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Do School Electrification and Provision of Digital Media Deliver Educational Benefits? First-year Evidence from 164 Tanzanian Secondary Schools *

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Abstract

This paper reports results from a one-year pilot study that evaluated impacts of partial school electrification and provisions of language-varied educational videos on achievements of O-level (11th-grade) students in northern Tanzania. The GivePower school program randomized 164 schools into six groups: G1 schools received two 0.12 kWh solar home systems including lights and TVs ("facilities"); G2, solar facilities and English videos; G3, solar facilities and bilingual videos; G4, English videos only; G5, bilingual videos only; and control schools. Solar facilities provided lights to 20% of classrooms and offices in recipient schools on average. Videos included two sets: one set, solving past 10 years of biology and geography exams; another set, encouraging self-esteem, habit formation, future orientation and other cognitive-behavioral character traits. After one year, the treatments did not produce significant achievement gains, although G2 schools (solar and English videos) reported large and significant increases in video-based instruction hours, and G3 schools (solar and Bilingual videos) suggested the highest test score gains. I estimate the impact of solar-facilities-enabled programs, averaged across video-provision status, to be 0.05σ on O-level test scores and 2.8 percentage points (pp) on pass rates, and rule out impacts larger than 0.13σ and 6.7 pp in the first year. Second year results are expected to be announced in 2018. (JEL I21, I28, O13, Z13)

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1 Introduction

One in seven people today lack access to electricity worldwide, and one in three lack access to education in their native language.[1] This paper ask two questions. First, does school electrification affect academic achievement? Second, does provision of complementary inputs, such as language-varied educational videos, affect academic achievement?

Does school electrification matter? The United Nations' Advisory Group on Energy and Climate Change lists proper functioning of schools among the foremost benefits of expanding electricity access (AGECC, 2010). In Tanzania, connecting schools is an important priority of the government's rural electrification program (IED, 2014).[2] Glewwe et al. (2013), however, observe that though their intuitions suggest school electrification should positively affect achievement, correlation studies in their review show noisy and insignificant relationships, and that credible causal evidence is lacking.

This paper's first contribution is to report the earliest known experimental evidence on this question, enabled by a generous research partnership called the GivePower school program. To preview the result, the first-year evidence is negative. Schools without electricity that randomly received free solar home systems, comprising lights for an average of 2.5 school rooms and two televisions, produced little gains in test scores or pass rates, compared to schools that did not receive such support. I estimate the average impact of solar-facilities-enabled programs to be 0.05σ on test scores in the junior-secondary (11th-grade O-level) exit exams and 2.8 percentage points (pp) on pass rates, and rule out impacts larger than 0.13σ and 6.7 pp in the programs' first year.[3]

Why did school electrification fail to deliver benefits? It may be helpful to review, here, a recent set of studies in seeming disagreement with each other about the impact of electrification on economic-growth-related variables. Earlier works have pointed to positive effects of electrification on female employment in South Africa (Dinkelman, 2011), educational investments in El Salvador (Barron and Torero, 2014) and economic growth in Brazil (Lipscomb et al., 2013); however, more recent experimental evaluations involving large-scale household grid connections in Kenya (Lee et al., 2016), India (Aklin et al., 2017) and Tanzania (Chaplin et al., 2017) report remarkably low and insignificant—even adverse—effects on comparable outcomes.[4] Studies focusing on links between education and household electrification have also found mixed and mostly null results.[5]

[1]Electricity access: World Bank (2015) estimate. Native-language education access: UNESCO (2016) figure, citing Walter and Benson (2012). UNESCO (2015) further estimates that a majority of primary and secondary schools in sub-Saharan countries lack electricity access.

[2]Importantly, though, data from this study's control group show that that the priority connection is usually at the scale of one household meter, and that electrifying the whole school may take many additional years to complete. Hence, the educational value of partial electrification is a relevant policy question.

[3]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.

[4]Aklin et al. (2017) report minimal response in household educational time use within one year. Chaplin et al. (2017) report minimal impact on household income within two to three years. Lee et al. (2016) estimate a net reduction in social welfare within two years, implying negative growth.

[5]Although Agoramoorthy and Hsu (2009), Samad et al. (2013) and Furukawa (2013) report longer hours spent on homework, and Khandker et al. (2013) find higher attendance rates of children in electrified households, these

These conflicting set of results suggest that the timing of environmental factors that facilitate growth effects of electrification may matter more than the literature previously thought. One may speculate that, over the cited periods in South Africa and El Salvador, what crucially mattered in the backdrop of electrification were availabilities of household-labor-saving appliances such as the refrigerator, water infrastructure that enabled laundry machines, and labor market conditions favorable enough to induce translations of time saved at home into work away from home.[6] Conversely, without the timely development of these complementary factors that raise people's effective demand for electricity's productive application, electrification by itself might long have remained merely an increased potential, bearing a low rate of return (Greenstone, 2014).[7]

The rest of the experimental arms studied in this paper examine this reasoning in the context of development education. They test whether jointly providing educational and motivational videos raises students' effective demand for electricity-enabled education.

While to my knowledge this paper is the first to examine the effect of interacting electricity, television and educational videos, it is noteworthy that a number of past studies have presented a sobering picture regarding potential impacts of school-based ICT interventions. In Israel and Peru, state-purchased computers provided to schools had no effect on test scores (Angrist and Lavy, 2002; Cristia et al., 2017). Internet connection subsidies had no effect on test scores in California (Goolsbee and Guryan, 2006). Provisions of a popular instructional computer program had no effect on reading skills in a large urban US district (Rouse and Krueger, 2004). In contrast, Machin et al. (2007) found some positive achievement impacts of subsidies for ICT-specific expenditures in the UK. In India, Banerjee et al. (2007) found dramatic achievement impacts of educational computer games, whose level of difficulty responded to 4th-grade students' ability to solve math problems. Although originating from different settings, these results suggest that one's expectation of the impact of providing a new educational technology on achievement, unless that technology can somehow dynamically respond to the eye level of students, should be low.

This suggestion is particularly emphasized by Glewwe et al. (2009), who report that textbooks provided in Busia County, Kenya, minimally affected average performance, but raised the performance of students who had pretested in the top quintile. The authors suggest that this was because while many students did not understand English well, the textbooks were in English (as was the language of instruction), and only the top students comprehended enough English to benefit. A logical extension of this suggestion is that crossing mother-tongue content with electric media may

studies do not report impacts on more direct measures of human capital such as graduation rates or test scores. Chen et al. (2017) and Grimm et al. (2017) find no response educational investments. Furukawa (2013) report negative and insignificant test score changes. Lipscomb et al. (2013) exceptionally reports large and significant changes in measures of educational attainment; however, their estimate concerns the impact of hydroelectric-plant placements on county-average outcomes, and as such may have been driven more by labor demand effects than by electrification-induced reductions in the cost of learning.

[6] Works that have studied the relevance of household electric appliances in developed countries include Greenwood et al. (2005), de V. Cavalcanti and Tavares (2008) and Coen-Pirani et al. (2010).

[7] The spirit of this reasoning is similar to that in Greenstone and Jack (2015), where the authors discuss complementary factors that determine the growth value of, and the marginal willingness to pay for, environmental quality in the developing world.

be able to raise the marginal effectiveness of partial school electrification by better meeting the eye level of native-language students.

While I sought to investigate these additional questions beyond the impact of school electrification, the results were weak and statistically imprecise. Additionally providing complementary videos did not significantly raise performance compared to providing electricity alone. Although jointly providing bilingual videos suggestively produced larger point estimates than providing electricity alone or providing English videos, neither the point estimates nor the differences were statistically significant. This was despite the fact that solar-receiving groups reported conducting a large number of video-based instruction per week and seeing substantial proportions of students remaining behind to use electricity after regular school hours each week. In the concluding sections, I discuss potentially important barriers that may need to be overcome before investments in school electrification can generate significant achievement gains. Finally, I note that these are preliminary findings from running the program for one year (13 months between September, 2015, and October, 2016). I am evaluating the second and final year of the program, whose results are expected to be announced in 2018.

The rest of the paper is organized as follows. Section 2 describes the institutional setting. Section 3 lays out the design and empirical framework. Section 4 presents and discusses the results. Section 5 concludes.

2 Institutional Setting

Tanzania represents a fast-growing sub-Saharan nation, with a population of approximately 54 million, per capita GDP of $960, growth rate of 7.0%, electricity access rate of 16% and junior secondary net enrollment ratio of 48.1%.[8] In terms of energy policy, Tanzania is focusing on expanding grid-line extensions from the current coverage of 36% of villages as of June, 2016, to 100% of villages by 2021 (REA, 2017).[9] In terms of education policy, Tanzania is focusing on raising pass rates in primary and secondary education (PO-RALG, 2014).

Why are pass rates important? First, to fail means to lose at least a year of effort put into attending school, with no certification to show for it. Re-enrolling in government schools is prohibited for failed students.[10] Alternative candidacy exams do not necessarily provide equivalent recognition, as regular passers receive priority over alternative passers in gaining admission to institutions of higher learning. On the other hand, many career and educational opportunities require a junior secondary pass as a prerequisite. These opportunities include security-guard jobs

[8]Population, per capita GDP and growth rate are 2015 figures, and electricity access rate is a 2014 figure (World Bank, 2015). Junior secondary gross enrollment ratio is a 2015 figure (PO-RALG, 2016). Sub-Saharan averages (number of countries with data), inverse-variance weighted by country population, are as follows: population, 6.6 million (48); per capita GDP, $1,800 (47); growth rate, 6.0% (46); electricity access rate, 37% (48); and secondary net enrollment ratio, 33% (16) (World Bank, 2015).

[9]It may take additional years or even decades, however, for all households in a village to eventually receive connections after a distribution line reaches the village (IED, 2014).

[10]Private schools, meanwhile, are costly alternative, with average fees exceeding 100 times that of government schools (Jingi, 2015).

and janitorial jobs, as well as vocational education programs, offered by both government and private institutions, to receive training as tour guides, clerks, hotel staff, private primary school teachers, ICT staff and other basic service workers.[11] Hence, passing is tied to student welfare.

Pass rates also track educational quality, an important development concern. The first panel of fig. 1 shows that, while the number of youths entering the junior-secondary system (grades 8 through 11) rose more than fivefold since the turn of the millennium, the number passing has lagged far behind. One sees that the gap between the two lines began opening up in 2007, and that it has since grown to account for more than 23% of Tanzania's junior-secondary age-group population, a generation of youths entering the nation's secondary education system yet failing to see their education through.[12] A similar story is reflected the second panel of fig. 1, which presents the historical trend in the government's targeted "pass rates," defined as the number of students passing their junior-secondary exit examinations (11th-grade O-levels) divided by the number students sitting for the same examinations. One can see a fall from 90% to 70% over the plotted period, suggesting a drop in the quality of education received by the average candidate.

What factors might be causing, and what policies might it take to remedy, such lagging achievement? I investigated two potential factors. First, motivated by the GivePower school program, whose initiation is described in the next section, I considered lack of electricity in newly established schools. Second, I noted Tanzania's language of education policy.

Lack of electricity might adversely affect learning by limiting the extent of time and means of study, yet many new secondary schools in Tanzania operated in villages without electricity access. For example, the 164 secondary schools the GivePower program came to engage comprised the universe of secondary schools without electricity in 23 targeted districts in 2015. These schools were approximately the last 20% of schools in these districts that were still without electricity access in 2015, and these schools tended to be young: on average they had been operating for 8.5 years, and all but 7 schools (96%) had been founded after the turn of the millennium.[13]

Given that the majority of schools in Tanzania are day schools, there was a question of whether school lighting would be of much value even before the project began. Field interviews suggested and survey evidence later verified, however, that substantial activity took place in schools even after regular hours.[14] The national curriculum document also strongly recommends co-curriculum activities (MoEVT, 2007). These recommended activities include sports and games, subject clubs (e.g. history club), conservation (e.g. tree planting), anti-corruption clubs, sex and HIV education,

[11] See, for example, www.vetamikumi.com/admission-en.htm and www.atc.ac.tz/index.php/admissions/vet-programmes. Beyond the lowest pass, which just requires passing in two subjects, passing more subjects or getting a higher division pass qualifies students to apply to progressively harder opportunities, such as community colleges and universities. Admittedly, a jump between failing and obtaining the lowest pass, the margin relevant in this paper, is a humble achievement; yet, it is not a meaningless achievement.

[12] 45.5% enrolled in 2013 minus 22.5% passing gives 23% of population (or 50.6% of students enrolled).

[13] Centralized data on the electrification status of government schools do not exist. These statistics were collected by interviewing individual district education officers, who themselves verified the statistics by calling individual school administrators.

[14] School surveys, discussed in later sections, verify that, in the control group, the gross participation of students in extracurricular activities per week was 44% in 2016.

leadership life skills education, peer education and remedial classes. Therefore, it seemed quite possible to expect at least some students to remain behind in school after regular hours and benefit from new school lighting.

Another factor noted to be possibly contributing to Tanzania's lagging educational quality was the nation's language policy, which abruptly switches the language of instruction in public schools from Swahili in grades 7 (end of primary school) to English in grade 8 (beginning of junior secondary school).[15] Past research into the value of such a switching of the instructional language mid-curriculum, a policy quite widespread across the developing world, has not found consensus. Regarding the impact of extending the years of mother-tongue education, some works suggest dramatic achievement benefits (see Trudell (2016) and Laitin and Ramachandran (2016) for review), while another work suggests a reduction in long-run incomes (Angrist and Lavy (1997)). This disagreement may be hinging on the fact that an average achievement effect is an integration of distributional effects: a language shock may be beneficial for children whose prior preparation and parental support can facilitate adaptation, but the same shock may force children without such support to remain lost in the language gap for many years, impeding and demotivating educational growth. As larger proportions of school-age population enter the education system, chances are these new entrants come from relatively less well-prepared backgrounds. No randomized-controlled trial has been evaluated on this topic to date, however, and while the program evaluated in this study by no means provides a direct test of this question, the program did seek to address at least a small part of this question by varying the language of explanation of the program video content.

A final piece of institutional detail I note here is the presence of a large number of youth empowerment organizations, an example of which is GivePower's program partner, Youth Shaping and Sharpening Movement (YSSM).[16] YSSM has operated in northern Tanzania since 2013 with a membership of approximately 70 volunteers. Their main activities are holding mentorship and motivational events for students across different townships. As volunteers, YSSM members are unpaid, yet local demand for such events measured by participation seems high.[17] As reiterated in the next section, while YSSM's facilitators have received no formal psychology training (similar in some sense to facilitators in Heller et al. (2017) and Blattman et al. (2017)), aspects of their programs seem to reflect characteristics of cognitive-behavioral therapies.[18] All of YSSM's mentorship events are conducted in Swahili.

[15]Relative to the rest of sub-Saharan African nations, Tanzania is on the longer side of conducting education in a local language before switching to a western language: sub-Saharan African nations on average conduct 3.4 years (standard deviation 2.7 years) of education in a local language before switching to a western language of instruction.

[16]The government's NGO Coordination Department currently lists 236 children and youth development organizations on its website. The webpage seems outdated, and the real number may be much larger.

[17]YSSM's internal records show that, in the two years leading up to the beginning of our GivePower school program (September, 2013 to August, 2015), YSSM held approximately 20 events per year with 280 students participating in each event on average, reaching a cumulative number of 10,930 students over the period.

[18]Heller et al. (2017) and Blattman et al. (2017) report that low-cost, cognitive-behavioral therapies delivered by minimally trained facilitators led to remarkable gains in pass rates and reductions in crime rates among youths in Chicago and Liberia. Their suggested mechanism—that the interventions worked by influencing patience, self-identity and values—may have applied in this study, though not strongly enough to be detected.

3 Methods

3.1 Sample Selection

The sample analyzed in this paper includes 164 schools, which comprised the universe of secondary schools that were without electricity in 20 northern Tanzanian districts in September, 2015, and that fielded 10,171 candidates in grade 11 taking junior secondary leaving examinations (O-levels) in November, 2016. This section gives a brief history of sample selection.

In 2015, Give Power Foundation, a US-based clean-energy nonprofit, began an initiative to donate the costs of purchasing solar systems for a large number of Tanzanian secondary schools to Off Grid Electric (OGE), a major solar energy equipment supplier headquartered in Tanzania. A research partnership was formed to investigate the impact of this initiative on the academic achievement of targeted schools. The research team consisted of the present author from the Energy Policy Institute at Chicago (EPIC) and YSSM, to operate under the care of the President's Office – Regional Administration and Local Government (PO-RALG).

The research team initially targeted the universe of schools without electricity in 23 northern Tanzanian districts, an intersection of districts to which the team's solar technology partner, Off Grid Electric (OGE), had expanded operations by 2015, and whose 2014 average pass rates were on the lower side compared to pass rates across the rest of the nation, as shown in fig. 2. The President's Office–Regional Administration and Local Government (PO-RALG) approved the initiation of this study and operations began in the study districts.

Through the government, YSSM and I requested a list of un-electrified schools, to be verified by district educational officers who were asked to call all the schools in their district. No up-to-date list of school electrification status had been maintained by the central government. The list received from the government contained 208 schools, and on September 1, 2015, the present author randomly allocated these schools into six assignment arms. The randomization and the assignment arms are described in detail in the next section.

Subsequent visits to deliver intervention materials led to an unexpected reduction in sample size. Out of 208 schools, 34 schools were already connected to the national grid or had solar facilities installed in all classrooms. Further 10 schools were relatively newly established schools, whose students were too young to register for the 2016 junior-secondary exit exams. These schools were excluded from the program, as they should never have been included in the sample in the first place.[19] Thus, the final sample came down to 164 schools. Power calculations are discussed in section 3.5.1.[20]

[19] No data were collected from these schools.

[20] It would have been ideal to re-randomize after verifying the electrification status more thoroughly; however, this was made difficult because the research team began with no budget in September, 2015, and was seeking to minimize the operational burden of our field installation partner, OGE, who was operating under schedule to finish GivePower installations by the end of September, 2015. Research funding was subsequently approved by the International Growth Centre (IGC) in November, 2015, enabling the research team to conduct a school survey in February, 2016; the survey retrospectively asked for school characteristics pertaining to August, 2015. Additional grants by the IGC and the Abdul Latif Jameel Poverty Action Lab (J-PAL) Post Primary Education Initiative enabled the conduction of a follow-up school survey of pedagogy and educational time use in October, 2016.

3.2 Design

Randomization was performed on the initial list of 208 schools. Schools were block-randomized by 2014 pass-rate tiers of six schools within each region (a higher administrative unit than district). Within each block of schools, the following six assignment groups were randomly assigned.[21]

1. "Solar x TV x No Video (G1)" receiving solar lights and TVs ("facilities") only;
2. "Solar x TV x English Videos (G2)" receiving solar facilities and English videos;
3. "Solar x TV x Bilingual Videos (G3)" receiving solar facilities and bilingual videos;
4. "No Solar x No TV x English Videos (G4)" receiving English videos only;
5. "No Solar x No TV x Bilingual Videos (G5)" receiving bilingual videos only;
6. "Control (G6)" receiving neither solar facilities nor video.

The initial randomization produced the following numbers of schools in each treatment group: 35 schools in G1, 35 in G2, 36 in G3, 35 in G4, 33 in G5 and 34 in G6. The subsequent reduction in sample size as described in section 3.1 changed the numbers in each group to: 26 schools in G1, 29 in G2, 31 in G3, 29 in G4, 24 in G5 and 25 in G6. This attrition was unbiased since the condition for selection was determined prior to the randomization.

The solar facilities included panels, lights, radios and TVs. OGE supplied two proprietary solar home systems called M120s, plus one 16-inch television and one 19-inch television. The battery systems could supply approximately 0.22 kWh of energy combined. This energy was enough to power, on average, lights for two classrooms and one office, plus the two televisions. When fully charged, the batteries could power all lights for approximately nine hours, and the two televisions for approximately four hours, per day. In the sense that not every room in the school was electrified—schools in the GivePower sample had approximately 10.6 classrooms and 2.5 offices on average—the electrification was partial.

Videos included two sets: one set solving past 10 years of biology and geography exams ("solutions videos"); and another set motivating self-esteem, perseverance, habit formation, future orientation and other cognitive-behavioral character traits ("motivational video"). The same production team recorded two versions of the videos: one version only in English; and another version mainly in Swahili. The productive team was mainly composed of YSSM team members with bachelor's degrees in education who were certified to teach in public schools. The videos made by highly motivated, certified teachers, seem to be of strong quality, and can be viewed online.[22]

The motivational video, interestingly, contained a number of phrases one would expect to find in cognitive-behavioral therapies, whose purpose according to the psychology literature is "to produce cognitive change—modification in the [participant's] thinking and belief system—to bring about enduring emotional and behavioral change" (Beck, 2011). Frequently from the very beginning, the

[21] That is, schools in each region were ranked by their 2014 pass rates, counted off in groups of six schools, and then assigned treatments randomly within this group using Stata's random number generator.

[22] https://www.youtube.com/playlist?list=PLWjakPMOMQnW3w57jnh3AIIFjcL2y17Oz

video employs phrases such as "change the way you think" and "change the way you act . . . especially on academic matters."

The motivational video merits a deeper investigation for a number of reasons. First, as explained earlier, YSSM's activities seem to be in popular demand, with no small number of participants volunteering to come each time. Programs elsewhere evaluated by Blattman et al. (2017) and Heller et al. (2017) have pointed to the potential effectiveness of targeting self-esteem and sense of identity in spurring human capital growth among economically disadvantaged populace. Digital media may be a promising channel to scale these programs.

The lengths of the English videos were: 2 hours for the Geography video; 1 hour and 23 minutes for the Biology video; and 30 minutes for the Motivational video, totaling 3 hours and 53 minutes. The lengths of the bilingual videos were 50 minutes for the Geography video, 1 hour and 10 minutes for the Biology video, and 40 minutes for the Motivational video, totaling 2 hours and 40 minutes. While the questions covered were equalized across videos, bilingual videos seem to have taken less time because teachers could speak more swiftly and repeat themselves less.

3.3 Data

This paper relies on three sets of data: school surveys, solar battery meter scans, and administrative student records.

Two short surveys were conducted, each survey no more than one sheet in length (two-pages, front and back). A baseline characteristics survey collected data on tuition and numbers of teachers, classrooms, offices and years of operation. A follow-up survey collected data on pedagogy and educational time use in October, 2016, right before the November O-level examinations were conducted.[23]

Since the gist of the intervention was providing lights, TV and videos, the follow-up survey focused on four aspects of educational inputs in particular: (1) electrification status; (2) multimedia usage; (3) lights usage; and (4) patterns of after-hours participation.[24] Electrification status was broken down into numbers of classrooms and offices with grid or solar access, and then was aggregated during analysis into a single measure of percentage of classrooms and offices with grid or solar electricity in school. Multimedia usage was first disaggregated down into hours of video viewing per average week during government-mandated hours and extra hours in classrooms and offices, and then was aggregated into a single measure of total hours of video viewing per average week. Lights usage was similarly dissected and then aggregated. Finally, patterns of

[23]The pre-analysis plans that document these surveys span two entries on the AEA RCT Registry: Seo (2015, 2016). The first entry, though its measurement plans could not be used because survey funds could not be obtained in time, contains the essential design, power calculations and qualitative hypotheses about after-hour attendance and demand. The second entry retains pre-committed surveys that were administered as described in this section (though not yet publicly released). The second entry also describes a different set of pilot treatments aimed at a younger cohort in these schools; but additional treatments did not target the graduating cohort examined in this paper and including the additional treatment indicators as controls do not affect any of the results reported in this paper.

[24]The follow-up survey also asked about water use in school and 9th-grade mathematics pedagogy for some other research, but these topics were not related to this study.

after-hours participation were asked in two ways: (A) the gross number of students participating in extracurricular activities per an average week; (B) the gross number of students using electricity after regular hours per an average week. These measures need not overlap, as self-study is not a formal extracurricular activity, yet students engaging in self-study can still be included in (B). In analysis, I divided measures (A) and (B) by the total number of students in school and took the proportions.[25] The survey did not ask about daytime attendance.

The analysis also relies on battery meter data scanned by surveyors using OGE's proprietary NFC-enabled smartphone application. The battery meter data kept records of daily watt-hours of battery usage, battery voltage and average current out from battery for approximately two months. The data were automatically sent to OGE's server upon each scan, and were later made available to the research team for analysis.

Lastly, the analysis examines the 11th-grade O-level outcomes of all students in the final sample schools, matched to their 9th-grade national promotional examination outcomes from 2014. Thus the final set of administrative data include (9th-grade) pretest scores and gender, allowing us to conduct additional checks of balance of randomization, to control for baseline test scores, and to conduct subgroup analyses segmented by these variables. The government reports subject level outcomes in grade brackets (corresponding numerical weights): A (5), B (4), C (3), D (2) and F (0).[26] The government takes each student's seven highest subject scores to compute the student's grade point average (GPA). The GPA is used to determine the student's level of certification (Divisions I, II, III, IV and Failure). The main outcome variables I examine are indicators for passing (getting at least Division IV), normalized GPA of seven best subjects, normalized GPA of solutions-video targeted subjects (included in the best seven) and normalized GPA of no-solutions-video targeted subjects (included in the best seven).

3.4 Sample Characteristics and Representation

Table 1 reports baseline sample mean and standard deviation of the control group (the first row). With an average of 64.5 students taking the O-levels (column (3)), GivePower schools are between the national median (57) and the mean (75) in terms of size. The schools have an average of 24.6 teachers, 10.6 classrooms and 2.4 offices. The schools have operated on average for 9.5 years. The annual tuition per student was \$69.9 in 2015, which was about 7.3% of Tanzania's GDP per capita in 2015.[27]

In table 4, in the first row, one can see that the control group's O-level pass rate was 58.2% in 2016. A school-wide pass rate of 58.2% corresponds to the bottom 25th percentile of all schools in the nation. The national pass rate in 2016 was 70%. Therefore, as also alluded to earlier in

[25]Unfortunately, the surveys did not ask about these proportions broken down into different grades. Yet, to the extent that it is the graduating cohort facing the high stakes examination at the end of the year, I presume (B) to highly correlate with electricity use by the graduating cohort.

[26]For further information, see MoEVT (2015).

[27]It is noteworthy that while Tanzanian public schools had the freedom to choose its own level of school feels prior to 2016, in 2016 the government restricted all school fees in public schools to approximately \$10 per student. Therefore, the program could not have affected school fees in 2016.

fig. 2, GivePower schools are relatively disadvantaged schools compared to schools in the rest of the nation. These schools were also the last 20% of schools in the project districts to have not received access to electricity.

In table 2, column (3), one can see that girls (55%) outnumber boys (45%) in these schools.[28]

3.5 Estimation Framework

This paper employs three sets of regression models. The first model estimates differences in outcomes in each of the five treatment groups against outcomes in the control group. The second model estimates the average difference between outcomes in solar-facilities-receiving schools against outcomes in non-solar-facilities-receiving schools, where the average is taken across video-provision status. The third model estimates the policy impacts of increasing within-school electrification rate (proportion of school rooms with grid or solar access), English-video viewing hours per week, and bilingual-video viewing hours per week, using an instrumental variables framework. These models are elaborated on below.

The first model employs a standard difference-in-means equation:

$$y_{ij} = \alpha + \sum_{g \in \{1,\ldots,5\}} \beta_g \times T_{i,g} + \gamma \times P_i + \delta_b + \epsilon_{ij}, \tag{1}$$

where y_{ij} is the explanatory variable of interest, with i indexing students and j schools. α is a constant term. β_g is the treatment effect of group g; $T_{i,g}$, the indicator for i's belonging in group g. γ is the coefficient on i's pretest performance, P_i, which the educational literature includes whenever available to increase the precision of outcome estimates when y is a test score outcome. For normalized test score variables, I use normalized pretest score control; for pass indicator variables, I use pretest score quartile indicators to discipline the linear probability model prediction, following Angrist and Lavy (2009). I do not employ P_i in most specifications, but employ it for achievement outcome variables. δ_b represents randomization-block fixed effects. y may only exist at the school level (y_j), in which case the analogous school-level regression is examined. I report standard errors clustered at the school level. I also report both unadjusted significance levels and Holmes-Bonferroni corrected p-values adjusted for five hypotheses testing in all specifications. Anticipating, I do not reject any null hypotheses on outcomes; therefore, other procedures that control the false discovery rate, such as that of Benjamini et al. (2006), give results similar to Holmes-Bonferroni.[29]

Note that β_2 and β_3 can be viewed as sums of component treatment effects, since the treatment of G2 bundles two treatments of G1 and G4, and the treatment of G3 bundles two treatments of G1 and G5. To elaborate, the treatment of "Solar x TV x English Videos (G2)" is, in fact, a bundle of two treatments: "Solar x TV x No Videos (G1)" and "No Solar x No TV x English Videos (G4)." Therefore, the treatment effect of G2 (β_2) can be thought of as the sum of three

[28] The national gender distribution is more equal: about 50:50. The unequal gender distribution in these rural schools may have arisen because parents in these areas prioritize boys' education over girls', tending to send boys to private schools or city schools while leaving girls behind in these remote public schools.

[29] These other corrections can be made available upon request.

component treatment effects: (A) the treatment effect of providing “Solar x TV” alone without English videos (β_1); (B) the treatment effect of providing “English videos” alone without solar facilities (β_4); and (C) the treatment effect of interacting the two exclusive treatments (θ_1). The analogous consideration holds for (β_3). In summary,

$$\beta_2 = \beta_1 + \beta_4 + \theta_1, \tag{2}$$

$$\beta_3 = \beta_1 + \beta_5 + \theta_2, \tag{3}$$

where θ_1 represents the interaction effect of combining G1 and G4, and θ_2 represents the interaction effect of combining G1 and G5.

The above discussion motivates my second regression model. Let $S = T_1 + T_2 + T_3$ represent the indicator for solar-facilities provision; $E = T_2 + T_4$, the indicator for English-videos provision; $B = T_3 + T_5$, the indicator for bilingual-videos provision. Consider the following regression equation:

$$y_{ij} = \alpha + \lambda_1 \times S + \lambda_2 \times E + \lambda_3 \times B + \gamma \times P_i + \delta_b + \epsilon_{ij}. \tag{4}$$

Of particular interest is λ_1, which estimates the treatment effect of providing solar facilities, averaged across the three video groups of “No Solar,” “English Videos” and “Bilingual Videos.” In expectation: $\lambda_1 = \frac{1}{3}\big(\beta_1 + (\beta_2 - \beta_4) + (\beta_3 - \beta_5)\big) = \beta_1 + \frac{\theta_1 + \theta_2}{3}$.[30] Going forward, I refer to this parameter as the “combined” average treatment effect of solar-facilities-enabled programs.

In table 6, I additionally employ an instrumental variables specification to further investigate channels of impact and form policy predictions. Specifically, I examine the effects of three explanatory variables: (A) proportion of school classrooms and offices with grid or solar access (“within-school electrification rate”); (B) hours of video instruction conducted in English; (C) hours of video instruction conducted in mixed-Swahili-and-English. I instrument for these variables using the five individual program treatment indicators.

Let these endogenous variables be represented by x_m, where m indexes the variables. I take the following first-stage equations,

$$x_{m,j} = \alpha + \sum_{g \in \{1,\ldots,5\}} \zeta_g \times T_{g,i} + \gamma_m \times P_i + \delta_{m,b} + \epsilon_{ij}, \quad m \in \{1, \ldots, M\}, \tag{5}$$

[30] To see this, consider the mechanics of the ordinary-least-squares estimator in a model with treatment effect $\eta(V)$ that depends on a discrete covariate V. In the current context, one can consider the mapping: $\{V = 0\} = \{\text{received neither videos}\}$, $\{V = 1\} = \{\text{received English videos}\}$, $\{V = 2\} = \{\text{received bilingual videos}\}$, $\eta_1 = \beta_1$, $\eta_2 = \beta_1 + \theta_1$, $\eta_3 = \beta_1 + \theta_2$. Then, $\eta^{OLS} = \frac{\sum_v \eta_v P[S=1|V=v](1-P[S=1|V=v])P[V=v]}{\sum_v P[S=1|V=v](1-P[S=1|V=v])P[V=v]} \approx \sum_v \eta_v P[V = v] = \beta_1 + \frac{\theta_1+\theta_2}{3}$, given $P[V = v] = 1/3, \forall v$. That is, η^{OLS} estimates the treatment effect of S averaged across groups V (i.e. the matching average treatment effect). The last approximation is exact if S is perfectly balanced given V; in the current evaluation's design, each of “No Video,” “English Videos” and “Bilingual Videos” groups has exactly half of schools receiving the solar facilities and half not receiving, in expectation.

and form predicted variables $\hat{x}_m$. Then, I estimate the following equation to get IV coefficients:

$$y_{ij} = \alpha + \sum_{m \in \{1,\dots,M\}} \beta_m \times \hat{x}_m + \gamma \times P_i + \delta_b + \epsilon_{ij}. \tag{6}$$

Standard errors are given by estimating these equations in one step.

3.5.1 Power

Appendix table A1 reports power calculations given the following two sets of assumptions: (A) power given assumptions made before the start of the experiment; (B) power that was realized with the final sample size and sample moments. Specifically, columns (1) and (3) of appendix table A1 were reported in the pre-analysis plan (Seo, 2015). The initially expected minimum-detectable effect size (MDE) for pass rates ranged from 0.04 in the combined specification (that is, the MDE for λ_1 in eq. (4), to 0.08 in the individual-comparison specification (that is, the MDE for β_g's in eq. (1)) with Holmes-Bonferroni alpha of 0.05 given five null hypotheses. The corresponding range for standardized test scores was between 0.16σ and 0.34σ; admittedly, I was optimistic about what the treatments could do to test scores. The power in the realized data increased dramatically because the government-provided pretest scores of sample students reduced the residual standard deviation by a substantial amount ($\approx 1/3$), as shown in columns (4)-(6). MDE's on pass rates range from 0.05 to 0.1, and MDE's on normalized test scores range from 0.11σ to 0.23σ.

When reporting results, I commit to reporting Holmes-Bonferroni-corrected p-values for most reported coefficients, and to interpreting results in light of these p-values. I also indicate unadjusted standard errors and levels of significance for perspective. When estimating the combined treatment effect averaged across all video groups (λ_1 in eq. (4)), I do adjust for multiple-hypotheses testing (MHT) since that is the singular hypothesis in which I am interested.

4 Results

4.1 Main Results

In this section, I examine: (1) balance of randomization on pre-intervention characteristics and attrition patterns; (2) impacts on patterns of school-level educational investments; (3) impacts on test score and pass rate outcomes.

4.1.1 Balance of Randomization and Attrition

Table 1 tests null hypotheses of whether various school characteristics were balanced across treatment groups using eq. (1) at the school level.

Column (1) checks for balance on the number of students who passed the 9th-grade promotional examinations in 2014 in these schools. I cannot reject the hypothesis that all treatment groups were balanced on this number of students. All of the MHT-unadjusted p values, while not reported, exceed 0.2, and after MHT correction exceed 1. While statistically insignificant, the class sizes in G1 schools and G5 schools tend to be 10% smaller than the control group class size.

Columns (2) and (3) examine similar hypotheses for the number of students registering for the O-levels (in January, 2016), and the number of students taking the O-levels (in November, 2016), respectively. My target is the ratio of the number of students passing to the number of students taking the O-levels; therefore, I would be concerned about the confounding effects of selective participation if participation were shown to be statistically different across treatment groups. I see little evidence for selective participation.[31]

Columns (4)-(9) compare school-level characteristics in August, 2015, as discussed in advance in section 3.4. The small and insignificant coefficients show that the randomization was balanced on these characteristics.

Columns (1)-(2) in table 2 examine student-level indicators. Columns (1) and (2) in particular examine attrition from November 2014 to January 2016, and from January 2016 to November 2016, respectively. Proportional differences in attrition are small and insignificant, not exceeding 1 pp in most cases. Column (4) of table 2 examines normalized 9th-grade pretest GPAs reported by the government. Although coefficients are not statistically significant, G1 and G3 schools are shown to have had -0.09 and -0.07 lower test scores, respectively, which are fairly large differences. For this reason, I control for these pretest scores P_i in analyses of outcomes, as specified in section 3.5.

Columns (5) and (6) examine solutions-video-targeted subjects' GPAs and no-solutions-video-targeted subjects' GPAs, respectively. No statistically significant difference is observed here, and the trend looks similar to that of column (4). Hence, in the interest of space and discipline, the only control I rely upon is the aggregate pretest GPA variable examined in column (4).[32]

4.1.2 Impacts on School Inputs and Educational Behaviors

Column (1) of table 3 compares differences across percentages of school rooms with grid or solar access, a variable I introduced in section 3.3. On average, schools selected to receive solar facilities report an additional 19% to 25% of school rooms electrified compared to the control group. As reported in column (1) of appendix table A2, each school received installations in 2.5 rooms on average, while as seen in table 1 column (7), schools on average had 10.6 classrooms; hence, these estimated effects on the proportions are reasonable. This variable is technically still endogenous, as schools could decide for themselves to invest in electric facilities or the government could provide during the 13-month observation window. Illustrating this point is the surprisingly large control group mean of 10.1%. Anticipating, the control group shows positive means for electricity use variables in subsequent columns also.

Column (2) compares hours of weekly multimedia usage across schools. G2 schools report to have watched 4.2 hours of more videos than control schools, which report to have watched only

[31]Ideally, I would examine the number of students enrolled in August, 2015, immediately before the treatment interventions began. I do not have reliable data on this, as schools do not closely track attendance records.

[32]Including other subjects' GPA controls additionally generally improve the precision of estimates, but do not affect qualitative analyses. These results are available upon request.

24 minutes per week. Interestingly, G1 schools which did not receive the GivePower-YSSM videos also report to have watched 1.7 hours more per week than the control group. Although statistically insignificant when the Holmes-Bonferroni correction is applied, this coefficient is almost twice as large as that reported by G3 schools, which did receive the GivePower-YSSM videos. This suggests a policy of providing television may encourage teachers to find ways to use the television on their own even if no content is provided, although it is questionable how educationally effective such an effort may be. Finally, G3 group reports to have used videos for 51 minutes longer than the control group per week, but the coefficient is insignificant and small compared to those reported by G1 and G2 schools.

Column (3) compares reported hours of lights usage per week across treatment groups. All coefficients are statically very imprecise. It is remarkable though that G3 schools report to have used almost 4 hours more of lights on average, compared to G2 schools which report to have used only 53 more minutes of lights.

Column (4) compares percentages of students participating in extracurricular activities. As discussed in advance in section 2, the average participation rate in the control group is high at 44%. The differences across treatment groups are small and insignificant. The probably of rejecting joint significance is high at 0.753.

Column (5) compares the percentage of students staying late to use electricity. The probably of rejecting joint-significance is 0.0003, and the corresponding F-statistic (not reported) is 6.81. Coefficients individually are not statistically precise; the only coefficient that is significant before MHT-correction is the coefficient for G3 schools, and this significance disappears after MHT-correction. The magnitudes are large, however. In G3 schools, 15.6 pp of more students stayed in school to use electricity, or approximately four times as many students in control schools. In G1 and G2 schools, the magnitudes are also large, with 6.9% more students being reported to have stayed in school after hours to use electricity every week.

Figure 4 plots the coefficients reported in columns (2)-(4) for a graphical comparison.

Figure 5 plots battery energy and approximate time usage estimates computed from the battery meter data as introduced in section 3.3. Two sets of facts are worthy of note. First, the average usage rate was large, the magnitude of energy usage was low, and the estimated time use was large. All solar boxes metered positive usage almost every day (except a small number of boxes that were stolen, as indicated in column (6) of appendix table A2).[33] All schools combined used 5 kWh per day (reported in column (4) of appendix table A2), which is a small number compared to the large usage rate, but not so surprisingly small given the small capacity of the solar boxes. All schools combined are estimated to have used a total of 1,505 hours per day (reported in column (5) of appendix table A2 (see appendix C for an analysis of the estimation process, which suggests that the measures may actually be underestimates). Field interviews suggested that many schools used the lights for *security lighting* at night, explaining the large number of hours. Second, while the

[33] Solar box thefts were fortunately not a large problem in this setting. This might have been because OGE's solar boxes can only operate if the correct voucher code is used, rendering them useless for thieves.

differences in usage among G1, G2 and G3 schools are not statistically significant, their orderings corroborate the survey evidence discussed above: G2 schools used the most energy, consistent with the report that G2 schools used more TV, while G3 schools used the most time, consistent with the report that G3 schools used more lights.

4.1.3 Achievement Impacts

Figure 6 reports the histograms of the raw GPA data realized in the pretest (left) and in the O-levels (right) by different treatment groups (rows). In each histogram, the control group GPAs are plotted in white in the background, while the indicated treatment group GPAs are overlaid in green in the foreground for comparison. The red vertical lines indicate the pass cutoff. In the third row, one can see some missing mass to the left of the pass threshold line that did not exist in the pretest score distribution, representing students who passed more in the G3 group than in the control group. A smaller but similar missing mass is also observed in the fifth row for the G5 group, but one can see that the G5 group was also performing relatively better than the treatment group before the intervention, due presumably to sampling variation. Hence I control for pretest GPAs in evaluating performance impacts in subsequent tables.

Columns (1), (3) and (5) in table 4 examine normalized GPAs. While coefficients are statistically insignificant, in column (1), which examines the aggregate GPA, G3 students shows a meaningful increase of 0.081σ, while other groups show coefficients less than half in size. In column (3), which examines solutions-video-targeted subjects' GPA, G1 students who did not receive the videos actually shows a larger increase than G2's or G3's, even though they did not receive GivePower-YSSM videos. In column (5) on the other hand, G3 students show a fairly large test score gain of 0.104σ, while other groups again show gains less than half as large.

Column (2) in table 4 reports achievement impacts on normalized test scores. While statistically insignificant, a pass rate impact of 0.059 pp is seen in G3 schools, a 10% increase compared to the control group mean pass rate of 58.2 percent, while the coefficients for all other treatment groups are less than a third in magnitude. It is noteworthy that the mean pass rate increase is larger in the no-video-subject performance than in the video-subject performance, although neither the impact nor the differences are statistically significant. Note that column (2)—indicators for getting at least two subject D's or above—need not be averages of columns (4) and (6), the means of indicators of getting a D or above in subjects belonging to respective sets.

Figure 7 provides a graphical summary of columns (1), (2), (4) and (6).

4.1.4 Impact of Solar-facilities-enabled Programs Averaged Across All Video Groups

Table 5 reports this paper's central results from estimating eq. (4); the main coefficients are from the first row of panel A, which report the impact of solar-facilities-enabled programs averaged across no-video, English-video and bilingual-video subgroups (λ_1), as explained in section 3.5.

Column (1) shows the impact on normalized O-level GPA, and the point estimate shows a statistically insignificant mean effect of 0.052σ. The corresponding confidence interval allows me to rule out an effect size of 0.129σ. Column (2) shows the impact on O-level pass rate, and the point estimate shows a statistically insignificant mean effect of 0.028. The corresponding confidence interval allows me to rule out an effect size of 0.067 pp.

Columns (3) and (4) show analogous impacts on averages across video subjects' test scores and pass rates. The magnitudes seen here are similar to those seen across columns (1) and (2). The MHT-unadjusted standard error is smaller in column (4), and the coefficient is borderline significant. I cannot reject that the treated students showed no different improvements in video subjects' performance than in no-video subjects' performance. Indeed, previously in table 4, "Solar x TV x No Video (G1)" schools, in fact, showed higher performance estimates in video subjects than "Solar x TV x English Videos (G2)" or "Solar x TV x Bilingual Videos (G3)" schools, although the differences were not statistically significant. Therefore, the video-versus-no-video distinction does not seem remarkable, and I conclude that the average impact on video subjects' performance was just as modest as the average impact on overall O-level performance.

Columns (5) and (6) show analogous impacts on averages across no-video subjects. The magnitudes, here, are also modest, and the coefficients are insignificant.

Figure 8 additionally reports that the combined average impacts of solar-facilities-enabled programs were 2.3 hours per week of video viewing (fig. 8a), and 14 gross percentage points of students staying late in school to use electricity (fig. 8b). Figure 8c and fig. 8d graphically summarize the above discussions of columns (1) and (2), respectively.

The second and third rows of coefficients reported in table 5 present the average impact of providing videos across solar-receiving and non-solar-receiving groups. The magnitudes are small and unremarkable.

4.1.5 Policy Predictions Using Instrumental Variables

Table 6 reports results from estimating eq. (6).

The first-row variable in panel A are rates. Therefore, one can multiply each coefficient in the first row by 10 to predict the school-level policy impact of additionally electrifying 10% of classrooms and offices, controlling for video-viewing hours, on each dependent variable. In column (1), for example, one can see that 10% of additional school rooms electrified would lead to 0.01σ gain in O-level GPA and 0.4 pp gain in pass rate; that is, such a policy would have virtually no effect on O-level performance. The magnitudes of the coefficients in columns (2), (5) and (6) are also very small in economic terms. The magnitudes of the coefficients in columns (3) and (4), the video subjects, are larger, but noisily estimated.

The magnitudes of the coefficients on the second-row variable, hours of English-videos viewing per average week, are small across the board. This is remarkable, given that schools treated with solar facilities and English videos reported substantial amounts of video viewing. This suggests that the conventional wisdom in the field that English-video viewing may be an effective means of instruction may be at odds with reality.

The magnitudes of the coefficients on the third-row variable, hours of bilingual-videos viewing per average week, are much larger, but very noisily estimated. The AP First-stage-F statistic is also weak for bilingual-videos viewing. Since schools did not engage in much bilingual-video viewing on average, these estimates seem unremarkable.

4.1.6 Distributional and Heterogeneous Impacts

Table A3 and fig. B1 report effects broken down by gender and pretest performance.

Specifically, table A3 breaks down coefficients estimated from eq. (4) into impacts on boys, impacts on girls, impacts on top-half students (students whose pretest GPA was above median) and impacts on bottom-half students (students whose pretest GPA was at or below median). No coefficient meets the 5% statistical significance benchmark, although there is suggestive evidence that the programs generally had larger effects on girls than on boys, and on bottom-half students than on top-half students.

Estimates shown in fig. B1a and fig. B1c break down the average impacts of solar-facilities-enabled programs (λ_1) as continuous functions of pretest performance percentile, while estimates in fig. B1b and fig. B1d further break down these functions by gender. Outcomes are O-level pass rates in (a) and (c), and normalized grade-point averages in (b) and (d). The values are computed by first estimating the outcome variable as a continuous function of pretest performance percentile using local-linear estimates with an Epanechnikov kernel bandwidth of 15, by treatment group (and gender). Then, the difference between treatment and control is taken (by gender) conditional on pretest performance percentile. It can be seen that the effects estimated for lower- and middle-class pretest performers are suggestively larger than the effects estimated for top-class pretest performers. It can also be seen that the effects for girls hover consistently above those of boys across the pretest performance distribution. None of the effects or differences shown are statistically significant, however.

4.2 Additional Comments on the Results

It is noteworthy that G2 schools, despite watching videos significantly more, saw only small and insignificant performance increases. It is also noteworthy that G1 schools reported watching more videos, despite not being given videos, suggesting that it is a conventional wisdom in the field that video viewing may be a helpful tool for education. Yet, overall, achievement impacts were weak and insignificant.

Estimated impacts on educational behavior combined with impacts on test scores suggest that students' watching more videos did little to spur concrete academic achievement. Students' staying later in school to use electricity after hours may have been more helpful, but the effect was not strong enough to be statistically detectable. These results suggest that giving students more opportunities to actively engage and struggle with the materials by themselves, and making these materials more approachable and conducive to self-study, may be more effective than encouraging passive viewings of videos alone.

Girls' benefits were suggestively larger than boys' even after controlling for girls' lower pretest achievement, consistent with past findings that females on average benefit more as electricity access grows (Dinkelman (2011)) and as educational opportunities expand (Angrist and Lavy (2009); Becker et al. (2010); Carrell and Sacerdote (2017)). There was also weak but suggestive evidence that the program benefited middle-to-low performers more than top performers. By construction, pass rates are mechanically harder to improve for top performers because their probability of passing is already very high, thus leaving little margin for improvement. Yet, similar pattern also seemed to hold for the GPA outcome, which is a continuous measure. It could be that top students had already been performing at the point of the learning curve where returns to additional self-study is low absent further innovations in learning methodology, while middle to bottom students had been lacking much opportunity for self study outside of regular school hours to begin with. These speculations, however, should be noted with caution since none of the distributional or heterogeneous effects were statistically significant.

The program was inexpensive, costing \$6.41 per student. While certainly by no means a fair comparison, for perspective I list here the per-pupil costs of some past school-based interventions cited in the introduction. Not taking into account inflation, Banerjee et al.'s (2007) 2001-2002 remedial education program in low income Indian primary schools cost \$2.25 per year per student, while their 2002-2003 4th-grade computer game program cost \$15.18 per student. Glewwe et al.'s (2009) 1996 textbooks provision program in Kenya cost \$2-3 per textbook. In the developed world, the Israeli high school graduation prize program evaluated by Angrist and Lavy (2009) awarded \$1,500 to every student who passed high school. The behavioral-therapy programs evaluated by Heller et al. (2017) cost \$1100-1850 per year per program participant in low-income Chicago public schools, and \$60 per year per participant in juvenile detention centers. The program of Blattman et al. (2017) cost \$530 including cash grant per participant in the youth program they evaluated in Liberia.

Taken together, the program showed clear limitations in the first year. Achievement gains were modest. Giving some lights and videos were not enough to encourage significant achievement gains.

4.3 Threats to Validity

I discuss a few conflicting lines of reasoning that may also explain the data.

First, it could have been the case that the provided videos were actually detrimental to learning, canceling out positive effects of electrification that might have been better realized had electrification been provided alone.

A counter argument to this line of reasoning is that, ex ante, there was no way of anticipating that the solutions videos would be of little avail. Indeed, the G1 group data suggest that schools not receiving the videos also thought seeking out videos on their own would bring educational benefits—indeed, this seems to be the conventional wisdom in the field.[34] Without the variegated

[34] I plan to ask in the next survey whether the schools exchanged videos. My prior is that it is unlikely that that the schools were actively trading videos between themselves, given that these schools are remote schools located far

treatment groups, the program could not have tested this hypothesis. Finally, the overall performance in G2 and G3 schools was not any lower than in G1 schools, but suggestively higher. This suggests that the videos at least did not hurt the performance of the students.

Another threat to validity I consider is statistical weakness. The unadjusted standard errors and the MHT-adjusted corrections are what they are as reported. Even if the study had fewer treatments, the conclusion that partial electrification alone would have brought little benefits without additional stimulation would have held up one and the same. Under the most minimal set of assumptions, the project sought to test with variegated treatments whether providing digital content jointly with electricity infrastructure could be more effective than providing either by itself. The results are as shown.

Although results are imprecise, the program evaluated in this study pioneered a large-scale school-level RCT intervention in Tanzania, a large sub-Saharan-African nation. Institutional details and observations from the field reported in this study, such as the potential link between language and motivation, and the contrast between learning by passive video viewing versus by active, intrinsically-motivated learning, may meaningfully inform future research.

5 Conclusion

Enabled by the GivePower school program, I tested the educational benefits of partial school electrification and language-varied educational videos in northern Tanzania with 164 relatively disadvantaged schools. These schools were the last 20 percent of schools still without electricity in the project districts in September, 2015. The average pass rate in these schools, at 58%, corresponded to the bottom 25th percentile of the national pass rate distribution.

I randomized the schools into the following five treatments. Treatment 1 provided solar lights, panels and TVs (Group 1). Treatment 2 provided solar facilities and English videos (G2). Treatment 3 provided solar facilities and bilingual videos (G3). Treatment 4 provided English videos without solar facilities (G4). Treatment 5 provided bilingual videos without solar facilities (G5). Videos included two sets: one set, solving past 10 years of biology and geography exams; another set, motivating self-esteem, perseverance, habit formation, future orientation and other cognitive-behavioral character traits.

After one year, solar facilities increased the percentage of classrooms and offices with electricity access by 19% to 25%. Solar-receiving schools reported to have watched between 0.8 and 4.2 hours more of videos per week, and to have seen between 7 and 16% of students in gross percentage per week staying behind in school to use electricity after regular hours. G2 schools (receiving solar and English videos) reported to have watched especially more videos: approximately 4.2 hours more per average week.

These behavior changes did not lead to significant achievement gains. While statistically imprecise, an economically meaningful average pass rate increase of 5.9 pp (or 10%) was observed in

away from each other.

G3, who received bilingual educational and motivational videos in addition to solar facilities. But overall, gains were weak and significant. I estimate the average impact of solar-facilities-enabled programs, where the average is taken across video-receiving and non-video-receiving schools, to be 0.05σ on O-level test scores and 2.8 percentage points (pp) on pass rates. I rule out impacts larger than 0.13σ and 6.7 pp in the first year. It may be the case that longer-term (two-year) results are different from the first year's. I am awaiting second (final) year results, whose examinations will be taken at the end of 2017 and announced in 2018.

In the first year, the program showed clear limitations. Giving lights and videos were not enough to elicit significant achievement gains. Schools did report that many students showed willingness to remain behind in school to use electricity (14 gross percentage points of students were reported to have stayed behind in school after regular hours to use electricity each week). These results suggest that motivating students with more opportunities to actively engage and struggle with the materials themselves, and making these materials more approachable and conducive to self-study, may constitute more effective policy than encouraging passive viewings of videos alone, in the Sub-Saharan junior secondary education setting

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Tables

Table 1: Balance of Baseline School Characteristics

	(1) # Passing 9th-grade (Nov. '14)	(2) # Entering 11th-grade (Jan. '16)	(3) # Taking O-levels (Nov. '16)	(4) Number of Teachers Employed	(5) Student-to-11th-grader Ratio	(6) Number of Years in Operation	(7) Number of Classrooms in School	(8) Number of Offices in School	(9) Annual Tuition per Student ($)
Mean of Control Group (G6)	74.08	65.84	64.52	24.60	4.585	9.535	10.61	2.448	69.93
Sd of Control Group (G6)	33.93	30.05	29.69	11.91	3.465	2.417	2.493	2.000	31.52
Panel A: *Treatment Mean - Control Mean*									
Solar x TV x No Video (G1)	-8.653	-7.012	-6.936	-4.182*	-0.533	0.512	-0.741	0.497	-8.142
	(10.11)	(9.002)	(8.827)	(2.469)	(0.738)	(0.761)	(0.965)	(0.597)	(6.315)
	[1]	[1]	[1]	[0.4645]	[1]	[1]	[1]	[1]	[1]
Solar x TV x English Videos (G2)	-0.734	0.0853	-0.246	0.440	-0.576	1.007	-0.0907	0.0232	-2.668
	(9.764)	(8.696)	(8.527)	(2.337)	(0.698)	(0.720)	(0.914)	(0.565)	(5.977)
	[1]	[1]	[1]	[1]	[1]	[0.656]	[1]	[1]	[1]
Solar x TV x Bilingual Videos (G3)	5.284	5.676	5.285	2.271	-0.703	0.381	0.343	0.503	-4.496
	(9.718)	(8.655)	(8.487)	(2.308)	(0.690)	(0.711)	(0.902)	(0.558)	(5.903)
	[1]	[1]	[1]	[1]	[1]	[1]	[1]	[1]	[1]
No Sol. x No TV x English Vid. (G4)	3.975	3.171	2.625	-1.292	-0.185	1.179	0.0650	0.388	-4.360
	(9.944)	(8.856)	(8.684)	(2.322)	(0.694)	(0.715)	(0.908)	(0.561)	(5.938)
	[1]	[1]	[1]	[1]	[1]	[0.51]	[1]	[1]	[1]
No Sol. x No TV x Bilingual Vid. (G5)	-6.646	-6.398	-6.322	-1.080	-0.317	0.859	-0.423	0.262	-3.252
	(10.14)	(9.032)	(8.857)	(2.497)	(0.746)	(0.769)	(0.976)	(0.604)	(6.387)
	[1]	[1]	[1]	[1]	[1]	[0.798]	[1]	[1]	[1]
Observations	164	164	164	164	164	164	164	164	164
R-squared	0.303	0.316	0.316	0.449	0.263	0.326	0.321	0.399	0.700
Sum of Student Weights	.	.	.	10.171	10.171	10.171	10.171	10.171	10.171
Pr > Joint F, All Treat.	0.658	0.635	0.660	0.143	0.917	0.573	0.887	0.904	0.876

Note : Difference-in-means coefficients compare school characteristics across five treatment groups and one control group. Each observation is a school. Columns (1)-(3) are administrative data, examining the number of students passing the 9th-grade promotional examinations (Nov. '14); the number registering (Jan. '16) for the junior-secondary exit exams (11th-grade O-levels); and the number taking the O-levels (Nov. '16), respectively. Columns (3)-(9) examine survey records retrospectively asked about August, 2015, inverse-variance-weighted by the number of students in column (3). Schools were block-randomized over 2014-pass-rate tiers of six schools within each region; block fixed effects included. Second row in parentheses are MHT-unadjusted robust standard errors. Levels of significance: *** p<0.01, ** p<0.05, * p<0.10. Third row in brackets are Holmes-Bonferroni corrected p-values (e.g. the smallest unadjusted p-value in each column is multiplied by 5).

Table 2: Balance of Student Characteristics

Column Type:	------------Attrition------------		-Demographics-	-------Normalized Pretest Grade-Point Averages-------		
	(1)	(2)	(3)	(4)	(5)	(6)
	Attritor Indicator (Nov. '14 -> Jan. '16)	Attritor Indicator (Jan. '16 -> Nov. '16)	Female Indicator	Aggregate (Best 7 Subjects)	Solutions-video-targeted Subjects	No-solutions-video-targeted Subjects
Mean of Control Group (G6)	0.129	0.0200	0.542	0.0166	0.0300	0.00947
Sd of Control Group (G6)	0.335	0.140	0.498	1.035	1.025	1.033
Panel A: *Treatment Mean - Control Mean*						
Solar x TV x No Video (G1)	-0.00484	0.00317	0.0243	-0.0957	-0.0811	-0.101
	(0.0127)	(0.00729)	(0.0231)	(0.0752)	(0.0891)	(0.0727)
	[1]	[1]	[1]	[1]	[1]	[0.845]
Solar x TV x English Videos (G2)	-0.00910	0.00681	0.0151	-0.00402	-0.0306	0.00874
	(0.0133)	(0.00695)	(0.0229)	(0.0922)	(0.107)	(0.0864)
	[1]	[1]	[1]	[1]	[1]	[1]
Solar x TV x Bilingual Videos (G3)	-0.00866	0.00463	-0.00914	-0.0725	-0.0802	-0.0660
	(0.0139)	(0.00659)	(0.0222)	(0.0705)	(0.0855)	(0.0668)
	[1]	[1]	[1]	[1]	[1]	[0.972]
No Sol. x No TV x English Vid. (G4)	0.0149	0.00674	0.0136	-0.0456	-0.0565	-0.0412
	(0.0135)	(0.00670)	(0.0196)	(0.0845)	(0.101)	(0.0786)
	[1]	[1]	[1]	[1]	[1]	[1]
No Sol. x No TV x Bilingual Vid. (G5)	0.00949	0.000536	0.00770	0.0721	0.00824	0.0942
	(0.0130)	(0.00587)	(0.0218)	(0.0806)	(0.0936)	(0.0758)
	[1]	[1]	[1]	[1]	[1]	[0.864]
Observations	11,697	10,405	10,171	10,171	10,141	10,171
R-squared	0.006	0.006	0.008	0.087	0.083	0.085
Clusters	164	164	164	164	164	164
Pr > Joint F, All Treat.	0.408	0.850	0.752	0.243	0.789	0.141

Note : Difference-in-means coefficients. Observations in column (1): students passing the 9th-grade promotional examinations in Nov. '14; in (2): students registering for the junior-secondary exit exams (11th-grade O-levels) in Jan. '16. In columns (3)-(7), observations are O-level takers in Nov. '16. Indicators in (1)-(2) turn on if students were absent from the O-levels. Column (3) examines normalized 9th-grade grade-point average (GPA) across a student's seven best subject scores, used by the government to determine promotion. In (4), each average is across solutions-video-targeted subjects (geography and biology) included in the best seven; in (5), across non-solutions-video-targeted subjects in the best seven. Schools were block-randomized over 2014-pass-rate tiers of six schools within each region; block fixed effects included. Second row in parentheses: MHT-unadjusted school-clustered standard errors. Levels of significance: *** p<0.01, ** p<0.05, * p<0.10. Third row in brackets: Holmes-Bonferroni corrected p-values (e.g. smallest unadjusted p-value in each column multiplied by 5).

Table 3: Impact on School Inputs and Educational Behavior

	(1) % of Classrooms and Offices with Grid or Solar Access	(2) Hours of Video Viewing per Average Week	(3) Hours of Lighting Use per Average Week	(4) % Attending Extracurricular Activities	(5) % of Students Staying Late to Use Electricity
Mean of Control Group (G6)	0.101	0.405	4.100	0.437	0.0531
Sd of Control Group (G6)	0.153	1.025	12.16	0.303	0.127
Panel A: *Treatment Mean - Control Mean*					
Solar x TV x No Video (G1)	0.239***	1.675**	1.987	-0.0119	0.0688
	(0.0409)	(0.787)	(3.899)	(0.112)	(0.0492)
	[0]	[0.1412]	[1]	[1]	[0.496]
Solar x TV x English Videos (G2)	0.189***	4.204***	0.875	0.0413	0.0690
	(0.0446)	(1.314)	(2.667)	(0.0900)	(0.0456)
	[0.00013]	[0.00875]	[1]	[1]	[0.496]
Solar x TV x Bilingual Videos (G3)	0.248***	0.848	3.961	-0.0339	0.156**
	(0.0473)	(0.737)	(4.056)	(0.0873)	(0.0748)
	[0]	[0.759]	[1]	[1]	[0.198]
No Sol. x No TV x English Vid. (G4)	0.0163	-0.0311	-2.509	0.0327	-0.0581
	(0.0452)	(0.660)	(2.990)	(0.0966)	(0.0396)
	[1]	[1]	[1]	[1]	[0.496]
No Sol. x No TV x Bilingual Vid. (G5)	-0.00274	0.246	1.924	-0.0740	-0.0617
	(0.0483)	(0.755)	(3.270)	(0.0887)	(0.0399)
	[1]	[1]	[1]	[1]	[0.496]
Observations	164	164	164	164	164
R-squared	0.564	0.369	0.315	0.320	0.339
Sum of Student Weights	10,171	10,171	10,171	10,171	10,171
Pr > Joint F, All Treat.	0	0.0141	0.429	0.753	0.000300

Note : Difference-in-means coefficients. Observations are responses on a survey of school administrators (October, 2016). Column (1) examines the percentage of classrooms and offices with grid or solar access. Columns (2) and (3) examine hours spent watching video and using lights during planned hours (regular plus extracurricular activity hours) per (an average) week, respectively. Column (4) examines the gross percentage of students attending extracurricular activities per week. Column (5) examines the gross percentage of students (all grade levels) using electicity after regular hours per week. Schools were block-randomized over 2014-pass-rate tiers of six schools within each region; block fixed effects included. Second row in parentheses: MHT-unadjusted robust standard errors. Levels of significance: *** p<0.01, ** p<0.05, * p<0.10. Third row in brackets: Holmes-Bonferroni corrected p-values (e.g. the smallest unadjusted p-value in each column is multiplied by 5).

Table 4: Impacts on November 2016 O-level Outcomes

Column Type:	-Certification Criteria (7 Best Subjects)-		-Video Subjects (Biology/Geography)-		-No Video Subjects-	
	(1)	(2)	(3)	(4)	(5)	(6)
	Z-score	Pass Rate	Z-score	Pass Rate	Z-score	Pass Rate
Mean of Control Group (G6)	0.00893	0.582	0.0284	0.424	-0.000848	0.435
Sd of Control Group (G6)	1.031	0.493	1.037	0.456	1.024	0.379
Panel A: *Treatment Mean - Control Mean*						
Solar x TV x No Video (G1)	0.0251	0.0140	0.0592	0.0345	0.00773	0.00393
	(0.0630)	(0.0313)	(0.0608)	(0.0283)	(0.0682)	(0.0261)
	[1]	[1]	[1]	[1]	[1]	[1]
Solar x TV x English Videos (G2)	0.0404	0.0187	0.0457	0.0288	0.0380	0.0132
	(0.0722)	(0.0365)	(0.0607)	(0.0282)	(0.0799)	(0.0290)
	[1]	[1]	[1]	[1]	[1]	[1]
Solar x TV x Bilingual Videos (G3)	0.0812	0.0594*	0.0276	0.0180	0.104	0.0398
	(0.0668)	(0.0320)	(0.0605)	(0.0265)	(0.0713)	(0.0262)
	[1]	[0.3235]	[1]	[1]	[0.74]	[0.65]
No Sol. x No TV x English Vid. (G4)	0.0104	0.000283	-0.0172	-0.00472	0.0224	0.00507
	(0.0714)	(0.0353)	(0.0716)	(0.0342)	(0.0730)	(0.0283)
	[1]	[1]	[1]	[1]	[1]	[1]
No Sol. x No TV x Bilingual Vid. (G5)	-0.0247	0.00668	-0.0348	-0.0105	-0.0187	-0.00731
	(0.0661)	(0.0314)	(0.0600)	(0.0274)	(0.0717)	(0.0272)
	[1]	[1]	[1]	[1]	[1]	[1]
Observations	10,171	10,171	10,169	10,169	10,171	10,171
R-squared	0.758	0.462	0.708	0.603	0.731	0.641
Clusters	164	164	164	164	164	164
Pr > Joint F, All Treat.	0.712	0.386	0.639	0.523	0.627	0.569

Note : Difference-in-means coefficients. Observations are takers of junior-secondary exit examinations (11th-grade O-levels) in Nov. '16. Columns (1) examines normalized GPA across seven best subjects; columns (2) passage (indicators for getting at least two subject D's). In columns (3)-(4) and (5)-(6), means are across video-targeted subjects (geography and biology) in the best seven, and non-video-targeted subjects in the best seven, respectively. Note that column (2) need not be an average of columns (4) and (6), which are means across indicators of getting at least a D in one subject. Two students sat for neither video-targeted subject examination. Odd columns control for pretest (9th-grade) normalized GPA (from 2014); even columns, for quartile indicators of the same pretest GPA (following Angrist and Lavy, 2009). Schools were block-randomized over 2014-pass-rate tiers of six schools within each region; block fixed effects included. Second row in parentheses are MHT-unadjusted school-clustered standard errors. Levels of significance: *** p<0.01, ** p<0.05, * p<0.10. Third row in brackets are Holmes-Bonferroni corrected p-values (e.g. smallest p-value in each column is multiplied by 5).

Table 5: Average Impact of Solar-facilities-enabled Programs, Controlling for Video Provision, on November 2016 O-level Outcomes

Column Type:	-Certification Subjects (7 Best)-		-Video Subjects (Bio./Geo.)-		-No Video Subjects-	
	(1)	(2)	(3)	(4)	(5)	(6)
	Z-score	Pass Rate	Z-score	Pass Rate	Z-score	Pass Rate
Mean of Control Group (G6)	0.00893	0.582	0.0284	0.424	-0.000848	0.435
Sd of Control Group (G6)	1.031	0.493	1.037	0.456	1.024	0.379
Panel A: *Explanatory Variables*						
Solar-facilities-enabled Programs Indicator	0.0520	0.0277	0.0616*	0.0323**	0.0464	0.0190
(G1+G2+G3)	(0.0387)	(0.0197)	(0.0349)	(0.0160)	(0.0425)	(0.0160)
	[-0.0244, 0.129]	[-0.0112, 0.0667]	[-0.00733, 0.130]	[0.0006, 0.0640]	[-0.0376, 0.130]	[-0.0127, 0.0506]
English Videos Provision Indicator (G2+G4)	0.0118	0.00197	-0.0154	-0.00513	0.0248	0.00659
	(0.0493)	(0.0248)	(0.0462)	(0.0221)	(0.0532)	(0.0202)
Bilingual Videos Provision Indicator (G3+G5)	0.0195	0.0278	-0.0332	-0.0138	0.0440	0.0163
	(0.0472)	(0.0228)	(0.0435)	(0.0198)	(0.0511)	(0.0194)
Observations	10,171	10,171	10,169	10,169	10,171	10,171
R-squared	0.758	0.462	0.708	0.603	0.730	0.640
Randomization Block FE	X	X	X	X	X	X
Clusters	164	164	164	164	164	164
Pr > Joint F, All Treat.	0.532	0.192	0.344	0.255	0.515	0.429

Note: Coefficients from regressions of column variables on three explanatory variables and controls. Observations are takers of junior-secondary exit examinations (11th-grade O-levels) in Nov. '16. Column variable and control definitions are as in Table 4. The first regressor is an indicator of providing solar panels and television (G1+G2+G3), and its coefficient identifies the impact of solar-facilities-enabled school programs averaged across no-video, English-video and bilingual-video subgroups. The second regressor is an indicator of providing English videos (G2+G4), and its coefficient identifies the average impact of English videos across solar-receiving and non-solar receiving schools; the third regressor is an indicator of providing bilingual videos (G3+G5), and its coefficient identifies the analogous impact of bilingual videos. Second row in parentheses are school-clustered standard errors. Levels of significance: *** p<0.01, ** p<0.05, * p<0.10. Third row in brackets in the first row of Panel A are 95% confidence intervals.

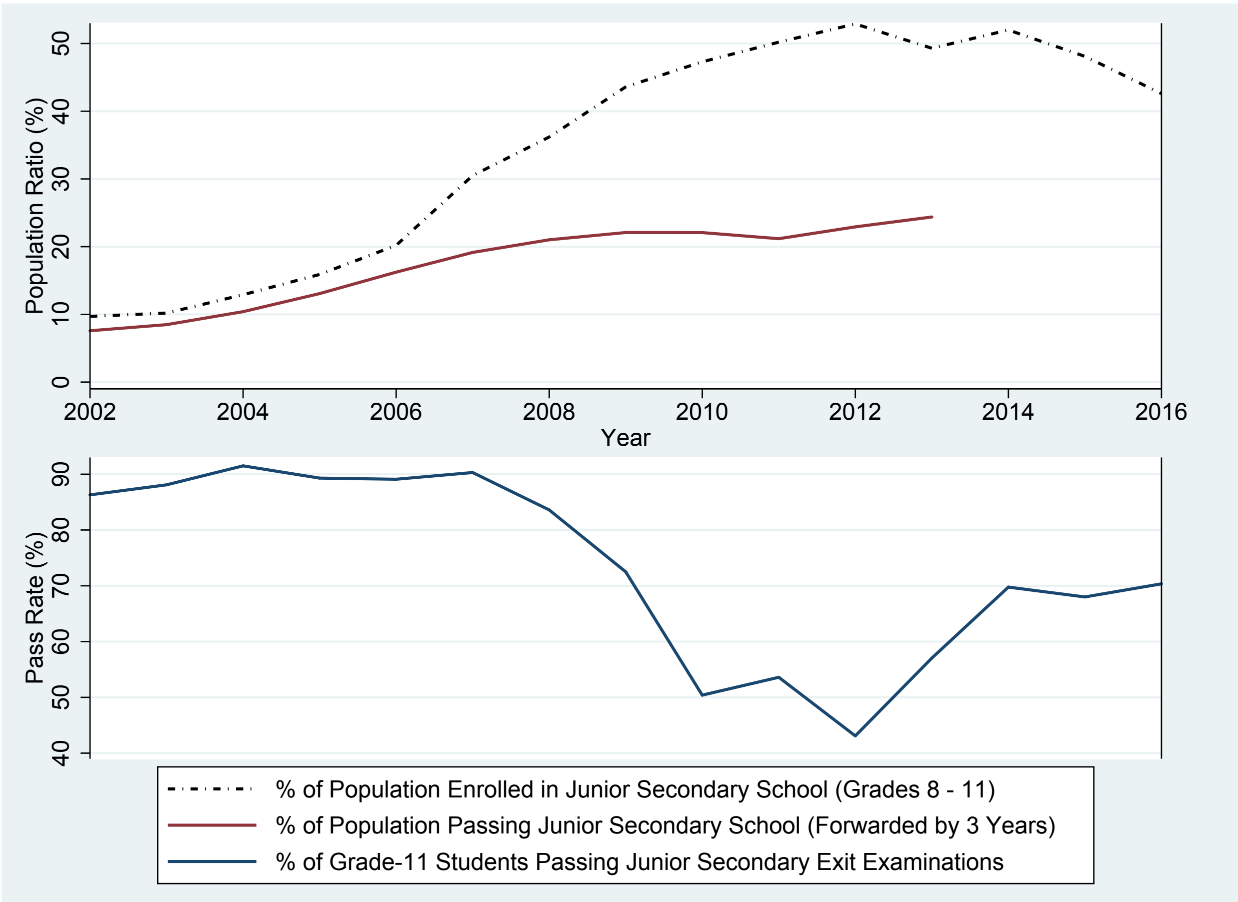
Table 6: Policy Prediction: Impacts of School Electrification and Language-varied Video Instruction on O-level Outcomes

Column Type:	-Certification Subjects (7 Best)-		-Video Subjects (Bio./Geo.)-		-No Video Subjects-	
	(1)	(2)	(3)	(4)	(5)	(6)
	Z-score	Pass Rate	Z-score	Pass Rate	Z-score	Pass Rate
Mean of Control Group (G6)	0.00893	0.582	0.0284	0.424	-0.000848	0.435
Sd of Control Group (G6)	1.031	0.493	1.037	0.456	1.024	0.379
Panel A: *Instrumented Variables*						
Proportion of Classrooms/Offices with Grid	0.114	0.0391	0.337	0.174*	0.00298	0.0110
or Solar Access ("Electrification Rate")	(0.225)	(0.116)	(0.217)	(0.103)	(0.248)	(0.0974)
Hour of Video Viewing per Average Week	0.0127	0.00497	5.13e-05	0.00105	0.0193	0.00627
x English Videos	(0.0386)	(0.0195)	(0.0329)	(0.0156)	(0.0428)	(0.0158)
Hour of Video Viewing per Average Week	0.0740	0.0633	-0.0506	-0.0256	0.131	0.0490
x Bilingual Videos	(0.0886)	(0.0449)	(0.0867)	(0.0402)	(0.0977)	(0.0370)
Observations	10,171	10,171	10,169	10,169	10,171	10,171
R-squared	0.759	0.460	0.705	0.600	0.729	0.640
Randomization Block FE	X	X	X	X	X	X
Clusters	164	164	164	164	164	164
AP First-stage F, Electrification Rate	19.97	19.96	19.96	19.95	19.97	19.96
AP First-stage F, English Video Hours	9.636	9.647	9.637	9.649	9.636	9.647
AP First-stage F, Bilingual Video Hours	4.570	4.572	4.570	4.572	4.570	4.572
Pr > Joint F, All Endog.	0.428	0.203	0.380	0.269	0.369	0.304

Note : Student-level IV regressions. Column variable and control definitions are as in Table 5. Three endogenous regressors are considered: (A) proportion of classrooms and offices with grid or solar access; (B) hours per average school week spent watching video, multiplied by the indicator of program English video provision; (C) hours per average school week spent watching video, multiplied by the indicator of program Bilingual video provision. Instruments are treatment indicators (G1-G5). Second row in parentheses are MHT-unadjusted school-clustered standard errors. Levels of significance: *** p<0.01, ** p<0.05, * p<0.10.

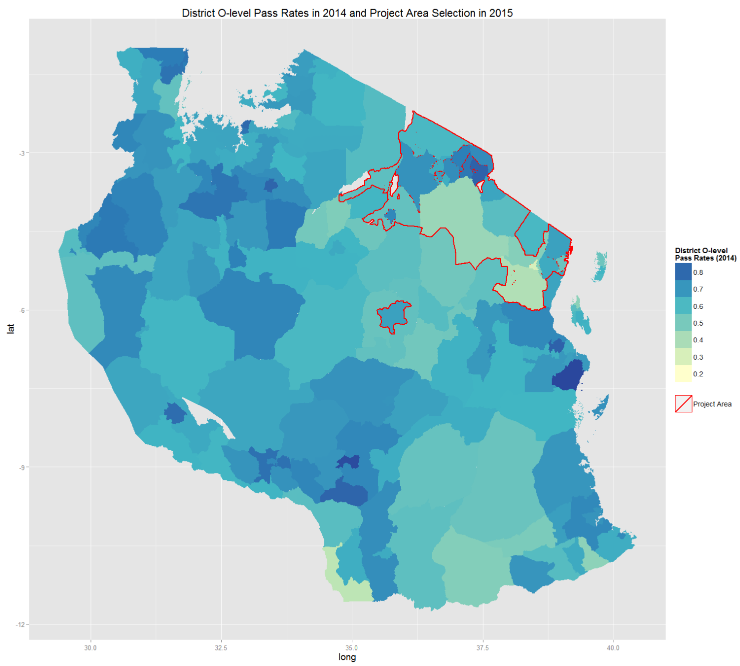
Figures

Figure 1: Pass Rates and Population Ratios in Tanzanian Junior Secondary Schools, 2002-2016



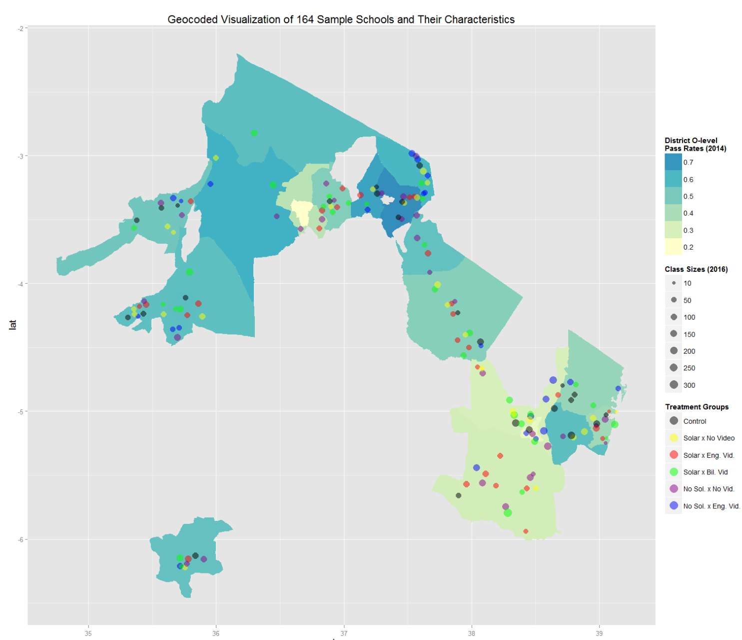
Note: The first line in dotted black shows gross junior-secondary-school enrollment (grades 8-11), expressed as a percentage of the official junior-secondary age-group population. The second line in maroon shows the total number of students newly receiving junior-secondary certification over the four-year period starting from the indicated year, expressed as a percentage of the official junior-secondary age-group population. (For example, in 2013, Tanzania had 3.5 million youths in the official age group corresponding to grades 8 through 11; had 1.7 million students actually enrolled in grades 8 through 11; and saw 0.7 million students succeeded in obtaining junior-secondary (O-level) certification between 2013 and 2016.) The third line in green shows the "O-level pass rate" as defined by the government: the number of 11th-grade students passing the indicated year's O-level exit examinations over the number of students sitting for the same examinations. Source: Government Data (MoEVT, 2005, 2010; PMO-RALG, 2014; PO-RALG, 2016).

Figure 2: District O-level Pass Rates in 2014 and Project Area Selection in September, 2015



Note: The research team initially targeted all schools without electricity in 23 northern Tanzanian districts (demarcated in red), an intersection of districts to which Off Grid Electric had expanded operations by 2015, and whose 2014 pass rates tended to be lower (of lighter color) than the rest's. Tanzania is a large nation, with a population of 56 million (2016 est.) and land area larger than the sizes of California, Oregon and Washington combined.

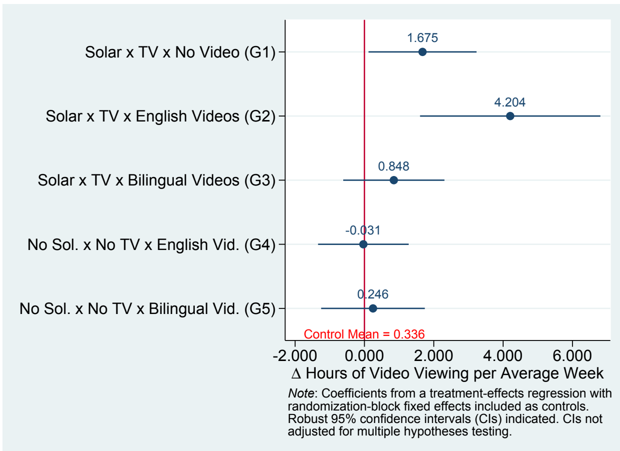
Figure 3: Map of 164 Sample Schools and Sizes of Schools



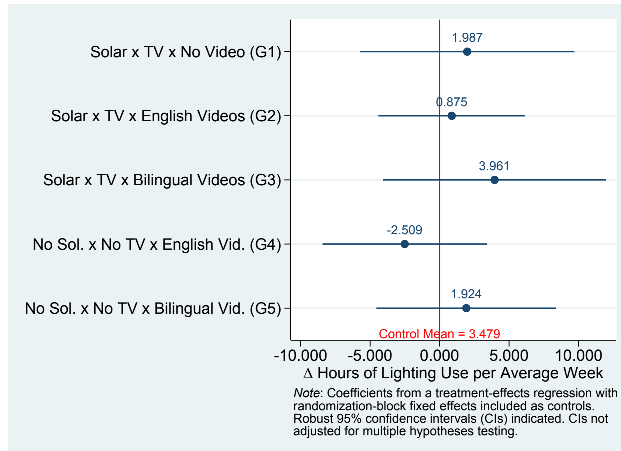
Note: Final sample of 164 schools with class sizes and random-assignments visualized. Districts are fewer than in fig. 2 by three, because some districts found with no un-electrified school were dropped before the interventions began. The size of the final project area was approximately the size of England.

Figure 4: Program Impacts on Pedagogy and Educational Time Use

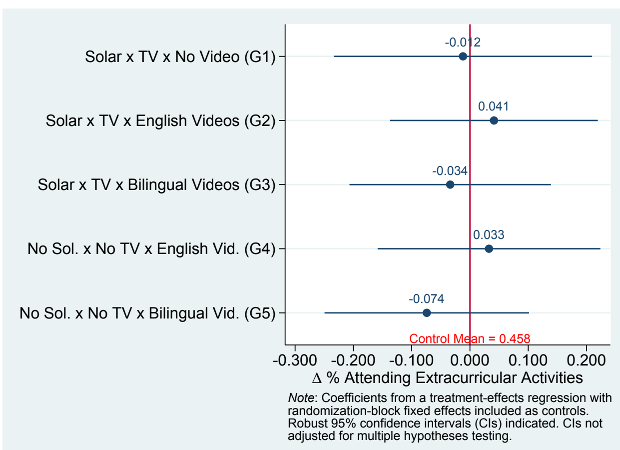
(a) Hours of Video Viewing per Average Week



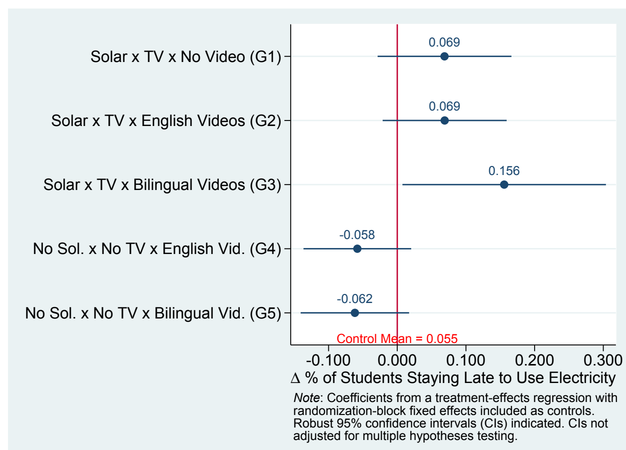
(b) Hours of Lighting Use per Average Week



(c) % Students Attending Extracurricular Activities



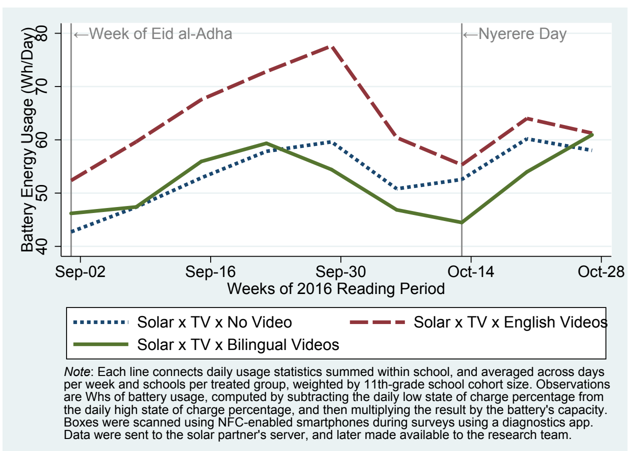
(d) % Students Staying Late to Use Electricity



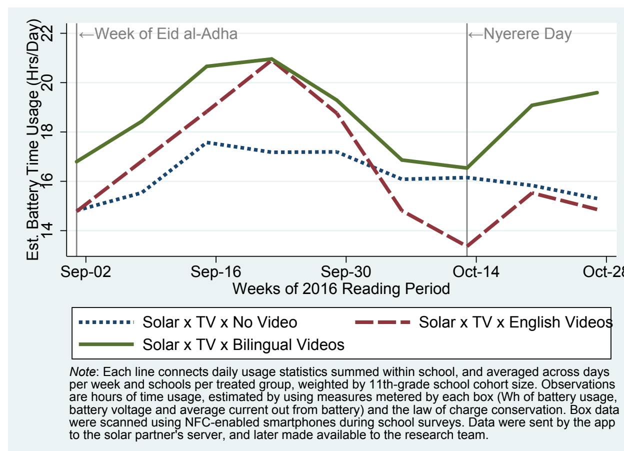
Note: Difference-in-means coefficients compare changes in educational behavior across five treatment groups and one control group. Responses were collected on a survey of school administrators conducted in October, 2016. Sub-figure (a) and (b) examine total hours spent watching video and using lights during planned hours (regular and extracurricular hours) in an average week, respectively. Sub-figure (c) examines the percentage of students attending extracurricular activities in an average week. Sub-figure (d) examines the percentage of students (all grade levels) using electricity after regular hours in an average week. Regressions control for randomization-block fixed effects. Regressions are weighted by the number of 11th-grade examination takers in each school. Robust 95% confidence intervals (CIs) indicated (not adjusted for multiple hypotheses testing).

Figure 5: Battery Usage Two Months Leading up to the Graduating Examinations in 2016

(a) Avg. Daily Battery Energy Usage (Wh/Day)

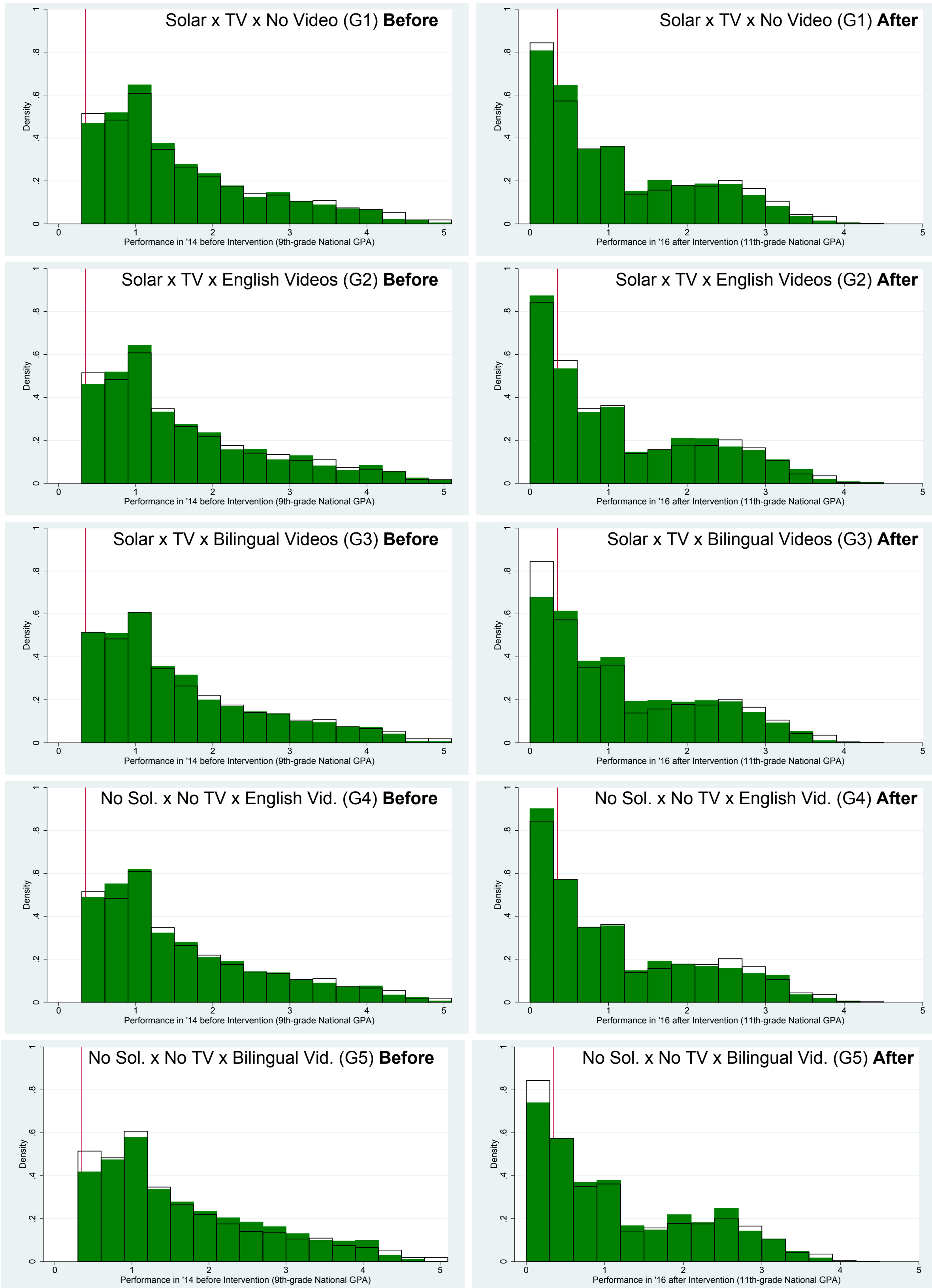


(b) Avg. Est. Battery Time Usage (Hrs/Day)



Note: These plots show daily metered battery usage (sub-figure (a)), and daily estimated battery time usage (sub-figure (b)), summed across boxes within school, and then averaged across days per week and schools per treated group. The battery meter did not record current out directly from solar panels; therefore, the total solar energy usage was greater. Appendix C suggests that the battery time formula—using Wh of battery energy usage, battery high voltage, average current out from battery and the law of charge conservation—tends to underestimate the time scale of usage. Appendix table A2 and appendix C include more details.

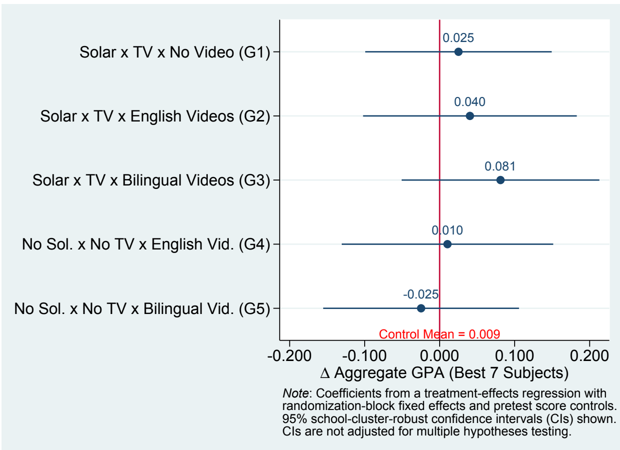
Figure 6: Histograms of National Examination Grade Point Averages before and after Treatment



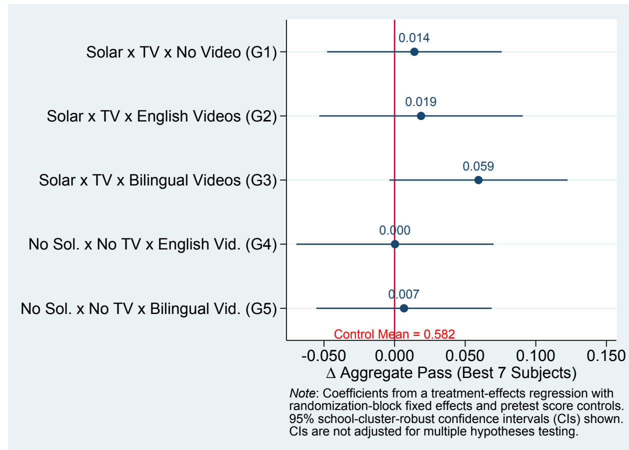
Note: Each row of plots shows histograms of treatment group GPAs before and after treatment, plotted in the foreground in green. Every plot also includes the control group's GPAs for comparison, in the background in white. The red lines indicate the pass GPA cutoff.

Figure 7: Impact on Outcomes

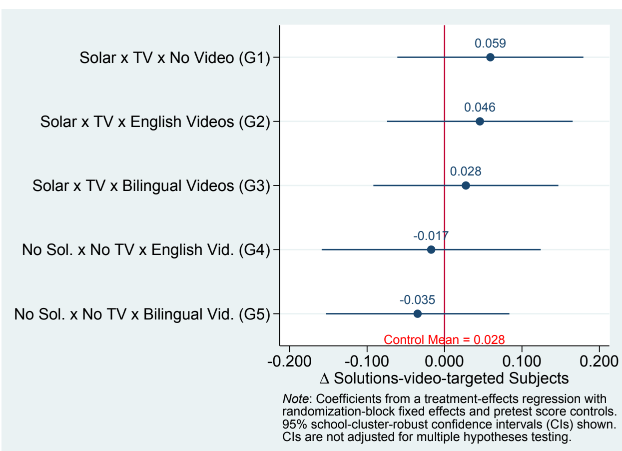
(a) Normalized Grade Point Average



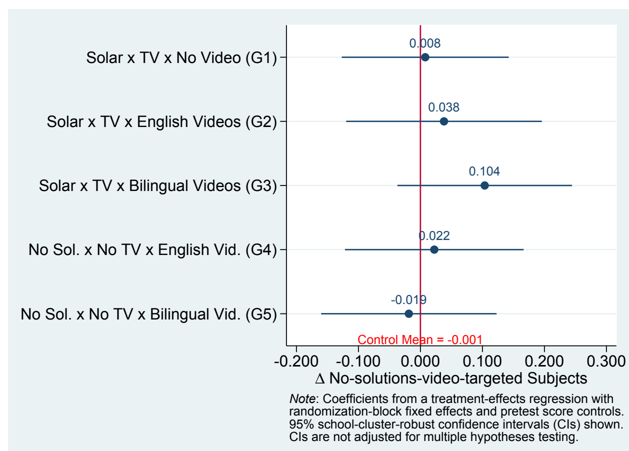
(b) O-level Certificate Pass Rate



(c) Solutions-video Targeted Subjects' GPA



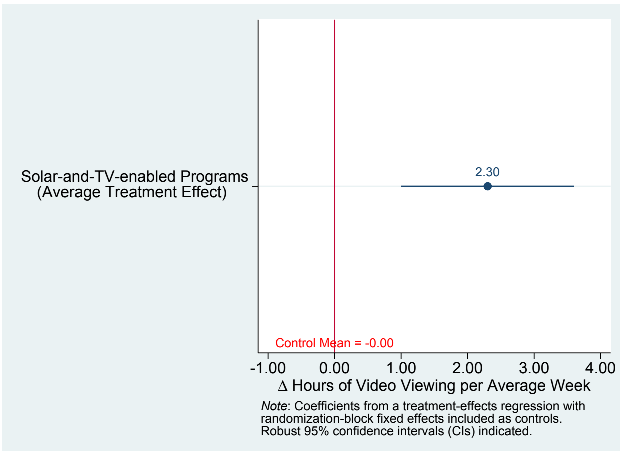
(d) No-solutions-video Targeted Subjects' GPA



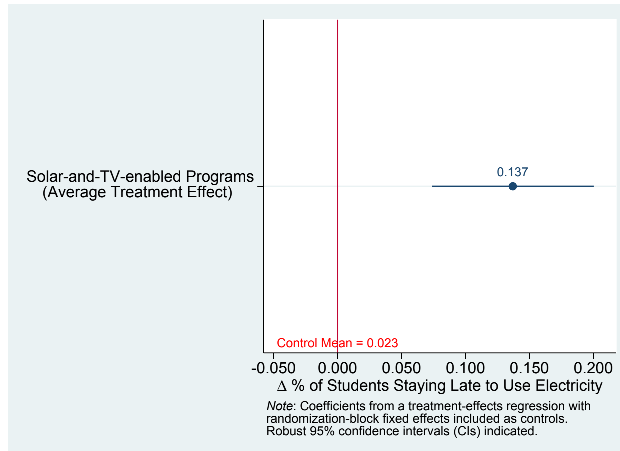
Note: Difference-in-means coefficients compare outcomes across five treatment groups and one control group. Observations are 11th-grade students sitting for the junior-secondary exit examinations (O-levels) in November, 2016. Sub-figure (a) examines normalized grade-point average (GPA) across a student's seven best subjects (used by the government to determine certification); sub-figure (b), indicator for passing junior-secondary school (or Division IV certification, whose threshold is getting at least two D's). In sub-figures (c) and (d), the GPAs are across solutions-video-targeted subjects (geography and biology) included in the best seven, and non-solutions-video-targeted subjects included in the best seven, respectively. Regressions control for normalized pretest GPAs (or, in the case of pass rates, pretest GPA quartiles) and randomization-block fixed effects. School-cluster-robust 95% confidence intervals indicated (not adjusted for multiple hypotheses testing).

Figure 8: Combined Average Impacts of Solar-and-TV-enabled Programs

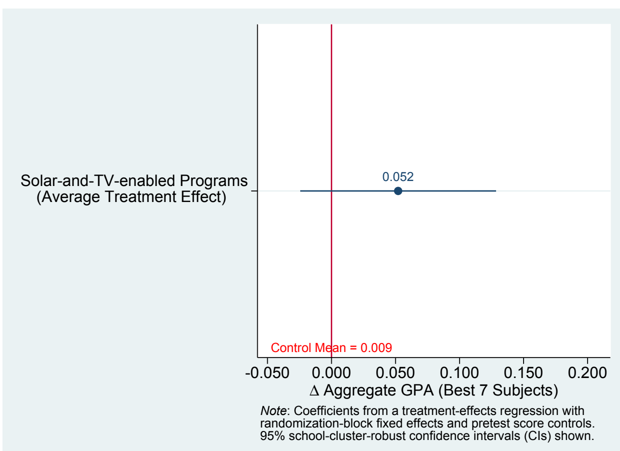
(a) Hours of Video Viewing per Average Week



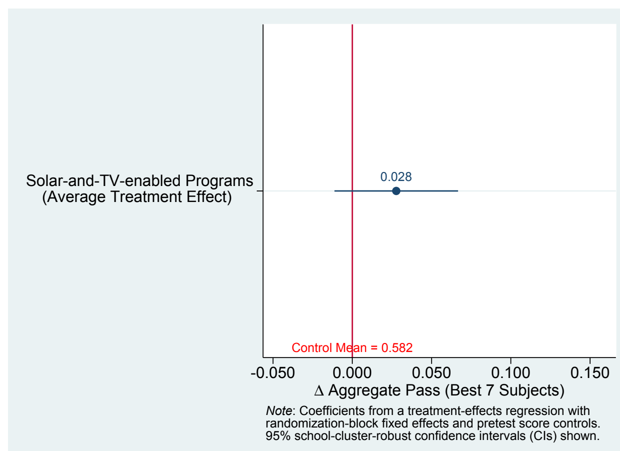
(b) % Students Staying Late to Use Electricity



(c) Normalized Grade Point Average



(d) O-level Certificate Pass Rate



Note: Coefficients from regressions of dependent variables on one explanatory variable and controls. In sub-figures (a) and (b), school-level dependent variables examine (a) total hours spent watching video during planned hours (regular and extracurricular hours) in an average week, and (b) the percentage of students (all grade levels) using electricity after regular hours in an average week. In sub-figures (c) and (d), student-level dependent variables examine (c) normalized grade-point average (GPA) across a student's seven best subjects (used by the government to determine certification); and (d), indicator for passing junior-secondary school (getting at least two D's). The explanatory variable is an indicator of providing solar panels and television (G1+G2+G3), and its coefficient identifies the average impact of solar-facilities-enabled school programs. Controls include an indicator of providing English videos (G2+G4) and an indicator of providing bilingual videos (G3+G5), as well as randomization-block fixed effects. In (a) and (b), regressions are weighted by the number of 11th-grade examination takers in each school, and robust 95% confidence intervals (CIs) are indicated. In (c), regressions additionally control for pretest GPA quartiles; in (d), normalized pretest GPAs; and school-cluster-robust 95% CIs are indicated.

A Appendix Tables

Table A1: Minimum Detectable Effect Sizes under Different Scenarios

Sample Evolution	---- *Pre-analysis with assumed moments* ----			-- *Final sample with final sample moments* --		
	(1)	(2)	(3)	(4)	(5)	(6)
Power:	80%	80%	80%	80%	80%	80%
Comparison Type	*Individual*	*Individual*	*Combined*	*Individual*	*Individual*	*Combined*
MHT Correction	No	Yes	-	No	Yes	-
Outcome Variables:						
Normalized GPA	0.277	0.338	0.160	0.186	0.227	0.106
Pass Rate	0.0692	0.0845	0.0400	0.0939	0.1145	0.0533
Parameters and Moments:						
τ_{α/2}	1.96	2.576	1.96	1.96	2.576	1.96
τ_{1-κ}	0.84	0.84	0.84	0.84	0.84	0.84
c	1	1	1	1	1	1
s	0	0	0	0	0	0
P	0.500	0.500	0.500	0.528	0.528	0.524
J (Treatment)	35	35	105	28	28	86
J (Control)	35	35	105	25	25	78
n	40	40	40	62	62	62
σ (GPA)	1	1	1	0.49	0.49	0.49
ρ (GPA)	0.15	0.15	0.15	0.23	0.23	0.23
σ (Pass Rate)	0.25	0.25	0.25	0.36	0.36	0.36
ρ (Pass Rate)	0.15	0.15	0.15	0.10	0.10	0.10

Note: This table reports minimum detectable effect sizes (MDEs) calculated under different clustered-randomized-design scenarios, using equation (12) of Duflo et al. (2007) (who themselves follow Bloom (2005)). The GivePower School Program initially planned to randomize 210 schools into five treatment groups and one control group, with 35 schools in each treatment group and 35 in the control group. The final sample came down to 164 schools, with approximately 28 schools in each treamtnet group and 25 in the control group. Columns (1)-(3) report originally assumed MDEs at the time of random assignment; assumptions were liberally made, and columns (1) and (3), specifically, were reported in the pre-analysis plan. Columns (4)-(6) report MDEs given moments realized in the final sample of 164 schools. These moments include: the average number of students per school (n); the residual intracluster correlation coefficients (ρ) and standard deviations (σ) of test scores after controlling for student pretest scores and randomization-block fixed effects; and ρ and σ of pass indicators, after controlling for pretest score quartile indicators and randomization-block fixed effects.

Table A2: Installation Details and Solar Facility Problems

	(1) # Rooms Receiving Program Solar	(2) # Classrooms Receiving Program Solar	(3) # Other Rooms Receiving Program Solar	(4) Battery Energy Consumption (Wh/Day)	(5) Estimated Battery Time Use (Hrs/Day)	(6) # of Solar Box and Appliance Thefts	(7) # of Other Solar Facility Problems	(8) # of Box Insurance Replacements
Treatment Indicators :								
Solar x TV x No Video (G1)	2.456***	1.402***	1.025***	54.47***	16.26***	0.174*	0.0546	0.0584
	(0.255)	(0.195)	(0.215)	(7.197)	(1.687)	(0.0900)	(0.0615)	(0.0640)
	[0]	[0]	[0.00003]	[0]	[0]	[0.275]	[1]	[1]
Solar x TV x English Videos (G2)	2.677***	1.854***	0.813***	67.55***	17.33***	0.0491	0.0777	0.0925*
	(0.200)	(0.223)	(0.174)	(5.755)	(1.214)	(0.0447)	(0.0506)	(0.0481)
	[0]	[0]	[0.00003]	[0]	[0]	[1]	[0.512]	[0.2515]
Solar x TV x Bilingual Videos (G3)	2.480***	1.802***	0.708***	54.31***	19.01***	0.0784	0.165***	0.117*
	(0.183)	(0.218)	(0.193)	(5.083)	(0.926)	(0.0761)	(0.0630)	(0.0591)
	[0]	[0]	[0.00108]	[0]	[0]	[0.915]	[0.0491]	[0.2515]
No Sol. x No TV x English Vid. (G4)	-0.0149	0.00747	-0.0221	0.435	0.380	-0.00668	-0.00895	-0.00645
	(0.130)	(0.146)	(0.117)	(3.420)	(0.653)	(0.0392)	(0.0355)	(0.0325)
	[1]	[1]	[1]	[1]	[1]	[1]	[1]	[1]
No Sol. x No TV x Bilingual Vid. (G5)	-0.0585	-0.0236	-0.0348	0.952	0.0778	-0.0306	-0.0282	-0.0230
	(0.154)	(0.147)	(0.146)	(3.775)	(0.658)	(0.0525)	(0.0412)	(0.0370)
	[1]	[1]	[1]	[1]	[1]	[1]	[1]	[1]
Control Mean	0	0	0	0	0	0	0	0
Control Sd	0	0	0	0	0	0	0	0
Observations	164	164	164	164	164	164	164	164
R-squared	0.807	0.715	0.461	0.766	0.857	0.304	0.376	0.329
Sum of Student Weights	10,171	10,171	10,171	10,171	10,171	10,171	10,171	10,171
Sum of Sample Values	222	146	78	5,005	1,505	6	15	13
Pr > Joint F, All Treat.	0	0	0	0	0	0.320	0.0483	0.120

Note : Difference-in-means coefficients. Observations are schools. Columns (1)-(3) examine surveyor-eye-inspected records. "Other rooms" include offices (71%); laboratories (10%); libraries (8%); dormitories (1%); and teacher houses (10%). Columns (2) and (3) report estimated usage statistics summed across boxes per school and averaged across days per two months leading up to November 2016 national examinations. Column (6) examines the number of solar appliance thefts occurring over the evaluation period; column (7), the number of solar facility problems due to technical or natural causes; column (8), the number of box warranty replacements. Regressions control for randomization-block fixed effects. First row in parentheses are MHT-unadjusted robust standard errors. Levels of significance: *** p<0.01, ** p<0.05, * p<0.10. Second row in brackets are Holmes-Bonferroni corrected p-values (e.g. the smallest unadjusted p-value in each column is multiplied by 5).

Table A3: Impacts on November 2016 O-level Outcomes by Gender and Pretest Performance

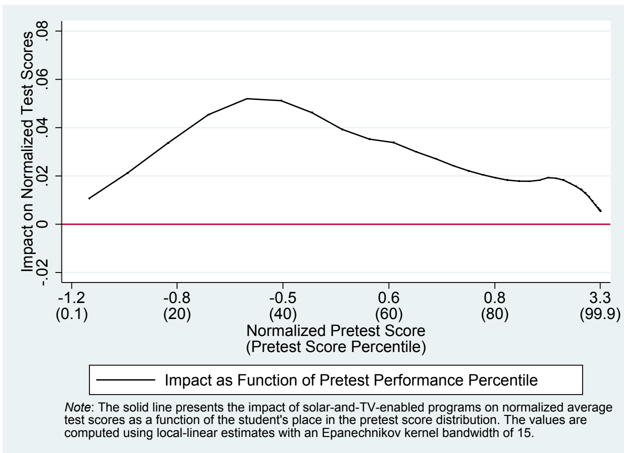
Column Type:	----Boy Students----		----Girl Students----		--Top-half Students--		--Bottom-half Students--	
	(1) Z-score	(2) Pass Rate	(3) Z-score	(4) Pass Rate	(5) Z-score	(6) Pass Rate	(7) Z-score	(8) Pass Rate
Mean of Control Group (G6)	0.205	0.622	-0.156	0.547	0.821	0.913	-0.676	0.302
Sd of Control Group (G6)	1.126	0.485	0.912	0.498	0.919	0.282	0.470	0.459
Panel A: *Explanatory Variables*								
Solar-facilities-enabled Programs Indicator	0.0153	0.00762	0.0794*	0.0415*	0.0419	0.0156	0.0622*	0.0363
(G1+G2+G3)	(0.0403)	(0.0199)	(0.0429)	(0.0237)	(0.0500)	(0.0161)	(0.0329)	(0.0273)
English Videos Provision Indicator (G2+G4)	-0.0399	-0.000437	0.0521	0.00673	-0.0219	-0.0132	0.0265	0.0191
	(0.0518)	(0.0270)	(0.0537)	(0.0275)	(0.0684)	(0.0253)	(0.0395)	(0.0304)
Bilingual Videos Provision Indicator (G3+G5)	0.00155	0.0220	0.0417	0.0359	-0.00295	0.00594	0.0377	0.0482
	(0.0526)	(0.0235)	(0.0494)	(0.0263)	(0.0659)	(0.0183)	(0.0399)	(0.0321)
Observations	4,545	4,545	5,626	5,626	4,674	4,674	5,497	5,497
R-squared	0.777	0.483	0.732	0.452	0.613	0.105	0.252	0.190
Randomization Block FE	X	X	X	X	X	X	X	X
Clusters	163	163	164	164	164	164	163	163
Pr > Joint F, All Treat.	0.766	0.616	0.222	0.145	0.833	0.557	0.213	0.218

Note : Coefficients from regressions of column variables on three explanatory variables and controls. Observations are takers of junior-secondary exit exams (11th-grade O-levels) in Nov. '16. Odd columns examine normalized GPAs across seven best subjects, used by the government to determine certification; even columns, passage. The coefficient on the first regressor, an indicator of providing solar panels and television (G1+G2+G3), identifies the impact of solar-facilities-enabled school programs averaged across no-video, English-video and bilingual-video subgroups. The coefficient on the second regressor, an indicator of providing English videos (G2+G4), identifies the average impact of providing English videos across solar-receiving and non-receiving schools; the coefficient on the third regressor, an indicator of bilingual videos provision (G3+G5), identifies the analogous impact of providing Bilingual videos. Columns (1)-(2) restrict sample to male students; columns (3)-(4), to female students; columns (5)-(6), to students pretesting in the top-half of November 2014 Form Two National Assessment (FTNA) outcomes; columns (7)-(8) restirct sample to students pretesting in the bottom-half of 2014 FTNA outcomes. Second row in parentheses are school-clustered standard errors. Levels of significance: *** p<0.01, ** p<0.05, * p<0.10.

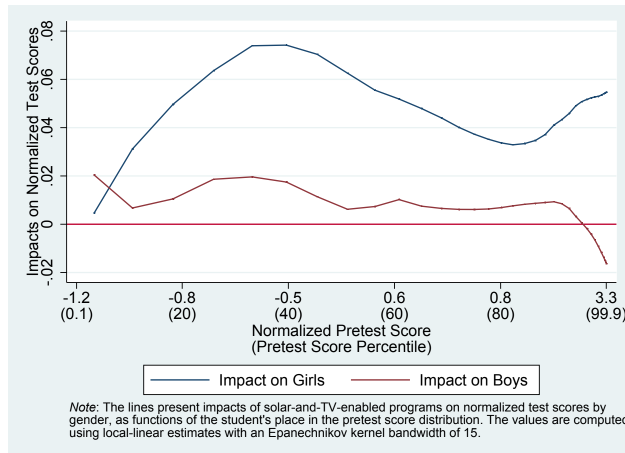
B Appendix Figures

Figure B1: Distributional and Heterogeneous Effects

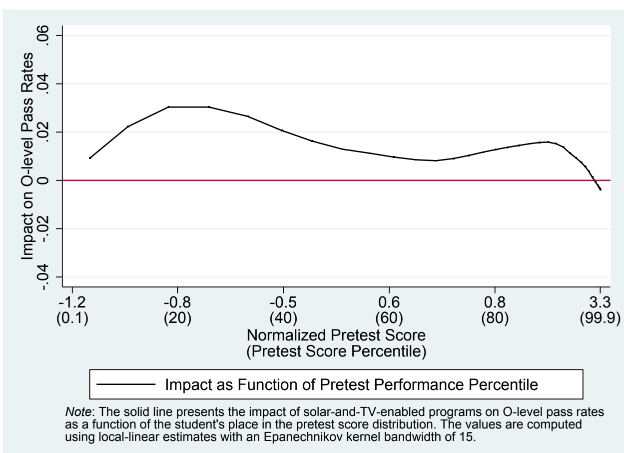
(a) Impact on Normalized GPA



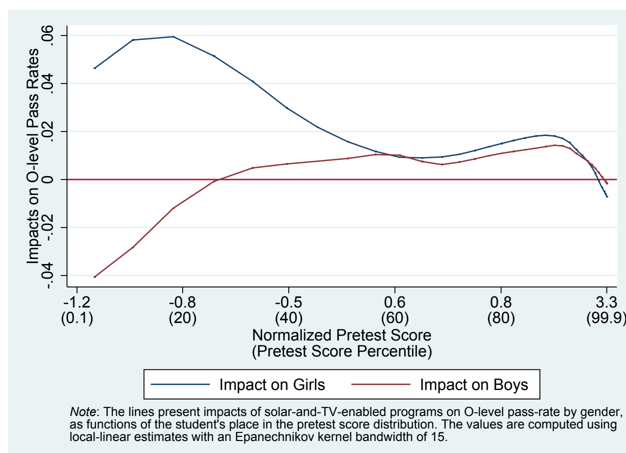
(b) Impacts on Normalized GPA by Gender



(c) Impact on O-level Pass Rate



(d) Impacts on O-level Pass Rate by Gender



Note: The lines present first-year achievement impacts of solar-and-TV-enabled programs as functions of pretest GPA percentile. Outcomes are O-level pass rates in sub-figures (a) and (c), and normalized grade-point averages in sub-figures (b) and (d). The values are computed using local-linear estimates with an Epanechnikov kernel bandwidth of 15. It should be noted that the effects and differences shown, while suggestive, are not statistically significant.

C Appendix Note on Estimation of Daily Hours of Battery Usage

This appendix section describes the formula I used to convert a set of daily usage statistics recorded by OGE's solar boxes into estimated daily hours of usage.[35]

Only about a half of the solar boxes come with firmware v38 or later, which records daily hours of usage. Older firmware does not track the total time, but tracks some common statistics including:

- H: "Daily High State of Charge (unit: %)";
- L: "Daily Low State of Charge (unit: %)";
- G: "Daily High Voltage (unit: mV)";
- C: "Daily Average Current Out (From Battery) (unit: mA)".

This availability offers us a chance to develop a common formula to estimate the daily hours of usage for all boxes. Following Chen (2004), note that because power is the rate at which energy is consumed, energy can be approximated as:

$$E = \int_0^T v(t)i(t)dt \approx V \times I \times T, \tag{c7}$$

where E denotes energy (a measure of the total number of joules dissipated by a circuit), V denotes the supply voltage, and I denotes the average current, and T represents time (measured in hours).

The available daily electric energy on the solar box is 107 Wh. Using this information, I have, $E = (H-L)/100 \times 107$ joule-hours of energy used per day. I also have $V = G/1000$ and $I = C/1000$, in appropriate units of voltages and amperes, respectively. Therefore, I can compute,

$$T = E/(V \times I) = (H - L)/(G \times C) \times 1.07 \times 10^7 \tag{c8}$$

as formula-predicted daily hours of battery usage.

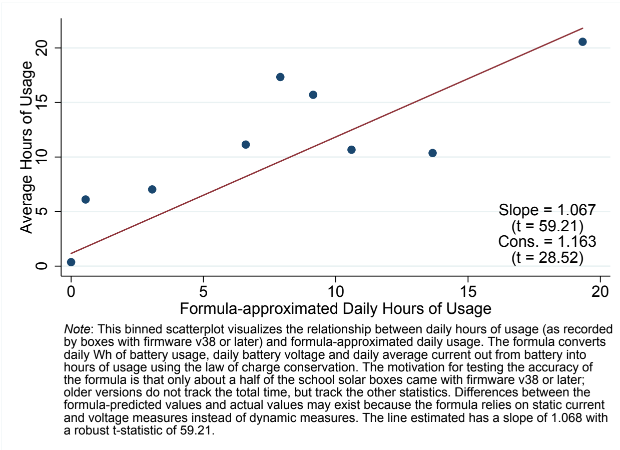
Differences between formula-predicted values and actual values can arise for a number of reasons. One has been alluded to already: the formula relies on static current and static voltage measures, instead of dynamic measures (c.f. the step approximating the energy integration above). Also, while v37 boxes records G (the daily average current out), v38 boxes does not record G but records the number of seconds of current outflow in different current range brackets: "0-249mA," "250-499mA," "500-999mA," "1000-1999mA," and "greater than 2000mA." Therefore, in order to obtain a comparable measure of G for a v38 box, I take a weighted average of current outflows in each bracket. To get the average current outflow parameter for each bracket, I use v37 box data to compute the mean of G across days on which G fell into the bracket of interest. In this way, I take 90mA, 350mA, 570mA, 1005mA, and 2000mA as the average currents for the respective brackets. Finally, whenever the estimated T is greater than 24, I truncate the estimate to 24.

How does this procedure perform? In the two binned-scatter plots in fig. C1, I conduct correlation studies between average daily hours of usage and formula-approximated daily hours of usage. Sub-figure (a) shows the binned-scatter correlation. Sub-figure (b) shows the correlation after taking out school effects (e.g. some schools may have two v38 boxes while others none then I am picking up clustering, not the usefulness of the formula) and date effects (e.g. holiday effects). The correlations seem close.

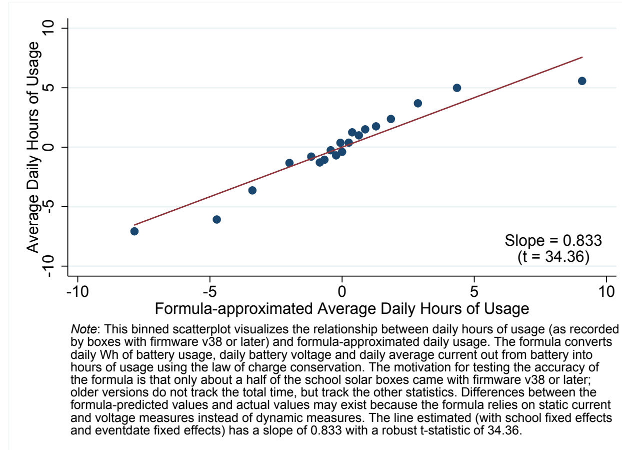
[35]I thank Solutions Cubed's David Brobst, who is on the design team of the solar box firmwares used in this study, for discussing this problem with us.

Figure C1: Binned-scatter Plots of V38 Hours of Usage and Formula-approximated Hours of Usage

(a) No Fixed Effect Removed



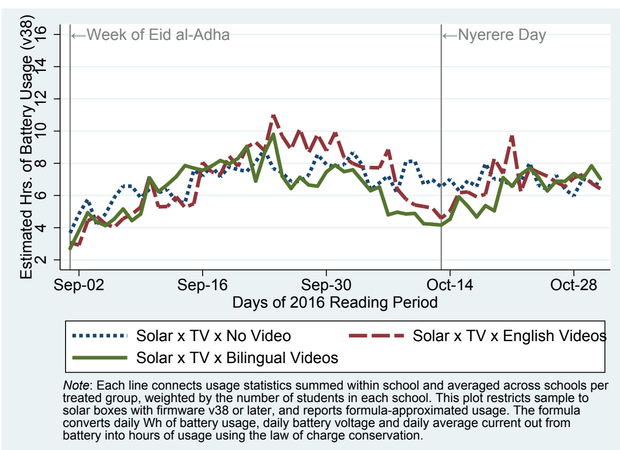
(b) School and Date Fixed Effects Removed



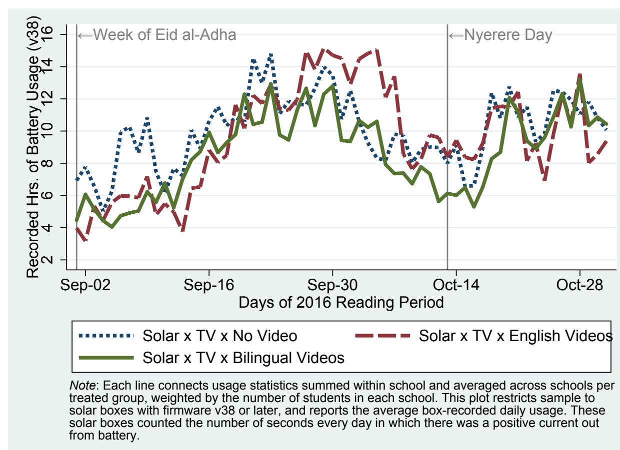
Next, in fig. C2 I first plot v38-recorded daily hours of battery usage, averaged by treatment groups; I then plot for comparison formula-predicted daily hours of battery usage (for the same v38 boxes), averaged by treatment groups:

Figure C2: Battery Time Usage Measures: Formula-approximated vs. V38-box-recorded

(a) Formula-approximated Usage Hours (V38 Boxes)



(b) Recorded Usage Hours (V38 Boxes)



It seems that while the formula tends to underestimate the scale of usage, the formula predicts well the directions of the differences in usage across treatment groups. Since the formula seems to be somewhat reliable in identifying these directions, perhaps there is merit to using this formula to augment the study's analyses with formula-predicted battery time-use patterns. It is remarkable that for the subset of v38 boxes, the differences between "Solar x TV x English Videos" and "Solar x TV x Bilingual Videos" seem small in these particular boxes, but the box with the older firmware suggests significantly greater hours of usage per day by the latter group. The full picture I get (showing averages within weeks for smoothing) is what is shown in fig. 5.

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