Disinterested, Lost, or Both? Valuing Productivity Thresholds Using an Educational Field Experiment[*]

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[**Work in progress**. Please click here for the latest draft.]

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Abstract

Student achievement of skills is critical to raising living standards even in Tanzania, where right now less than 20 percent of high school students are passing their national promotional mathematics test. In order to understand whether this level of achievement is because (1) students are disinterested in the curriculum; (2) lost and unable to follow the curriculum; or (3) both of the above; I conduct a field experiment with 6,201 students, 170 high schools, and a three-year follow-up, providing students with (1) money pegged to math test scores, (2) technologies to ease the effort costs of learning, or (3) both of the above. I find that money or technologies alone make limited impact on test scores, while both together produce a large, complementary effect (0.3σ), especially on the scores of students below the top 20 percent. To rationalize these results, based on detailed surveys of study habits matched to outcomes, I estimate a model of students who recognize the benefits and costs of learning, including certain fixed costs of even trying to study that they only incur if there are sufficiently large benefits. I use the model to value test scores and promotion; compute welfare implications of the interventions; and simulate counterfactual outcomes from lowering the promotional cutoff—a low-cost policy option that would offer more students a realistic chance of promotion and, therefore, a higher expected return from marginal effort. The model suggests that *both* providing the experimental inputs *and* incentivizing the students by doubling their chances of promotion in mathematics will induce a modest but meaningful endogenous response of student knowledge, by reducing the share of students who are giving up on learning at the margin from 79 percent to 52 percent. By explaining treatment complementarities, the estimated model extends a standard model of classroom learning from the previous literature to reflect a higher degree of realism about developing community contexts.

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1 Introduction

Student achievement of skills is critical to raising living standards across nations, which are now nearing universal primary school enrollment and 75 percent secondary school enrollment (Lee and Lee, 2016). The productivity of these classroom hours often raises questions, however; the recent World Development Report documents detailed evidence of classroom learning failures from a wide variety of contexts, warning of a global "learning crisis" (World Bank, 2018). Tanzania provides a case in point, where right now less than 20% of high school students are passing the national promotional mathematics test.[1]

In order to understand whether Tanzania's current level of mathematics achievement is because (1) students are disinterested in the curriculum; (2) lost and unable to follow the curriculum; or (3) both of the above; I conduct a field experiment with 170 high schools and a three-year follow-up, providing students with (1) money pegged to math test scores, (2) technologies to ease the effort cost of learning, or (3) both of the above. Specifically, Group 1 students received money-reward contracts for marks to be obtained on a year-end curriculum-based test ("Incentives (G1)"). Group 2 students received inputs combining free solar-energy access, bilingual textbooks, and videos ("Technologies (G2)"), that were designed to better show students not just what but *how* to study, based on Glewwe, Kremer and Moulin's (2009) suggestion that a mismatch between one's mother tongue and the language of instruction may hinder learning, and Fryer's (2011) that students not knowing how to study may also hinder learning. Group 3 students received both of the above ("Both (G3)"), testing the interaction effect of interventions directly provided to students. Each treatment was delivered in the beginning of each year, and the same treatment groups were followed for a period of three years between 2016 and 2018.

Results show that money or technologies alone make limited impact on test scores (0.05 and 0.09σ, respectively), while both together make a large impact (0.33σ for the both treatment effect and 0.19σ for the interaction effect), especially on the scores of students just below the top 20 percent (0.35σ for the both treatment effect and 0.33σ for the interaction effect).[2] Detailed surveys of study behavior matched to outcomes point to increased hours of study and more attentive engagement during study—particularly with the provided books—as the mechanisms for improvement. Why does providing both of these interventions deliver results where either alone cannot, and why does the interaction work more strongly for some than others?

In order to rationalize these results, based on detailed survey of study habits matched to outcomes, I estimate a model of students who recognize the benefits and costs of learning that generates key patterns of treatment effects. I begin from a benchmark model of classroom learning with student self-agency based on the current literature's standard (Todd and Wolpin, 2018). I then hypothesize a simple additional feature: students face certain fixed costs of even trying to study that they will only incur if there are sufficiently large benefits—which I refer to as productivity thresholds, or "traps," in this paper. In particular, I consider minimum interest and minimum entering grade-level knowledge (or "preparedness") as key thresholds. These thresholds can be motivated from fixed (non-convex) adjustment costs. The productivity thresholds tested in this paper may be viewed as a parsimonious way of capturing more complex micro-foundations that may be at play in the classroom-learning setting, such as heterogeneous learning-curve concerns (Loerch, 2001).[3] I show that this feature helps explain key treatment-effect patterns, in particular a hump-shaped relationship between the interaction effect and student pretest score, that the standard model cannot. Since these models give different policy implications, selecting models that offer a

[1] About 40% of age-group youths sit as candidates, which means that among Tanzania's 10 million men and women aged 18 to 27, only 6% have passed high school mathematics; passing mathematics is a prerequisite to training in STEM-related occupations, and hence may be high stakes for some students (NECTA, 2019).

[2] The effects reported are from the second year of the program, as shown in column 6 of table 3; column 9 of the same table shows that year 3 results are similar. The "interaction effect" refers to the G3 (both) treatment effect minus the G1 (incentives) and G2 (technology) effects.

[3] This paper demonstrates a flexible approach that uses field-experimental variations that can be employed to test a variety of such and other learning models in the classroom-learning context.

higher degree of realism has direct implications for policy effectiveness, which I assess in counterfactual simulations.

I use the model to value test scores and promotion; welfare implications of the interventions; and counterfactual outcomes from lowering the promotional cutoff—a low-cost policy option that would offer more students a realistic chance of promotion and, therefore, a higher expected return from marginal effort. The results suggest that conditional on providing the experimental technologies, lowering the cutoff will generate a modest but meaningful endogenous response of student knowledge, particularly by reducing the share of students who are "giving up" on learning the curriculum from 79% to 52%.

In doing so, this work demonstrates how to use field-experimental treatment variations to estimate parameters of a student-agency-based model of classroom learning based on the current literature's standard, and identify "productivity traps" that hinder learning. While hundreds of field experiments have examined classroom-level test scores, and an extensive body of work has studied skill production functions, few models have focused on student self-agency: that is, students explicitly choosing their own levels of effort and performance in the classroom. My work provides an intuitive, clarifying measure of classroom efficiency from this perspective: the percentage of students who are meaningfully accumulating knowledge, as opposed to seeming from the data as if they are turning off their minds during the year lacking the ability to follow the material.

My work builds on multiple recent advances from three strands of the literature. First, my work builds on insights from recent field experiments in education involving incentives (Fryer, 2011; Hirshleifer, 2017), complementarities (Behrman et al., 2015; Mbiti et al., 2019), and teaching at the right level (Duflo, Dupas and Kremer, 2011; Banerjee et al., 2016). Second, my work builds on the technology of skill formation literature that estimates skill production functions that take a general, agnostic stance on student agency (Cunha and Heckman, 2008; Cunha, Heckman and Schennach, 2010; Agostinelli and Wiswall, 2016). Third, my work builds on a recent exercise of structural estimation of classroom learning that endogenizes a key choice variable in the process of skill formation: student effort (Todd and Wolpin, 2018).

Adding to these works, this paper provides the first experimental test of complementarity between interventions provided strictly to students as opposed to teachers. It delivers valuations of test scores that cannot be inferred from treatment effects alone, since these effects confound the willingness-to-pay (WTP) for knowledge with the cost of effort. This paper also suggests fixed costs of investment as sources of complementarities that get estimated in more agnostic production functions. Finally, this paper provides the first case study of using field-experimental treatment variations to assess structural parameters of classroom learning with student self-agency, which may be of particular relevance to developing-country contexts where the first generations of secondary-school attendees—with limited parental experience in education—are beginning to rise (Banerjee et al., 2013). By providing a measure of classroom efficiency from this perspective, I argue that the work can be used to inform the targeting of educational investments and curriculum design. For the context of Tanzania in particular: (1) whether the high bar of the test is constraining the efficiency of mandatory hours students are being asked to spend in the mathematics classroom; (2) why providing additional inputs alone, or lowering the bar alone without providing additional inputs, are expected to be fruitless; and (3) how, conditional on providing more inputs, the bar may be better targeted.[4]

This paper proceeds as follows. Section 2 reports the details of the field experiment and reduced-form results. Section 3 outlines the conceptual framework. Section 4 describes the structural-estimation framework and its results. Section 6 concludes.

[4] Students in Tanzania's junior secondary schools—approximately 1.5 million in number—are mandated to spend at minimum 100 hours of classroom-learning on mathematics each year, more so than on any other subject.

2 Experiment and Reduced-form Findings

2.1 Setting

In the beginning of 2016, the study team partnered with 9th-grade students in 170 rural Tanzanian high schools, President's Office – Regional Administration and Local Government (PO-RALG) and seven other organizations, to implement the "Sharpening Mathematics Review (SMR)" project, a collection of interventions developed to support mathematics education of secondary school students.[5] The sample of schools selected were the population of all schools without electricity in 23 northern Tanzanian districts, enlisted in September, 2015.[6] The districts selected were the intersection of where Zola Electric, a national energy company (and research partner), was servicing at the time, and where the government deemed performance to be relatively low in terms of pass rates.[7]

My sample represent a "middle class" of students across the nation. Figure 1a shows O Level aggregate and selected subject-level pass rates nationwide between 2012 and 2015. Out of 1.4 million students who sat for these examinations across these four years, 55% of students managed to obtain O Level certification, which requires obtaining at least two D's or one C on subject-level examinations out of seven best subjects taken.[8] It can also be seen, re-scaled on the right-hand-size y-axis, that the same number re-scaled corresponded to 23% out of 3.4 million youths in the official age group population. That is, less than a quarter of youths in the junior-secondary age group were able to obtain certification over these years. Figure 1b shows the corresponding figures in sample schools, where the pass rates are generally lower, reflecting the selection criterion of relatively lower performance. The average pass rate in the selected sample schools corresponded to the bottom 25th percentile of nationwide school pass-rate distribution. Note that, as can be seen in fig. 1a, if a student is enrolled in secondary-school, the student is already in the top 40% of the nation in terms of educational attainment. Hence, this group of students could be seen as representing a "middle class" of students in terms of educational attainment among the age-group population.

Secondary mathematics has been of particular interest to the government because the subject-level performance has remained poor. It can seen that 40 to 60% of students passed Swahili, English and Biology, whereas the pass rate for mathematics stood below 15% nationwide. Among the sample schools, the pass rate for mathematics has stood below 6%.[9]

2.2 Design

The SMR project involved 170 classrooms from 170 schools, one randomly selected ninth-grade classroom from each school. Between 2015 and 2016, these classrooms were randomly divided into four groups: Incentives (G1), Technology (G2), Both of the above (G3) and Control (C). Incentives provided cash to students for scores on year-end mathematics tests. Technology provided solar-energy access, bilingual textbooks and videos, with emphases not just on what to study but how to practice. The treatments were delivered every year for three years,

[5] These organizations include Zola Electric, GivePower foundation, Energy Policy Institute at Chicago, Youth Shaping & Sharpening Movement, International Growth Centre, Abdul Latif Jameel Poverty Action Lab and Chicago Booth PhD Program Office.

[6] The project initially identified 173 schools, but two schools closed as the program was beginning in 2016 and another school was disqualified from taking government examinations because of having been found with a teacher who had helped students cheat on prior examinations. Data were not collected from these schools. Note that junior-secondary mathematics examinations in Tanzania are very difficult to cheat on without teacher's collusion, because all questions are in free response format. See further discussions in section 2.5.

[7] Districts are Tanzania's 2nd-order-administrative units (ADM2).

[8] O Level certification requires obtaining at least two D's or one C out of five required subjects and two optional subjects: the five required subjects include Swahili, English, Biology, Civics and Mathematics. A grade of D means that the student obtained 29.5 marks out of 100 total marks on the subject level examination. Civics data is omitted in the figures to conserve on space, but the pass rate looks similar to Swahili and English pass rates.

[9] Re-taking the test requires repeating at least two years or attending a private school, both of which are cost-prohibitive for most students in the nation. Re-takers also lose post-O-level public-tuition support for which regular candidates qualify.

with some variation between years, as detailed below. The details, including the implementation timeline, can also be found diagrammatically summarized in figs. B1 and B2.[10]

- **Year 1**: Schools were randomized into 3 treatment groups and 1 control group, with some additional variation in incentive-contract amounts within each classroom.

  G1 ("Incentives Only") Students were given a fixed piece-rate incentive contract for each mark to be scored on an end-of-the-year curriculum-based mathematics test (42 schools).

  G1.1 1/4 of students were promised $0.125 per mark.
  G1.2 1/4 were promised $0.25 per mark.
  G1.3 1/4 were promised $0.50 per mark.
  G1.4 1/4 were promised $0.75 per mark.

  G2 ("Technology Only") Students received 9th-grade mathematics textbooks with Swahili chapter summaries. Schools received solar panels (covering approximately two large classrooms and one office), two TVs (one 16-inch and one 19-inch), and a set of 15-hour mathematics videos covering the full 9th-grade curriculum (44 schools).

  G3 ("Both") Students received both Incentives and Technology (44 schools).

  C ("Control") Students received neither Incentives nor Technology (40 schools).

- **Year 2**: Same treatments continued in year 2, except with the incentive contracts unified.

  G1 ("Incentives Only") Students were promised $0.50 per mark.

  G2 ("Technology Only") Students were given 10th-grade textbooks and videos.

  G3 ("Both") Students received both Incentives and Technology.

  C ("Control") Students received neither Incentives nor Technology.

- **Year 3**: Same treatments continued in year 3, except schools that had been without solar also received solar in the beginning of the year. Hence, the Technology variation only involved textbooks and videos in year 3.

  G1 ("Incentives Only") Students were promised $0.50 per mark.

  G2 ("Technology Only") Students were given 11th-grade textbooks and videos.

  G3 ("Both") Students received both Incentives and Technology.

  C ("Control") Students received neither Incentives nor Technology.

The equalization of piece rates in the beginning of year 2 was for the concern that invidious effects of comparison might hurt intrinsic motivation of some suggestion and demotivate learning.[11] The equalization of the solar variation in the beginning of year 3 was to honor an initial agreement with the government that non-receiving schools would receive the same solar facilities within two years.[12]

Figure 3 shows photographs from the field capturing some typical deliveries of the interventions. Figure B3 provides linked access to online copies of the program's textbooks and videos. Table A2 provides initial and realized power calculations (c.f. table A1 for reference estimates from past works).

[10] Currently under preparation is a draft photo essay depicting some of these activities: link.

[11] I thank an anonymous JPAL referee for this comment. There were a number of anecdotal reports in year 1 that some students were displeased with this variation even after being informed that the variation was random. In year 2, I took the referee's suggestion and equalized the rates. See section 2.5 for further discussion.

[12] The treatment effects between years 2 and 3 remained similar, suggesting that solar was not the binding component within technology. See section 5 for further discussion.

In February, 2016, YSSM field team conducted the Form 1 (grade-8) SMR survey and examinations ("Year 0 Mock Test"), and immediately after the survey and examinations, the field team signed the incentive agreements with the students and distributed the textbooks and videos.[13] Only students who were present during this visit on an arbitrary weekday were enrolled in the program. While participation was voluntary, all students agreed to participate. I take the data from February, 2016, as predetermined student characteristics.[14]

In October, 2016, YSSM field team conducted F2 (grade-9) survey and examinations ("Year 1 Mock Test"), whose data constitute end-of-first-year observations.

In November, 2016, NECTA conducted the promotional FTNA examination, which serve as auxiliary set of observations for the sample students.

In February 2017, the project reinforced a similar design, but with the piece rate per mark equalized across students at $0.50 per mark.

In October, 2017, YSSM field team conducted F3 (grade-10) survey and examinations ("Year 2 Mock Test"), whose data constitute end-of-second-year observations.

In February, 2018, the half of the chools that had not received solar facilities received the same solar facilities that the other half of schools had received two years prior. The rest of the treatment variation continued; therefore, in year 3, the "Technology" variation consisted only of textbooks and videos.

In October, 2018, YSSM field team conducted F4 (grade-11) survey and examinations ("Year 3 Mock Test"), which data constitute third year outcomes.

In November, 2018, the sample students took their O Level examinations ("Year 3 Real Test").

In terms of the data, this study relies on student characteristics predetermined at February, 2016; end-of-year-1 observations; end-of-year-2 observations; and end-of-year-3 observations.

2.3 Reduced-form Estimating Equation

I report difference-in-means estimates, based on the following equation:

$$Q_{ijt} = \beta_t^0 + \sum_{g\in\{1,2,3\}} \beta_t^g \times G_i^g + X_{it}\zeta + \epsilon_{ijt}, \tag{1}$$

where i indexes students; j, schools. The index t denotes evaluation year (the year end).[15] The parameter β^0 is a constant term. Q_{ij}, the explanatory variable of interest, may only exist at the school level (Q_j), in which case the analogous school-level regression is examined. β^G represents the treatment effect of group G. G_i indicates the group to which i belongs. ζ is the vector of coefficients on i's covariates, X_i, which can include age, commute distance, pretest Z-score or a flexible polynomial in attendance propensity score.[16] Because of non-response, controls were missing at random for approximately 10% of observations (i.e., the missingness of individual responses pertaining to relevant control variables was balanced on treatment indicators). Results are robust to multiple (stochastic) imputation, as further discussed in section 5. Attrition controls are also further discussed in section 5. I report standard errors clustered at the school level, the level of the independent unit of the randomized draw

[13] As the PO-RALG was a direct partner, all tests and surveys were known as President's Office Mathematics Evaluation (POME) tests and POME surveys. As such, though nonbinding, the tests carried an air of authoritative government examination.

[14] Because of field partners' expense schedules and timing issues with the funding, Technology Support groups (G2 and G3) received pilot installations of solar facilities (for one classroom) toward the end of 2015, before textbooks, videos and incentive contracts were delivered in the beginning of 2016. See Seo (2016) and Seo (2017) for additional details. I take the stance that the test scores on February, 2016 (year 0) serve as pretest scores, given that these tests were administered before the incentive contracts, textbooks, and videos were delivered. This assumption means that the pilot solar exposure in October and November of 2015 did not affect the students' mathematics learning (December is a vacation month for these students). I test this assumption in table 1, and find that year 0 test scores are balanced on the treatment indicators.

[15] Results are robust to whether I use difference-in-difference specifications.

[16] X_i also includes randomization-block (five-region) indicators.

(Bertrand, Duflo and Mullainathan, 2004). I report both unadjusted significance levels, and Benjamini, Krieger and Yekutieli (2006) sharpened two-stage q-values (adjusted for three hypotheses) as described in Anderson (2008).

2.4 Reduced-form Results

Table 1 reports means of student characteristics and tests for their balance. It can be seen in panel A of table 1 that students were on average 15 years old and 56% female at the onset of the evaluation. Less than a quarter of primary guardians completed secondary school. Over two thirds of the parents engaged in farming or fishing, while less than a tenth of the parents engaged in technical or managerial occupations.[17] In contrast, only 1% of students desired to engage in farming or fishing; 93% of students desired technical or managerial occupations. Note that, as alluded to in section 2.1, approximately half of these students were expected to either drop out or fail the junior secondary pass examinations within three years in spite of these hopes.

In panel B of table 1, I see that 75% of students reported that their "intended area of focus" was Science (as opposed to Arts or Commerce, the two other tracks that students can elect to follow starting in grade 10). Despite such a stated preference for the Science track, only 5% passed the mathematics evaluation in February, 2016 (panel C). Due to random chance, age and commute distance were not balanced during randomization. Hence, I check robustness to including these as controls going forward.

As can also be seen in the balance tables, not every student responded to all questions, and for approximately 10% of students the control variables age (4.13%), commute distance (7.66%) and pretest score (0.06%) were missing. The missingness of these variables was balanced on treatment indicators (regression results omitted, here, though available upon request). I use multiple imputation to enhance the robustness of the results to missing data. Briefly, multiple imputation uses a regression-based procedure to generate multiple copies of the data set, each of which contains different estimates of the missing values. I relied the multiple imputation by chained equations algorithm using Stata's MI package to generate 10 imputed data sets. The imputation process included five variables (age, pretest score, commute distance, math study hours, and other study others) as well as seven auxiliary variables (female, STEM-occupation intended, class size and four region indicators). After creating the complete data sets, I estimated the multiple regres-sion models on each filled-in data set and subsequently used Rubin's (1987) formulas to combine the parameter estimates and standard errors into a single set of results. Note that methodologists currently regard multiple imputation as a "state of the art" missing data technique when the data is missing at random, because not only does it improve the accuracy and the power of the analyses relative to other missing data handling methods, but it also gives full consideration to every sample observation (Enders, 2010). All results are robust and implications do not change if I repeat the same analyses just dropping all observations with missing controls.[18] As discussed next, although controls were missing at random, attendance on subsequent examinations was not, requiring separate methods to deal with selection not at random.

Table 2 tests whether absences from the mock tests were balanced. Absences were substantial and selective. In year 1, as shown column (1), 80% of students showed up on the date of the follow-up evaluation. All three treatment groups (G1-G3) saw higher attendance than the control group. It is noteworthy that even though students in the Technology Only group (G2) were not promised any money rewards, more students showed up on the test day also from G2, suggesting that the program study aids were able to encourage students to at least show up more to the evaluation in year 1. The statistical significance of this effect disappears from G2 in subsequent years, however. In year 2, significantly more students showed up only from G1 and G3; in year 3, significantly more stu-

[17] Occupations were categorized by ISCO classifications during data entry based on free responses.

[18] Compare tables 3 to 5 and tables A3 to A5.

dents showed up only from G3.[19] Aggregate attendance fell to 67% and 66% in years 2 and 3. The especially sharp fall between years 1 and 2 was due to students no longer being enrolled in the same grade; reasons for no longer being enrolled—omitted from this table because of space constraints—include failing the 9th-grade promotional examination (12%), quitting school (6%), and transferring to a different school (4%).[20]

Columns (4) and (6) show that not only attendance on test day but also formal enrollment in school were higher among incentivized groups, in line with evidence from past literature that incentives raise enrollment (Barrera-Osorio et al., 2011). These higher enrollment statistics do not translate into higher rates of students graduating, however, showing that marginally incentivized enrollments do not help students complete school when a stringent graduation test stands in the way.

The substantial and selective attrition motivates the need to control for attrition. I mainly use the non-parametric control-function approach of Heckman (1990), using commute distance to instrument for attendance. I use the probit analogues of columns (1), (3) and (5) to form selection propensity scores for each student and year, and include a third-degree polynomial of these propensity scores alongside age and pretest scores as controls for selection.[21] Going forward, I report results from this "selection-corrected" specification, in addition to results from (1) specification without any controls, and (2) specification with the standard controls (pretest score—which is standard in education economics—as well as age and commute distance—variables of imbalance). Even though commute distance is my theoretically-preferred instrument for selection, the variable was not balanced because of random chance, a contextual weakness of this method. Yet, the results are robust not only to this check but also to various other imputation-based checks.[22]

Table 3 reports achievement impacts. In year 1, the estimated effects were noisy though suggestive: the highest coefficient ranged between 0.123σ and 0.183σ for the Both group. In years 2 and 3, the treatment effects were large and highly significant only for the Both group: the coefficients ranged between 0.280σ and 0.415σ for the Both group, while being much weaker and insignificant for the other groups.[23] Since weaker students were more likely to be absent, selection correction was expected to adjust the estimate upward if only the students who were present from the treatment groups were considered; however, selection correction also accounts for potentially weaker effects from absent students across the board. In aggregate, selection correction as well as controls ends up slightly attenuating the coefficients, but not by so much to influence the overall implications. In Panel C, I report linear combination of treatment effects; in particular, $\beta^3 - \beta^2 - \beta^1$ tests the complementarity between interventions G1 and G2. It is shown that the study was underpowered to detect the complementarity effect in this setting, although the estimated magnitudes are meaningful in years 2 and 3, ranging between 0.131σ and 0.186σ. These estimates are comparable to those from past works that have reported different combinations of "Incentives Only," "Technology/Inputs Only" or "Both" treatments targeted at students across various settings. As seen in table A1, results from past works, when collated together, suggest that the complementarity effect may be large. This work provides, to my knowledge, the first cleanly identified evidence on the magnitude of potential complementarity between student-level inputs, though underpowered.[24] In some specifications that drop observations with missing controls, the coefficients are significant (c.f. table A3).

[19] All regressions control for age, pretest score and commute distance; results are robust when the controls are dropped.

[20] Unlike the O Levels, the 9th-grade promotional examination can be retaken by repeating the grade.

[21] Results are robust to using polynomials of degrees one to ten.

[22] Methods I have tried include imputing zeros, imputing means, and predicting outcomes based on pretest score percentiles. Although these imputation-based methods are neither theoretically founded nor designed to address selective attrition, these other methods generate similar, qualitatively equivalent results, and are available upon request.

[23] This pattern of first-year results being weaker than subsequent-year results was also seen in Mbiti et al. (2019). See section 2.5 for further discussion.

[24] A clean identification of a complementary effect requires a full-factorial (fully interacted) experimental design; as reviewed by (Mbiti et al., 2019), such a design been rarely seen in the education economics literature, because of its difficulty of implementation.

Table 4 examines effects on reported hours per week of mathematics study. The impacts are aligned with those on achievement in that the impacts show up only for the Both group. Whereas the treatment effects were weak and insignificant for G1 and G3, the effects were strong and significant for G3, ranging from 1.0 additional hour per week in year 1 to 2.3 additional hours per week in year 3.[25] Although the complementarity effects are somewhat noisy, and the coefficient marginally significant only in some specifications with controls, the effects show up in economically meaningful manners across specifications in years 2 and 3: on the order of 1.3 hours per week, against the control mean of approximately six hours per week (c.f. table A4).

Table 5 reports performance effects in year 2 and year 3, disaggregated by pretest performance quintiles (following Glewwe, Kremer and Moulin 2009).[26] The interaction effects display a hump-shaped pattern across the pretest quintiles. In both years 2 and 3, the effects rise from none for the bottom two quintiles to reach approximately a third of a standard deviation for the fourth quintile, falling back down somewhat and becoming noisy for the top quintile. The pattern suggests that a significant mass of students toward the upper-middle of the pretest performance distribution are under-performing particularly because they are lacking not only in technologies that facilitate learning, but also in motivation to learn. Estimates becoming noisy for the top quintile (the pass threshold bites for students at approximately 90th percentile of the performance distribution) also suggest heterogeneous responses in this quintile (c.f. table A5).

Table 6 examines effects on proxies of reported usage of inputs beyond just the dimension of aggregate time: (1) hours of study in school after 6pm (proxying for usage of lights), (2) printed-materials usage (proxying for usage of books), (3) ICT and multimedia usage (proxying for usage of videos), and (4) total teacher hours. Effects are generally null for variables (1) and (4), suggesting that students may be able to find other means of sourcing light in school (kerosene, lamps, etc.). Teachers do not particularly increase teaching effort as proxied by teaching hours. Effects are large and significant for variables (2) and (3) in both G2 and G3 groups, but are not aligned with the performance effects in that they show up for G2 (not just for G3). I take these pieces of evidence as justifications for some of my modeling choices: effective student effort can be summarized by a uni-dimensional measure (e.g. hours of mathematics study); technologies can be summarized by a singular technology parameter; and teacher effort is orthogonal to the treatment interventions studied.

Table A10 reports effects on O Level mathematics certification examination (CSEE) grades and breakdown of the difference in results from that of year 3's mock test. In contrast to the mock test results whose values can range continuously from 0 to 100, CSEE grades are reported only in grade brackets: 0 to 29.5 marks translate into F; 30 to 44.5 marks into D; 45 to 64.5 marks into C; 65 to 74.5 marks into B; and 75 to 100 marks into A. For each student, I take the midpoint of their corresponding grade bracket range and convert these midpoints into Z-scores. Column (1) shows the result of regressing these Z-scores on the treatment indicators. No significant effect is seen.

The null effects stand in contrast to results seen on the mock test, shown in column (2), bracketed and reconverted to Z-scores in the analogous way. On the mock test, the Both group shows 0.166σ higher test scores, but on the real test, this difference reduces to null, suggesting that 40 percent of the difference between effects on the mock test and effects on the real test is attributable to lower resolution of the real test's grade brackets (given that at higher resolution the difference was 0.28σ). Columns (3) and (4) show this difference disaggregated into improvements among students whose scored higher on the O level than on the mock test, versus deteriorations among students who scored lower on the O level than on the mock test. Column (3) shows that approximately 20 percent of the difference between columns (1) and (2) is attributable to control students catching up; 40 percent, to erasing of knowledge gains from the both treatment group within the month-long interval between the mock

[25] The effects on G3 hours are significant even in year 1; however, G3 performance effects are too noisy to reject the null in year 1.

[26] Due to space constraints, year 1 results are omitted; year 1 results show trends that are similar but weaker and less precise than those from the other years.

test and the real test.

Columns (5)-(6) repeat the above analysis using pass indicators. On the top row of the table, between columns (5) and (6), it is shown that, among students in the control group, there is a fairly large pass-rate improvement of approximately three percentage points from the mock test to the real test. In contrast, G3 row shows that the pass rate falls by 4.7 percentage points from the mock test to the real test. Approximately 40% of the difference comes from fewer students improving from the mock test to the real test, and 60% from students deteriorating from the mock test to the real test.

2.5 Validity of Reduced-form Results

In this subsection, I discuss some threats to validity posed by (1) possibilities of cheating, (2) unusual differences between effects in year 1 and subsequent years, and (3) the difference between the mock test results and the final certification test outcome.

As for (1), cheating was not a concern in this study, as the mathematics examinations were difficult to cheat on: all questions were in free response format, and marks were given only to those students who demonstrate valid steps. In year 3, following Mbiti et al. (2019), I randomly provided five different versions of the examination to students. I did not inform the students that the test versions were varied. As seen in table 3, treatment effects remained essentially equivalent across years 2 and 3. I asked markers to indicate any student who attempted to copy answers from a version different from the student's own; less than 0.1% of students attempted to copy.

As for (2), I consider a few reasons why year 1 did not see any significant achievement gain while subsequent years did.

The first is that students may not have found the incentive contracts to be credible in year 1. This pattern of first-year results being weaker than subsequent-year results was also seen in Mbiti et al. (2019), who provided cash incentives to teachers in Tanzanian primary schools; they hypothesized that this was because teachers in the first year did not believe that cash would actually be provided in the first year. I hypothesize a similar reason in this setting. I find mixed evidence on this hypothesis. On the one hand, since students in incentives-support groups attended the tests with significantly higher probability, it could not have been the case that they thought the probability of payout was zero. Also, lacking belief in the contract is inconsistent with spending significantly higher number of hours on mathematics, as reported among the Both group. On the other hand, I find suggestive evidence across columns (1) and (6) of table A6 that, in year 1, the highest piece-rate groups did not attend the test at a significantly higher rate, while in year 2 they did, possibly suggesting lagged effects of belief "after seeing" rather than what the surveyors were announcing in the beginning of each year.

The second is that aversion to inequality in piece rates in year 1 may have discouraged some or all students in year 1; invidious effects of comparison may have hurt the intrinsic motivation of some students and demotivated them from learning. In columns (5) of table A6, I see that a nontrivial portions of students report that the differential piece rates were "demotivating" (as opposed to "motivating" or "did not matter"), and in column (9) I see that almost a fifth of students at least recognize that the piece rate promises were "unfair." Perhaps relatedly, I see in columns (3) and (8) that test scores were generally lowest in groups promised the lowest piece rate, though estimates are somewhat noisy.

The third is that the test in year 2 was too difficult, or relatedly, that the textbook in year 1 might not have been as well aligned with the test in year 1 than in subsequent years. The circumstantial evidence for this hypothesis is simply that the Both group studied for significantly more hours in all years, but the treatment effects are large and significant only post year 1. Perhaps making the discussion moot, even these effects disappear on the final year's O Level examination.

All of these reasons (as well as potentially others not discussed) may have contributed to the treatment effect being essentially null in year 1. For the structural estimation, I focus on estimating the model in year 2, taking year 0 and year 1 responses as predetermined characteristics.

As for (3): again, I do not see a large or meaningful effect on the actual O Level promotional mathematics test (0.02σ in test score and 0.1 p.p. in pass rate), even though the latter test covered equivalent curriculum subtopics and took place only a month after the incentivized mock test administered in the third year. As discussed, the government only provides access to five letter-grade brackets of the O Level test as opposed to the raw scores that can be seen on the mock test; less than 20 percent of students receive anything above the grade "F" on the O Level test, leaving little room for any improvement to be observed for the vast majority of students. Yet, there were fade out effects even among the top students, who in the both group had shown mock pass rate effect as high as 4.8 p.p. I consider circumstantial hypotheses of whether this discrepancy occurred due to (i) the distance between the mock test and the real test in terms of content and difficulty; (ii) control students catching up on the real test; (iii) lower resolution of real-test brackets; (iv)) a differential rate of cheating on the mock test than on the real test specific only to the both group; (v) the one-month lag.

First, the difference between the mock test result and the real test result does not seem to come from variability in test subtopic.[27] In table A7, I examine how maximum marks for each subtopic were allocated on the respective topics. Out of 38 subtopics (syllabus chapters) available across all form years, only 25 topics were asked on the two examinations, and 20 subtopics out of these were commonly asked. In fact, 80 out of the 100 total marks were asked on exactly the same topics of questions with exactly the same weights, and 10 to 16 marks were asked on the same topics just with a slightly different set of weights. Therefore, the two tests were remarkably similar.[28] Indeed, regression analysis (not reported here) of mock-test marks reweighted to match the real test weights (discarding the off-topic marks) predict equivalently large performance gain, suggestively ruling out at least subtopic variability as an explanation for the difference.

Second, the breakdown presented in table A10 suggests that control students being more motivated on the real exam and "catching up," may explain a fifth of the discrepancy.

Third, a large part of the discrepancy seems to have stemmed from the real test being much more crudely measured; that is, in grade brackets as opposed to continuous marks.[29] The both treatment effect in column (2) table A10, 0.210σ, is between 62% and 88% of the coefficient estimated in table 3. Therefore, it could be that between 12% to 38% of gains are being underestimated on the real test because of variation among failing students being unmeasured. Yet, the lower resolution does not explain why high-performing regressed.

Fourth, differential rates of cheating on the mock test than on the real test seems to be an unnatural explanation: this would mean that students in the both group were somehow able to cheat selectively more than students in the money-only group. Yet, these two groups were incentivized in the same manner.

Fifth, the one-month lag seems to have played a large role. Test anxiety on the real test may have differentially affected students who experienced performance gains on the mock test. Students who experienced performance gains on the mock test may have expended less effort during the month intervening the mock test and the real test, leading to relatively lower performance on the real test. Given that students in the both group report to have spent significantly more hours studying math throughout each year for all three years, all of these explanations would suggest knowledge gains from additional investments of effort induced by these treatments are vulnerable to being mismeasured when performance tests are at least a month apart.

[27] Links to online copies of these tests are provided in fig. B3.

[28] NECTA has been aware of the SMR project, and SMR mock questions were submitted to NECTA prior to test administration in all years. It may not have been a coincidence that the subtopics were distributed so similarly.

[29] It has not been possible to obtain the raw scores underlying the grade brackets awarded on the real test.

In all, the analyses suggest that control students catching up may account for 20% of the discrepancy; the lower resolution, 40%; and the one-month lag, 40%. The findings highlight how difficult it can be to measure performance when the resolution of the measurement is low; they also highlight the difficulty of the test faced by students in this setting, and the economic boundaries of policy-relevant treatments by which the students can and cannot be helped.[30] Yet, these analyses of validity do not threaten the main finding of performance gains caused by the interventions seen over the course of the program.

3 Conceptual Framework

I present a model of students who choose optimal effort, given (1) utility benefits to knowledge and (2) costs of effort, including (i) costs governed by the parameters of the knowledge production function that vary with effort and (ii) fixed costs motivated by learning-curve considerations.

3.1 The Knowledge Production Function

Let i denote a student taught by teacher j in year t. Student i chooses her level of effort, E_{it}, taking as given parameters that govern the productivity of her effort: $K_{i,t-1}$, her initial level of knowledge; A_{jt}, the ability of her teacher; R_{jt}, the effort of her teacher; δ_i, the "depreciation" rate of knowledge; and τ, the efficiency of commonly supplied educational technology (e.g. textbook).[31] The amount of knowledge she comes to possess at the end of year t is given by,

$$\begin{aligned} K_{it} &= (1-\delta_i)K_{i,t-1} + \left(\tau A_{jt}^{\gamma_0} R_{jt}^{\gamma_1}\right) K_{i,t-1}^{\alpha_0} E_{it}^{\alpha_1} \\ &= (1-\delta_i)K_{i,t-1} \times \left[1 + \left(\frac{1}{1-\delta_i}\tau A_{jt}^{\gamma_0} R_{jt}^{\gamma_1}\right) K_{i,t-1}^{\alpha_0 - 1} E_{it}^{\alpha_1}\right], \end{aligned} \tag{2}$$

where knowledge achieved equals $(1-\delta_i)K_{i,t-1} \geq 0$ if the student invests zero effort. The value-added specification takes the previous year's knowledge, $K_{i,t-1}$, as a sufficient statistic for student endowments and effort from all previous years.[32] This class of functions can also be written in a cumulative form in which end-of-year knowledge depends on all past inputs and endowments.[33]

[30] While previous works have reported that teacher impacts on test scores can "fade out" rapidly in subsequent grades, this paper uniquely shows that achievement gains accumulated over three years can fade out as rapidly as in one month.(Banerjee et al., 2007; Rothstein, 2010; Carrell and West, 2010; Jacob, Lefgren and Sims, 2010; Behrman et al., 2015; Mbiti, Romero and Schipper, 2019). Chetty, Friedman and Rockoff (2014) find, in their particular data, "fade-out and re-emergence" effects, whereby teachers' impacts on earnings are similar to predictions based on the cross-sectional correlation between earnings and contemporaneous test score gains, echoing findings of early childhood interventions (Deming, 2009; Heckman, Pinto and Savelyev, 2013; Chetty et al., 2011).

[31] The "depreciation" in a loose sense: capturing both how the total knowledge level might regress if not put to use, and how difficult it might be to transfer knowledge of content of the previous year to performance on this year's content. I allow δ_i to vary linearly with $K_{i,t-1}$ to reflect the notion that, for example, when the curriculum content is changing fast year to year, better performing students in the previous year have more performance level to lose by not studying this year, especially where test scores can commonly be zero.

[32] As noted by Todd and Wolpin (2018), this tradition dates back to at least Ben-Porath (1967) who, in eqs. (2) and (4), assumes $\dot{K}_t = \beta_0 \left(s_t K_t\right)^{\beta_1} D_t^{\beta_2} - \delta K_t$, where $\dot{K}_t$ represents the time (year) derivative of knowledge; s_t, the fraction of time spent learning; D_g, other inputs. Setting $\beta_0 D_t^{\beta_2} \equiv \tau A_{jt}^{\gamma_0} R_{jt}^{\gamma_1}$ and $s_t^{\beta_1} K_t^{\beta_1 - 1} \equiv K_{i,t-1}^{\alpha_0 - 1} E_{it}^{\alpha_1}$ leads to the equivalence between his model and this model.

[33] That is, $K_{it} = f(\boldsymbol{\eta}_i) \prod_{s=1}^{t} (1-\delta) \left[1 + \left(\frac{1}{1-\delta}\tau A_{js}^{\gamma_0} R_{js}^{\gamma_1}\right) K_{i,s-1}^{\alpha_0 - 1} E_{is}^{\alpha_1}\right]$, where $\boldsymbol{\eta}_i$ represents a vector of student endowments at time 0, and $f(\boldsymbol{\eta}_i)(1-\delta)^t$ what the student's knowledge would be at the end of year t if she were to invest zero effort each year.

3.2 The Student's Decision Problem

Consider a risk-neutral student i whose utility from knowledge net of effort cost is given by:

$$U_i(E_i) = \pi_i K_i + \sum_l \theta_l \mathbf{1}\{K_i + \epsilon_i^S \geq T_l\} - \frac{c}{p}(E_i)^p,$$

where $S_i = K_i + \epsilon_i^S$ is a test score that measures end-of-year knowledge with a normally distributed error term; T_l is the cutoff for letter grade l; θ_l is the student's utility benefit from letter grade l; π_i is the student's marginal utility of knowledge net of the certification value. I now focus my analysis on a single year, and drop the time subscript for notational convenience ($t \equiv 1$).

The student's decision problem is to maximize her expected utility,

$$\mathbf{E}\, U_i(E_i) = \pi_i K_i + \sum_l \theta_l \Phi\left(\frac{K_i - T_l}{\sigma^s}\right) - \frac{c_i}{p}(E_i)^p, \tag{3}$$

where $\Phi(\cdot)$ represents the cumulative distribution function (CDF) of a standard normal deviate.[34] Note that, because eq. (2) implies a one-to-one mapping between E_i and K_i, eq. (3) is invariant whether the student maximizes it with respect to E_i or K_i.

The decision problem with respect to K_i implies the first-order condition:

$$\underbrace{\pi_i + \sum_l \frac{\theta_l}{\sigma^S}\phi\left(\frac{K_i^* - T_l}{\sigma^S}\right)}_{\substack{\text{marginal benefit of} \\ \text{knowledge, } MB(K^*)}} = \underbrace{a(K_i^* - b)^\lambda}_{\substack{\text{marginal cost of} \\ \text{knowledge, } MC(K^*)}}, \tag{4}$$

where $\phi(\cdot)$ represents the probability density function (PDF) of a standard normal deviate. Note that, for simplicity of notation, I have let $a \equiv \frac{c_i}{\alpha_1}\left[\left(\tau A_j^{\gamma_0} R_j^{\gamma_1}\right) K_{i0}^{\alpha_0}\right]^{-\frac{p}{\alpha_1}}$, a parameter governing the scale of the marginal-knowledge-cost curve; $b \equiv (1 - \delta_i)K_{i0}$, a parameter governing the horizontal intercept of the marginal-cost curve; and $\lambda \equiv \frac{p - \alpha_1}{\alpha_1}$, a parameter governing the curvature of the marginal-cost curve.[35]

Going forward, I reduce the dimension of letter grades to one, for two reasons: (1) getting the lowest passing grade is one that matters for eligibility to training in higher-level technical and science-related subjects in this setting; (2) in my sample, there are only a very small mass of students for whom the higher-level cutoffs matter. In order to reflect the fact that the presence of higher letter grades may nevertheless act as continued motivators for these top students, I empirically consider a promotional chance value of the form: $\frac{\theta}{\sigma^S}\phi\left(\frac{\max\{K_i - T, 0\}}{\sigma^S}\right)$.

As seen, the left-hand side of eq. (4) is a constant plus bell curves (that are increasing with K_i), and the right-hand side is a power curve. The nonlinear marginal-benefit structure gives students who are closer to the letter-grade cutoff (T) higher motivational push than it gives students who are farther away from the threshold. Conversely, the structure demotivates students for whom the cutoff is set too far out of reach; these students would begin the year seeing little reason to invest effort in performance.

[34] By the 2-fold rotational symmetry of the standard normal CDF, $\Pr\left\{\epsilon_i^S \geq T - K_i\right\} = 1 - \Phi\left(\frac{T - K_i}{\sigma^s}\right) = \Phi\left(\frac{K_i - T}{\sigma^s}\right)$.

[35] Intuitively, the higher the productivity of student effort, the lower the scale parameter; the higher the output elasticity of student effort, the higher the curvature. The intercept represents the level of knowledge in the case of zero student effort.

Finally, I specify minimum productivity thresholds as introduced in the previous section: $\underline{\pi}$ and $\underline{K}_0$. That is,

$$E^*_{PT} = \begin{cases} E^*_{CD} & \text{if } \pi^*_{i,CD} \geq \underline{\pi} \text{ and } K_{0i} \geq \underline{K}_0 \\ 0 & \text{otherwise} \end{cases},$$

where $\pi^*_{i,CD} = MB(K^*_{CD})$, the marginal benefit from the benchmark case. Note that PT can be motivated from fixed costs of the form $g_i \mathbf{1}\{E > 0\}$.

I argue that the two thresholds provide a parsimonious way of capturing heterogeneous learning curves. In particular, $\underline{K}_0$ may be viewed as a basic entry requirement to making any improvement in the curriculum from the minimum score. Meanwhile, $\underline{\pi}$ may be viewed as entry requirements that are progressively higher for better performing students, governed by the condition $\mathbf{E}\,U(\pi_i, K_{0i}) > \mathbf{E}\,U(\underline{\pi}, K_{0i})$.[36] These situations may be likened to students learning the basics of algebra: $K_{0i} < \underline{K}_0$ may be likened to, for example, students knowing only addition and subtraction and not multiplication; $K_{0i} > \underline{K}_0, \pi^*_i < \underline{\pi}$ may be likened to students knowing multiplication as well, but still needing to incur some fixed effort costs to digest more complicated concepts in the curriculum (e.g. quadratic equations), in order to generate positive value added.

I discuss empirical identification of the model in section 4.5 and appendix C.

3.3 Mapping to Treatment Effects

The Incentive Only (G1) group can be thought of as receiving a shock to the net-utility parameter, $\pi_i \to \pi_i + \pi^{\$}\nu$, where ν represents the dollar amount of incentive per unit knowledge, and $\pi^{\$}$ represents utils per dollar. The Technology Only (G2) group can be thought of as receiving a shock both to the technology parameter and to the minimum-preparedness-threshold parameter: $\tau^{\text{cons}} \to \tau^{\text{cons}} + \tau^{\text{SMR}}$, and $\underline{K}_0^{\text{cons}} \to \underline{K}_0^{\text{cons}} + \underline{K}_0^{\text{SMR}}$, where "cons" stands for status-quo ("constant") level of technology and "SMR" stands for the paper's evaluated program, "Sharpening Mathematics Review." This latter assumption is to reflect the fact that providing more understandable books, such as bilingual books, reduce the threshold-level preparation required for learning the curriculum (Kremer, Miguel and Thornton, 2009). The Both (G3) group can be thought of as receiving positive shocks to all three parameters, $(\pi_i, \tau^{\text{cons}}, \underline{K}_0^{\text{cons}}) \to (\pi_i + \pi_i + \pi^{\$}\nu, \tau^{\text{cons}} + \tau^{\text{SMR}}, \underline{K}_0^{\text{cons}} + \underline{K}_0^{\text{SMR}})$.

3.4 Valuations and Welfare

This framework admits evaluation of utils in dollar terms based on revealed-preference theory. Students value certification at $\frac{\theta}{\pi^{\$}}$ dollars. The marginal value of knowledge net of the certification value is $\frac{\pi_i}{\pi^{\$}}$ dollars. These valuations jointly characterize students' willingness to pay for knowledge. Similarly, I can assess the value of each student's marginal hour of time. I can also compare and contrast the welfare increases caused by the treatment interventions, and evaluate the welfare increase caused by the interaction effect of incentive and technology. Total revealed-preferred student welfare is given by $\int \frac{1}{\pi^{\$}} \mathbf{E}\,U_i \,\mathrm{d}i$.[37]

3.5 Counterfactual Scenarios and Optimal Certification Threshold

In defining policy objectives, communities may have different preferences as to how to assign relative weights over different educational outcomes. A community may be interested in maximizing aggregate knowledge, $\int K_i \,\mathrm{d}i$.

[36] Clearly, $\mathbf{E}\,U(\underline{\pi}, K_{0i})$ is increasing in K_{0i}.

[37] Such linear translations into welfare terms are commonly seen in public economics. Carleton et al. (2018), for example, multiply value-of-statistical-life estimates with mortality effects of adaptation to climate change to evaluate mortality-specific economic benefits of adaptation.

A community may be interested in maximizing aggregate effort, $\int E_i \, \mathrm{d}i$.[38] A community may be interested in maximizing private welfare of students, $\int \mathbf{E}\, U_i \, \mathrm{d}i$.[39] In considering counterfactual certification policies below, I assume that the community's objective function is to maximize effort; analogous considerations could be made for alternative objectives.[40]

Recall that two key ideas of this paper are that (1) the extent to which students deem certification "attainable" may be a strong motivator of student performance, and that (2) educational technologies (or learning materials) may be strong complements to motivation. To the extent that (1) is important, reducing the threshold may be as powerful a motivator as providing cash incentives for test scores, especially for students toward the middle of the rising portion of the marginal benefits curve. Yet, to the extent that (2) is important, a policy of reducing the threshold (or otherwise making the certification test more accessible) may not lead to significant increases in effort, if students are also commonly lacking in the means by which to practice learning. Hence, a community's optimal certification policy may depend crucially on the level of educational technology commonly supplied in the community. That is, it may be important to consider,

$$\left\{T^*, \tau^*\right\} = \underset{\{T,\tau\}}{\arg\max} \int \mathbf{E}\, E_i^*(T,\tau)\, \mathrm{d}i - p_\tau \tau, \tag{5}$$

where p_τ stands for the supply cost of technology τ.

The assumption that θ remains constant in counterfactual scenarios warrants a discussion. I justify it based on two contextual reasons. First, in this setting, the margin of additional passing in mathematics is to come from those already expected to have secured seats at the A Level by passing the O Level via other subjects. Second, field interviews suggest that mathematics education at the A Level involves little teaching and mostly self- and group-study given printed materials, assuaging congestion concerns.[41]

A potentially meaningful corollary to this analysis is that a community may not be able to reap significant benefits from policies designed to make aspects of a certification process (content/curriculum/threshold) more "accessible," without also commonly supplying appropriate technologies to practice with (and vice versa). Across educational settings, there may be systemic barriers that make it difficult for communities to consider pulling both policy levers at the same time. Yet, an easy potential barrier to address may be perspective: holding technology fixed, a high threshold could seem optimal because lowering it might just mean passing more students who have not learned much; holding a high threshold fixed, supplying a more efficient learning technology might not generate much enthusiasm among students, leading to community inaction. Anticipating, however, I show that estimated empirical magnitudes of these implications are modest.

4 Structural Estimation

I rely on simulated-maximum-likelihood estimation, with intent to estimate three categories of structural parameters:

[38] There may be value apart from increasing knowledge in curricular content, for example, in getting students to practice following directions or collaborating with each other. Although there is a one-to-one mapping between knowledge and effort in this framework, individuals differ in how their effort maps to knowledge, and effort is also a convex function of knowledge. This implies that maximizing average knowledge would mean prioritizing higher-performing students, and maximizing average effort would mean prioritizing lower-performing students.

[39] If social returns to outcomes such as knowledge and effort exceed private returns, however, a community may prioritize these other outcomes over private welfare alone. For example, there may exist knowledge spillovers, and student discount rates may be lower than the community-wide discount rate.

[40] A related angle is to consider to which objective the government's current certification policy maps the closest.

[41] One reason why θ may fall with the number of students who pass is if there is a market response of wages to decreasing quality of certified labor. A possible scenario in which θ falls with T is that the government, because of seat constraints, randomly selects a fixed number who may proceed to higher learning among students who pass; hence, θ falls linearly with the number of students who pass.

(1) the parameters of the production and utility functions, as outlined above;

(2) the parameters of a latent-factor model, which specifies how endowments are being determined by exogenous initial conditions, some of which I assume are measured in survey responses and others (classroom- and individual-level unobservables) I draw from simulations;

(3) the parameters of a measurement-error model, which specifies how knowledge, effort and endowments are being measured.

I calculate the joint likelihood of observed measurement outcomes of each classroom j based on my assumption about the measurement-error distribution, taking the average across simulation draws.[42] I integrate the likelihoods of each classroom over the whole sample of classrooms, and then maximize the sample joint likelihood over the parameter vector space. The structural assumptions (2) and (3) provide a high degree of flexibility in accounting for uncertainties that may be inherent in student test-score and survey data.

I additionally model selection explicitly to account for attrition, and truncation explicitly to account for latent scores measured with binding bounds. I also model explicitly how the piece-rate incentive may differentially affect those with higher probabilities of getting the minimum score (0 marks in this setting), since only the part of a student's latent score above the minimum mark is awarded the linear piece-rate incentive.

4.1 Latent Factor Structure

I assume student endowments, $\boldsymbol{\eta}_{ij} = \{K_{i0}, \pi_i, A_j, R_j\}$, are latent factors measured with error.[43] Each factor $\eta_{ij} \in \boldsymbol{\eta}_{ij}$ depends on a set of exogenous initial conditions, $\boldsymbol{X}_{ij}^{\eta}$, and unobserved classroom- and individual-difference components, μ_j^{η} and ω_{ij}^{η}:

$$K_{ij0} = \boldsymbol{X}_{ij}^{K_0} \boldsymbol{\beta}^{K_0} + \mu_j^{K_0} + \omega_{ij}^{K_0}, \tag{6}$$

$$\pi_{ij} = \boldsymbol{X}_{ij}^{\pi} \boldsymbol{\beta}^{\pi} + \mu_j^{\pi} + \omega_{ij}^{\pi} + \pi^{\$} \times (G_i^1 + G_i^3), \tag{7}$$

$$A_j = \boldsymbol{X}_j^{A} \boldsymbol{\beta}^{A} + \mu_j^{A}, \tag{8}$$

$$R_j = \boldsymbol{X}_j^{R} \boldsymbol{\beta}^{R} + \mu_j^{R}. \tag{9}$$

All difference components are assumed to be mean zero, orthogonal to each other and orthogonal to observed characteristics, for a given endowment. Across endowments, the difference components may be freely correlated.[44] Note that in eq. (7), $\pi^{\$}$ identifies the per-mark utility benefit of the experimental piece-rate incentive.

4.2 Truncation and Selection Structures

In order to account for truncation, I modify eq. (3) in the following way.

$$\mathbf{E}\, U_i(E_i) = \pi_i K_i + \pi^{\$}(K_i - K^{\min})\Phi\left(\frac{K_i - K^{\min}}{\sigma^{\mathrm{s}}}\right) + \pi^{\$}\sigma^{\mathrm{s}}\phi\left(\frac{K_i - K^{\min}}{\sigma^{\mathrm{s}}}\right) + \theta\,\Phi\left(\frac{K_i - T}{\sigma^{s}}\right) - \frac{a}{\lambda+1}(K_i - b)^{\lambda+1}, \tag{10}$$

where the first of the two added terms describes utility benefit of SMR incentives scaled by $\Pr\{S_i > K^{\min}\}$, and the second term describes the expected truncation bonus.[45]

[42] Classroom j and teacher j are equivalent in this setting.

[43] Assuming no student gets negative utility from knowledge, I impose that the latent factors are bounded from below by zero.

[44] In implementation, I do not allow for difference components at some levels to conserve on parameters.

[45] If $S_i < K^{\min}$, the student gets $\pi_i K_i$. If $S_i > K^{\min}$, the student gets $\pi_i K_i + \pi^{\$}(S_i - K^{\min})$.

The modification of eq. (4) follows accordingly:

$$\pi_i + \pi^{\$}\Phi\left(\frac{K_i - K^{\min}}{\sigma^{s}}\right) + \frac{\theta}{\sigma^S}\phi\left(\frac{K_i^* - T}{\sigma^S}\right) = a(K_i^* - b)^{\lambda}. \tag{11}$$

Selection is modeled in the following manner. Students are assumed to be required to pay a random test attendance cost:

$$\zeta_{ij} = \boldsymbol{X}^{\zeta}_{ij}\boldsymbol{\beta}^{\zeta} + \epsilon^{\zeta}_{ij}, \tag{12}$$

where ϵ^{ζ}_{ij} is assumed to be an independent standard normal. If a student does not attend, the student reduces learning by ι, modeled as a portion of effort. On test day, student attends if

$$[\mathbf{E}\,U_i(E_i^*) - \mathbf{E}\,U_i(E_i^* - \iota)] \times \beta^{\text{Udiff}} > \zeta_i, \tag{13}$$

and avoids the test otherwise. While a student's test score outcome is missing if the student is absent, the data includes ϵ^{ζ}_{ij} from the Year 0 survey, allowing this method to work. Therefore, the likelihood of observing an absent student's observation is given by $\Phi(\zeta_i - [\mathbf{E}\,U_i(E_i^*) - \mathbf{E}\,U_i(E_i^* - \iota)] \times \beta^{\text{Udiff}})$, and the likelihood of observing a present student's observation is scaled by $1 - \Phi(\zeta_i - [\mathbf{E}\,U_i(E_i^*) - \mathbf{E}\,U_i(E_i^* - \iota)] \times \beta^{\text{Udiff}})$.

4.3 Measurement Structure

Given $m = 1, \ldots, M^{\eta}$ measures for each latent factor η, I assume measurement equations given by,

$$K^m_{ij0} = \beta_0^{K_0,m} + \beta_1^{K_0,m} K_{ij0} + \epsilon_{ij}^{K_0,m}, \quad m = 1, \ldots, M^{K_0}, \tag{14}$$

$$\pi^m_{ij} = \beta_0^{\pi,m} + \beta_1^{\pi,m}\pi_{ij} + \epsilon_{ij}^{\pi,m}, \quad m = 1, \ldots, M^{\pi}, \tag{15}$$

$$A^m_{j} = \beta_0^{A,m} + \beta_1^{A,m} A_j + \epsilon_{j}^{A,m}, \quad m = 1, \ldots, M^{A}, \tag{16}$$

$$R^m_{j} = \beta_0^{R,m} + \beta_1^{R,m} R_j + \epsilon_{j}^{R,m}, \quad m = 1, \ldots, M^{R}. \tag{17}$$

All measurement errors are assumed to be uncorrelated with all of the latent variables (both observed and unobserved components) and with each other.

I also treat student effort as a latent variable measured with error.[46] Given M^E measures of student effort, the effort measurement equation is given by,

$$E^m_{ij} = \beta_0^{E,m} + \beta_1^{E,m} E_{ij} + \epsilon_{ij}^{E,m}, \quad m = 1, \ldots, M^{E}. \tag{18}$$

Students determine their levels of effort by solving the knowledge decision problem (eq. (3)), which is fully determined by the latent endowments and fixed production-function parameters.

I additionally estimate a location parameter of latent student effort, one that may be interpreted as reporting or measurement bias. That is, I estimate $\underline{E}$ in the relationship,

$$E^{\text{latent}}_{ij} = \underline{E} + E^*_{ij} + \epsilon^{E^{\text{latent}}}_{ij}. \tag{19}$$

[46] This methodology allows us to reduce the dimension of the effective input space to one; see Cunha and Heckman (2008) for further theoretical discussion. See section 5, table 4 and table 6, for evidence that hours of mathematics study provides a singular measure of effort that exhibits movements closely aligned with those of the performance outcomes, while alternative candidate inputs that students may be making choices over do not exhibit movements aligned with those of the performance outcomes.

Several interpretations may apply to $\underline{E}$. If positive, the implication may be that students overstate their effort, or that this part of effort represents "unproductive" hours: for example, students report substantial percentages of time in response to such questions as "what percentage of this mathematics study time were you paying attention and not copying from friends?" If negative, the implication may be that students are spending additional time of study elsewhere: for example, the SMR survey in year 2 queried student's hours of self-study for a measure of effort, yet students also spend required class hours learning mathematics and also engage in group studies.

End-of-year knowledge is a latent variable measured with error by end-of-year test marks, S_{ij}:

$$S_{ij} = K_{ij} + \epsilon^{S}_{ij}, \tag{20}$$

where K_{ij} is determined by eq. (2).

A measurement outcome consists of (i) test marks, measures of effort levels and measures of initial knowledge and preferences for each student, and (ii) measures of effort levels and ability of teachers. A measurement outcome is moreover conditioned by absence indicators, where present outcomes are missing (but not past outcomes) for absent students. I denote the set of measurement outcomes for classroom j as:

$$O^{M}_{j} = \left\{ S_{ij}, E^{\boldsymbol{m}}_{ij}, K^{\boldsymbol{m}}_{ij0}, \pi^{\boldsymbol{m}}_{ij}, A^{\boldsymbol{m}}_{j}, R^{\boldsymbol{m}}_{j}, Absence_{ij} : i = 1, \ldots, N_j \right\}. \tag{21}$$

I additionally denote the set of observable determinants of latent endowments as $O^X = \left\{ \boldsymbol{X}^{\boldsymbol{\eta}}_{ij} \right\}$.

I assume that the vector of unobservables and measurement errors are jointly normal. Specifically, let $\boldsymbol{\upsilon}^{\mu} = \left\{ \mu^{\boldsymbol{\eta}}_{j} \right\}$ represent the vector of classroom-level observables, $\boldsymbol{\upsilon}^{\omega} = \left\{ \omega^{K_0}_{ij}, \omega^{\pi}_{ij} \right\}$ the vector of student-level unobservables, and $\boldsymbol{\upsilon}^{\epsilon} = \left\{ \epsilon^{\boldsymbol{\eta},\boldsymbol{m}}_{ij}, \epsilon^{E,\boldsymbol{m}}_{ij}, \epsilon^{S}_{ij} \right\}$ the vector of measurement errors. The unobservables $\boldsymbol{\upsilon}^{\mu}$ and $\boldsymbol{\upsilon}^{\omega}$ have joint distributions F_{μ} and F_{ω}, assumed to be normal with variance-covariance matrices Σ_{μ} and Σ_{ω}. The measurement errors $\boldsymbol{\upsilon}^{\epsilon}$ have a joint distribution F_{ϵ}, assumed to be normal with a diagonal variance-covariance matrix Σ_{ϵ}.

4.4 Likelihood Function

Estimation is carried out by simulated maximum likelihood. The likelihood contribution of classroom j is the joint density of O_j. The estimation routine proceeds as follows:

1. Guess a vector of parameters, $\{\alpha_0, \alpha_1, \gamma_0, \gamma_1, \boldsymbol{\delta}, \tau^{\text{cons}}, \tau^{\text{SMR}}, \pi^{\$}, \theta, \boldsymbol{\beta}^{\boldsymbol{\eta}}, \Sigma_{\mu}, \Sigma_{\omega}, \Sigma_{\epsilon}, \underline{E}, \underline{\pi}, \underline{K}_0, \underline{K}^{SMR}_0, \iota, B^{\boldsymbol{m}}\}$, where $B^{\boldsymbol{m}}$ denotes the set of $\beta^{\boldsymbol{\eta},\boldsymbol{m}}_0$ and $\beta^{\boldsymbol{\eta},\boldsymbol{m}}_1$ parameters of the measurement error equations.
2. Draw shocks $\boldsymbol{\upsilon}^{\mu}$ and $\boldsymbol{\upsilon}^{\omega}$ for all students $i = 1, \ldots, N_j$ and classrooms $j = 1, \ldots, J$.
3. Given the shocks and observed determinants O^X, compute the values of endowments $\boldsymbol{\eta}_{ij}$.[47]
4. Compute K^* and E^* for all students in all classrooms for all draws $d = 1, \ldots, D$.[48]
5. For each draw d, given the joint measurement-error distribution F^{ϵ}, calculate the joint likelihood of the measurement outcome O^M_j.[49] Denote this joint density as $f_j(d)$.
6. Compute the mean value of the joint density across draws: $\mathcal{L}_j = \frac{1}{D} \sum_d f_j(d)$.
7. Repeat for all $j = 1, \ldots, J$ classrooms. The likelihood of the entire sample is $\prod^{J}_{j=1} \mathcal{L}_j$.
8. Repeat steps 1 through 7, maximizing the sample likelihood over the space of parameter vectors.

[47] I set to zero any latent factor that is negative.

[48] It is computationally more efficient to search for K^* first and then back out E^* by inverting eq. (2), a technical innovation.

[49] Some survey measures are continuous, some ordered-categorical, some dichotomous, and all bounded. In all cases, I assume a continuous latent measure that underlies the observed measure; I treat the bounded measures as truncated, the dichotomous variables as probits, and ordered-categorical variables as ordered probits.

4.5 Identification

An innovation in this paper is demonstrating empirical identification even in the case of nonlinear returns to knowledge given the possibility of certification. Apart from the innovation, identification is via Todd and Wolpin (2018) and Cunha, Heckman and Schennach (2010). I discuss identification separately for the parameters of the production function, and the parameters in the latent-factor and measurement-error equations.

First, suppose I have perfect measurements of A_j, R_j, K_{i0} and E_{ij}. Combining eq. (20) and eq. (2) gives us:

$$S_{ij} = (1-\delta_i)K_{i0} \times \left[1 + \left(\frac{1}{1-\delta}\tau A_j^{\gamma_0} R_j^{\gamma_1}\right) K_{i0}^{(\alpha_0-1)} E_i^{\alpha_1}\right] + \epsilon_{ij}^S.$$

Given that the only student-level unobservable is the test-score measurement error, which is assumed to be orthogonal to the determinants of K_i, identification of the production function parameters follows immediately from independent variations in the perfect measurements.

I do not assume to have perfect measurements of A_j, R_j, K_{i0} and E_{ij}; however, with multiple measures, this framework folds into a special case of Theorem 2 in Cunha, Heckman and Schennach (2010). The measurement equations of section 4.3, together with the implicit determination of E^* as an argument optimum of the student's decision problem, correspond to the system of nonlinear measurement equations given by (3.7) in Cunha, Heckman and Schennach (2010). Given the orthogonality conditions, the parametric forms of the measurement equations, and distributional assumptions about the measurement errors, I can invoke their theorem to identify the production function parameters.

I can easily identify the parameters in the latent-factor and measurement-error equations, based on the linearity of the equations and orthogonality of the unobserved terms. For illustration, consider two measures of a latent factor η, with measurement equations,

$$\eta_{ij}^{m1} = \eta_{ij} + \epsilon^{\eta,m1}, \tag{22}$$

$$\eta_{ij}^{m2} = \gamma_0^{\eta,m2} + \gamma_1^{\eta,m2}\eta_{ij} + \epsilon^{\eta,m2}, \tag{23}$$

where, without loss of generality, I allow for only a student-level unobservable. Note the normalization $\gamma_0^{\eta,m1} = 0$ and $\gamma_1^{\eta,m1} = 1$, which establishes $m1$ as the metric of η_{ij}. The latent-factor equation is given by,

$$\eta_{ij} = \gamma_0^\eta + \gamma_1^\eta X_{ij}^\eta + \omega_{ij}^\eta, \tag{24}$$

where, wit=hout loss of generality, I assume X_{ij}^η is scalar; there is only a student-level unobservable; and all unobservables are orthogonal to each other and to X_{ij}^η. Note that γ_0^η and γ_0^η are identified upon regressing η_{ij}^{m1} on X_{ij}^η after substituting eq. (24) into eq. (22). The factor loading in the second measurement equation, $\gamma_1^{\eta,m2}$, is given by $\text{Cov}\left(\eta_{ij}^{m2}, X_{ij}^\eta\right) \Big/ \left[\gamma_1^\eta \text{Var}\left(X_{ij}^\eta\right)\right]$; the location parameter, $\gamma_0^{\eta,m2}$, is then identified by passing the line through the means. The variance of the unobservable, $\text{Var}\left(\omega_{ij}^\eta\right)$, is derived from the covariance between the two measurements, $\text{Cov}\left(\eta_{ij}^{m1}, \eta_{ij}^{m2}\right)$.[50] The measurement error variances are derived from the variances of the measures. The same argument applies to all other measures.

To complete the identification argument, I address the last remaining threat, which is that the nonlinear marginal benefit of knowledge may admit multiple levels of knowledge that deliver the same utility. My response is that the curvatures of the marginal-benefit and marginal-cost curves imply that they generally cross each other at a unique

[50] $\text{Cov}\left(\eta_{ij}^{m1}, \eta_{ij}^{m2}\right) = \gamma_1^{\eta,m2}\text{Var}\left(\eta_{ij}\right) = \left(\gamma_1^\eta\right)^2 \text{Var}\left(X_{ij}^\eta\right) + \text{Var}\left(\omega_{ij}^\eta\right)$.

optimum; though it is true that the two curves can cross each other at up to three points, even in cases of multiple intersections, the level of utility is generally the highest at a unique point, and dual maxima occur only in a degenerate case of measure zero when the latent factors are being drawn from a continuous distribution. See appendix C for a proof.

4.6 Empirical Determinants and Measures

For the structural estimation, I estimate the model in year 2, taking year 0 and year 1 responses as predetermined characteristics.[51] The full list of determinants and measures can be seen in table A8 and table A9. Many of these choices follow Todd and Wolpin (2018). I estimate two models: the model benchmark model without productivity thresholds, and the proposed model with productivity thresholds.

Items 20 through 31 list the variable determinants assumed for $K0$. These include female indicator; year 0 math score; age; FTNA nonmath average; and parental education (high school or above).

Items 33 through 36 list the variable determinants assumed for π_0. These include female indicator; parental education; FTNA nonmath average; and whether the students' reported desired job was a STEM occupation.

Items 38 through 45 list the variable determinants assumed for A_j. These include being a full-time math teacher (as opposed to part-time or substitute teacher whose specialty is another subject); has bachelor's degree; number of years taught math; number of years taught math squared.

Items 47 through 51 list the variable determinants assumed for R_j. These include the total number of teaching hours per week; being a full-time math teacher (as opposed to part-time or substitute teacher whose specialty is another subject); has bachelor's degree; number of years taught math; number of years taught math squared.

Items 53 through 57 list the variable determinants assumed for attendance probability. These include the utility difference between attending and dodging; parental education (high school or above); female; age; parental education and commute distance (which serves as an instrument for selection correction).

Items 68 through 95 indicate measures used for the latent factors. $K0$ has two measures: year 1 math score; and FTNA math grade. π_0 has two measures: year 0 reported hours of study of non-math subjects; and year 0 reported degree to which student likes math (a categorical ordered from 1 ("never") to 4 ("always")). A_j has two measures: the proportion of students in class reporting that the teacher "always knows the subject"; the proportion reporting that the teacher "always has control of class." R_j has two measures: the proportion of students in class reporting that the teacher "always cares that the student learn"; the proportion reporting that the teacher "always cares that the students pay attention." E has three measures: year 2 reported hours of math study; year 2 reported percentage of attention paid on homework; and year 2 reported degree of effort expended on the test (a categorical ordered from 1 ("low") to 4 ("high")).

All test marks are converted to scaled scores with mean 500 and standard deviation 100, following the convention of many previous empirical works, as well as international educational authorities such as Program for International Student Assessment (PISA) and the Southern and Eastern Africa Consortium for Monitoring Educational Quality (SACMEQ).

[51] A key condition for identification—two or more independent measures of previous year test scores—is not met in years 1 or 3.

5 Structural Estimation and Simulation Results

5.1 Parameter Estimates across Models

Table 7 reports selected parameters and simulated treatment effects compared against the data. Table 8 reports valuation estimates. Model 1 refers to the benchmark model. Model 2 refers to the model with productivity thresholds.

Panel A reports estimated production function parameters. α_0, the output elasticity of knowledge endowment, is large; that this parameter is greater than 1 was also seen in Todd and Wolpin (2018). In contrast, α_1, the output elasticity of effort, is estimated to be one seventh of α_0. Teacher ability, measured at least within the range of variation observed in the data, is also small, and more or less zero in model 2; teacher effort is not predicted to matter within the range of variation observed in this setting.

The baseline "depreciation" rate, $\delta^{\text{cons}} = 0.148$, is estimated to be large in model 1; small in model 2.[52] In both models, The depreciation rate is seen to fall as performance level rises: for each 100 rise in scaled score (1 standard deviation) from the minimum score, model 1 predicts depreciation falls by 0.0121; model 2 predicts a fall by 0.0333.[53] Interestingly, model 1 predicts a large depreciation rate even for the bottom students while model 2 does not leads to a crucial difference in value added predicted for students between these models. Model 1 simulations suggest that control-group students are gaining an average knowledge of 0.65σ in one year, and even those from the bottom pretest quintile (those getting average percentage marks of three out of hundred) are gaining 0.53σ of knowledge in one year—almost three and a half years' worth of progress according to nationally-normed scales in other contexts.[54] In contrast, model 2 simulations suggest that control-group students are gaining 0.07σ of knowledge throughout the year and bottom students are gaining 0.0003σ of knowledge, perhaps more consistent with the empirically observed patterns that students from the bottom quintile consistently score around 3 marks out of 100 year to year and less than 0.003% passed the final O level mathematics examination.

The SMR technology is seen to improve value-added factor productivity (τ) by 15% in model 1, and 35% in model 2. Anticipating, this leads model 1 to overestimate the effect of technology on average treatment effect and predict positive effects even for students below the top inconsistent with the observed treatment effects, while leading model 2 to better match the effect of technology on top students only given the productivity thresholds.

Model 1 ascribes a lower marginal utility of income (4.79) than model 2 (14.1). As can be seen in the second row of table 8, this leads model 1 to ascribe a value of $13.2 per 10 scaled-score units, while model 2 to ascribe a value of $4.20. π_{scale}, a scale parameter included to potentially control for scale bias in reported measures for π_i, varies within 10% of each other across models. The value of promotion is estimated to be much larger in model 1 ($289) than model 2 ($26.6). Model 1 ascribes a lower cost elasticity of effort, lower location bias (or amount of unproductive effort), and lower noise, consistently predicting higher productivity and efficiency of the system than model 1.

At the bottom of panel A, I report the proportion of students model 2 attributes as being either "disinterested" ($\pi_i < \underline{\pi}$); "lost" ($K_{0i} < \underline{K}_0$); both disinterested and lost; and neither. Only 12.8% of students are seen to be meaningfully accumulating knowledge in model 2, whereas in model 1, by construction, every student is equating her marginal cost of effort to a highly estimated marginal benefit of knowledge.

[52] Again, this is "depreciation" in a loose sense: capturing both how the total knowledge level might regress if not put to use and how difficult it might be to transfer knowledge of content of the previous year to performance on this year's content.

[53] For top students (e.g. five standard deviations above the minimum score), this means that a standard deviation gain in knowledge endowment halves in less than four years, suggesting that even investments at younger ages would not remain effective if only temporarily applied.

[54] An often-targeted benchmark of success in educational interventions is 0.1σ. In an analysis of nationally-normed tests in the US (originally adapted from Hill et al. (2008)), Lipsey et al. (2012) note that students typically gained an average achievement of 0.16σ across reading, math, science and social studies over their 11th-grade academic year.

In panel B, column (Data) reproduces estimates from column (6) of table 3, and the bottom row of table 5. Columns (1) and (2) report differences in simulated means across treatment groups, leaving the original balance of student characteristics across treatment groups intact. Columns (1') and (2') report differences in simulated means across treatments, assuming that the whole sample gets shocked with each treatment. Although both models underhit the magnitudes of treatment effects seen in the reduced-form data, model 2 consistently generates the patterns of effects better than model 1. Strikingly, in column (1'), the interaction effects are seen to be off by two to three orders of magnitude; essentially, model 1 does not allow for any meaningful interaction effect to occur. In contrast, though attenuated, model 2 generates the observed pattern of hump-shaped dynamics in the interaction effects with respect to the students' pretest performance levels.

In panel C, columns (2) and (2') report differences in simulated proportions of students who are gaining knowledgeâĂŤi.e., those who are neither disinterested nor lostâĂŤacross treatments, providing a clear intuition for how the treatment effect and hump-shaped patterns were generated. In particular, the both group is seen to have raised the proportion of students meaningfully accumulating by 20 p.p., generating the patterns of average and heterogeneous interaction effects.

Panel A of table 8 reports model-based valuation estimates for each considered educational good. At the bottom of each horizontal panel, it can be seen that model 1 consistently predicts net welfare loss from the interaction effect (more cost effective to just give books than to give both books and incentivize students), while model 2 consistently predicts a welfare gain, intuitively from students crossing the productivity thresholds.

The top of panel B of table 8 reports mean effort cost estimates per 0.01σ, 0.10σ, and 0.2σ of performance gain; what happens given the technology (G2) shock; and mean program-treatment cost estimates. The effort costs show a highly convex and inelastic structure in both models, but because everyone is pushing themselves to the inelastic margin in model 1, the cost estimates in model 1 are much higher.

The rest of the 82 parameters estimated are reported in table A8. Standard errors are works in progress.

5.2 Counterfactual Simulation Results

Figure 4 plots simulated educational outcomes from counterfactual promotion cutoffs, given a constant θ scenario.

Again, promotion cutoffs are minimum absolute marks of test scores a student must achieve in order to have the option of continuing to study mathematics beyond the O Levels. Plots indicate model-simulated outcomes, *conditional on distributing the experimental technology nationally*, from a counterfactual policy of lowering the current promotion cutoff (indicated in red and also labeled "T: 29.50") to a hypothetical cutoff (indicated in green) simulated to double the number of students passing in equilibrium. Plotted are nationally allocated estimates, given that each experimental sample observation is weighted nationally using the ratio between a Weibull density fitted to FTNA grade distribution and the density of the experimental sample's year 1 marks. "Share Accumulating Knowledge" refers to the proportion of students who are neither disinterested nor lost, and therefore meaningfully accumulating knowledge during the year.

As seen in the "Promotion Rate" plot, lowering the absolute mark from 29.5 to 19.5 (model 1) and to 20.5 (model 2) would double the endogenous proportion of students passing the test; the mark is lower for model 1 because there is less scope of endogenous response in model 1 where incentives have limited effects.

As seen in the "Knowledge" plot, in model 1, such a policy change would have a negligible, 0.004σ effect on knowledge, despite model 1 attributing a much greater monetary value to passing. This is because model 1 predicts that all students are pushing themselves to a highly inelastic portion of the knowledge cost curve and are leaving no effort on the table. Model 2, on the other hand, predicts that such a policy change would have a modest but meaningful effect on endogenous response of student knowledge, on the order of 11 scaled-score points (0.11σ), by raising the share of students meaningfully accumulating knowledge by 20 p.p.

6 Conclusion

This paper presented an economic framework for analyzing how students' cost-benefit consideration affects educational outcomes, using the method to rationalize data from a unique field experiment that was designed to assess the performance of Tanzania's secondary mathematics education system.

The field experiment tested to what extent the lack of student interest, the lack of basic inputs, or the lack of both thereof might explain the system's performance. I find that neither performance-based incentives, nor free solar-energy access, nor bilingual textbooks, nor videos by themselves could lead to a meaningful performance gain. Providing all these inputs together, on the other hand, showed strong and significant impacts on year-to-year incentivized mock tests over a period of three years (0.3σ), suggesting that a substantial number of students at baseline were lacking in both interest and requisite learning support.

I then presented results of structural estimates of the students' cost-benefit considerations. In the model, heterogeneous students balance the cost of effort against the benefit of knowledge, given (i) probabilistic distance to the promotional cutoff, and (ii) preference for knowledge net of the distance value. On the cost side, I tested two simple specifications: model 1, which has a convex variable effort; and model 2, which has not only a convex variable effort, but also threshold costs which are functions of minimum interest and minimum preparedness thresholds that may prevent some students from even starting to study. Comparisons of model-generated treatment effects against reduced-form treatment effects (untargeted moments) show that model 2 generates key treatment effect patterns, while model 1 cannot, attributing a higher degree of realism to model 2.

Based on model 2, simulations of a counterfactual policy of *both* providing the experimental technologies to students *and* lowering the promotional cutoff to encourage more students to learn suggest that this policy would lead to modest but meaningful gains in endogenous responses of student knowledge, by reducing the share of students who are giving up on learning at the margin from 79% to 52%. By explaining treatment complementarities, the proposed model extends a standard model of classroom learning from the previous literature to reflect a higher degree of realism about developing community contexts.

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Tables

Table 1: Balance of Student Characteristics

	(1)	(2)	(3)	(4)	(5)
Statistic:	Control Mean	G1 - C	G2 - C	G3 - C	N
(Statistic:)	*(Control Sd.)*	*(Se.)*	*(Se.)*	*(Se.)*	*(%missing)*
A. Basic Demographics and Household Background, Feb. '16					
Female indicator	0.555	0.008	-0.023	0.019	6201
	(0.497)	(0.026)	(0.028)	(0.023)	(0)
Age	15.033	-0.142	-0.169**	-0.253***	5945
	(1.268)	(0.087)	(0.078)	(0.075)	(.0413)
Commute Distance (km)	3.189	-0.888***	-0.418	-0.387	5726
	(3.929)	(0.339)	(0.380)	(0.529)	(.0766)
Boarding student indicator	0.064	-0.053**	0.047	-0.001	6165
	(0.245)	(0.026)	(0.051)	(0.033)	(.00581)
Household has grid power	0.237	-0.019	0.000	0.012	6198
	(0.426)	(0.039)	(0.040)	(0.040)	(.000484)
Primary guardian finished secondary school or above	0.229	0.010	0.018	0.014	6159
	(0.420)	(0.025)	(0.024)	(0.023)	(.00677)
Primary guardian's occupation: Farming or Fishing	0.703	-0.042	-0.012	-0.033	5992
	(0.457)	(0.039)	(0.034)	(0.040)	(.0337)
Primary guardian's occupation: Technical or Managerial	0.097	-0.007	-0.024	-0.014	5992
	(0.296)	(0.017)	(0.015)	(0.015)	(.0337)
B. Educational Outlook, Preferences and Investments, Feb. '16					
Future occupation aimed for: Farming or Fishing	0.011	-0.004	-0.003	-0.004	5881
	(0.102)	(0.004)	(0.004)	(0.004)	(.0516)
Future occupation aimed for: Technical or Managerial	0.933	0.004	0.011	0.002	5881
	(0.251)	(0.014)	(0.013)	(0.013)	(.0516)
Intended area of focus: Science (not Arts or Commerce)	0.752	0.010	0.035	0.028	6127
	(0.432)	(0.027)	(0.027)	(0.027)	(.0119)
Likes math: Always (4 on a scale of 4)	0.604	0.018	-0.016	0.085*	6147
	(0.489)	(0.049)	(0.054)	(0.051)	(.00871)
Average Textbook Ownership (Nonmath Subjects)	0.071	-0.001	-0.015	-0.025	6198
	(0.141)	(0.017)	(0.016)	(0.015)	(.000484)
Textbook Ownership (Mathematics)	0.118	-0.008	-0.013	-0.038	6198
	(0.322)	(0.033)	(0.033)	(0.027)	(.000484)
Hrs/wk of non-math study after regular class hours	5.610	-0.194	-0.148	0.056	6139
	(2.630)	(0.262)	(0.240)	(0.252)	(.01)
Hrs/wk of math study after regular class hours	3.981	-0.079	-0.032	0.106	6132
	(2.586)	(0.220)	(0.220)	(0.230)	(.0111)
C. Mathematics Examination Results, Feb. '16					
Normalized mathematics marks, Feb. '16	-0.064	0.083	0.066	0.082	6197
	(0.924)	(0.079)	(0.103)	(0.089)	(.000645)
Pass rate (got 29.5 marks or above), Feb. '16	0.061	0.012	0.019	0.020	6197
	(0.240)	(0.015)	(0.020)	(0.016)	(.000645)

Note: Each row of coefficients from a regression of the row variable on three treatment indicators (G1-G3). Regressions include randomization-block (five-region) fixed effects. Reported in parentheses: standard errors clustered at the school level. Levels of significance: *** p<0.01, ** p<0.05, * p<0.10.

Table 2: Attendance on Dates of Examinations and O Level (Aggregate) Pass Indicator

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Timing of Observation:	Year 1 (Oct. '16)		Year 2 (Oct. '17)		Year 3 (Oct. '18)		Year 3 (Nov. '18)
Explained Variable:	Attended	Enrolled	Attended	Enrolled	Attended	Enrolled	O Level Pass
Control (C) Mean Proportion	0.804	0.982	0.668	0.762	0.655	0.740	0.598
(Std. Dev.)	(0.397)	(0.135)	(0.471)	(0.426)	(0.476)	(0.439)	(0.490)
A. Treatment Variables							
Incentives Only (G1)	0.0388*	0.00977	0.0875***	0.0645**	0.0428	0.0605**	0.00674
	(0.0213)	(0.0131)	(0.0265)	(0.0256)	(0.0301)	(0.0282)	(0.0376)
	[0.0350]	[1]	[0.00400]	[0.0400]	[0.187]	[0.0670]	[1]
Technology Only (G2)	0.0500**	0.00279	0.0442*	0.0267	0.0315	0.0408	-0.0197
	(0.0213)	(0.0131)	(0.0247)	(0.0257)	(0.0293)	(0.0270)	(0.0359)
	[0.0350]	[1]	[0.0260]	[0.158]	[0.235]	[0.0670]	[1]
Both (G3)	0.0506**	0.00886	0.0603**	0.0395*	0.0572**	0.0490**	-0.00461
	(0.0219)	(0.0134)	(0.0241)	(0.0232)	(0.0249)	(0.0239)	(0.0314)
	[0.0350]	[1]	[0.0140]	[0.100]	[0.0740]	[0.0670]	[1]
B. Control Variables							
Age	-0.0199***	0.000218	-0.0278***	-0.0241***	-0.0311***	-0.0282***	-0.0549***
	(0.00471)	(0.00109)	(0.00535)	(0.00511)	(0.00599)	(0.00549)	(0.00671)
Year 0 (Feb. '16) Z-score	0.0462***	0.000621	0.0869***	0.0758***	0.0987***	0.0793***	0.171***
	(0.00603)	(0.00199)	(0.00833)	(0.00726)	(0.00826)	(0.00731)	(0.0104)
Commute Distance (km)	-0.00330	-0.00109	-0.00750***	-0.00751***	-0.00806***	-0.00732***	-0.00850***
	(0.00225)	(0.000799)	(0.00186)	(0.00201)	(0.00239)	(0.00203)	(0.00254)
Block (Five-region) FE	X	X	X	X	X	X	X
Observations	6,201	6,201	6,201	6,201	6,201	6,201	5,965
R-squared (Mean)	0.0390	0.00530	0.0869	0.0790	0.0878	0.0748	0.158
Clusters	170	170	170	170	170	170	170
F, Commute Distance = 0	2.161	-	16.32	-	11.40	-	-
Pr. > Joint F, All Treat. = 0	0.0925	0.483	0.0111	0.0805	0.146	0.154	0.905

Note: Difference-in-means coefficients. "Enrolled": enrolled in project school on exam date. "O Level Pass": obtained junior-secondary certificate. Column (7) drops transferred students. Controls were missing at random for about 10% of students; estimates were obtained using multiple impuation and combined using Rubin's (1987) formulas. In parentheses: school-cluster-robust standard errors. Levels of significance: *** p<0.01, ** p<0.05, * p<0.10. In brackets: Benjamini-Krieger-Yekutieli (2006) sharpened two-stage q-values (for three hypotheses).

Table 3: Effects on Performance

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Timing of Observation:	Year 1 Z-score (Oct. '16)			Year 2 Z-score (Oct. '17)			Year 3 Z-score (Oct. '18)		
Specification:	Non-missing	Year 0 Controls	Selection Corrected	Non-missing	Year 0 Controls	Selection Corrected	Non-missing	Year 0 Controls	Selection Corrected
A. Treatment Variables									
Incentives Only (G1)	0.0732	0.0407	0.101	0.147	0.118	0.0541	0.120	0.105	0.101
	(0.0935)	(0.0716)	(0.0918)	(0.0944)	(0.0741)	(0.0935)	(0.0925)	(0.0737)	(0.0750)
	[0.628]	[0.393]	[0.321]	[0.138]	[0.0820]	[0.602]	[0.247]	[0.186]	[0.218]
Technology Only (G2)	0.134	0.110	0.175	0.137	0.122	0.0908	0.0589	0.0403	0.0295
	(0.129)	(0.0835)	(0.126)	(0.127)	(0.0745)	(0.0914)	(0.130)	(0.0805)	(0.0881)
	[0.628]	[0.393]	[0.321]	[0.222]	[0.0820]	[0.475]	[0.422]	[0.307]	[0.366]
Both (G3)	0.157	0.123	0.183*	0.415***	0.372***	0.331***	0.352***	0.301***	0.280***
	(0.103)	(0.0748)	(0.104)	(0.104)	(0.0767)	(0.0843)	(0.102)	(0.0781)	(0.0811)
	[0.628]	[0.393]	[0.321]	[0.00100]	[0.00100]	[0.00100]	[0.00300]	[0.00100]	[0.00300]
B. Linear Combinations of Estimators, Other Tests and Details									
β_{G3} - β_{G1}	0.0834	0.0819	0.0820	0.268**	0.253***	0.277***	0.232**	0.196**	0.179**
	(0.100)	(0.0668)	(0.0679)	(0.104)	(0.0876)	(0.0905)	(0.106)	(0.0870)	(0.0870)
β_{G3} - β_{G2}	0.0228	0.0122	0.00772	0.278**	0.250***	0.240***	0.293**	0.260***	0.251***
	(0.133)	(0.0793)	(0.0804)	(0.136)	(0.0897)	(0.0875)	(0.141)	(0.0942)	(0.0888)
β_{G3} - β_{G2} - β_{G1}	-0.0504	-0.0285	-0.0933	0.131	0.132	0.186	0.173	0.155	0.149
	(0.163)	(0.106)	(0.133)	(0.165)	(0.116)	(0.139)	(0.168)	(0.120)	(0.121)
Block (Five-region) FE	X	X	X	X	X	X	X	X	X
Observations	5,251	5,251	5,251	4,518	4,518	4,518	4,354	4,354	4,354
R-squared (Mean)	0.0536	0.570	0.574	0.0688	0.498	0.499	0.0566	0.473	0.478
Clusters	170	170	170	170	170	170	170	170	170
Pr. > Joint F, All Treat. = 0	0.458	0.323	0.353	0.00150	0.000100	0.000700	0.00780	0.00210	0.00350

Note: Difference-in-means coefficients. Columns (2), (5) and (8) controls: year-0 score, age and commute distance. Column (3), (6) and (9) use Heckman's (1990) nonparametric control-function approach, using a (3rd-degree) polynomial of probit attrition-propensity score instrumented with commute distance. Controls were missing at random for about 10% of students; estimates were obtained using multiple impuation and combined using Rubin's (1987) formulas. Standard errors: clustered by school. Levels of significance: *** p<0.01, ** p<0.05, * p<0.10. Third row in brackets: Benjamini-Krieger-Yekutieli (2006) sharpened two-stage q-values (for three hypotheses) as described in Anderson (2008).

Table 4: Effects on Effort (Reported Hours Per Week of Mathematics Study)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Timing of Observation:	Year 1 Math Study (Hrs/wk)			Year 2 Math Study (Hrs/wk)			Year 3 Math Study (Hrs/wk)		
Specification:	Non-missing	Year 0 Controls	Selection Corrected	Non-missing	Year 0 Controls	Selection Corrected	Non-missing	Year 0 Controls	Selection Corrected
Control (C) Mean	4.099	4.099	4.099	5.747	5.747	5.747	6.118	6.118	6.118
(Std. Dev.)	(2.979)	(2.979)	(2.979)	(5.703)	(5.703)	(5.703)	(6.193)	(6.193)	(6.193)
A. Treatment Variables									
Incentives Only (G1)	0.378	0.347	0.368	0.490	0.417	-0.277	0.602	0.616	0.587
	(0.345)	(0.349)	(0.377)	(0.480)	(0.454)	(0.560)	(0.545)	(0.517)	(0.514)
	[0.226]	[0.272]	[0.283]	[0.333]	[0.317]	[1]	[0.372]	[0.308]	[0.342]
Technology Only (G2)	0.522	0.500	0.517	0.490	0.462	0.114	0.289	0.289	0.279
	(0.402)	(0.400)	(0.444)	(0.551)	(0.492)	(0.529)	(0.592)	(0.550)	(0.543)
	[0.226]	[0.272]	[0.283]	[0.333]	[0.317]	[1]	[0.685]	[0.545]	[0.619]
Both (G3)	0.918**	0.887**	0.902**	1.662***	1.604***	1.153**	2.333***	2.296***	2.279***
	(0.383)	(0.381)	(0.414)	(0.558)	(0.516)	(0.563)	(0.615)	(0.594)	(0.604)
	[0.0570]	[0.0690]	[0.102]	[0.0110]	[0.00700]	[0.145]	[0.00100]	[0.00100]	[0.00100]
C. Linear Combinations of Estimators, Other Tests and Details									
β_{G3} - β_{G1}	0.540	0.540	0.534	1.172**	1.188**	1.431***	1.730***	1.680***	1.692***
	(0.374)	(0.371)	(0.368)	(0.512)	(0.488)	(0.497)	(0.585)	(0.573)	(0.579)
β_{G3} - β_{G2}	0.396	0.387	0.385	1.171**	1.142**	1.040*	2.043***	2.008***	2.001***
	(0.424)	(0.416)	(0.416)	(0.579)	(0.526)	(0.528)	(0.628)	(0.597)	(0.603)
β_{G3} - β_{G2} - β_{G1}	0.0181	0.0399	0.0170	0.681	0.725	1.317*	1.441*	1.392*	1.413*
	(0.547)	(0.543)	(0.568)	(0.754)	(0.699)	(0.756)	(0.829)	(0.789)	(0.786)
Observations	5,251	5,251	5,251	4,518	4,518	4,518	4,354	4,354	4,354
R-squared (Mean)	0.0884	0.102	0.103	0.0264	0.0616	0.0625	0.0549	0.0939	0.0951
Clusters	170	170	170	170	170	170	170	170	170
Pr. > Joint F, All Treat. = 0	0.123	0.141	0.185	0.0276	0.0185	0.0322	0.00120	0.00100	0.00150

Note: Difference-in-means coefficients. Columns (2), (5) and (8) controls: year-0 score, age and commute distance. Column (3), (6) and (9) use Heckman's (1990) nonparametric control-function approach, using a (3rd-degree) polynomial of probit attrition-propensity score instrumented with commute distance. Controls were missing at random for about 10% of students; estimates were obtained using multiple impuation and combined using Rubin's (1987) formulas. Standard errors: clustered by school. Levels of significance: *** p<0.01, ** p<0.05, * p<0.10. Third row in brackets: Benjamini-Krieger-Yekutieli (2006) sharpened two-stage q-values (for three hypotheses) as described in Anderson (2008).

Table 5: Year 2 and Year 3 Outcomes by Year 0 Performance Quintiles

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Timing of Observation:	Year 2 Z-score (Oct. '17)					Year 3 Z-score (Oct. '18)				
Pretest Quintile:	Bottom	2nd	3rd	4th	Top	Bottom	2nd	3rd	4th	Top
Control (C) Mean	-0.731	-0.585	-0.444	-0.119	0.638	-0.588	-0.574	-0.484	-0.0816	0.712
(Std. Dev.)	(0.319)	(0.391)	(0.557)	(0.673)	(0.935)	(0.502)	(0.436)	(0.593)	(0.761)	(1.049)
A. Treatment Variables										
Incentives Only (G1)	0.205**	0.0975	0.0989	0.0271	-0.0322	0.107	0.115	0.233**	-0.00402	0.0375
	(0.0883)	(0.0885)	(0.0969)	(0.123)	(0.156)	(0.0768)	(0.0699)	(0.0987)	(0.118)	(0.149)
	[0.0230]	[0.374]	[0.448]	[1]	[0.387]	[0.199]	[0.114]	[0.0200]	[0.548]	[1]
Technology Only (G2)	0.0617	0.0520	0.00311	-0.00523	0.346*	0.0183	0.0462	0.0378	-0.136	0.119
	(0.0788)	(0.0771)	(0.0720)	(0.110)	(0.193)	(0.0830)	(0.0737)	(0.0723)	(0.114)	(0.201)
	[0.170]	[0.503]	[0.864]	[1]	[0.0810]	[0.380]	[0.216]	[0.251]	[0.309]	[1]
Both (G3)	0.250***	0.236***	0.375***	0.351***	0.449***	0.171**	0.267***	0.393***	0.227**	0.269*
	(0.0846)	(0.0792)	(0.0882)	(0.113)	(0.147)	(0.0834)	(0.0757)	(0.0942)	(0.111)	(0.151)
	[0.0110]	[0.0100]	[0.00100]	[0.00700]	[0.00800]	[0.144]	[0.00200]	[0.00100]	[0.147]	[0.300]
B. Linear Combinations of Estimators, Other Tests and Details										
$\beta_{G3} - \beta_{G1}$	0.0453	0.139*	0.276***	0.324***	0.481***	0.0640	0.152*	0.160	0.231**	0.231*
	(0.0867)	(0.0814)	(0.0983)	(0.105)	(0.167)	(0.0866)	(0.0907)	(0.117)	(0.108)	(0.139)
$\beta_{G3} - \beta_{G2}$	0.188**	0.184**	0.372***	0.356***	0.103	0.153*	0.221**	0.355***	0.363***	0.150
	(0.0832)	(0.0813)	(0.0840)	(0.0997)	(0.203)	(0.0882)	(0.0916)	(0.0914)	(0.102)	(0.192)
$\beta_{G3} - \beta_{G2} - \beta_{G1}$	-0.0164	0.0868	0.273**	0.329**	0.135	0.0457	0.106	0.122	0.367**	0.112
	(0.125)	(0.119)	(0.130)	(0.158)	(0.269)	(0.119)	(0.117)	(0.138)	(0.158)	(0.250)
Observations	4,518	4,518	4,518	4,518	4,518	4,354	4,354	4,354	4,354	4,354
R-squared (Mean)	0.505	0.505	0.505	0.505	0.505	0.481	0.481	0.481	0.481	0.481
Clusters	170	170	170	170	170	170	170	170	170	170
Pr. > Joint F, All Treat. = 0	0.0102	0.0251	0.000100	0.00110	0.00380	0.138	0.00470	0.000100	0.00540	0.259

Note: Difference-in-means coefficients. Controls as in columns (6) and (9) of Table 3: age, year-0 score, randomization-block (five-region) indicators, and a (3rd-degree) polynomial of attrition-propensity score. Controls were missing at random for about 10% of students; estimates were obtained using multiple impuation and combined using Rubin's (1987) formulas. Standard errors: clustered by school in parentheses. Levels of significance: *** p<0.01, ** p<0.05, * p<0.10. In brackets: Benjamini-Krieger-Yekutieli (2006) sharpened two-stage q-values (for three hypotheses).

Table 6: Other Potentially Relevant Inputs (Students' Technology Usage and Teachers' Time Input)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Timing of Observation:	Year 2 Proxies for Inputs Usage (hrs/wk)				Year 3 Proxies for Inputs Usage (hrs/wk)			
Explained Variable:	Studied in School after 6pm	Printed Material Usage	ICT and Multimedia Usage	Total Teacher Hours	Studied in School after 6pm	Printed Material Usage	ICT and Multimedia Usage	Total Teacher Hours
Control (C) Mean	3.171	5.611	0.0381	6.404	8.465	6.372	0.135	6.681
(Std. Dev.)	(6.942)	(6.313)	(0.752)	(3.046)	(11.01)	(6.820)	(0.798)	(2.676)
A. Treatment Variables								
Incentives Only (G1)	-1.407	0.231	-0.0187	-0.158	-1.421	1.127*	-0.0837	-0.0650
	(1.328)	(0.702)	(0.0710)	(0.830)	(1.699)	(0.669)	(0.117)	(0.653)
	[1]	[0.329]	[0.360]	[1]	[1]	[0.0330]	[0.189]	[1]
Technology Only (G2)	0.931	2.494***	0.344***	0.292	1.229	2.032***	1.453***	0.394
	(1.516)	(0.611)	(0.111)	(0.714)	(1.762)	(0.643)	(0.266)	(0.579)
	[1]	[0.00100]	[0.00700]	[1]	[1]	[0.00200]	[0.00100]	[1]
Both (G3)	0.914	3.582***	0.235***	-0.0246	0.865	4.294***	1.321***	0.00369
	(1.176)	(0.643)	(0.0850)	(0.625)	(1.782)	(0.857)	(0.252)	(0.578)
	[1]	[0.00100]	[0.00700]	[1]	[1]	[0.00100]	[0.00100]	[1]
B. Linear Combinations of Estimators, Other Tests and Details								
$\beta_{G3} - \beta_{G1}$	2.321**	3.351***	0.254***	0.134	2.286	3.167***	1.404***	0.0687
	(0.924)	(0.691)	(0.0741)	(0.717)	(1.698)	(0.761)	(0.227)	(0.618)
$\beta_{G3} - \beta_{G2}$	-0.0172	1.088*	-0.109	-0.316	-0.365	2.262***	-0.132	-0.390
	(1.161)	(0.640)	(0.117)	(0.618)	(1.586)	(0.745)	(0.331)	(0.542)
$\beta_{G3} - \beta_{G2} - \beta_{G1}$	1.390	0.857	-0.0903	-0.158	1.056	1.135	-0.0483	-0.325
	(1.987)	(0.971)	(0.139)	(1.035)	(2.355)	(0.979)	(0.348)	(0.830)
Observations	4,518	4,518	4,518	170	4,354	4,354	4,354	170
R-squared (Mean)	0.0857	0.0827	0.0324	0.0951	0.152	0.0992	0.209	0.0547
Clusters	170	170	170	-	170	170	170	-
Pr. > Joint F, All Treat. = 0	0.0221	0	0	0.941	0.406	0	0	0.854

Note: Difference-in-means coefficients. Controls: age, year-0 score, randomization-block (region) indicators, and a polynomial of attrition-propensity score (degree 3). Controls were missing at random for about 10% of students; estimates were obtained by multiple impuation and combined via Rubin's (1987) formulas. "Total Teacher Hours" sum teaching, preparing and tutoring hours; regressions are weighted by classroom size. In parentheses: school-cluster-robust standard errors. Levels of significance: *** p<0.01, ** p<0.05, * p<0.10. In brackets: Benjamini-Krieger-Yekutieli (2006) sharpened two-stage q-values (for three hypotheses).

Table 7: Selected Model Parameters and Simulated Treatment Effect Comparisons with Data

A. Model Parameters and Estimates			*B. Treatment Effects on Knowledge (Gross vs. Value-Added)*						*C. % Gaining Knowledge*	
[Statistic]	(1)	(2)	*[Statistic]*	(Data)	(1)	(2)	(1')	(2')	(2)	(2')
[Param.]			*[All, G.]*							
α_0	1.08	1.08	β_{G1}	5.41	-1.34	1.98	0.168	3.58	6.05	6.86
α_1	0.140	0.129	β_{G2}	9.08	11.5	6.03	10.4	5.37	4.39	4.58
γ_0	0.163	0.00100	β_{G3}	33.1	14.5	19.4	10.6	15.5	22.3	20.8
γ_1	0.00108	0.00208	$\beta_{G3\text{-}G2\text{-}G1}$	18.6	4.39	11.4	0.0253	6.52	11.9	9.32
δ_{cons}	0.148	0.0200								
$\Delta\delta_{K0}$	1.21E-04	3.33E-04	*[All, VA.]*							
τ_{cons}	0.0457	0.0437	β_{G1}	-	-0.241	3.03	0.144	3.50	-	-
τ_{SMR}	0.00677	0.0151	β_{G2}	-	10.6	5.56	10.4	5.44	-	-
$\pi_{\$}$	4.79	14.1	β_{G3}	-	11.6	16.74	10.6	15.52	-	-
π_{scale}	0.900	1.00	$\beta_{G3\text{-}G2\text{-}G1}$	-	1.16	8.16	0.232	6.58	-	-
θ_{cons}	1350	396								
θ_{STEM}	54.4	-33.7	*[Quintiles, G.]*							
p	2.58	3.58	$\beta_{G3\text{-}G2\text{-}G1}$ *[Q5]*	13.5	2.23	10.1	0.0164	6.92	7.69	4.75
$\underline{E}$	1.41	5.61	$\beta_{G3\text{-}G2\text{-}G1}$ *[Q4]*	32.9	11.6	22.2	0.0242	10.2	21.1	15.5
ι	1E-8	3.11	$\beta_{G3\text{-}G2\text{-}G1}$ *[Q3]*	27.3	4.22	11.5	0.0293	7.23	14.0	12.4
σ_S	60.1	60.9	$\beta_{G3\text{-}G2\text{-}G1}$ *[Q2]*	8.68	2.73	7.02	0.0297	4.55	9.38	8.23
$\underline{K}_0$	-	554	$\beta_{G3\text{-}G2\text{-}G1}$ *[Q1]*	-1.64	0.944	3.38	0.0281	2.75	5.32	5.20
$\underline{K}_{0,SMR}$	-	-155								
$\underline{\pi}$	-	7.89	*[Quintiles, VA.]*							
			$\beta_{G3\text{-}G2\text{-}G1}$ *[Q5]*	-	0.451	8.26	0.0144	6.89	-	-
[% Baseline]			$\beta_{G3\text{-}G2\text{-}G1}$ *[Q4]*	-	2.62	13.5	0.0220	10.3	-	-
Disinterested	-	86.1	$\beta_{G3\text{-}G2\text{-}G1}$ *[Q3]*	-	1.16	8.33	0.0271	7.33	-	-
Lost	-	78.0	$\beta_{G3\text{-}G2\text{-}G1}$ *[Q2]*	-	0.874	5.32	0.0275	4.64	-	-
Both D. & L.	-	77.0	$\beta_{G3\text{-}G2\text{-}G1}$ *[Q1]*	-	0.339	2.90	0.0265	2.81	-	-
Neither	100	12.8								

Note: Model (1) refers to the benchmark model; (2), the model with productivity traps. Top of panel A reports selected parameter estimates. In the bottom of panel A, "disinterested" refers to students whose marginal valuation of knowledge fell short of minimum interest threshold; "lost" refers to students whose entering grade-level knowledge fell short of minimum preparedness threshold; "both" refers to those both disinterested and lost; "neither" refers to neither disinterested nor lost. In panel B, column (Data) reproduces estimates from column (6) of table 3, and the bottom row of table 5. Columns (1) and (2) report differences in simulated means across treatment groups, leaving the original balance of student characteristics across treatment groups intact. Columns (1') and (2') report differences in simulated means across treatments, assuming that the whole sample gets shocked with each treatment. In panel C, columns (2) and (2') report differences in simulated proportions of students who are gaining knowledge—i.e., those who are neither disinterested nor lost—across treatments.

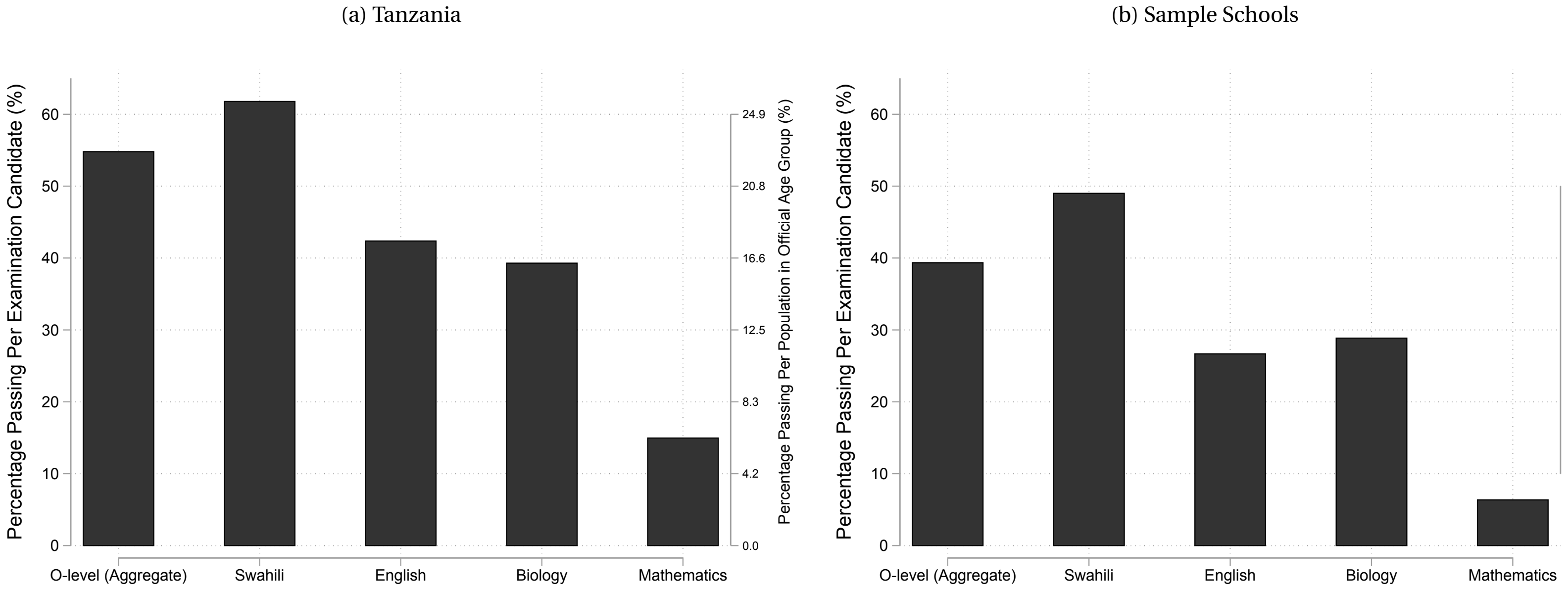
Table 8: Revealed-Preference–Based Valuations of Achievement and Inputs across Models

A. Mean Valuation Estimates ($ per good)				*B. Mean Cost Estimates ($ per good)*		
[Sample]	(1)	(2)	(2; 0 if $MB < \pi$)	(Data)	(1)	(2)
[All]						
+1 Ki (0.01σ)	1.32	0.42	0.0801	-	1.42	0.0230
+10 Ki (0.10σ)	13.2	4.20	0.801	-	71.7	3.26
+20 Ki (0.20σ)	26.4	8.41	1.60	-	17100	169
+10 Ki given G2	13.8	4.31	1.01	-	54.55	2.048
$E[\theta]$	289	26.6	-	-	-	-
$E[\theta\phi/\sigma] \times 10$	3.23	0.272	-	-	-	-
Incentives (G1)	5.49	7.11	-	5.30	5.06	4.98
Technology (G2)	14.6	2.90	-	6.13	(same as left)	
Both (G3)	20.7	13.8	-	13.1	11.8	11.9
Complementarity	0.557	3.81	-	1.72	0.605	0.782
[Q5]						
+10 Ki (0.10σ)	21.9	5.12	2.79	-	70.9	14.1
+20 Ki (0.20σ)	43.7	10.2	5.58	-	1680	721
+10 Ki given G2	23.1	5.25	3.19	-	61.9	8.42
$E[\theta\phi/\sigma] \times 10$	10.2	0.885	-	-	-	-
Incentives (G1)	11.5	17.55	-	10.5	11.4	11.8
Technology (G2)	27.3	7.86	-	6.13	(same as left)	
Both (G3)	39.7	29.8	-	19.9	18.4	19.2
Complementarity	0.866	4.39	-	3.22	0.880	1.27
[Q4]						
+10 Ki (0.10σ)	12.6	4.26	0.610	-	63.0	0.576
+20 Ki (0.20σ)	25.2	8.51	1.22	-	6350	41.2
+10 Ki given G2	13.5	4.43	0.956	-	50.7	0.762
$E[\theta\phi/\sigma] \times 10$	2.96	0.24	-	-	-	-
Incentives (G1)	5.76	7.18	-	5.90	5.49	5.22
Technology (G2)	14.5	3.44	-	6.13	(same as left)	
Both (G3)	20.9	16.5	-	13.4	12.3	12.5
Complementarity	0.624	5.88	-	1.35	0.665	1.15

Note: [From top to bottom:] Rows labeled "+*s* Ki" refer to adding *s* scaled-score units of knowledge given the status quo learning environment. "+10 Ki given G2" refers to +10 scaled-score units given additionally the experimental technology (G2). "$E[\theta]$" refers to promotion; "$E[\theta\phi/\sigma] \times 10$" refers to how much the chance of promotion affects the valuation of adding 10 scaled-score units. The rest of the rows refer to valuations of the treatment interventions and their interaction effect. Model (1) refers to the benchmark model; (2), the model with productivity traps. Column (2; 0 if $MB < \pi$) shows "perceived" estimates as if students whose interests are lower than the minimum interest threshold valued each additional scaled-score unit at 0. In panel B, column (Data) shows mean cost of each treatment as seen in program data; columns (1) and (2) show simulated effort costs and program costs across models.

Figures

Figure 1: O Level Pass Rates for Secondary School Students (2012–2015)

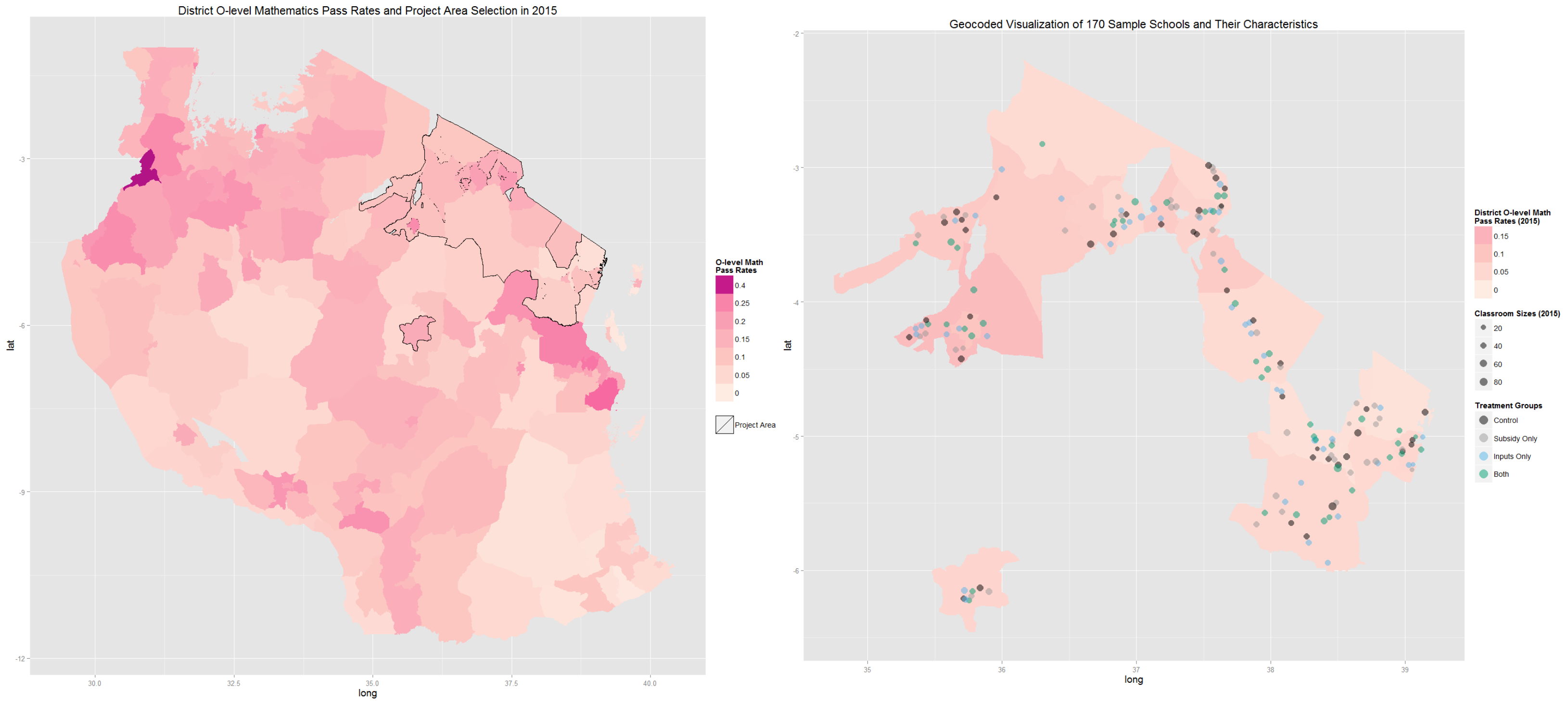


(a) Tanzania

(b) Sample Schools

Note: Author's calculations using government data (PO-RALG, 2016; NECTA, 2019). Figure 1a plots the total numbers of students who passed as percentages over the total number of secondary school students who sat for O Level Certification examinations between 2012 and 2015; approximately 1.4 million students sat for these examinations over these four years. The right-hand-size y-axis plots the numbers of students who passed over the number of youths who belonged in the official secondary-school age group population (approximately 3.4 million youths). O Level certification requires obtaining at least two D's or one C out of five required subjects and two optional subjects; the five required subjects include Swahili, English, Civics, Biology and Mathematics—the pass rate for Civics look similar to Swahili and English pass rates. Figure 1a plots the numbers of students who passed in the SMR sample schools over the number of students who sat for the examinations across these four years (44,804 students).

Figure 2: National Mathematics Performance and Characteristics of Project Schools (2015)

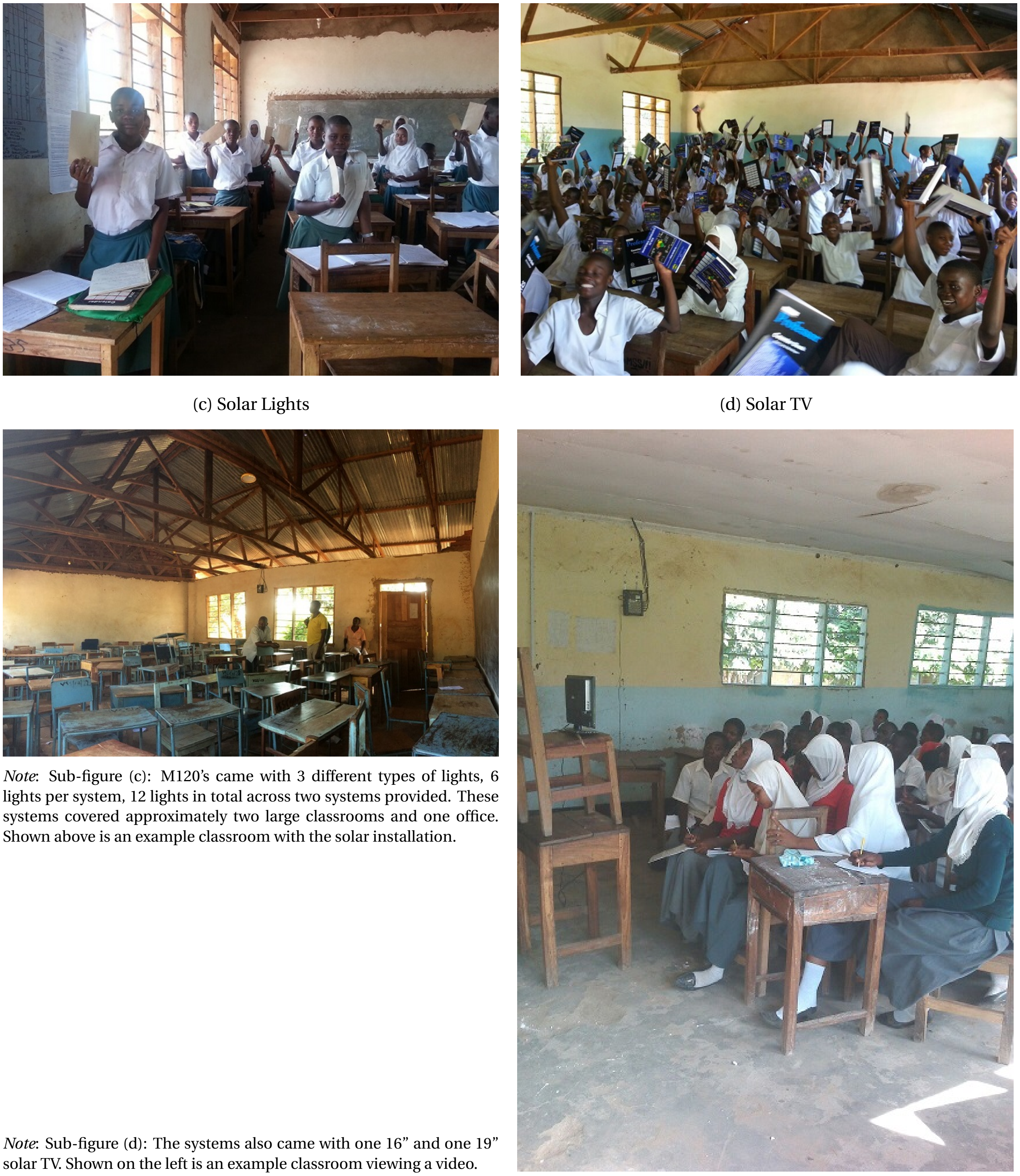


(a) Tanzania

(b) Sample Schools

Note: Author's calculations using government and project survey data (NECTA, 2019). The fills in fig. 2a are pass rates of all schools; the fills in fig. 2b are pass rates of sampled schools. The research team initially targeted all schools without electricity in 23 northern Tanzanian districts (demarcated in black). Districts shown in fig. 2b are three fewer than in fig. 2a, because some districts were found with no un-electrified school and were dropped.

Figure 3: Subsidies, Textbooks, Solar Lights and Solar TVs



(a) Incentives

(b) Textbooks

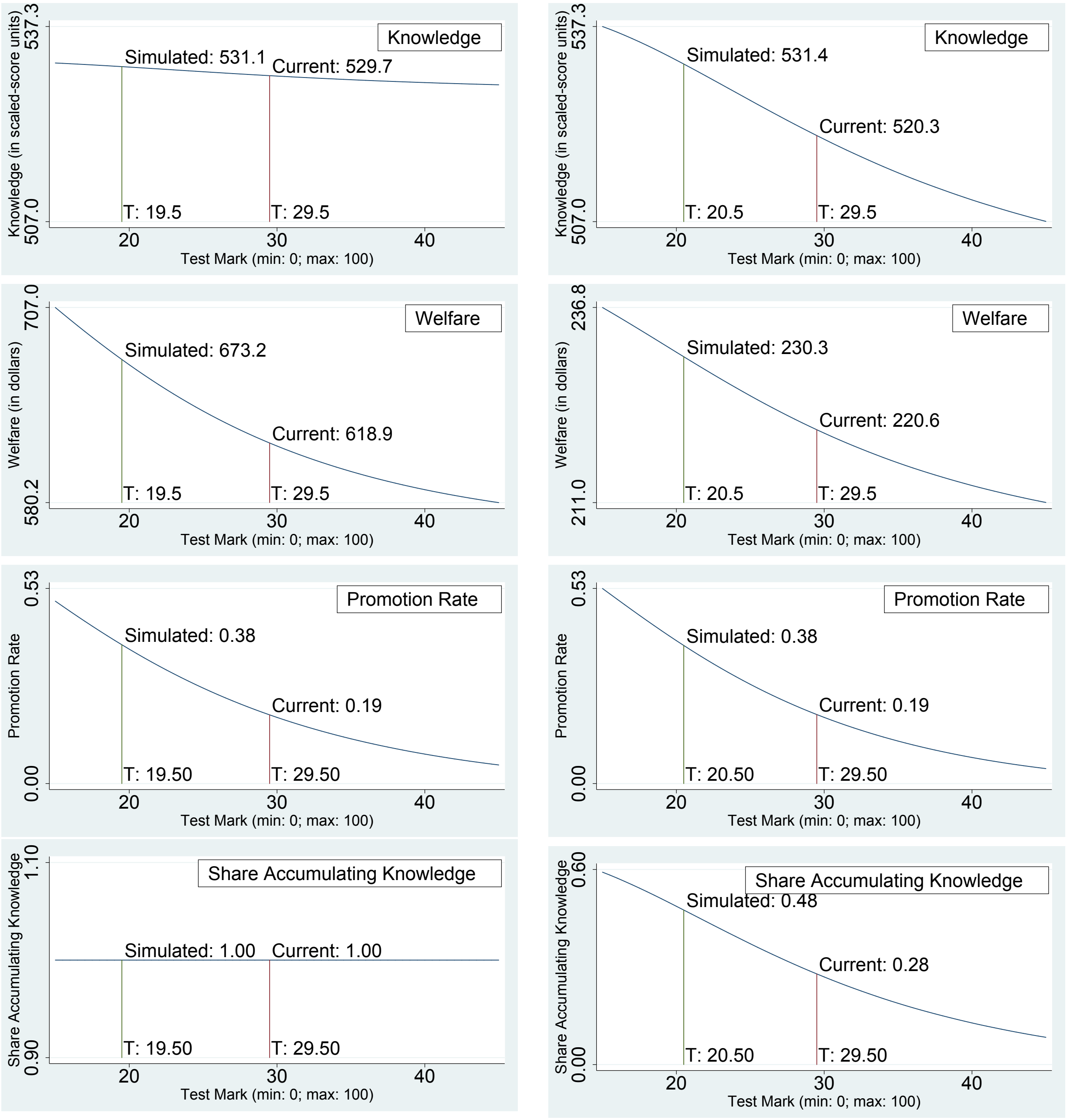
(c) Solar Lights

(d) Solar TV

Note: Sub-figure (c): M120's came with 3 different types of lights, 6 lights per system, 12 lights in total across two systems provided. These systems covered approximately two large classrooms and one office. Shown above is an example classroom with the solar installation.

Note: Sub-figure (d): The systems also came with one 16" and one 19" solar TV. Shown on the left is an example classroom viewing a video.

Figure 4: Impacts of Counterfactual Promotion Cutoffs on Outcomes Conditional on Providing Technology (G2)


(a) Benchmark Model

(b) Model with Productivity Thresholds

Note: Constant θ assumed. Promotion cutoffs are minimum absolute marks of test scores a student must achieve in order to have the option of continuing to study mathematics beyond the O Levels. Plots indicate model-simulated outcomes, conditional on distributing the experimental technology nationally, from a counterfactual policy of lowering the current promotion cutoff (indicated in red and also labeled "T: 29.50") to a hypothetical cutoff (indicated in green) simulated to double the number of students passing in equilibrium. Each experimental sample observation is weighted nationally using the ratio between a Weibull density fitted to Form Two National Assessment grade distribution and the density of the experimental sample's year 1 marks. "Share Accumulating Knowledge" refers to the proportion of students who are meaningfully accumulating knowledge during the year (or, in other words, students who are paying the entry cost of studying and not giving up on learning the curriculum).

A Appendix Tables

Table A1: A Review of Five Selected Student Performance Subsidy Experiments

(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
					Unit Subsidy†			Average Treatment Effect(s)‡			
Study	Setting	Unit Period [All Periods]	Subject(s)	Grade(s)	$	% GDPPC	Technology / Inputs	Incentives Only	Technology Only	Both	Incentives | Tech.
Fryer (2011)	New York	2 Months [1 Year]	Reading, Math	4, 7	$0.20~$0.40	>0.001%	-	−0.12σ ~ −0.05σ			
Fryer (2011)	Chicago	5 Weeks [1 Year]	Core Courses	9	$0.50	0.001%	-	0.09σ			
Behrman et al. (2015)	Mexico	1 Year [3 Years]	Math	10, 11, 12	$8	0.1%	Teacher Incentives	0.17σ ~ 0.32σ	−0.05σ ~ 0.14σ	0.19σ ~ 0.63σ	0.23σ ~ 0.53σ
Hirshleifer (2017)	Mumbai, Pune	40 Days [80 Days]	Math	4, 5, 6	$0.03	0.002%	Tablet, Softwares				0.24σ
This Study (2018)	Northern Tanzania	1 Year [3 Years]	Math	9, 10, 11	$0.50	0.05%	Solar Energy, Bilingual Textbooks, Videos	-0.04σ ~ 0.05σ	-0.06σ ~ 0.08σ	0.13σ ~ 0.28σ	0.05σ ~ 0.30σ

Note: Author's compilation. Selection was based on whether student incentives in the experiment could be approximated as "unit subsidies": piece-rate payment contracts per percentage mark on period-end achivement test or report card. In each experiment, students were randomized into groups receiving Incentives Only, Technology (or Inputs) Only, Both (Incentives and Technology), or none of the above. Treatments were delivered in the beginning of each period over multiple periods. Columns (9)-(11) show the range of reported period-cohort-specific difference-in-means treatment effects. Column (12) reports differences between the Both treatment effect and Technology Only treatment effect; hence, column (12) identifies the effectiveness of the incentive when technology were provided to students, whereas by comparison column (9) identifies the effectiveness of the incentive when the technology was not provided to students. Hirshleifer (2017) did not include a control group that did not receive any treatment beyond what is available in the normal schooling environment (but included Technology Only, Both, and an additional treatment group that received subsidies on input usage whose effect is beyond the scope of this paper and is ommitted); in her case, the Both against Technology Only treatment effect identifies the analogous difference for column (12).

† Columns (6) and (7) refer to approximate size of performance contract promised to students per end-of-period percentage mark. Values in column (7) are obtained by dividing those in column (6) by the respective national Gross Domestic Product per capita.

‡ In Behrman et al. (2015), "Both" treatment was not a simple sum of "Incentive" and "Inputs" treatments, but students additionally received rewards from peer student performance, teachers from peer-teacher performance, and school administrators from school-wide performance.

Table A2: Minimum Detectable Effect Size Calculations

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Timing of Observation:	Year 0 (Feb. '16)		Year 1 (Oct. '16)		Year 2 (Oct. '17)		Year 3 (Oct. '18)	
Power:	80%	80%	80%	80%	80%	80%	80%	80%
MHT Correction:	None	Bonferroni	None	Bonferroni	None	Bonferroni	None	Bonferroni
<u>Outcome Variables:</u>								
Normalized Mathematics Marks	0.237	0.274	0.203	0.234	0.231	0.267	0.224	0.258
<u>Parameters and Moments:</u>								
Number of Hypotheses	1	3	1	3	1	3	1	3
J_{T} (Treatment Size)	43	43	43	43	43	43	43	43
J_{C} (Control Size)	40	40	40	40	40	40	40	40
n (Cluster Size)	36.453	36.453	30.888	30.888	26.576	26.576	25.612	25.612
τ_{α/2}	1.96	2.39	1.96	2.39	1.96	2.39	1.96	2.39
τ_{1-κ}	0.84	0.84	0.84	0.84	0.84	0.84	0.84	0.84
p (Treatment Share)	0.518	0.518	0.518	0.518	0.518	0.518	0.518	0.518
c (Compliance among Treated)	1	1	1	1	1	1	1	1
s (Defiance among Control)	0	0	0	0	0	0	0	0
ρ (Residual Intracluster Corr.)	0.138	0.138	0.232	0.232	0.250	0.250	0.222	0.222
σ (Residual Std. Dev.)	0.958	0.958	0.650	0.650	0.711	0.711	0.724	0.724

Note : This table reports minimum detectable effect sizes (MDEs) calculated under different clustered-randomized-design scenarios, using equation (12) of Duflo et al. (2007) (Bloom (2005)). The Sharpening Mathematics Review School Program randomized 170 9th-grade classrooms, each sampled from a distinct school, into three treatment groups and a control group, with approximately 43 schools in each treatment group and 40 in control, targeting normalized mathematics marks on follow-up curriculum-based tests. Odd columns report MDE with alpha unadjusted for multiple-hypotheses testing; evens report MDEs with alpha adjusted for three independent hypotheses using Bonferroni. The moments used include the average number of students (n), the intracluster correlation coefficients (ρ) and standard deviations (σ) of normalized mathematics test scores. In columns (1)-(2), moments are calculated based on year 0 (Feb. '16) F1 (grade 8) results; power reported controls for age, commute distance and randomization-block (five-region) indicators. Columns (3)-(4) report the power realized on year 1 (Oct. '16) F2 (grade 9) results, controlling for age, commute distance, randomization-block (five-region) indicators and year-0 marks (administered before the program's incentive contracts were announced and before year-1 math curriculum textbooks and videos were delivered). Columns (5)-(6) report the analogous power for year 2 (Oct. '17) F3 (grade 10) results; Columns (7)-(8), for year 3 (Oct. '18) F4 (grade 11) results.

Table A3: Effects on Performance (Observation Missing Controls Dropped)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Timing of Observation:	Year 1 Z-score (Oct. '16)			Year 2 Z-score (Oct. '17)			Year 3 Z-score (Oct. '18)		
Specification:	Non-missing	Feb. '16 Controls	Selection Corrected	Non-missing	Feb. '16 Controls	Selection Corrected	Non-missing	Feb. '16 Controls	Selection Corrected
A. Treatment Variables									
Incentives Only (G1)	0.0727	0.0413	0.0503	0.147	0.0789	-0.0433	0.119	0.0657	0.0401
	(0.0935)	(0.0756)	(0.0875)	(0.0944)	(0.0696)	(0.0804)	(0.0926)	(0.0721)	(0.0708)
	[0.619]	[0.641]	[1]	[0.138]	[0.244]	[1]	[0.249]	[0.572]	[0.617]
Technology Only (G2)	0.134	0.0859	0.0817	0.137	0.0691	0.0134	0.0587	-0.0248	-0.0562
	(0.129)	(0.0808)	(0.0985)	(0.127)	(0.0656)	(0.0703)	(0.130)	(0.0674)	(0.0677)
	[0.619]	[0.407]	[1]	[0.222]	[0.244]	[1]	[0.426]	[0.908]	[0.617]
Both (G3)	0.157	0.136*	0.127	0.415***	0.371***	0.283***	0.349***	0.309***	0.241***
	(0.102)	(0.0811)	(0.0984)	(0.104)	(0.0797)	(0.0816)	(0.102)	(0.0832)	(0.0824)
	[0.619]	[0.397]	[1]	[0.00100]	[0.00100]	[0.00300]	[0.00300]	[0.00100]	[0.0120]
B. Linear Combinations of Estimators, Other Tests and Details									
β_{T3} - β_{T1}	0.0842	0.0950	0.0770	0.268**	0.292***	0.326***	0.230**	0.244***	0.201**
	(0.100)	(0.0715)	(0.0711)	(0.104)	(0.0886)	(0.0901)	(0.106)	(0.0872)	(0.0873)
β_{T3} - β_{T2}	0.0232	0.0504	0.0457	0.278**	0.302***	0.269***	0.290**	0.334***	0.297***
	(0.133)	(0.0761)	(0.0759)	(0.136)	(0.0863)	(0.0849)	(0.141)	(0.0839)	(0.0832)
β_{T3} - β_{T1} - β_{T2}	-0.0496	0.00908	-0.00467	0.131	0.223**	0.312***	0.171	0.268**	0.257**
	(0.163)	(0.107)	(0.115)	(0.165)	(0.112)	(0.119)	(0.168)	(0.111)	(0.111)
Block (Five-region) FE	X	X	X	X	X	X	X	X	X
Observations	5,251	4,697	4,697	4,518	4,079	4,079	4,351	3,919	3,919
R-squared	0.054	0.547	0.551	0.069	0.483	0.485	0.057	0.461	0.466
Clusters	170	170	170	170	170	170	170	170	170
Pr. > Joint F, All Treat. = 0	0.456	0.356	0.588	0.00140	0.000100	0.00140	0.00860	0.000700	0.00470

Note : Difference-in-means coefficients. Columns (2), (5) and (8) control for year-0 score, age and commute distance. Column (3), (6) and (9) use Heckman's (1990) nonparametric control-function approach, using a (degree-3) polynomial of probit attrition-propensity score instrumented with commute distance. Standard errors: clustered by school. Levels of significance: *** p<0.01, ** p<0.05, * p<0.10. Third row in brackets: Benjamini-Krieger-Yekutieli (2006) sharpened two-stage q-values (for three hypotheses) as described in Anderson (2008).

Table A4: Reported Hours Per Week of Mathematics Study (Observation Missing Controls Dropped)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Timing of Observation:	Year 1 Math Study (hrs/wk)			Year 2 Math Study (hrs/wk)			Year 3 Math Study (hrs/wk)		
Specification:	Non-missing	Feb. '16 Controls	Selection Corrected	Non-missing	Feb. '16 Controls	Selection Corrected	Non-missing	Feb. '16 Controls	Selection Corrected
Control (C) Mean	4.099	4.149	4.149	5.747	5.783	5.783	6.118	6.080	6.080
(Std. Dev.)	(2.978)	(3.002)	(3.002)	(5.701)	(5.757)	(5.757)	(6.191)	(6.060)	(6.060)
A. Treatment Variables									
Incentives Only (G1)	0.382	0.401	0.322	0.490	0.398	-0.429	0.603	0.567	0.542
	(0.346)	(0.356)	(0.378)	(0.480)	(0.459)	(0.563)	(0.545)	(0.516)	(0.514)
	[0.220]	[0.211]	[0.395]	[0.333]	[0.602]	[0.809]	[0.371]	[0.378]	[0.414]
Technology Only (G2)	0.522	0.469	0.352	0.490	0.284	-0.103	0.289	0.156	0.136
	(0.402)	(0.414)	(0.440)	(0.551)	(0.490)	(0.512)	(0.592)	(0.535)	(0.529)
	[0.220]	[0.211]	[0.395]	[0.333]	[0.602]	[1]	[0.684]	[0.698]	[0.782]
Both (G3)	0.915**	1.033***	0.912**	1.662***	1.799***	1.193**	2.326***	2.352***	2.328***
	(0.383)	(0.388)	(0.407)	(0.558)	(0.522)	(0.573)	(0.615)	(0.582)	(0.604)
	[0.0580]	[0.0270]	[0.0870]	[0.0110]	[0.00300]	[0.133]	[0.00100]	[0.00100]	[0.00100]
C. Linear Combinations of Estimators, Other Tests and Details									
$\beta_{T3} - \beta_{T1}$	0.532	0.631*	0.590	1.172**	1.401***	1.622***	1.724***	1.786***	1.786***
	(0.374)	(0.376)	(0.369)	(0.512)	(0.497)	(0.501)	(0.587)	(0.584)	(0.600)
$\beta_{T3} - \beta_{T2}$	0.393	0.564	0.559	1.171**	1.515***	1.296**	2.038***	2.197***	2.192***
	(0.424)	(0.428)	(0.426)	(0.579)	(0.526)	(0.530)	(0.629)	(0.596)	(0.610)
$\beta_{T3} - \beta_{T1} - \beta_{T2}$	0.0108	0.163	0.238	0.681	1.117	1.725**	1.435*	1.630**	1.650**
	(0.547)	(0.558)	(0.574)	(0.754)	(0.703)	(0.737)	(0.829)	(0.788)	(0.790)
Observations	5,251	4,697	4,697	4,518	4,079	4,079	4,351	3,919	3,919
R-squared	0.088	0.102	0.102	0.026	0.060	0.061	0.055	0.101	0.102
Clusters	170	170	170	170	170	170	170	170	170
Pr. > Joint F, All Treat. = 0	0.124	0.0719	0.156	0.0275	0.00470	0.0122	0.00130	0.000400	0.000900

Note : Difference-in-means coefficients. Columns (2), (5) and (8) controls: year-0 score, age and commute distance. Column (3), (6) and (9) use Heckman's (1990) nonparametric control-function approach, using a (degree-3) polynomial of probit attrition-propensity score instrumented with commute distance. Standard errors: clustered by school. Levels of significance: *** p<0.01, ** p<0.05, * p<0.10. Third row in brackets: Benjamini-Krieger-Yekutieli (2006) sharpened two-stage q-values (for three hypotheses) as described in Anderson (2008).

Table A5: Year 2 and Year 3 Outcomes by Year 0 Performance Quintiles (Observation Missing Controls Dropped)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Timing of Observation:			Year 2 Z-score (Oct. '17)					Year 3 Z-score (Oct. '18)		
Pretest Quintile:	Bottom	2nd	3rd	4th	Top	Bottom	2nd	3rd	4th	Top
Control (C) Mean	-0.734	-0.577	-0.432	-0.0891	0.640	-0.570	-0.574	-0.467	-0.0571	0.695
(Std. Dev.)	(0.289)	(0.402)	(0.549)	(0.685)	(0.935)	(0.536)	(0.452)	(0.608)	(0.781)	(1.051)
A. Treatment Variables										
Incentives Only (G1)	0.172**	0.0382	0.0258	-0.0849	-0.165	0.0726	0.0884	0.200**	-0.104	-0.0338
	(0.0858)	(0.0861)	(0.0878)	(0.114)	(0.145)	(0.0776)	(0.0698)	(0.101)	(0.112)	(0.151)
	[0.0490]	[0.973]	[1]	[0.443]	[0.207]	[0.545]	[0.262]	[0.0530]	[0.309]	[1]
Technology Only (G2)	-0.00467	0.0253	-0.0377	-0.0768	0.212	-0.0507	0.0297	-0.0286	-0.211*	-0.0357
	(0.0514)	(0.0760)	(0.0631)	(0.104)	(0.176)	(0.0704)	(0.0724)	(0.0670)	(0.111)	(0.165)
	[0.448]	[0.973]	[1]	[0.443]	[0.207]	[0.545]	[0.451]	[0.288]	[0.214]	[1]
Both (G3)	0.246***	0.208**	0.320***	0.286**	0.404***	0.140	0.236***	0.342***	0.160	0.273*
	(0.0823)	(0.0842)	(0.0862)	(0.116)	(0.150)	(0.0887)	(0.0805)	(0.0981)	(0.117)	(0.156)
	[0.0100]	[0.0460]	[0.00100]	[0.0450]	[0.0250]	[0.545]	[0.0120]	[0.00200]	[0.214]	[0.325]
B. Linear Combinations of Estimators, Other Tests and Details										
$\beta_{T3} - \beta_{T1}$	0.0738	0.170**	0.294***	0.371***	0.569***	0.0670	0.148	0.142	0.265**	0.307**
	(0.0946)	(0.0826)	(0.0955)	(0.104)	(0.169)	(0.0923)	(0.0957)	(0.124)	(0.104)	(0.138)
$\beta_{T3} - \beta_{T2}$	0.251***	0.183**	0.357***	0.363***	0.192	0.190**	0.207**	0.371***	0.371***	0.309*
	(0.0768)	(0.0849)	(0.0839)	(0.104)	(0.205)	(0.0867)	(0.0952)	(0.0904)	(0.103)	(0.159)
$\beta_{T3} - \beta_{T1} - \beta_{T2}$	0.0785	0.145	0.332***	0.448***	0.357	0.118	0.118	0.170	0.476***	0.343
	(0.114)	(0.117)	(0.121)	(0.148)	(0.253)	(0.117)	(0.120)	(0.141)	(0.154)	(0.220)
Observations	4,079	4,079	4,079	4,079	4,079	3,919	3,919	3,919	3,919	3,919
R-squared	0.492	0.492	0.492	0.492	0.492	0.469	0.469	0.469	0.469	0.469
Clusters	170	170	170	170	170	170	170	170	170	170
Pr. > Joint F, All Treat. = 0	0.00260	0.0640	0.000400	0.00140	0.00430	0.116	0.0279	0.000100	0.00410	0.111

Note : Difference-in-means coefficients. Controls are as in columns (3), (6) and (9) of Table 3: age, year-0 score, randomization-block (five-region) indicators, and a (3rd-degree) polynomial of attrition-propensity score. Standard errors: clustered by school in parentheses. Levels of significance: *** p<0.01, ** p<0.05, * p<0.10. Third row in brackets: Benjamini-Krieger-Yekutieli (2006) sharpened two-stage q-values (for three hypotheses) as described in Anderson (2008).

Table A6: Unequal Piece Rates in Year 1: Effects on Outcomes and Perceptions on Fairness

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Year 1 (Piece rates were unequal)					Year 2 (Piece rates were equal)			
	Present	Hrs/wk of Math Study	Year 1 Z-score	Subsidy was Motivating: "Always"	Unequal Piece-rates were "Demotivating"	Present	Hrs/wk of Math Study	Year 2 Z-score	Year 1 Piece-rates were "Unfair"
A.Incentives Only (G1) Constant Term and Incremental Effects of Piece Rates (Base Group = "Promised $0.125 / Mark")									
Incentives Only (G1)	0.0287	0.401	0.0418	0.774***	0.0903***	0.100***	-0.00783	-0.105	0.181***
	(0.0273)	(0.425)	(0.0880)	(0.0388)	(0.0155)	(0.0323)	(0.676)	(0.0830)	(0.0420)
Incentives Only (G1) x Promised $0.25 / Mark	0.0261	0.0474	-0.00486	0.0595**	-0.00212	-0.0228	-0.364	0.116**	0.00292
	(0.0263)	(0.225)	(0.0366)	(0.0290)	(0.0185)	(0.0320)	(0.466)	(0.0457)	(0.0225)
Incentives Only (G1) x Promised $0.5 / Mark	-0.0232	-0.176	0.0450	0.0693**	-0.0240	-0.0287	-0.941*	0.0451	-0.0406
	(0.0265)	(0.222)	(0.0451)	(0.0327)	(0.0209)	(0.0303)	(0.500)	(0.0464)	(0.0275)
Incentives Only (G1) x Promised $0.75 / Mark	0.0265	-0.184	-0.00741	0.0753**	-0.0323**	-0.0116	-0.351	0.0871	-0.0478*
	(0.0242)	(0.237)	(0.0543)	(0.0331)	(0.0160)	(0.0288)	(0.502)	(0.0604)	(0.0288)
B. Both (G3) Constant Term and Incremental Effects of Incremental Piece Rates (Base Group = "Promised $0.125 / Mark")									
Both (G3)	0.0426	0.746	0.0586	0.779***	0.0752***	0.0425	1.198*	0.197**	0.231***
	(0.0261)	(0.466)	(0.102)	(0.0585)	(0.0171)	(0.0301)	(0.643)	(0.0916)	(0.0488)
Both (G3) x Promised $0.25 / Mark	-0.00170	0.363	0.0732*	0.0129	-0.0161	0.0237	-0.353	0.0878	-0.0207
	(0.0266)	(0.278)	(0.0397)	(0.0298)	(0.0188)	(0.0338)	(0.507)	(0.0652)	(0.0400)
Both (G3) x Promised $0.5 / Mark	0.0106	0.194	0.131**	0.00243	-0.00366	-0.00717	0.100	0.133*	-0.0546*
	(0.0230)	(0.284)	(0.0533)	(0.0258)	(0.0208)	(0.0296)	(0.562)	(0.0705)	(0.0316)
Both (G3) x Promised $0.75 / Mark	0.0156	0.105	0.0715	0.00867	-0.00930	0.0645**	0.238	0.122*	-0.0595
	(0.0268)	(0.271)	(0.0558)	(0.0272)	(0.0164)	(0.0293)	(0.663)	(0.0641)	(0.0368)
Selection and Other Ctrls.	X	X	X	X	X	X	X	X	X
Observations	5,508	4,697	4,697	4,697	4,697	5,508	4,079	4,079	4,079
R-squared	0.036	0.103	0.552	0.673	0.038	0.080	0.062	0.487	0.120
Clusters	170	170	170	170	170	170	170	170	170
Pr. > Joint F, All Incremental Effects = 0	0.532	0.800	0.206	0.320	0.204	0.155	0.393	0.0978	0.210

Note : Difference-in-means coefficients. Observations: year 1 (Oct. '16) and year 2 (Oct. '17) survey responses. Controls include age, year-0 score, randomization-block (five-region) indicators, and a (3rd-degree) polynomial of selection-propensity score (probit analogues of columns (1) and (3) in Table 2). Levels of significance: *** p<0.01, ** p<0.05, * p<0.10. Third row in brackets: Benjamini-Krieger-Yekutieli (2006) sharpened two-stage q-values (for three hypotheses) as described in Anderson (2008).

Table A7: Breakdown of Subtopics Evaluated on Year 3 Mock and Real Tests

	(1)	(2)
Test Type (Timing):	Mock (Oct. ’18)	Real (Nov. ’18)
Statistic:	Total Marks	Total Marks
A. Shared Subtopics, Shared Marks		
Form 1, Chapter 2, Fractions, Decimals and Percentages	1.5	1.5
Form 1, Chapter 9, Ratios, Profit and Loss	3	3
Form 2, Chapter 1, Exponents and Radicals	1.5	1.5
Form 2, Chapter 2, Algebra	7	7
Form 2, Chapter 3, Quadratic Equations	3	3
Form 2, Chapter 4, Logarithms	1.5	1.5
Form 2, Chapter 8, Pythagoras Theorem	3	3
Form 2, Chapter 9, Set Theory	3	3
Form 2, Chapter 10, Statistics	5	5
Form 3, Chapter 2, Functions	7	7
Form 3, Chapter 4, Rates and Variations	5	5
Form 3, Chapter 5, Sequence and Series	6	6
Form 3, Chapter 6, Circles	1.5	1.5
Form 3, Chapter 8, Accounts	10	10
Form 4, Chapter 1, Coordinate Geometry	3	3
Form 4, Chapter 2, Areas and Perimeters	3	3
Form 4, Chapter 4, Probability	5	5
Form 4, Chapter 5, Trigonometry	3	3
Form 4, Chapter 6, Vectors	3	3
Form 4, Chapter 8, Linear Programming	5	5
(Subtotal Marks of Panel A)	(80)	(80)
B. Shared Subtopics, Different Marks		
Form 2, Chapter 1, Exponents and Radicals	1.5	
Form 2, Chapter 2, Algebra	2	
Form 2, Chapter 3, Quadratic Equations	1.5	
Form 2, Chapter 4, Logarithms	1.5	
Form 2, Chapter 10, Statistics		5
Form 3, Chapter 2, Functions	4	
Form 3, Chapter 4, Rates and Variations		2
Form 4, Chapter 2, Areas and Perimeters		3
Form 3, Chapter 6, Circles		0.5
Form 4, Chapter 8, Linear Programming		5
(Subtotal Marks of Panel B)	(10.5)	(15.5)
C. Different Subtopics, Different Marks		
Form 1, Chapter 1, Numbers		3
Form 1, Chapter 10, Real Numbers		1.5
Form 1, Chapter 4, Approximations	3	
Form 2, Chapter 5, Congruence and Similarity	1.5	
Form 3, Chapter 7, The Earth as a Sphere	5	
(Subtotal Marks of Panel C)	(9.5)	(4.5)

Note: Breakdown of topics covered on year 3 program mock test (Oct. ’18) and O Level mathematics certification test (Nov. ’18).

Table A8: Structural Parameter Estimates: Benchmark Model

#	Value	Name	Label
1	1.0801	alpha0	alpha0
2	0.14	alpha1_cons	alpha1
3	0.1631	gamma0	gamma0
4	0.0011	gamma1_cons	gamma1
5	0.1478	delta_cons	delta constant
6	0.0001	delta_K0	delta (K0 - Kmin)**1 coef.
7	0.0457	tau_cons	tau constant
8	0.0068	tau_smrtech	tau SMR tech. coef.
9	4.7938	pi_incent	pi Y2 SMR incent. $ coef.
10	0.9	pi0scale	pi0scale
11	1350	theta_cons	theta constant
12	54.355	theta_stem	theta science-intended coef.
13	1E-08	iota	iota (effort of attending mock)
14	1.4136	Ebar	student-effort location
15	2.5775	Epower	student-effort convexity
16	-	K_th_cons	K_th constant
17	-	K_th_smrtech	K_th SMR tech. coef.
18	-	pi_th_cons	pi_th constant
19	8.5888	coef(K0,det,1,cons)	constant
20	0.1711	coef(K0,det,1,co1)	female indicator
21	0.4764	coef(K0,det,1,co2)	Y0 math score
22	1.0697	coef(K0,det,1,co3)	age
23	0.4445	coef(K0,det,1,co4)	FTNA nonmath average
24	-0.907	coef(K0,det,1,co5)	parental education
25	4.4662	coef(pi0,det,1,cons)	constant
26	0.0936	coef(pi0,det,1,co1)	female indicator
27	0.0019	coef(pi0,det,1,co2)	FTNA nonmath average
28	0.0793	coef(pi0,det,1,co3)	parental education
29	0.1914	coef(pi0,det,1,co4)	intended occupation = STEM
30	0.6598	coef(Aj,det,1,cons)	constant
31	0.0409	coef(Aj,det,1,co1)	is fulltime math teacher
32	0.0373	coef(Aj,det,1,co2)	has bachelor's degree
33	0.0117	coef(Aj,det,1,co3)	# years teaching math
34	-5E-04	coef(Aj,det,1,co4)	# years teaching math^2
35	0.4638	coef(Rj,det,1,cons)	constant
36	-1E-04	coef(Rj,det,1,co1)	total teacher hrs/wk
37	0.0215	coef(Rj,det,1,co2)	is fulltime math teacher
38	0.0203	coef(Rj,det,1,co3)	has bachelor's degree
39	0.0119	coef(Rj,det,1,co4)	# years teaching math
40	-3E-04	coef(Rj,det,1,co5)	# years teaching math^2
41	-2.395	coef(xi,det,1,cons)	constant
42	-0.009	coef(xi,det,1,co1)	util. diff.
43	-0.113	coef(xi,det,1,co2)	female indicator
44	0.0875	coef(xi,det,1,co3)	age
45	-0.119	coef(xi,det,1,co4)	parental education
46	0.0159	coef(xi,det,1,co5)	commute distance
47	29.173	sigma(K0,det,1,mu)	sigma of school random effect
48	0.7698	sigma(pi0,det,1,mu)	sigma of school random effect
49	0.2264	sigma(Aj,det,1,mu)	sigma of school random effect
50	0.2087	sigma(Rj,det,1,mu)	sigma of school random effect
51	0.0961	rho(K0cpi0c,det,1,mu)	rho(mu_K0, mu_pi0)
52	0.0062	rho(K0cAjc,det,1,mu)	rho(mu_K0, mu_Aj)
53	0.9999	rho(AjcRjc,det,1,mu)	rho(mu_Aj, mu_Rj)
54	15	sigma(K0i,det,1,omega)	sigma of indiv. random effect
55	0.0088	sigma(pi0i,det,1,omega)	sigma of indiv. random effect
56	0.9999	rho(K0ipi0i,det,1,omega)	rho(omega_K0, omega_pi0)
57	2995.6	coef(K0,meas,1,var)	m1 error variance
58	0.0172	coef(K0,meas,2,slope)	ftnamath slope
59	10.528	coef(K0,meas,2,cut1)	ftnamath cut1
60	1.3258	coef(K0,meas,2,diff2)	ftnamath cut2 - cut1
61	1.5553	coef(K0,meas,2,diff3)	ftnamath cut3 - cut2
62	1.0283	coef(K0,meas,2,diff4)	ftnamath cut4 - cut3
63	7.9404	coef(pi0,meas,1,var)	m1 error variance
64	0.631	coef(pi0,meas,2,slope)	likes_math slope
65	1.2086	coef(pi0,meas,2,cut1)	likes_math cut1
66	0.3803	coef(pi0,meas,2,diff2)	likes_math cut2-cut1
67	1.5752	coef(pi0,meas,2,diff3)	likes_math cut3-cut2
68	0.0122	coef(Aj,meas,1,var)	m1 error variance
69	-0.002	coef(Aj,meas,2,cons)	teacher_control constant
70	0.8933	coef(Aj,meas,2,slope)	teacher_control slope
71	0.0142	coef(Aj,meas,2,var)	teacher_control variance
72	0.0134	coef(Rj,meas,1,var)	m1 error variance
73	0.1371	coef(Rj,meas,2,cons)	teacher_attention constant
74	0.9154	coef(Rj,meas,2,slope)	teacher_attention slope
75	0.0112	coef(Rj,meas,2,var)	teacher_attention variance
76	39.723	coef(Ei,meas,1,var)	m1 error variance
77	0.3256	coef(Ei,meas,2,slope)	per_atte slope
78	0.3562	coef(Ei,meas,2,cut1)	per_atte cut1
79	0.9553	coef(Ei,meas,2,diff2)	per_atte cut2-cut1
80	0.9471	coef(Ei,meas,2,diff3)	per_atte cut3-cut2
81	1.0183	coef(Ei,meas,2,diff4)	per_atte cut4-cut3
82	3614.2	coef(Ki,meas,1,var)	m1 error variance

Notes: "K_th" refers to the $\underline{K}_0$ threshold. "pi_th" refers to the $\underline{\pi}$ threshold. "cons" is short for constant; "coef" and "co," coefficient"; "det," latent-factor-determinant equations; "meas," measurement-errror equations; "var," variance.

Table A9: Structural Parameter Estimates: Productivity-Thresholds Model

#	Value	Name	Label
1	1.0801	alpha0	alpha0
2	0.1285	alpha1_cons	alpha1
3	0.001	gamma0	gamma0
4	0.0021	gamma1_cons	gamma1
5	0.02	delta_cons	delta constant
6	0.0003	delta_K0	delta (K0 - Kmin)**1 coef.
7	0.0437	tau_cons	tau constant
8	0.0151	tau_smrtech	tau SMR tech. coef.
9	14.077	pi_incent	pi Y2 SMR incent. $ coef.
10	1.000	pi0scale	pi0scale
11	395.69	theta_cons	theta constant
12	-33.73	theta_stem	theta science-intended coef.
13	3.1143	iota	iota (effort of attending mock)
14	5.6134	Ebar	student-effort location
15	3.5845	Epower	student-effort convexity
16	554.05	K_th_cons	K_th constant
17	-154.9	K_th_smrtech	K_th SMR tech. coef.
18	7.8875	pi_th_cons	pi_th constant
19	8.3879	coef(K0,det,1,cons)	constant
20	-2.185	coef(K0,det,1,co1)	female indicator
21	0.4764	coef(K0,det,1,co2)	Y0 math score
22	1.3197	coef(K0,det,1,co3)	age
23	0.4445	coef(K0,det,1,co4)	FTNA nonmath average
24	-3.37	coef(K0,det,1,co5)	parental education
25	4.6662	coef(pi0,det,1,cons)	constant
26	0.0867	coef(pi0,det,1,co1)	female indicator
27	0.0018	coef(pi0,det,1,co2)	FTNA nonmath average
28	0.0849	coef(pi0,det,1,co3)	parental education
29	0.0958	coef(pi0,det,1,co4)	intended occupation = STEM
30	0.6161	coef(Aj,det,1,cons)	constant
31	0.0715	coef(Aj,det,1,co1)	is fulltime math teacher
32	-0.015	coef(Aj,det,1,co2)	has bachelor's degree
33	0.0057	coef(Aj,det,1,co3)	# years teaching math
34	0.0001	coef(Aj,det,1,co4)	# years teaching math^2
35	0.4301	coef(Rj,det,1,cons)	constant
36	-0.006	coef(Rj,det,1,co1)	total teacher hrs/wk
37	0.0423	coef(Rj,det,1,co2)	is fulltime math teacher
38	-0.007	coef(Rj,det,1,co3)	has bachelor's degree
39	0.0159	coef(Rj,det,1,co4)	# years teaching math
40	-2E-04	coef(Rj,det,1,co5)	# years teaching math^2
41	-2.395	coef(xi,det,1,cons)	constant
42	-0.002	coef(xi,det,1,co1)	util. diff.
43	-0.114	coef(xi,det,1,co2)	female indicator
44	0.0855	coef(xi,det,1,co3)	age
45	-0.102	coef(xi,det,1,co4)	parental education
46	0.017	coef(xi,det,1,co5)	commute distance
47	31.985	sigma(K0,det,1,mu)	sigma of school random effect
48	0.6823	sigma(pi0,det,1,mu)	sigma of school random effect
49	0.2118	sigma(Aj,det,1,mu)	sigma of school random effect
50	0.2048	sigma(Rj,det,1,mu)	sigma of school random effect
51	0.1816	rho(K0cpi0c,det,1,mu)	rho(mu_K0, mu_pi0)
52	-0.014	rho(K0cAjc,det,1,mu)	rho(mu_K0, mu_Aj)
53	0.9999	rho(AjcRjc,det,1,mu)	rho(mu_Aj, mu_Rj)
54	11.25	sigma(K0i,det,1,omega)	sigma of indiv. random effect
55	0.1875	sigma(pi0i,det,1,omega)	sigma of indiv. random effect
56	0.9999	rho(K0ipi0i,det,1,omega)	rho(omega_K0, omega_pi0)
57	3042.3	coef(K0,meas,1,var)	m1 error variance
58	0.0172	coef(K0,meas,2,slope)	ftnamath slope
59	10.528	coef(K0,meas,2,cut1)	ftnamath cut1
60	1.3346	coef(K0,meas,2,diff2)	ftnamath cut2 - cut1
61	1.5553	coef(K0,meas,2,diff3)	ftnamath cut3 - cut2
62	1.032	coef(K0,meas,2,diff4)	ftnamath cut4 - cut3
63	8.0357	coef(pi0,meas,1,var)	m1 error variance
64	0.631	coef(pi0,meas,2,slope)	likes_math slope
65	1.2733	coef(pi0,meas,2,cut1)	likes_math cut1
66	0.383	coef(pi0,meas,2,diff2)	likes_math cut2-cut1
67	1.5563	coef(pi0,meas,2,diff3)	likes_math cut3-cut2
68	0.0126	coef(Aj,meas,1,var)	m1 error variance
69	-0.015	coef(Aj,meas,2,cons)	teacher_control constant
70	0.9154	coef(Aj,meas,2,slope)	teacher_control slope
71	0.0136	coef(Aj,meas,2,var)	teacher_control variance
72	0.0128	coef(Rj,meas,1,var)	m1 error variance
73	0.1382	coef(Rj,meas,2,cons)	teacher_attention constant
74	0.9122	coef(Rj,meas,2,slope)	teacher_attention slope
75	0.0102	coef(Rj,meas,2,var)	teacher_attention variance
76	39.486	coef(Ei,meas,1,var)	m1 error variance
77	0.2973	coef(Ei,meas,2,slope)	per_atte slope
78	0.2019	coef(Ei,meas,2,cut1)	per_atte cut1
79	0.94	coef(Ei,meas,2,diff2)	per_atte cut2-cut1
80	0.9334	coef(Ei,meas,2,diff3)	per_atte cut3-cut2
81	1.0086	coef(Ei,meas,2,diff4)	per_atte cut4-cut3
82	3708	coef(Ki,meas,1,var)	m1 error variance

Notes: "K_th" refers to the $\underline{K}_0$ threshold. "pi_th" refers to the $\underline{\pi}$ threshold. "cons" is short for constant; "coef" and "co," coefficient"; "det," latent-factor-determinant equations; "meas," measurement-errror equations; "var," variance.

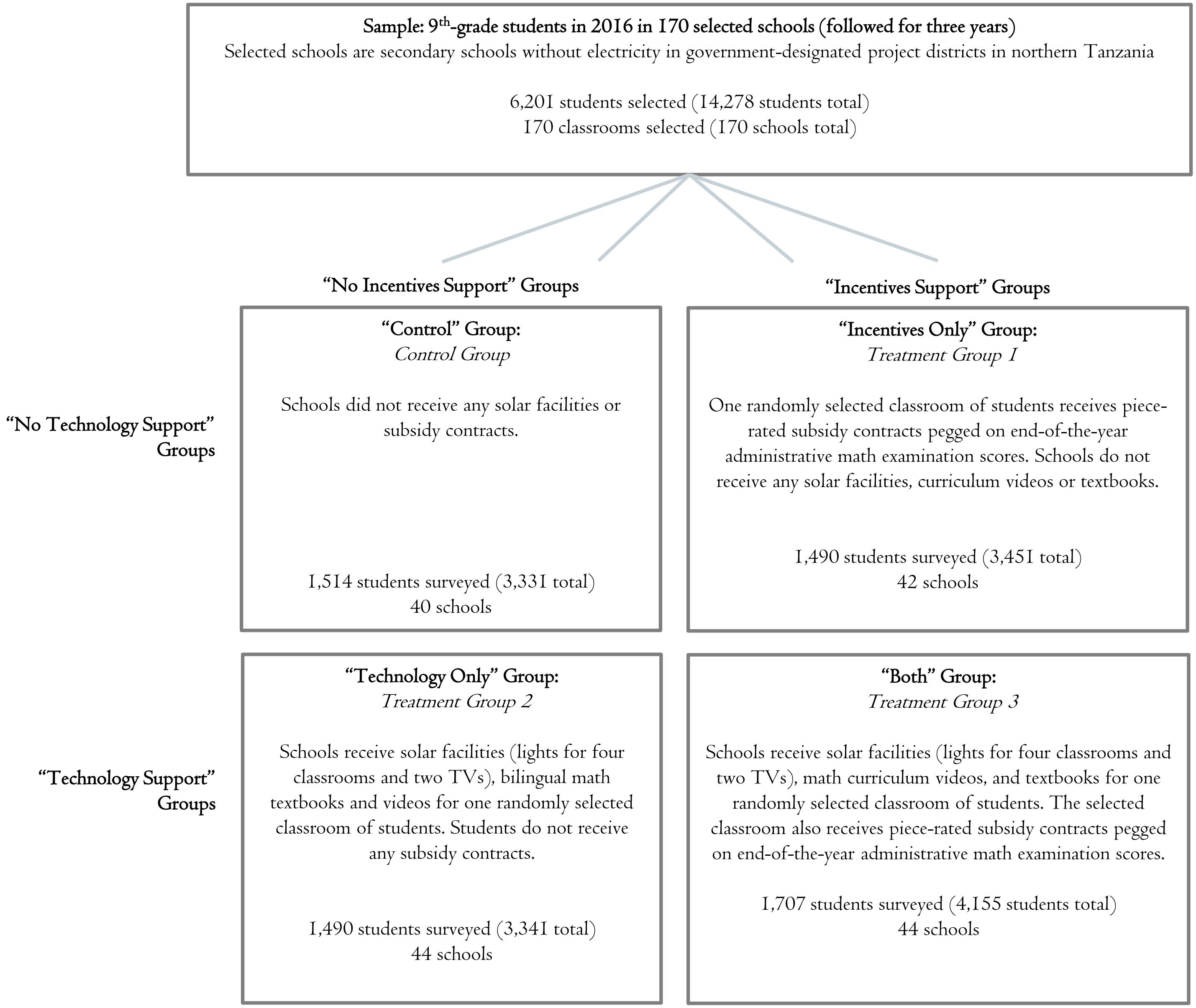
Table A10: Effects on O Level Mathematics Certification Examination Results and Breakdown of Difference from Mock Test

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome Unit:	Z-score Converted from Grade Brackets				Pass Indicator			
Variable Type:	Nov. '18 O Level	Oct. '18 Mock Test	Improvement on O Level	Deterioation on O Level	Nov. '18 O Level	Oct. '18 Mock Test	Improvement on O Level	Deterioation on O Level
Control (C) Mean	-0.0480	-0.0975	0.114	-0.0649	0.0962	0.0651	0.0401	-0.00900
(Std. Dev.)	(0.880)	(0.784)	(0.521)	(0.272)	(0.295)	(0.247)	(0.196)	(0.0945)
A. Treatment Variables								
Incentives Only (G1)	-0.0174	0.0215	-0.0195	-0.0194	-0.00755	0.00960	-0.00793	-0.00921
	(0.0495)	(0.0510)	(0.0257)	(0.0229)	(0.0161)	(0.0149)	(0.00929)	(0.00714)
	[1]	[0.816]	[0.818]	[0.362]	[1]	[1]	[0.652]	[0.178]
Technology Only (G2)	0.00332	0.0381	-0.00496	-0.0298	-0.0118	-0.00162	-0.00246	-0.00770
	(0.0494)	(0.0788)	(0.0282)	(0.0279)	(0.0132)	(0.0169)	(0.0109)	(0.00633)
	[1]	[0.816]	[1]	[0.362]	[1]	[1]	[1]	[0.178]
Both (G3)	0.0245	0.166***	-0.0495**	-0.0921***	0.000843	0.0483***	-0.0203**	-0.0272***
	(0.0443)	(0.0617)	(0.0225)	(0.0318)	(0.0136)	(0.0182)	(0.00842)	(0.00981)
	[1]	[0.0250]	[0.0960]	[0.0140]	[1]	[0.0270]	[0.0550]	[0.0190]
C. Linear Combinations of Estimators, Other Tests and Details								
β_{T3} - β_{T1}	0.0419	0.145**	-0.0300	-0.0727**	0.00839	0.0387**	-0.0123	-0.0180
	(0.0508)	(0.0612)	(0.0218)	(0.0363)	(0.0159)	(0.0187)	(0.00755)	(0.0110)
β_{T3} - β_{T2}	0.0212	0.128	-0.0445*	-0.0623	0.0126	0.0500**	-0.0178*	-0.0195*
	(0.0512)	(0.0898)	(0.0254)	(0.0409)	(0.0124)	(0.0206)	(0.00981)	(0.0108)
β_{T3} - β_{T1} - β_{T2}	0.0386	0.106	-0.0250	-0.0429	0.0202	0.0404	-0.00986	-0.0103
	(0.0708)	(0.100)	(0.0356)	(0.0463)	(0.0206)	(0.0255)	(0.0132)	(0.0130)
Observations	5,965	5,965	5,965	5,965	5,965	5,965	5,965	5,965
R-squared (Mean)	0.427	0.320	0.113	0.0385	0.378	0.277	0.0666	0.0139
Clusters	170	170	170	170	170	170	170	170
Pr. > Joint F, All Treat. = 0	0.869	0.0523	0.101	0.0295	0.725	0.0488	0.0594	0.0370

Note: Difference-in-means coefficients. Columns (1)-(4) concern Z-scores converted from the midpoint of each student's grade bracket: 88 (A), 67 (B), 52 (C), 37 (D) and 15 (F). Columns (5)-(8) concern pass indicators given by a grade of D or above. Controls: age, year-0 score, commute distance. Regressions drop transferred students. Controls were missing at random for about 10% of students; estimates were obtained using multiple impuation and combined using Rubin's (1987) formulas. Standard errors: clustered by school. Levels of significance: *** p<0.01, ** p<0.05, * p<0.10. In brackets: Benjamini-Krieger-Yekutieli (2006) sharpened two-stage q-values (for three hypotheses).

B Appendix Figures

Figure B1: Experimental Design



Sample: 9th-grade students in 2016 in 170 selected schools (followed for three years)
Selected schools are secondary schools without electricity in government-designated project districts in northern Tanzania

6,201 students selected (14,278 students total)
170 classrooms selected (170 schools total)

"No Incentives Support" Groups

"Incentives Support" Groups

"No Technology Support" Groups

"Control" Group:
Control Group

Schools did not receive any solar facilities or subsidy contracts.

1,514 students surveyed (3,331 total)
40 schools

"Incentives Only" Group:
Treatment Group 1

One randomly selected classroom of students receives piece-rated subsidy contracts pegged on end-of-the-year administrative math examination scores. Schools do not receive any solar facilities, curriculum videos or textbooks.

1,490 students surveyed (3,451 total)
42 schools

"Technology Support" Groups

"Technology Only" Group:
Treatment Group 2

Schools receive solar facilities (lights for four classrooms and two TVs), bilingual math textbooks and videos for one randomly selected classroom of students. Students do not receive any subsidy contracts.

1,490 students surveyed (3,341 total)
44 schools

"Both" Group:
Treatment Group 3

Schools receive solar facilities (lights for four classrooms and two TVs), math curriculum videos, and textbooks for one randomly selected classroom of students. The selected classroom also receives piece-rated subsidy contracts pegged on end-of-the-year administrative math examination scores.

1,707 students surveyed (4,155 students total)
44 schools

Figure B2: Experimental Timeline

September 2015

- Pilot Phase Solar Installations: Technology Support groups randomly selected and pilot systems tested (lights for 1~2 classrooms and one 16" TV)

November 2015

- Grant awarded for full study
- Classrooms sampled and cross-randomized into Incentives groups

February 2016

- Pre-incentives Examination & Survey: Mock test for 8th-grade students administered
- Year 1 Incentives Contracts and Technology Support Delivered: Contracts announced and books & videos delivered to respective support groups

May 2016

- Main Phase Solar Installations: Additional solar systems (lights for 1~2 additional classrooms and one 19" TV) installed for Technology Support groups

October 2016

- Year 1 Mock Examination & Survey: Administered for 9th-grade students
- Incentive awards distributed

November 2016

- '16 National Promotional Examinations: administered for 9th-grade students

February 2017

- Year 2 Incentives Contracts and Technology Support Delivered: Contracts announced and books & videos delivered to respective support groups

October 2017

- Year 2 Mock Examination & Survey: Administered for 10th-grade students
- Incentive awards distributed

December 2017

- Solar Installations for Remaining Schools: Solar systems installed in Technology Support groups also installed in remaining groups, as promised. Only the original Technology Support groups continue receiving new textbooks and videos.

February 2018

- Year 3 Incentives Contracts and Technology Support Delivered: Contracts announced and books & videos delivered to respective support groups

October 2018

- Year 3 Mock Examination & Survey: Administered for 11th-grade students
- Incentive awards distributed

November 2018

- Year 3 Certification Examination: Administered for 11th-grade students

Figure B3: Textbooks, Videos and Final Year Tests

Period	Learning Material	URL
Year 1 (Form 2) [Delivered Feb., 2016]	Sharpening Mathematics Review Textbook	https://tinyurl.com/temp-year1-yssm-textbook
	Sharpening Mathematics Review Videos	https://tinyurl.com/year1-yssm-video
Year 2 (Form 3) [Delivered Feb., 2017]	Sharpening Mathematics Review Textbook	https://tinyurl.com/temp-year2-yssm-textbook
	Sharpening Mathematics Review Videos	https://tinyurl.com/year2-yssm-video
Year 3 (Form 4) [Delivered Feb., 2018]	Sharpening Mathematics Review Textbook	https://tinyurl.com/temp-year3-yssm-textbook
	Sharpening Mathematics Review Videos	https://tinyurl.com/year3-yssm-video
End of Year 3 (Form 4) [Administered Oct., 2018]	Sharpening Mathematics Review (Mock) Test	https://tinyurl.com/year-3-mock-test
End of Year 3 (Form 4) [Administered Nov., 2018]	Certificate of Secondary Education Examination – Basic Mathematics (Real) Test	https://tinyurl.com/year3-real-test

C Appendix Proof

Proposition C.1. *Let $MB(K\,|\,\pi,\theta,T,\sigma) = \pi + \frac{\theta}{\sigma}\phi\left(\frac{K-T}{\sigma}\right)$ (a bell curve plus a constant), and $MC(K\,|\,a,b,\lambda) = a(K-b)^{\lambda}$ (a power curve with a horizontal intercept). Then, MB and MC cross each other within the interval $(b+(\pi/a)^{1/\lambda},\infty)$ in at least one place and at most three places. When the two curves intersect once, the intersection is the argument maximum of the integral: $U(K) = \int_b^K MB - MC \,\mathrm{d}K$. When they intersect twice, the argmax is either the first or the second intersection point and is unique. When they intersect three times, the argmax is generally unique, can involve no more than two points, and involves two points only in a degenerate case, a case of measure zero when some parameters are drawn from a continuous distribution.*

Proof Note that intersection points remain where they are when I divide through both MB and MC by θ. For notational simplicity, I restrict my attention to this case. I also restrict my attention to the case in which MC is strictly convex ($\lambda > 1$); the concave case ($\lambda \leq 1$) is straightforward and omitted.

First, I show that MB and MC intersect at least once. As K increases from $K = b$, the position of MC rises from below π, crossing π from below at $K = b + (\pi/a)^{1/\lambda}$. Thereafter, as $K \to \infty$, $MC \to \infty$ and $MB \to \pi$. By the intermediate value theorem, MB and MC intersect at least once.

Second, I show that MB intersects MC in at most three places. Let $\tilde{MB} = MB - \pi$ and $\tilde{MC} = MC - \pi$. I show this by demonstrating that (1) the log rates of change of $\tilde{MB}$ and $\tilde{MC}$ intersect each other in at most two points; (2) each of the three segments of the range of K partitioned by these two intersection points (before the first point, between the two points, and after the second point) admits at most one intersection point of MB and MC.

To see (1), note that the log rate of change of $\tilde{MB}$ falls linearly in K, while the log rate of change of $\tilde{MC}$ falls from infinity and is strictly convex in K.[55] These log rates of change intersect in at most two places, since, by definition, a strictly convex curve intersects any line in at most two places.

To see (2), note that, if the log rates intersect in two places, since the log rate of MC falls from infinity, the log rate of $\tilde{MC}$ must remain (i) above the log rate of $\tilde{MB}$ before, (ii) below the log rate of $\tilde{MB}$ between, and (iii) above the log rate of $\tilde{MB}$ after, the two intersection points. Since MC starts from below MB, the earliest instance at which MC crosses MB is from below; at this point MC is rising more quickly than MB, i.e., $MC'(K^*) > MB'(K^*)$, $K^* = \min\{K\,|\,MC(K) = MB(K)\;\&\;K \geq b\}$. At this intersection, $MB = MC$; therefore, $\frac{MC'}{MC} > \frac{MB'}{MB}$, which only case (i) admits. After the earliest instance at which MC crosses MB, the two curves intersect again only if the rate of change (and the log rate of change) of MC falls sufficiently below that of MB, which only case (ii) admits. If the two curves intersect twice, after the two intersection points, the rate of change (and the log rate of change) of MC eventually crosses from below, and remains above after, the rate of change (and the log rate of change) of MB, which only case (iii) admits. Since no further crossing of the rates of change occurs beyond (iii), MB and MC intersect in at most three places.

When the two curves intersect once, MC remains always below before, and always above after, K^*; hence K^* identifies the argmax K. When the two curves intersect twice, one of the two intersection points is tangential.[56] MC remains always equal to or below before, and always equal to or above after, either the first or the second intersection point; hence, the argmax K is either the first or the second intersection point, and is uniquely identified. Lastly, let $K_{p1} < K_{p2} < K_{p3}$ denote the three intersection points when the two curves intersect three times. The middle intersection point immediately follows an interval in which MB remains below MC; hence, K_{p2} cannot be an argmax.

[55] $\frac{\tilde{MB}'}{\tilde{MB}} = \frac{T-K}{\sigma^2}$, $\frac{MC'}{MC} = \frac{\lambda}{(K-b)-\pi(K-b)^{1-\lambda}/a}$, and $\left(\frac{\tilde{MC}'}{\tilde{MC}}\right)'' = \frac{2\lambda\left(1+(\lambda-1)\pi(K-b)^{-\lambda}/a\right)}{\left[(K-b)-\pi(K-b)^{1-\lambda}/a\right]^3} + \frac{\lambda^2(\lambda-1)\pi(K-b)^{-1-\lambda}/a}{\left[(K-b)-\pi(K-b)^{1-\lambda}/a\right]^2} > 0.$

[56] Assume for contradiction that neither intersection point is tangential. Then, it must be the case that, after the first intersection point, MC crosses MB from above. In the limit, however, MC must rise from below above MB at least once again, a contradiction.

Finally, I show that in general the argmax is unique, while dual argmax occur only in degenerate cases. I show this by demonstrating that (1) given parameter $\eta \in \boldsymbol{\eta} = \{T, \sigma, \pi, a, b, \lambda\}$, the number of values of η that satisfy $D(\eta, K_{p1}, K_{p3}) = U(K_{p3}, \eta \,|\, \boldsymbol{\eta} - \{\eta\}) - U(K_{p1}, \eta \,|\, \boldsymbol{\eta} - \{\eta\}) = 0$ is at most one; (2) given $\eta = \sigma$, the set of values is also degenerate (one can easily check numerically that the number of values is at most two, and that these values never occur in practice, which I verify during estimation).

To see (1), note that by the envelope theorem, $\frac{\mathrm{d}D(\eta, K_{p1}(\eta), K_{p3}(\eta))}{\mathrm{d}\eta} = \frac{\partial D(\eta, K_{p1}, K_{p3})}{\partial \eta}$, whose sign one can easily check is either strictly positive or negative, meaning $D = 0$ for at most one value of η.[57]

To see (4), I show that the case of $\frac{\mathrm{d}D}{\mathrm{d}\sigma} = 0$ is non-generic; if this claim is true, then the case of $D(\sigma, K_{p1}, K_{p3}) = 0$ is non-generic, since where the derivative changes its sign is non-generic. Note that $\frac{\mathrm{d}D}{\mathrm{d}\sigma} = -\phi\left(\frac{K_{p3}-T}{\sigma}\right)\frac{K_{p3}-T}{\sigma} + \phi\left(\frac{K_{p1}-T}{\sigma}\right)\frac{K_{p1}-T}{\sigma} = 0 \Rightarrow \sigma MB'(K_{p3}) = \sigma MB'(K_{p1})$. This equation pins down K_{p1} and K_{p3} as two roots of a Lambert's W relation for a given value of $MB'(K)$; since the relation is determined entirely by the shape of the MB curve and not by the local argmax constraints $MB(K_l) = MC(K_l)$, $l = 1, 3$, both this relation and the local argmax constraints are not satisfied for generic K_{p1}, K_{p3}.[58]

[57] Intuitively, $D(\eta, K_{p1}, K_{p3})$ is the difference between two areas: Area 1, below MC and above MB from K_{p1} to K_{p2}; and Area 2, below MB and above MC from K_{p2} to K_{p3}. The sign of $\frac{\partial D}{\partial \eta}$ shows that changing any $\eta \in \boldsymbol{\eta} - \{\sigma\}$ in a unidirectional way, holding all other parameters constant, strictly changes either Area 1 or Area 2 in a unidirectional way, while changing the other area in the opposite way. Hence, Area 1 can equal Area 2 for at most one value of η.

[58] A helpful way to think through this is to visualize how K_{p1} and K_{p0} comove as σ increases, where K_{p0} is the point that satisfies $MB'(K_{p0}) = MB'(K_{p3}), K_{p0} < K_{p3}$. As σ increases from zero, the top of the MB bell curve (located at $K = T$) falls to cross MC from above and continues falling, pushing K_{p3} leftward from $K = T$, allowing the relation $MB'(K_{p3}) = MB'(K_{p0})$ to hold and K_{p0} to exist, and pushing K_{p0} rightward from $-\infty$ as $MB(K_{p0})$ moves up along the MB curve. At the same time, K_{p1}, if it exists, grows rightward from beyond $b + (\pi/a)^{1/\lambda}$ to satisfy the local argmax constraint as $MC(K_{p1})$ moves along the MC power curve. In general, K_{p0} and K_{p1} do not overlap because they move at two distinct speeds, one determined solely by the shape of the MB curve and the other additionally influenced by the shape of the MC curve: $\frac{\mathrm{d}K_{p0}}{\mathrm{d}\sigma} = \frac{MB''(K_{p3})}{MB''(K_{p0})}\frac{\mathrm{d}K_{p3}}{\mathrm{d}\sigma} - \frac{\frac{\partial MB'(K_{p3})}{\partial\sigma} - \frac{\partial MB'(K_{p0})}{\partial\sigma}}{MB''(K_{p0})}$, $\frac{\mathrm{d}K_{p1}}{\mathrm{d}\sigma} = -\frac{\frac{MC'(K_{p3})}{MB'(K_{p3})} - 1}{\frac{MC'(K_{p1})}{MB'(K_{p1})} - 1}\frac{MB''(K_{p1})}{MB''(K_{p3})}\frac{\mathrm{d}K_{p3}}{\mathrm{d}\sigma}$. In numerical simulations, K_{p0} and K_1 overlap at most once; equivalently, the difference in two utilities at K_{p1} and K_{p3} are convex in σ: $\frac{\mathrm{d}^2 D}{\mathrm{d}\sigma^2} = \frac{\mathrm{d}^2 U(K_{p3})}{\mathrm{d}\sigma^2} - \frac{\mathrm{d}^2 U(K_{p1})}{\mathrm{d}\sigma^2} > 0$, where $\frac{\mathrm{d}^2 U(K)}{\mathrm{d}\sigma^2} = \frac{x(x - yz(z^2-1))}{y + xz}$, $x \equiv \left(a(K-b)^\lambda - \pi\right)\frac{1}{\sigma}$, $y \equiv \lambda a(K-b)^{\lambda-1}$, $z \equiv \frac{K-T}{\sigma}$.