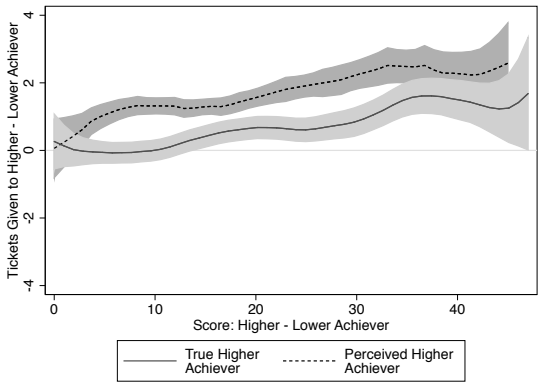
(a) **Workbooks (Complements)**



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

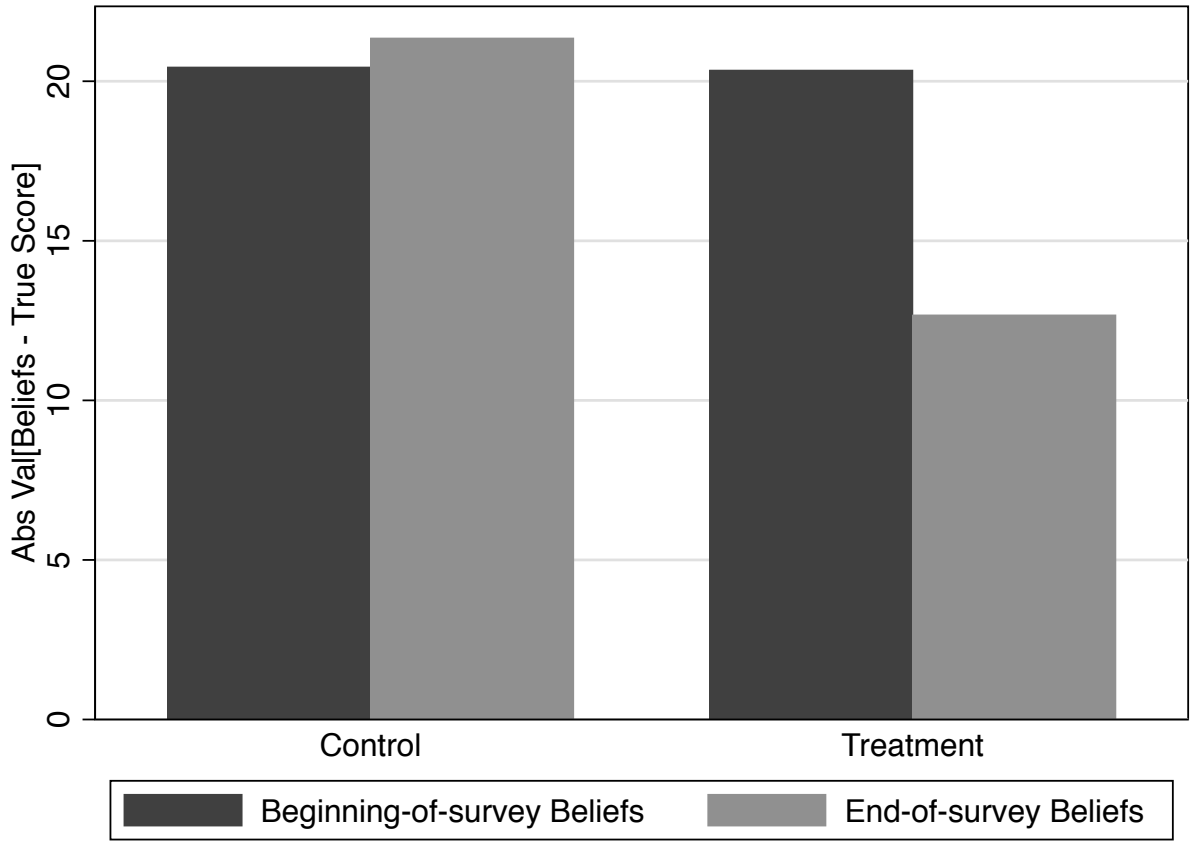


(c) **Secondary School Lottery**



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

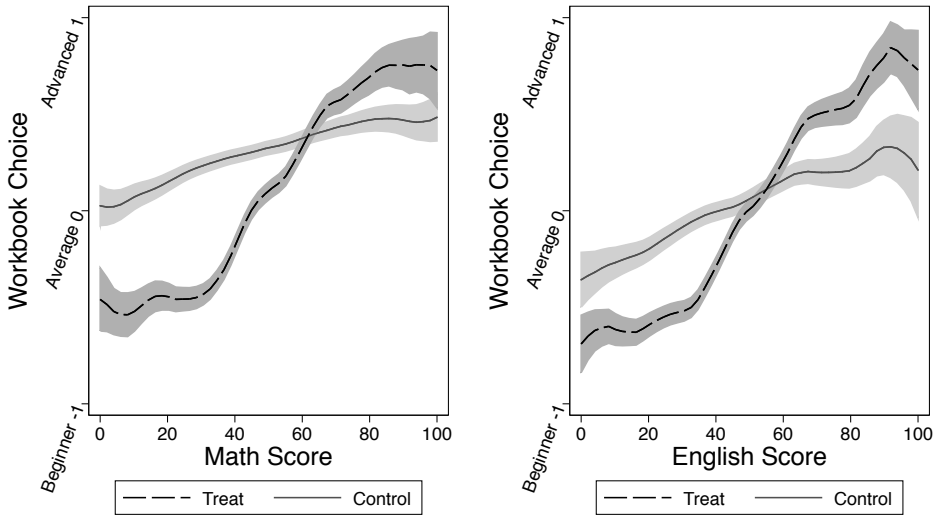
Figure 4: Information shifts parents' beliefs towards their children's true achievement



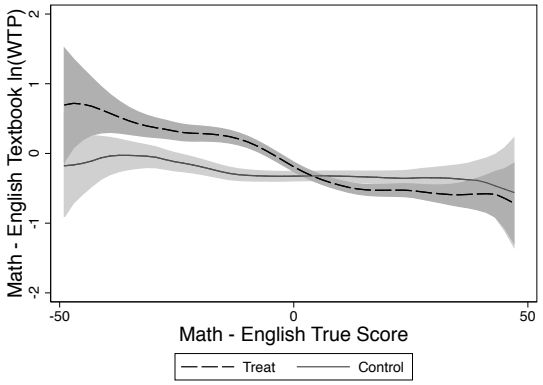
Notes: Data source is baseline survey data. The dark gray bars show the absolute value of the difference between children's true term 2 2011-2012 achievement test scores and their parents' "beginning of survey" beliefs about those scores, which were elicited at the beginning of the baseline survey (before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

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

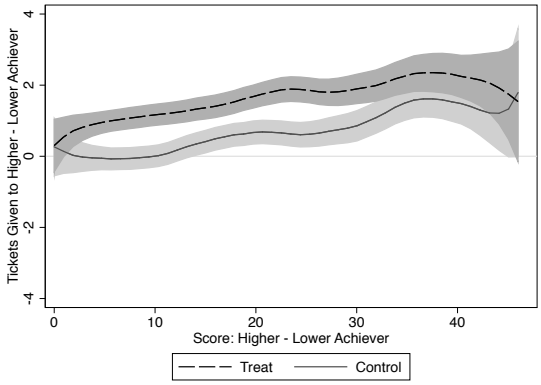
(a) **Workbooks (Complements)**



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



(c) **Secondary School Lottery**

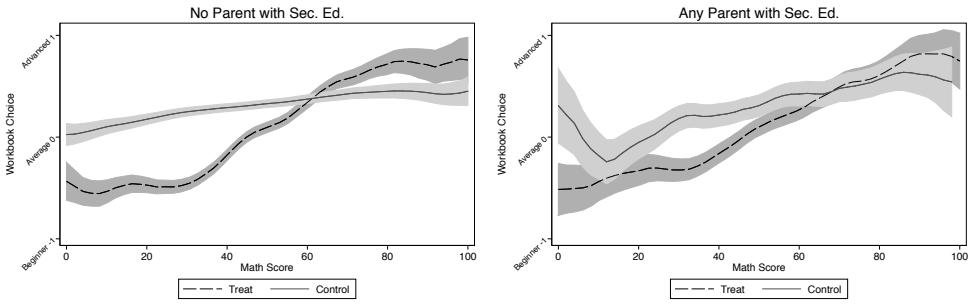


Notes: Data source is baseline survey data. All lines are local linear regression lines with investments as the dependent variable and true achievement as the x-axis. For the workbook graphs, the dependent variable is the parent's choice among 3 level-specific workbooks which are parametrized as -1 (beginner),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

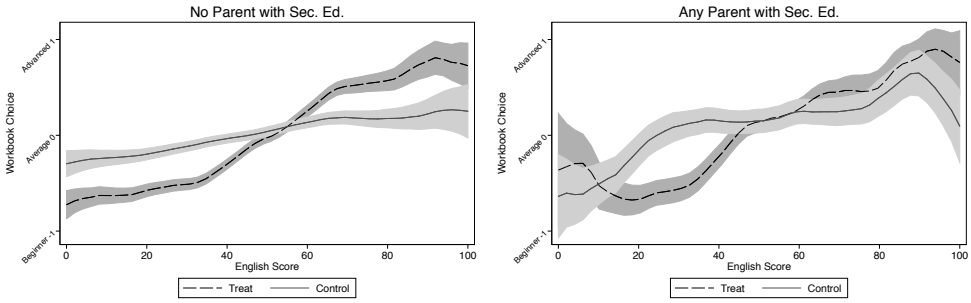
Figure 6: Heterogeneity in treatment effects by parent education (Textbooks, workbooks, secondary school lottery)

(a) **Workbooks (Complements)**

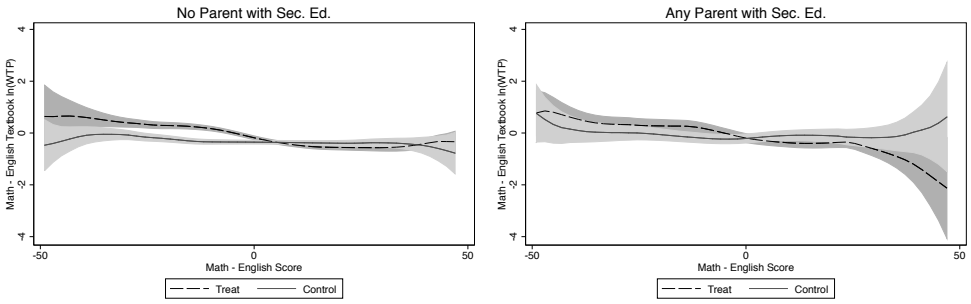
Math Workbooks



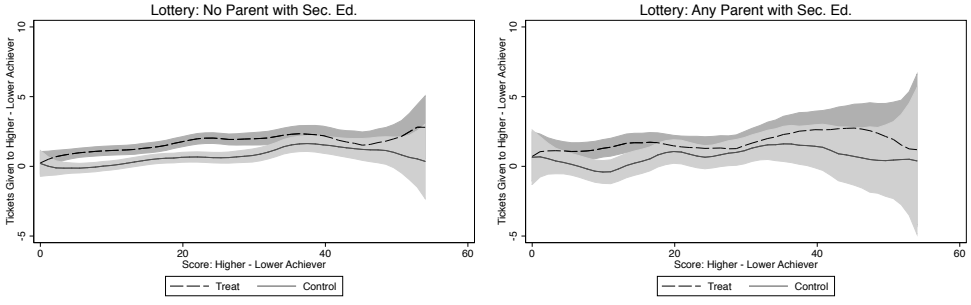
English Workbooks



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

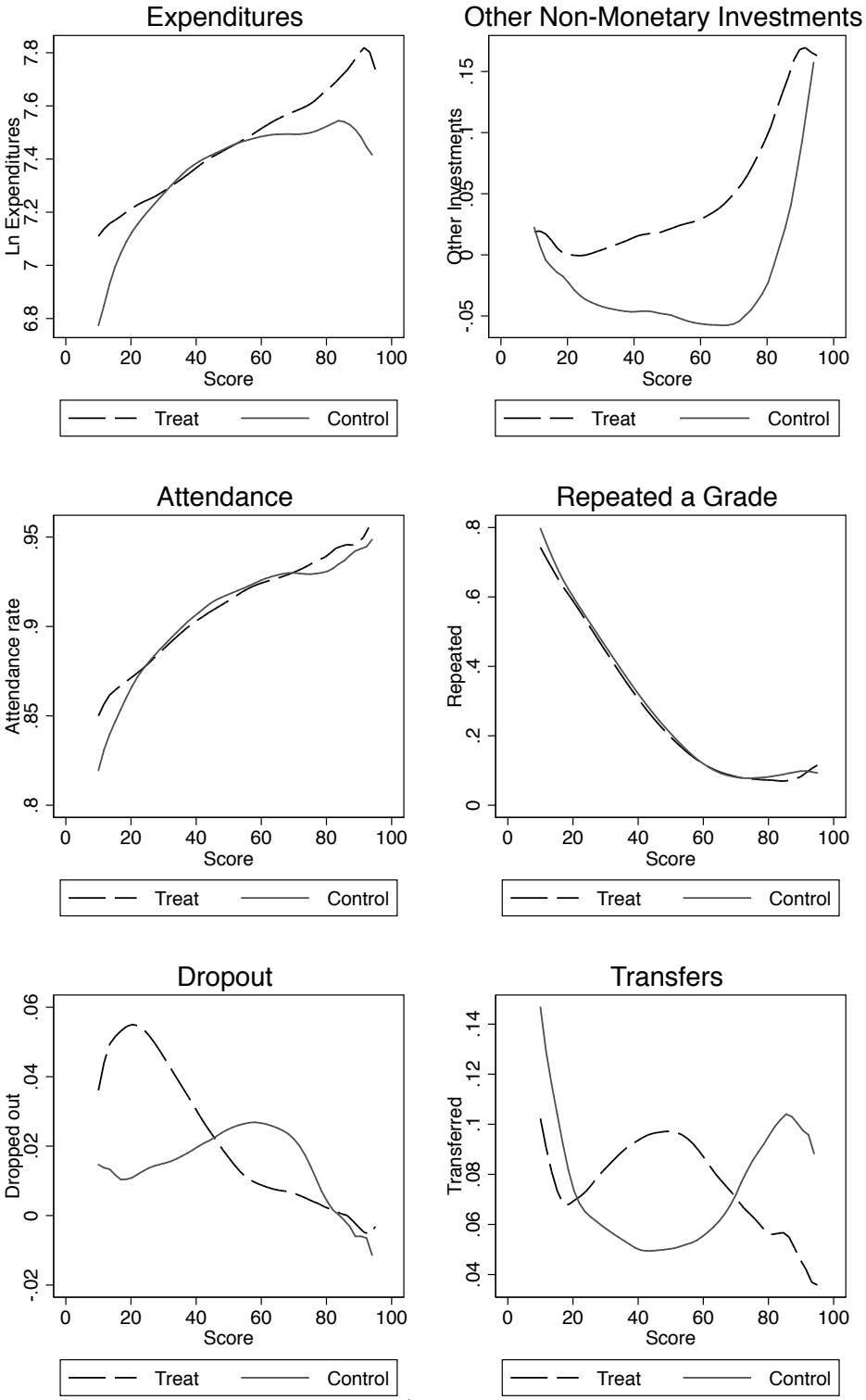


(c) **Secondary School Lottery**



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

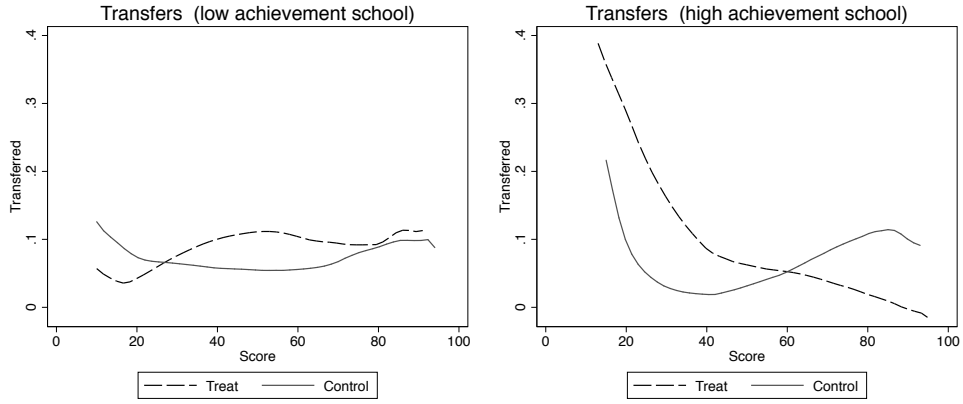
Figure 7: Treatment effects: Longer-run outcomes



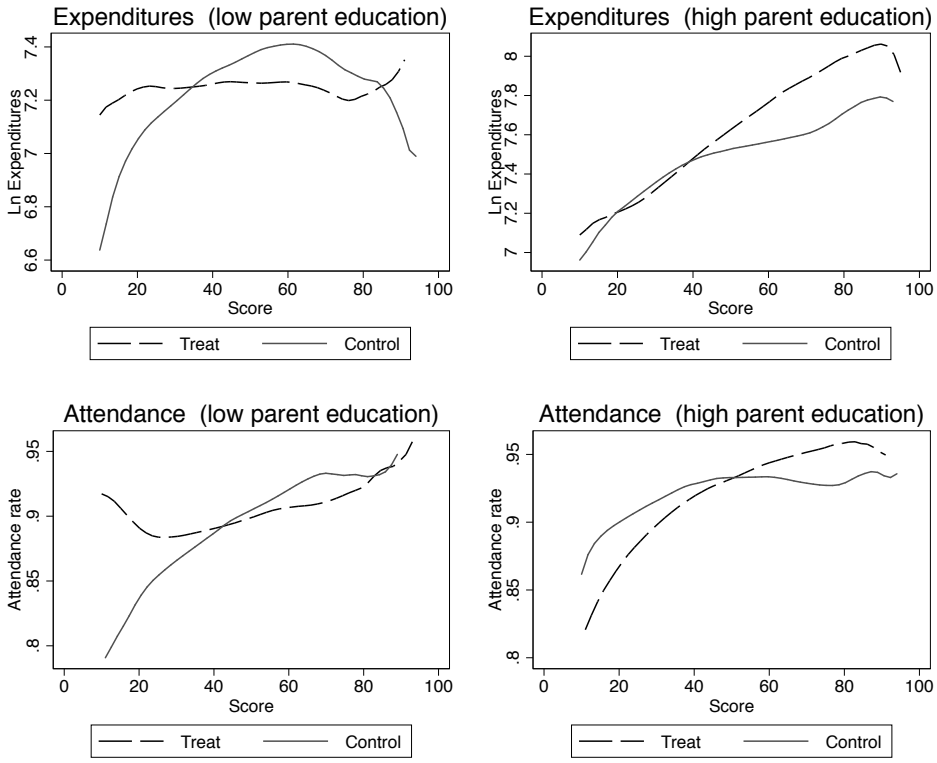
Notes: Data sources are endline survey data (expenditures, other non-monetary investments, homework help, asked for homework help, dropout, and transfers) and data collected from schools (attendance rate, grade repetition). All lines are local linear regression lines with investments as the dependent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

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

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



(b) **Heterogeneity by parent education**



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

Table 1: Baseline Summary Statistics

	Full Sample		Control	Treat	Treat − Control		
	Mean	SD	Mean	Mean	Mean	Std. Error	p-val T=C
Respondent Background							
Female	0.77	0.42	0.77	0.76	-0.01	0.02	0.37
Primary education 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)

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Dependent Var: Math Workbook Choice						
Treat × Math Score	0.013***	0.012***	0.012***	0.011***	0.011***	0.011***
	[0.00093]	[0.0017]	[0.0017]	[0.0017]	[0.0017]	[0.0017]
Math Score	0.0065***	0.0081***	0.0081***	0.0084***	0.0084***	0.0085***
	[0.00065]	[0.0012]	[0.0012]	[0.0012]	[0.0012]	[0.0012]
Treat	-0.91***					
	[0.049]					
Household FE		✓	✓	✓	✓	✓
Observations	5,239	5,239	5,239	5,239	5,239	5,239
R-squared	0.217	0.695	0.695	0.696	0.696	0.699
Panel B. Dependent Var: English Workbook Choice						
Treat × English Score	0.013***	0.013***	0.013***	0.013***	0.013***	0.013***
	[0.00096]	[0.0017]	[0.0017]	[0.0017]	[0.0017]	[0.0017]
English Score	0.0076***	0.0089***	0.0089***	0.0086***	0.0085***	0.0085***
	[0.00073]	[0.0012]	[0.0012]	[0.0012]	[0.0012]	[0.0012]
Treat	-0.68***					
	[0.048]					
Household FE		✓	✓	✓	✓	✓
Observations	5,239	5,239	5,239	5,239	5,239	5,239
R-squared	0.204	0.710	0.710	0.714	0.715	0.716
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***	-0.014***
	[0.0022]	[0.0038]	[0.0038]	[0.0039]	[0.0039]	[0.0039]
Math − English Score	-0.0030*	-0.0016	-0.0015	-0.00048	-0.00041	-0.00046
	[0.0016]	[0.0025]	[0.0025]	[0.0028]	[0.0028]	[0.0027]
Treat	0.15***					
	[0.041]					
Household FE		✓	✓	✓	✓	✓
Observations	5,183	5,183	5,183	5,183	5,183	5,183
R-squared	0.033	0.601	0.601	0.602	0.602	0.603
Panel D. Dependent Var: Lottery tickets received						
Treat × (Higher-scoring Sibling)	0.98***	0.98***	0.98***	0.94***	0.95***	0.95***
	[0.13]	[0.22]	[0.22]	[0.21]	[0.22]	[0.22]
Treat × (Overall Score)		0.0017	0.0017	0.0052	0.0036	0.0044
		[0.0090]	[0.0090]	[0.0088]	[0.0091]	[0.0090]
Higher-scoring Sibling	0.53***	-0.16	-0.17	-0.16	-0.19	-0.18
	[0.091]	[0.16]	[0.15]	[0.15]	[0.16]	[0.15]
Overall score		0.034***	0.034***	0.031***	0.033***	0.033***
		[0.0064]	[0.0064]	[0.0063]	[0.0064]	[0.0064]
Household FE	✓	✓	✓	✓	✓	✓
Observations	5,258	5,258	5,258	5,258	5,080	5,080
R-squared	0.105	0.125	0.129	0.161	0.175	0.180
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 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). *** 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]								
Performance Measure:	(1) Overall	(2) Overall	(3) Overall	(4) Math	(5) Math	(6) Math	(7) English	(8) English	(9) English
Secondary Education +	-3.552***	-3.530***	-2.200***	-4.870***	-4.957***	-4.864***	-1.877**	-1.874**	-1.119
	[0.720]	[0.720]	[0.751]	[0.867]	[0.867]	[0.921]	[0.836]	[0.832]	[0.870]
Observations	5,019	5,019	5,019	5,021	5,021	5,021	5,021	5,021	5,021
R-sqaured	0.005	0.007	0.047	0.006	0.020	0.050	0.001	0.005	0.028
Dep var mean	20.41			25.82			21.42		
Performance Measure:	(10) Chichewa	(11) Chichewa	(12) Chichewa	(13) Math−Eng	(14) Math−Eng	(15) Math−Eng	(16) Child2−1	(17) Child2−1	(18) Child2−1
Secondary Education +	-4.464***	-4.206***	-2.484***	-1.472*	-1.461*	-2.090**	-1.945*	-1.790*	-1.190
	[0.868]	[0.864]	[0.935]	[0.830]	[0.834]	[0.922]	[0.998]	[1.002]	[1.095]
Observations	5,021	5,021	5,021	5,021	5,021	5,021	2,514	2,514	2,514
R-squared	0.005	0.024	0.059	0.001	0.005	0.027	0.001	0.009	0.029
Dep var mean	23.84			22.12			18.73		
Col. Specification Details									
Child and Parent Controls		✓	✓		✓	✓		✓	✓
School FE			✓			✓			✓

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

Table 4: Relationship between beliefs and true achievement: Heterogeneity by parent education

	Dependent Variable = Respondent's Beliefs about Child's Score			
	(1)	(2)	(3)	(4)
Subject:	Overall	Math	English	Chichewa
Score	0.305***	0.189***	0.276***	0.234***
	[0.014]	[0.015]	[0.017]	[0.013]
Score × (Secondary Education +)	0.158***	0.205***	0.123**	0.116***
	[0.043]	[0.043]	[0.050]	[0.043]
Secondary Education +	-7.405***	-12.589***	-0.794	-5.558**
	[2.369]	[2.188]	[2.616]	[2.821]
Observations	5,019	5,021	5,021	5,021
R-squared	0.122	0.060	0.090	0.086

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

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

Dependent Variable = Abs Val [End-of-Survey − Beginning-of-Survey Beliefs]				
	(1)	(2)	(3)	(4)
Treat × (Parent years of education)		-0.37***	-0.32***	-0.29***
		[0.11]	[0.11]	[0.098]
Treat	8.29***	10.0***	14.6***	-3.30***
	[0.36]	[0.64]	[1.11]	[1.26]
Parent years of education		-0.021	-0.023	-0.019
		[0.061]	[0.061]	[0.061]
Treat × (Overall Score)			-0.10***	0.067***
			[0.019]	[0.018]
Overall Score			0.0043	0.016
			[0.0090]	[0.012]
Treat × Abs Val [Believed − True overall score]				0.48***
				[0.030]
Abs Val [Believed − True overall score]				0.028
				[0.019]
Observations	4,984	4,951	4,951	4,951
R-squared	0.126	0.133	0.143	0.305
Dep Var Mean in Treat	13.72			
Dep Var Mean in Control	5.456			

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

Table 6: Treatment effect heterogeneity by parent education

	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	All	Treat Only	All	All	Treat Only	All	All	Treat Only	All	All	Treat Only
Treat × Score	0.013***	0.019***		0.012***	0.016***		-0.013***	-0.017***		0.0013	0.0033	
	[0.00093]	[0.0016]		[0.00096]	[0.0017]		[0.0022]	[0.0037]		[0.0095]	[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	0.0065***	0.0029**	0.021***	0.0077***	0.0061***	0.022***	-0.0030*	0.000055	-0.017***	0.035***	0.033***	0.038***
	[0.00066]	[0.0011]	[0.0012]	[0.00073]	[0.0013]	[0.0011]	[0.0016]	[0.0026]	[0.0027]	[0.0071]	[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	-0.91***	-1.22***		-0.68***	-0.79***		0.15***	0.30***		0.99***	1.05***	
	[0.049]	[0.086]		[0.048]	[0.086]		[0.041]	[0.071]		[0.22]	[0.40]	
Treat × (Parent yrs of educ.)		0.065***			0.023			-0.032***			-0.015	
		[0.015]			[0.015]			[0.012]			[0.069]	
Parent yrs of educ.	0.0022	-0.039***	0.023**	0.0083**	-0.0029	0.019*	0.0079	0.024***	-0.0083	-0.0028	-0.00044	-0.016
	[0.0034]	[0.011]	[0.010]	[0.0035]	[0.012]	[0.0097]	[0.0062]	[0.0084]	[0.0089]	[0.020]	[0.050]	[0.047]
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	5,203	2,588	5,203	5,203	2,588	5,183	5,183	2,575	2,611	2,611	1,299
R-squared	0.216	0.220	0.292	0.206	0.207	0.325	0.033	0.035	0.059	0.047	0.047	0.028
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. Each observation is a child (cols (1)-(9)) or a household (cols (10)-(12)).
*** p<0.01, ** p<0.05, * p<0.1

Table 7: Information treatment effects: Longer-run outcomes

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

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

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

Independent Variable:	(1) Treat	(2) Treat × Score	(3) Treat × Parent Yrs. Educ.	(4) Treat × Score × Parent Yrs. Educ.	(5) Control group mean: Below-median parent educ.	(6) Control group mean: Above-median parent educ.	(7) Treat	(8) Treat × Score	(9) Treat × Female	(10) Treat × Score × Female	(11) Control group mean: Female =0	(12) Control group mean: Female =1
Dependent Variables												
Panel A. Dropout and transfer (from endline survey data)												
Dropout	0.068	-0.0015	-0.003	0.0001	0.033	0.005	0.055	-0.001	0.003	-0.0001	0.014	0.027
	[0.039]*	[0.0007]**	[0.005]	[0.0001]			[0.026]**	[0.0005]**	[0.043]	[0.0008]		
Transfer	-0.08	0.002	0.024	-0.0005	0.056	0.059	-0.022	0.0017	0.099	-0.003	0.04	0.071
	[0.065]	[0.0013]*	[0.013]*	[0.0003]*			[0.053]	[0.001]*	[0.071]	[0.0014]***		
Panel B. Investments (from endline survey data)												
Total educ. expenditures	1,027	-31.04	-185.69	6.219	2,089	2,653	274.43	-7.055	-360.59	14.73	2,248	2,375
	[562.97]*	[14.21]**	[125.02]	[3.471]*			[439.93]	[9.836]	[667.1]	[14.1]		
ln(Total educ. expenditures)	0.369	-0.009	-0.056	0.0014	7.293	7.489	0.0005	0	0.174	-0.004	7.354	7.403
	[0.203]*	[0.004]**	[0.037]	[0.0007]**			[0.156]	[0.003]	[0.222]	[0.004]		
Avg. std. effect across other non-monetary investments[a,b]	0.06	-0.0003	0.004	0	-0.091	0.075	0.03	0.0007	0.063	-0.001	0.008	-0.035
	[0.097]	[0.0018]	[0.02]	[0.0004]			[0.084]	[0.0016]	[0.11]	[0.002]		
Avg. std. effect across other chores[c]	0.124	-0.0017	-0.024	0.0006	-0.021	0.026	-0.039	0.0003	0.045	0.002	-0.127	0.125
	[0.17]	[0.003]	[0.033]	[0.0006]			[0.152]	[0.003]	[0.204]	[0.004]		
Panel C. Attendance and grades (from data collected from schools)												
Attendance rate following baseline survey	0.08	-0.0015	-0.018	0.0003	0.894	0.927	-0.045	0.0009	0.073	-0.0016	0.906	0.917
	[0.044]*	[0.0008]*	[0.008]**	[0.0001]**			[0.039]	[0.0007]	[0.051]	[0.0009]*		
End-of-year grade	-0.0013	0.0003	0.03	-0.0008	1.94	1.99	-0.052	0.001	0.35	-0.008	2.024	1.901
	[0.161]	[0.004]	[0.031]	[0.0007]			[0.136]	[0.003]	[0.195]*	[0.004]*		

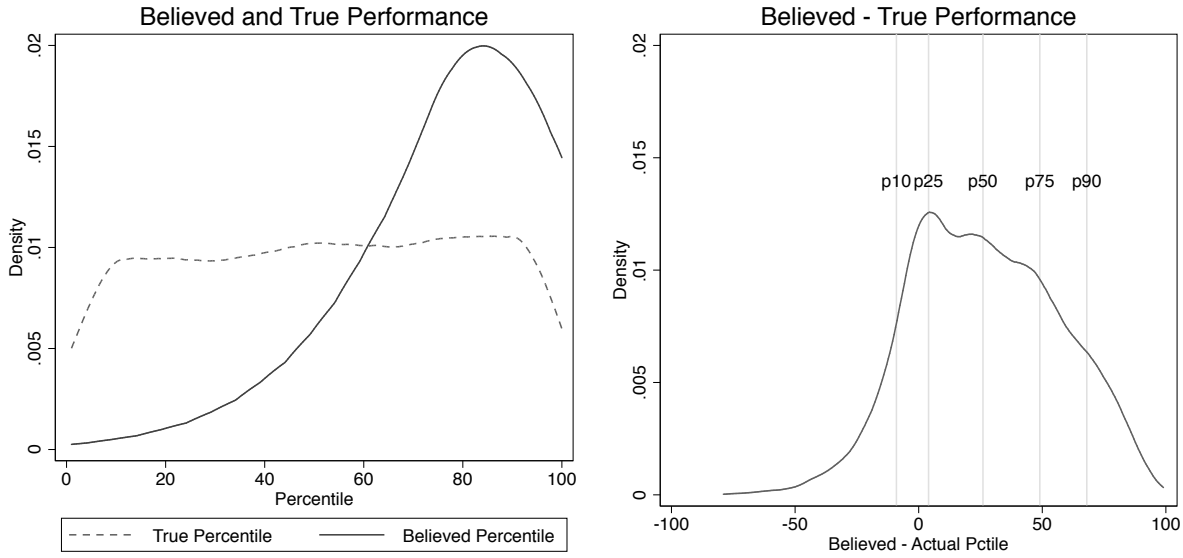
Notes. Data sources are endline survey and endline data collected from schools. Each observation is a child. Standard errors clustered at the household level. All regressions control for child gender, child baseline achievement, grade fixed effects, school fixed effects, and the baseline value of the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

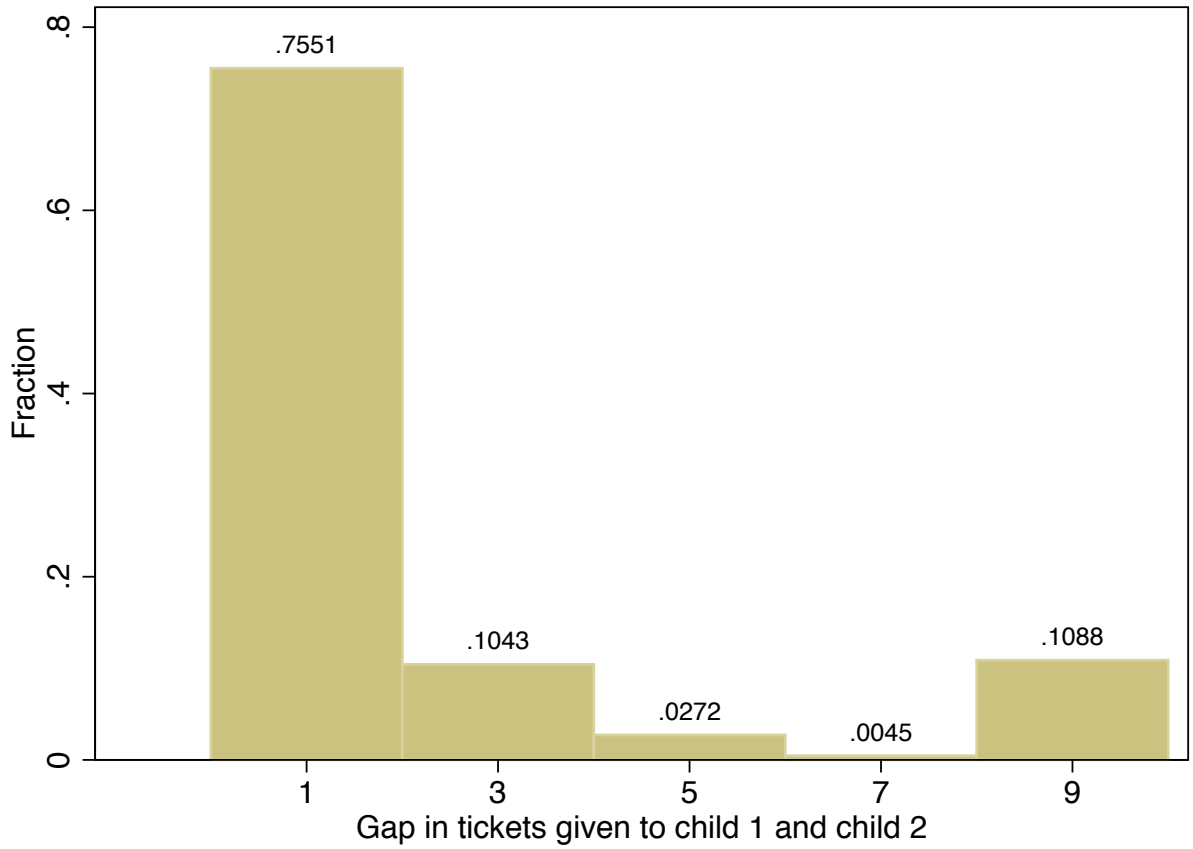
c. Average across 2 chores measures: hours of chores and # times fetched water.

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 sec-ondary educ.	Parent is literate	Years of educ.	Above-median educ.	At least sec-ondary 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]								
Performance Measure:	(1) Overall	(2) Overall	(3) Math	(4) Math	(5) English	(6) English	(7) Chichewa	(8) Chichewa
Child grade	0.754***	0.862***	0.785***	0.965***	0.273	0.395*	0.053	0.118
	[0.168]	[0.183]	[0.216]	[0.234]	[0.217]	[0.236]	[0.207]	[0.225]
Secondary education × Child grade		-0.818		-1.535**		-0.898		-0.492
		[0.595]		[0.756]		[0.765]		[0.713]
Household FE	✓	✓	✓	✓	✓	✓	✓	✓
Observations	5,052	5,019	5,054	5,021	5,054	5,021	5,054	5,021
R-squared	0.682	0.681	0.666	0.668	0.592	0.593	0.690	0.689

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

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

	Math Workbook		**English Workbook**		**Textbook WTP**		**Lottery**		**Attendance**	
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		Percent days attended (0-100)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treat × True score	0.013***	0.016***	0.013***	0.017***	-0.013***	-0.015***	0.0013	0.014	0.013	0.077
	[0.00093]	[0.00090]	[0.00096]	[0.00088]	[0.0022]	[0.0021]	[0.0095]	[0.0092]	[0.049]	[0.053]
Treat × Believed score		-0.015***		-0.015***		0.011***		-0.034***		-0.17***
		[0.0010]		[0.00086]		[0.0021]		[0.0067]		[0.057]
Treat	-0.91***	-0.054	-0.68***	-0.0011	0.15***	0.046	0.99***	1.00***	0.92	8.81**
	[0.049]	[0.068]	[0.048]	[0.052]	[0.041]	[0.044]	[0.22]	[0.21]	[3.40]	[4.35]
True score	0.0065***	0.0016***	0.0076***	0.0013**	-0.0030*	-0.000012	0.035***	0.012*	0.084**	0.042
	[0.00065]	[0.00057]	[0.00073]	[0.00058]	[0.0016]	[0.0015]	[0.0071]	[0.0066]	[0.035]	[0.036]
Believed score		0.022***		0.023***		-0.021***		0.069***		0.10**
		[0.00072]		[0.00060]		[0.0014]		[0.0045]		[0.044]
Observations	5,239	5,233	5,239	5,233	5,183	5,177	2,611	2,606	1,769	1,764
R-squared	0.217	0.374	0.204	0.405	0.033	0.097	0.047	0.145	0.116	0.121
p-val: (Treat × Perf) + (Treat × Educ.)=0		0.257		0.217		0.143		0.040		0.140

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

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

	(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 7: Correlations between beliefs and endline survey outcomes (Control group only)

Independent Variable:	Baseline beliefs	Avg. baseline/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 8: Effect of Information on Endline Outcomes (Detailed data)

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

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 9: Transfer results: Heterogeneity by school type

	Dependent Variable = Transferred		
	(1)	(2)	(3)
Treat	0.030**	0.022	0.016
	[0.014]	[0.019]	[0.022]
Treat × High-achievement school			0.040
			[0.046]
Treat × Score		0.016	0.041
		[0.025]	[0.029]
Treat × Score × High-achievement school			-0.097*
			[0.053]
Observations	1,781	1,781	1,781
R-squared	0.039	0.039	0.040
Dep Var Mean in Control	0.06		
p-val: (Treat × Score)=0			
p-val: (Treat × Score) + (Treat × Score × High quality)=0			0.208

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				
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

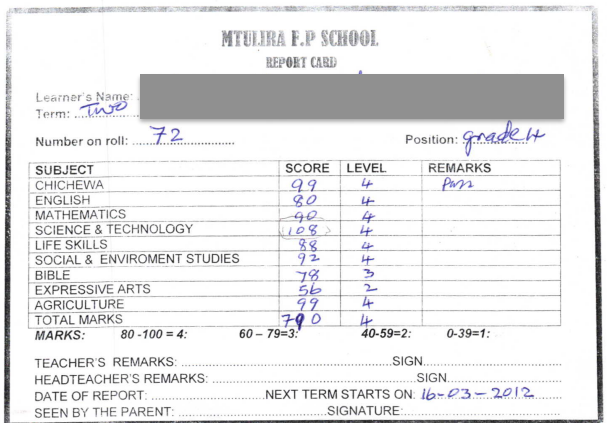
Sample price list

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	Pm
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	

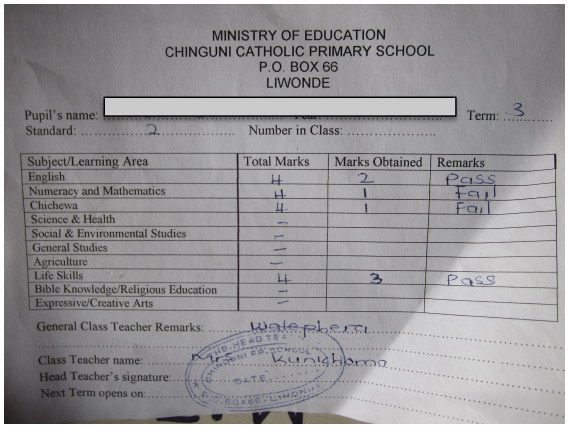
MARKS: 80 -100 = 4: 60 – 79=3: 40-59=2: 0-39=1:

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: Welepherri

Class Teacher name: Kunkhomo

Head Teacher's signature:

Next Term opens on:

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