Powering Education:[*]

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Abstract:

More than 1.3 billion people worldwide have no access to electricity and this has first-order effects on several development dimensions. We focus on the link between access to light and education. We evaluate an intervention that distributed solar lamps to 7th grade pupils in rural Kenya and monitored their educational outcomes throughout a year at a quarterly frequency. In the identification strategy we account for spillover effects and we find a positive and significant effect on treated students as well as a positive and significant spillover effect on control students. In a class with the average treatment intensity of our sample (where 43% of students have solar lamps), treated students experience math grade increases of 0.88 standard deviations. Moreover, we find a positive effect of treatment intensity on control students; increasing a classs proportion of students with lamps by 10% increases the grades of control students by 0.22 standard deviations. We exploit household geolocation to disentangle within- class and geographical spillover effects. We show that geographical spillovers do not have a significant impact, and within-school interaction is the main source of spillover effects. Finally, we provide suggestive evidence that the mechanism through which lamps affect students is by increasing rates of co-studying at school, especially after sunset.

1   Introduction

More than 1.3 billion people worldwide lack access to electricity and 40%
of whom live in Sub-Saharan Africa (IEA, 2013). This means that roughly
a quarter of humanity lives without electricity to light their homes in the
evenings, and are without power in the workplace during the day. This
'energy poverty' strongly constrains the standard of living of such people.

In Africa, the electrical power grid reaches only about 400 million of the
continent's 1 billion people. In urban and semi-urban areas, over 30% of
people have access to grid electricity. This figure drops to less than 2% in
rural areas. The electrical power grid is expanding slowly and unevenly.
Governments and the private sector are working to reach deeper into re-
mote areas but financial, political and logistical barriers have proven to be
significant obstacles.

The link between energy access and education is an important and under-
explored dimension of development. Looking at aggregate data, we can
see a strong correlation between rates of electrification and primary school
completion (Figure 1).[1] The lack of access to electric light limits opportuni-
ties to study after sunset and it constrains students' allocation of activities
throughout the day. In developing countries, it is not uncommon to find
students of all ages gathering to read at night under the lights of a gas sta-
tion or a shop (Figure 2).[2] . However, in rural areas, the lack of such basic
infrastructure means that even this option may not be possible. Electrifica-
tion of rural areas in developing countries is a long and costly process. By
the time this occurs, generations of students risk being affected by the lack
of lighting, undermining the process of human capital accumulation.

In this project, we evaluate the impact that solar lamps, which are a readily
available source of lighting, have on education. We distributed solar lamps

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[1]Interestingly, the R-squared of this simple regression of primary education completion
on electrification (0.6) is substantially higher than that of educational completion on per-
capita income (0.34).

[2]The first picture, which made the headlines of major newspapers, shows Daniel Cabr-
era, a nine-year-old boy from the Philippines, studying under the lights of a McDonalds
restaurant. The second picture was taken in Guinea and was published by the New York
Times and the BBC.

to 7th grade students in off-grid rural areas, randomizing the treatment at the pupil level. However, during the fieldwork, we collected evidence indicating that a substantial degree of lamp-sharing and co-studying was taking place. These gave rise to spillover effects from treatment, violating the stable unit treatment value assumption needed for identification. To circumvent this problem and to account for spillovers, our identification strategy exploits both variation in whether a student was given a lamp, as determined through treatment randomization, as well as variation in class treatment intensity (i.e. the share of students in a given class who received lamps).

The variation in treatment intensity was not part of our original design. As we explain later in detail, we exploit the quasi-experimental variation in treatment intensity across classes that arose from name mismatches between official school transcripts and our baseline survey; these administrative glitches led to variation in treatment intensity across classes. We show that this source of variation in the data is uncorrelated with any observable outcome or covariate of interest related to school quality or student background. Moreover, by exploiting the panel dimension of the data, we can control for all unobservable time-invariant characteristics both at the student and class level that can affect our results. Ultimately, our identification strategy follows the approach of the deworming paper of Miguel & Kremer (2004). In that case too randomization was at the pupil level and, after realising the presence of spillovers, the identification relied on the variation of treatment intensity across clusters retrospectively.

The results show a positive and significant effect of the intervention on students' grades, which are our outcome of interest. Treated students in a class with the average treatment intensity of our sample, which is about 43%, experience an increase in math grades of 0.88 standard deviations. Moreover, lamp distribution affects control students too - increasing treatment intensity by 10% improves their math grades by 0.22 standard deviations. All results are robust to wild-bootstrapping as in Cameron et al. (2008) and to randomisation inference as in Rosenbaum (2002).[3]

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[3]We have grades also on english, swahili, science and social science. Nevertheless, we focus our study on grades in mathematics because it was often the main focus of study

To further disentangle the source of spillovers, we exploit treatment intensity both across classes and geographical areas around pupils. Spillover effects appear to be driven by within-class interactions between students and there is not robust evidence of geographical spillovers. Moreover, using a survey on students' habits and time use, we find suggestive evidence that the mechanism through which the lamps affected students is by increasing co-studying at school after sunset. Our results show that access to light through solar lamps has a significant impact on education and, at least in the short-term, it can be an effective tool to limit the drawbacks on human capital accumulation due to the lack of access to light. Finally, the presence of spillovers and the fact that they arise from within-class interaction and higher co-studying in class, it suggests that a successful policy, whose focus is on education, does not need to distribute a lamp to every pupil, but it can provide lamps to the schools covering only a fraction of students.

Our paper is related to the large body of literature on inputs to education in developing countries. Promoting human capital accumulation is one of the key steps in the development process. The literature shows the importance of building school infrastructure (Duflo, 2001; Burde & Linden, 2013); the relevance of providing free primary education (Lucas & Mbiti, 2012); the effect of subsidies to households and pupils on enrolment (Schultz, 2004; Angrist & Lavy, 2009; Ambler et al., 2015); the impact of monetary incentives on teachers' performance (Muralidharan & Sundararaman, 2011); and the role of learning external technologies such as mobile phones (Aker et al., 2014). Soft inputs like information on schooling returns (Jensen, 2010) and involving parents in school management (Gertler et al., 2012) have also been found to have positive impacts on educational outcomes. Our paper is more related to the literature that analyses the role of complementary inputs to education, such as deworming programs (Miguel & Kremer, 2004) and flip-charts (Glewwe, 2002). Given the lack of electrification in developing countries, our contribution is to investigate the importance of access to light as a key input for education and to measure the spillover effects that

groups at school after the regular lectures and the subject that treated pupils studied the most in the evenings. The results for other subjects are not significant. However, our results on mathematics grades are robust to correction for multiple hypothesis testing with the other subjects.

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access to light can have on students.

There has been a growing number of studies on the the effects of solar energy on households. For instance Grimm et al. (2016) looked at the effects of basic photovoltaic kits on households and they some effect on domestic productivity, health, and on the environment. Furukawa (2014) and Kudo et al. (2017) focused on the relation between solar lamps and education in Uganda and Bangladesh, but, contrary to our results, they did not find an effect on school grades. The main differences between their study and ours, apart from different experimental contexts, is that their design involved students over different grades and their identification strategy did not completely account for spillover effects.

Our paper is also related to the literature on energy access and development, such that of Dinkelman (2011), Rud (2012), Lipscomb et al. (2013), Lee et al. (2016a), Lee et al. (2016b), and Lenz et al. (2017), . These papers are concerned with the effects of energy access on employment, industrialization, human development, housing values, and poverty. These studies examined the impact of electrification finding mixed results. They look at large region-wide technology shock, whereas we evaluate the effect of providing a solar lamp, which is a smaller and individual shock, but it gives a more accessible and easy to implement source of lighting. Moreover, our study focuses on education, which is a subject not specifically covered by the literature on large scale electrification.

The paper is structured as follows: Section 2 describes the intervention; Section 3 explains the source of variation in the data for identification; Section 4 focuses on the the estimation strategy and results; Section 5 discusses the mechanisms of our findings; and Section 6 concludes.

2 Project structure, randomization, and compliance

The experiment involved about 300 students in 7$^{\text{th}}$ grade across 13 classes in the Loitokitok and Nzaui districts, relatively close to the Tanzanian border

and Mount Kilimanjaro (Figure 3). This is an off-grid rural area where household electrification rates are below 2.6%.

The project started with a baseline survey in June 2013. We interviewed students and collected end of term grades from school transcripts. Lamps were distributed to the treatment group in September 2013, at the beginning of a new school term.[4] We then collected end of term grades for the treatment and control groups in November 2013, March 2014, and June 2014. We also ran extensive interviews of students in November 2013 and March 2014.

The baseline survey covered 341 pupils. We were able to match 286 of these with the transcripts of grades provided by the school. This constituted our core sample, within which we conducted treatment randomization. We distributed solar lamps to 143 pupils and, in order to mitigate resentment and in the interest of fairness, control students were promised that they would receive a lamp at the end of the experiment.[5] We randomized treatment assignment at the pupil level so that each had a mix of treatment and control group. We chose this level of randomization to maximize statistical power, given the budget and the size of our sample. In our randomization strategy, we sought balance between treatment and control groups on marks, which was our dependent variable, and balanced distribution of genders, classes, and household wealth.[6]

Given our limited sample size and the number of variables that we wanted to balance, we followed Bruhn & McKenzie (2009) and used a re-randomisation method. We selected an allocation of lamps that minimized the statistical difference in means between control and treatment groups out of 10,000 draws (the so-called *MinMax t-stat* method). We preferred this method to stratification, because our sample size would have constrained the number of

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[4]The academic year starts in January. This implies that the students in our sample started in 7th and finished in 8th grade. This contributed to attrition, as some students in our sample did not graduate to 8th grade or changed schools. As we discuss in Section 3, attrition is unrelated to treatment status.

[5]Students in the control group received their lamps in September 2014.

[6]We constructed a wealth index using a principal component analysis based on household characteristics (e.g. type of walls, water, and toilet facilities) and goods owned (e.g. radio, telephone, bicycle, etc.).

variables we could stratify on. In this way, we avoided strong imbalances of several variables without forcing close balance on each. Moreover, we chose re-randomization rather than pairwise matching, because attrition would have posed the risk of losing too many observations, potentially invalidating the experiment. In the econometric specifications, we follow the recommendation of Bruhn & McKenzie (2009) to improve inference by including the balancing variables in the regressions and also by running permutation tests as in Rosenbaum (2002).

Balance between treatment and control groups was well maintained throughout the study (Table 1). Moreover, we show that our sample is balanced on other relevant variables for which we did not explicitly seek balance, such as hours of studying, school attendance, and mothers' education. We also run a multiple-hypothesis test as in List et al. (2016) in order to check that balance was maintained across all these variables simultaneously; this is confirmed to be the case.

We ran two student surveys, 3 and 6 months after treatment. During these follow-ups, we asked specific questions about lamp usage and appropriation. About 84% of treated students reported that the lamp was kept in their household at night. The remaining 16% said that the lamp was kept in school at night. The lamps were durable and broke in only three cases. In all the other cases the lamps were reported to be in good condition. Moreover, in all cases, students declared that the solar charge was sufficient for all or most of the activities they wanted to carry out with the lamp.

A possible threat to the intervention comes from attrition due to missing marks. Marks are our main dependent variable of interest, but these are not always available for all students in our sample, as not all of them sit the end of term exams. Specifically, grades data are missing for 13% of our initial sample in Term 1; this increases to 23% in Term 2, and to 39% in Term 3.[7] To investigate attrition more in details, in Table 2 we run a fully interacted model where, for each term, attrition is regressed on treatment interacted

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[7]Between Term 2 and Term 3 students switched from 7th to 8th grade, but not all of them managed to get promoted, this partly explain the increase in attrition for the final Term.

with a set of key variables such as grades at baseline, age, gender, number of children in the household, parents' education, socio-economic status. We find that these characteristics are not related to attrition. The joint F-statistic for the interaction terms is not significant in term 1 and 2 (p-values are 0.30 and 0.14), whereas it is significant at the 10% level in term 3 (p-value is 0.08). This may raise some concern that attrition in term 3 could affect our results. Therefore, as a robustness, we focus on term one only from our analysis and the results are actually reinforced, so that attrition is likely to bias our estimates downwards.

3   Spillovers and source of variation in the data

In our experimental setting the stable unit treatment assumption turned to be violated due to contamination between treated and control students. For instance, as we discovered in mid-line surveys, 48% of treated pupils declared to share the lamp with other students. Moreover, even if there is not a direct sharing of the lamp, treated and control students interact in class and more than 50% of pupils declared to co-study regularly with other classmates. Therefore, spillovers could arise from either lamp sharing and "knowledge sharing" through co-studying between treated and control students.

In order to account for spillover effects, we exploit the variation in class treatment intensity, that is, the proportion of students in a given class who received a lamp. The variation in treatment intensity between classes was not part of our original experimental design, but, as we argue below, it is still a valid source of variation for identification. Our strategy is very similar to the deworming paper of Miguel & Kremer (2004). In that case too randomization was at the pupil level and, after realising the presence of spillovers, their identification relied on the variation of treatment intensity across clusters retrospectively.

In our case variation in treatment intensity arose during the process of matching the names of students that we surveyed at baseline, with the names of students listed in the official school transcripts. Starting from

the full sample of 341 students surveyed at baseline, a match with transcripts was achieved for only 286 students. We randomised treatment over this sample only, as it was the one with grades,. Given that the match rated differed across classes, this led to variation in treatment intensity ranging between 14% and 62% (Table 3).

Mismatches occurred for reasons such as: misspellings of names in the survey; the use of baptismal names in the survey and traditional names in the transcript; and inverting first names and surnames in the transcripts. Our enumerators did not use the transcripts as a reference for names when interviewing students, but they directly asked students what their name was. For example, a mismatch occurred when we had two "Mwendwa Makenzi" names in the survey, but one of them was a misspelling of "Mwenda Makenzi" as reported in the transcripts. Therefore, we could not distinguish between the two pupils. Similarly, we had a mismatch when "Wambua Kyalo", as registered in the transcript, used his baptismal name, "Jonathan Kyalo", when surveyed. Arguably, mismatches because of name misspellings and of inverting names and surnames were random. One might wonder whether a student introducing herself with a traditional or a baptismal name was random too. We are not aware of studies that shows that self-reporting of baptismal vs. traditional names is associated to some type of bias. This is especially unlikely to affect the results in the context of our study, which was characterized by a homogenous ethnic and social setting. From our fieldwork experience, this type of name reporting depends on how many cases of homonymy there are in a class, and has a random nature. In any case, as long as the self-reporting bias is time-invariant, we are able to control for this concern through student and class fixed effects.

Table 4 shows that matched and unmatched students were not statistically different in key observable characteristics such as hours of study, wealth, mothers' education, source of light etc.[8] Moreover, the matching was unrelated to which interviewer each student had. Table 5 reinforces this point by looking at the balance at the class level. It shows that treatment intensity across classes was balanced over gender, teacher experience, wealth, and

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[8]We run univariate regressions, over the full sample of students, of the baseline values of a set of dependent variables on a dummy on names' matching.

most grades.[9] There was a significant imbalance in grades for science, but the content of the Kenyan Primary School syllabus generates little complementarity between science and mathematics. In fact, science covers topics like plants, human diseases and similar issues.[10] The coefficient on teacher experience is large in magnitude, although statistically insignificant, but the negative sign work against our finding as classes with higher treatment intensity tend to have teacher with less experience.[11] Even if name mismatches and variation in treatment intensity at baseline are uncorrelated with key observable variables, they could still be correlated with unobservable variables that drive our results. To address this issue, in the econometric specification we exploit the panel dimension of the data set absorbing individual and class fixed effects, so that we account for time-invariant unobservable student and class level characteristics that can affect our results, such as differences in grading leniency across classes or in students' ability, quality of administrative records, ethnic composition of students (hence how they report their name).

4   Identification strategy and results

Standard econometric specifications, where we directly compare the performance of students in treatment and control groups, are unable to find a significant intention-to-treat effect of solar lamps.[12] This is not surprising given that in our experimental setting the SUTVA turned to be violated. For this reason, we implement an identification strategy that accounts for spillovers. We rely on the econometric specification of Baird et al. (2014) that allows for the identification of spillovers on the control group, spillovers

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[9]We run univariate regressions of the baseline values of a set of dependent variables on class treatment intensity. Variation in the data is at the class level; hence, given the low number of clusters, both the statistical significance and absolute values of the coefficients matter. The table says that as treatment intensity increases by 10% a class tends to have grades in mathematics that are 0.01 standard deviations lower, with a statistically insignificant value.

[10]As a robustness, in the regressions where the dependent variable is the mark in mathematics, we control for science, as well as for the other subjects. When we do so our results are slightly reinforced in statistical significance and very similar in magnitude.

[11]The coefficient says that moving from a class with 30% to one with 50% of saturation, teachers tend to have about one year less of teaching experience.

[12]We have run OLS estimates on a pooled cross-section, lagged-dependent variable, and first-difference specifications; results available upon request.

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on the treated group, and treatment on the uniquely treated. In its original formulation, this methodology involves a two-step randomization process: treatment intensity is firstly randomized across clusters; then, individual treatment is randomized within clusters. As argued above, our first step is as good as random and the second step was randomized explicitly.

In our setting, we do not have a "pure" control group. Therefore, we follow McIntosh et al. (2014) and we impose a functional form for the effect of treatment intensity on control students. To do so we investigate the relationship between grades of control students and treatment intensity fitting a local polynomial smoother. We find a positive and linear relationship (Figure 4), so that imposing a linear functional form is appropriate for the interval of our data.[13] Therefore, our econometric specification is:

$$y_{ijt} = \beta Treatment_{ij} + \mu(TI_j * \delta_t) + \gamma(TI_j * Treatment_{ij} * \delta_t) + \delta_t + s_{ij} + \epsilon_{ijt} \tag{1}$$

where $Treatment_{ij}$ indicates if a student i in class j is treated; TI_j is the share of treated students in class j; δ_t is a time dummy variable for the post-treatment period and s_{ij} are individual fixed effects.

Estimating regression (1) as a difference in difference model between a specific term date and grades at baseline is equivalent to estimating this simplified version in first difference. This allows us to net out the effects of student- and class-level unobservable (and observable) time-invariant characteristics, such as students' ability, the level of leniency across teachers of different schools, and other variables that on principle could bias the results. This is particularly important given the quasi-experimental nature of the class-level variation of treatment intensity. Hence, we run the following specification:

$$\Delta y_{ijt} = \alpha + \beta \; Treatment_{ij} + \mu \; TI_j + \gamma(TI_j \; * \; Treatment_{ij}) + \epsilon_{ijt} \tag{2}$$

β is the treatment effect on the uniquely treated (TUT) and captures the theoretical intention-to-treat effect at the point of zero saturation; μ is the saturation slope for the control group and captures spillovers on the con-

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[13]As a robustness test we added a squared term on treatment intensity in the main specification presented below, but the quadratic term turned to be insignificant.

trol group; γ is the differential of the saturation slope for the treated group compared to the control, so that $\mu + \gamma$ captures the spillover on treated students. This specification allows to compute the intention-to-treat measure as the sum of the treatment on uniquely treated and of spillovers on treated students such that $ITT(TI) = E(\Delta y_{ijt}|Treat_{ij} = 1, TI_j = TI) - E(\Delta y_{ijt}|Treat_{ij} = 0, TI_j = 0)$.

The results of this regression are presented in Table 6. We have a low number of clusters as in Burde & Linden (2013); hence, we account for this by presenting the p-values of the test statistics using the wild-cluster bootstrap procedure as in Cameron et al. (2008) and by re-calculating the p-values using randomization inference as in Rosenbaum (2002). The results are consistent across all procedures.[14]

The results show a positive and significant effect, such that treated students in a class with average saturation had mathematics score improved by 0.88 standard deviations.[15] The effect increases with the level of saturation and ranges between 0.57 standard deviations at 16% saturation and 1.1 standard deviations at 62% saturation. Moreover, we can see that there are significant, positive spillover effects on the control group. The estimates of μ are positive, significant, and large in magnitude such that a 10% increase in saturation raises the math scores of the control group by 0.22 standard deviations.[16] In Table 7 we replicate the baseline results by focusing on

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[14]Results are also robust also to multiple hypothesis testing for grades in other subjects using a Bonferroni method that corrects for correlation across the tested outcomes, as in Aker et al. (2016) and Sankoh et al. (1997). Given the five subjects that students cover and a correlation among other subjects at baseline of 0.43 that was observed at the start of the study, the adjusted p-value is equal to $padj = 1 - (1 - p(math))^{g(math)}$, where $g(math) = 5^{r(.math)}$ and $r(.math)$ is the average correlation between other subjects excluding math. So, the adjusted p-value for the intention-to-treat (at average saturation) and for the spillover effect are 0.06 and 0.055 respectively.

[15]The average class saturation in our sample is 43%. The effect we measure is given by the linear combination of $\beta + 0.43 \times (\mu + \gamma)$. The results are robust to excluding the clusters with the lowest or highest treatment intensity. When dropping the class with the lowest level of saturation the ITT is 0.66 standard deviations and it remains significant at the 10% level. Results do not change when excluding the class with the highest treatment intensity.

[16]Given the higher marks in science that characterize classes with higher treatment intensity shown in Table, as a robustness we run Equation 2 by controlling for grades in science; the results turns to be slightly more significant and very similar in terms of magnitude. This applies also if we control for subjects other than science. As an additional robustness, we collapse treated and untreated outcomes by cluster and we run Equation 2

data from Term 1 only. This is the term with lowest level of attrition and it confirms the main results from a qualitative and quantitative point of view.

Potentially, externalities may take place, because interaction occurs not just at school, but also at home or around households. Students live in clusters of houses called *bomas*. There are no roads or illumination to connect bomas, so pupils are unlikely to move between them at night, as they may get lost or encounter wild animals. However, on principle, students could interact around *bomas* during daylight or on their way to and from school. Therefore, we check whether spillovers arose from the geographical proximity of treated and control students' houses. We collected the geographical coordinates of houses and we used this information to construct a measure of geographical treatment intensity. For each student, we compute the percentage of treated students within a radii of 500, 1000, and 1500 meters from their homes. We rely on the following specification to identify the overall experimental effect accounting for both within-class and geographical externalities, thereby disentangling the two effects:

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\Delta y_{ijt} = \alpha + \beta \ Treatment_{ij} + \mu \ TI_j + \gamma(TI_j \ * \ Treatment_{ij}) + \atop \sigma \ GTI_{ik} + \phi(GTI_{ik} \ * \ Treatment_{ij}) + \epsilon_{ijt} \tag{3}
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where GTI_{ik} is the geographical treatment intensity around student i within a radius of $k = 0.5, 1, 1.5 \ km$ [17].

Table 8 shows that a positive and significant total effect of treatment is confirmed, such that a treated student in a class and location with average saturation improved their grades by 0.9 standard deviations. The results show that a 10% increase in geographical treatment intensity within 1km leads to an increase in the grades of control students by 0.047 standard deviations, but this result is not robust to randomization inference. Similarly,

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on school-term level observations. The results confirm a positive spillover effect such that a class with a 10% higher treatment intensity improves grades by 0.25 standard deviations (p-value 0.015). The results on treated students in a class with average saturation show an improve in grades by about one standard deviation (p-value 0.018).

[17]We report the results for geographical treatment intensity within 1km only. Measures based on distances of 0.5 or 1.5 kms yielded the same results. Details available upon request.

geographical spillovers on treated students are not robust to randomization inference. Finally, we find that spillovers on control students associated to class treatment intensity remain stable. We interpret these results as indicating that within-class spillovers, rather than geographical spillovers, account for the bulk of spillover effects.

5 Mechanisms of treatment and spillover effects: suggestive evidence from study habits

The analysis of study habits and the distribution of student activities over the day provides some insight into the underlying mechanism through which lamps can affect treated students and generate spillovers on controls. We find evidence consistent with lamps influencing study habits. Specifically, the availability of lamps appears to trigger increased co-studying at school during the early evenings among both treated and control students.

Our dependent variables of interest refer to study habits. Students were asked: i) if they usually study with other pupils; ii) the location of co-studying (at home or at school); and iii) at what time of day they study (before or after sunset). We run the specification in Equation (2) as a single cross-section using these responses as the dependent variable. The set of dependent variables are dummies; so, the regression specifications are a linear probability model and the coefficients should be interpreted in probabilistic terms.[18] Given the lack of baseline values for the dependent variables and the reliance on a single cross-section, we are cautious in giving a causal interpretation of these findings and we think of them as suggestive evidence of the mechanism underlying the effect of lamps on grades.

In Table 9 we report the coefficients of the intention-to-treat and the spillover effects on controls for a set of study habits. The results on co-studying are positive and significant, such that in a class with the average treatment intensity of our sample (43%), the incidence of co-studying in treated students increase by 45 percentage points. Moreover, a 10% increase in treatment intensity raises the incidence of co-studying for control students by 10

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[18]As a robustness check, we also run a probit specification and all results are confirmed.

percentage points. If we decompose co-studying by location and timing, a stronger effect occurs for the variable *studying with others at school after sunset*. This suggests that an important channel through which the lamp affects student performance is by allowing pupils to study together during a period of the day that was previously less feasible due to the lack of light.

These results are consistent with the responses in our surveys. In fact, 48% of treated respondents declared they shared the lamp with other people when studying; 60% of these that they shared the lamps with students of the same class and the remaining shared primarily with siblings. When studying with other students, about 90% of pupils reported doing so at school. Moreover, looking at the record of students' activities over different hours of the day, we find a significant increase in the incidence of studying at school after sunset for both treated and control students, and also a slight reduction in working at home before sunset. This is shown in Figure 5 and Figure 6 that report the total treatment effect on treated students in averagely-saturated classes, and the spillover effects on controls for specific activities over different hours of the day. The coefficients in the figures are estimated using Specification 2 with bands representing standard errors.[19]

There are various reasons that can explain why the lamps allowed students to spend more time at school in the evening. Anecdotal evidence from experience in the field suggests that the most plausible explanations are that, first, some lamps are used in class, allowing the room to be lit in the darker hours;[20] second, the lamps allow students to walk home safely later in the day, when sunset and darkness are approaching; and, finally, for treated pupils, students are no longer required to go home early to do chores because they, or their parents, can undertake them more efficiently during the evenings with the use of the lamps. The policy implication of the first point is that electrifying schools, so that students can spend more time in school and study together after class, can have a significant impact on human capital accumulation.

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[19]We report cluster-adjusted standard errors. For ease of illustration we have not reported the standard errors from randomization inference, but the main results are robust to the use of this approach.

[20]In some cases, teachers were keeping the lamps in the school, and in other cases students occasionally brought the lamps with them to school.

As for the impact of co-studying on exam scores, we are unable to determine whether this was due to better lighting itself, or to the benefits of studying together. However, given that only 48% of treated students stated they shared the lamp with other students, lamp sharing is unlikely to account for all the spillover effects. Sharing of knowledge through interaction between students, as well as a competition effects (where control students increase study effort to remain competitive with treated students), are plausible candidates. Further studies using network data on study partners could help to identify the different sources of spillovers.

An alternative explanation could be related to the income effect that the lamps generate. Das et al. (2013) showed that increasing school inputs may affect household spending responses and, in turn, learning outcomes. The lamp can help generate savings on other lighting fuels, kerosene in particular. Indeed, evidence from student surveys and household expenditure surveys indicates that families with treated students experience a reduction in fuel expenditure of about 60–90 Ksh ($0.66–$1) per week. This is equivalent to around 10–15% of the median weekly income of the households in our sample. Moreover, time-use analysis of parents shows that the lamps allow mothers to do chores more effectively at night, freeing time for other activities, especially paid work, during the day and in the evenings.

To explore the possibility that improved learning outcomes could be attributed to income effects associated with the lamp, we ran a household expenditure survey after one year at the end of our experiment and we did not find significant differences across expenditure categories between treatment and control groups. So the mechanisms highlighted by Das et al. (2013) do not seem to hold in this context. Our findings are not inconsistent with their results, given that they found an effect on household expenditure in the second year, and only if the input was anticipated. Our survey expenditure was conducted after the first year and the input was not anticipated. Additionally, income effects would only explain spillover effects on grades if the income effect itself spills over onto control households. These considerations strengthen our confidence that the income effect on grades is unlikely to explain the observed effects of the lamps on grades.

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6 Conclusions

This study presents a novel experiment that assesses the effect of access to light on education. We document an overall positive effect of solar lamps on education in rural Kenya. Once our identification strategy takes into account the presence of spillovers, we are able to find a positive and significant intention-to-treat effect and a positive and significant spillover effect on the control group.

Our results show that, in the context of our intervention, solar lamps, which are a simple, easily available, and cheap off-grid solution to the lack of electrical lighting, can be an effective complementary input to education. However, solar lamps should not be seen as a substitute for electrification, but as a short-term and practical solution for limiting the drawbacks to human capital accumulation that result from a lack of electricity.

We are also able to disentangle within-class and geographical spillovers. Most of the spillovers arose from within-class interactions, while the geographical proximity of treated and control students did not have a statistically robust effect. The mechanisms through which spillovers arise seem to be related to increased co-studying at school, especially after sunset. This suggests that solar lamps can be particularly effective when there are study groups at school after lectures.

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Figures
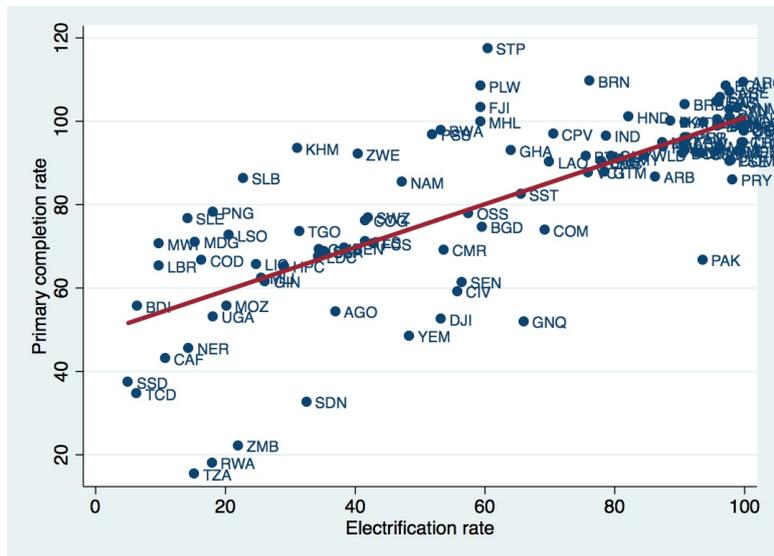
Figure 1: Electricity access and primary schooling completion rate (WDI data, electricity<100%)

Figure 2: Students studying with a lack of electrification (Associated Press and Facebook).

Figure 3: Map of study area
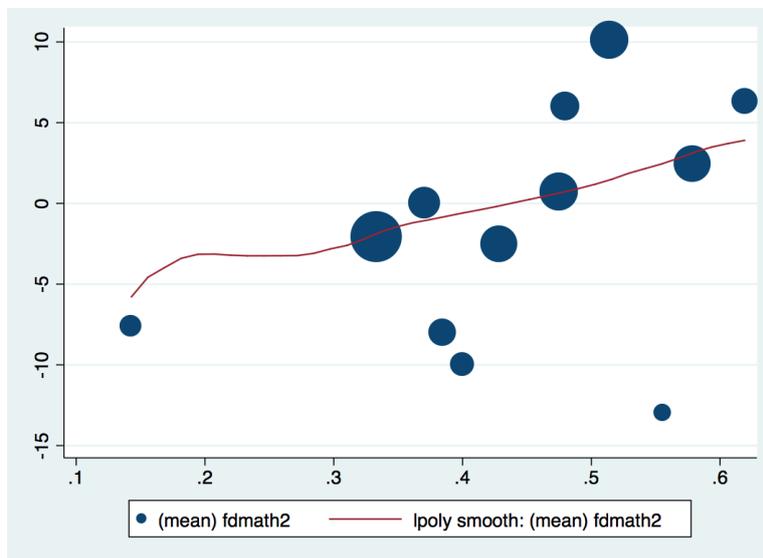
Figure 4: Local polynomial smoother fitted to control groups' grades in math and class treatment intensity
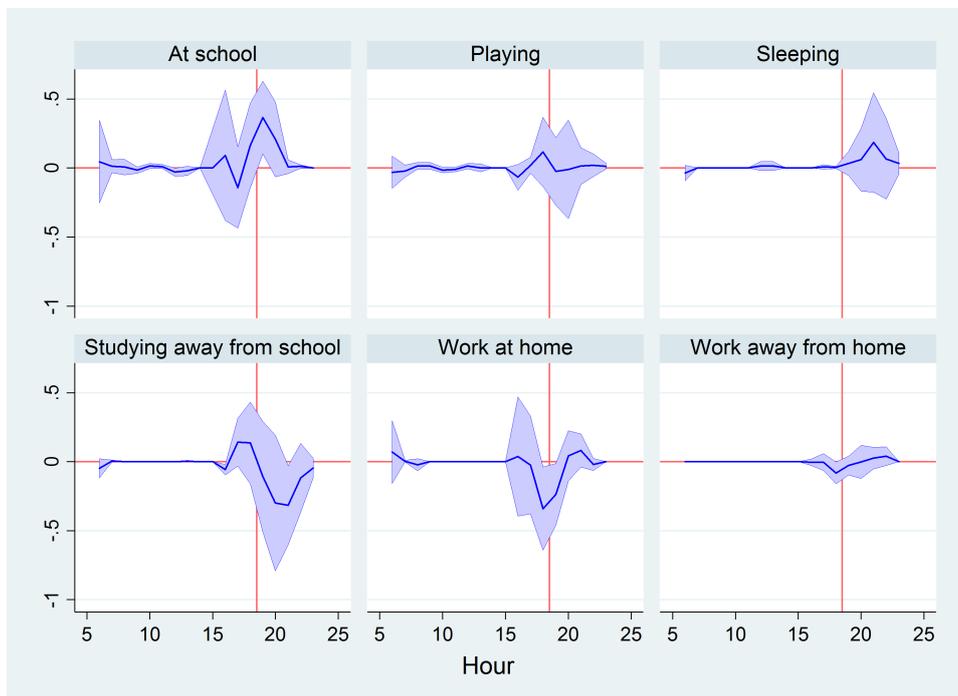
Figure 5: The Figure reports the results, for a set of students' activities in different hours of the day, of the intention-to-treat effect for treated students at average class saturation. The results come from a series of cross-sections that takes the form of $Time\ Use_{ij} = \alpha + \beta\ Treatment_{ij} + \mu\ TI_j + \gamma(TI_j\ *\ Treatment_{ij}) + \epsilon_{ij}$. Shaded areas are confidence intervals from clustered standard errors.
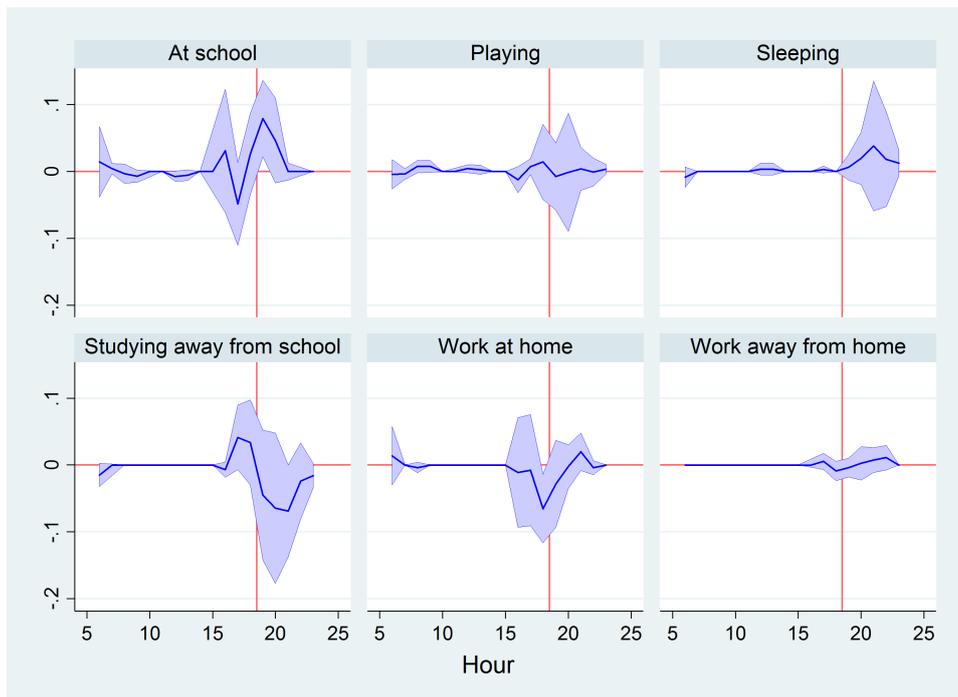
Figure 6: Students' activities by time of the day. Estimates of spillover effects on control students (shaded area = clustered standard errors).

The Figure reports the results, for a set of students' activities in different hours of the day, of spillover effects on control students. The results come from a series of cross-sections that takes the form of $Time\ Use_{ij} = \alpha + \beta\ Treatment_{ij} + \mu\ TI_j + \gamma(TI_j\ *\ Treatment_{ij}) + \epsilon_{ij}$. Shaded areas are confidence intervals from clustered standard errors.

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Tables

Table 1: Balance between treatment and control on baseline variables

Explanatory variable: treatment	Initial randomization		End of experiment	
Balanced variables	Coefficient	*p*-value	Coefficient	*p*-value
Mathematics	0.16	0.20	0.22	0.21
English	-0.05	0.66	-0.07	0.60
Kiswahili	-0.02	0.83	-0.08	0.55
Science	0.04	0.75	-0.08	0.95
Social Studies	-0.12	0.29	-0.15	0.33
Gender	0.00	0.95	0.00	0.99
Wealth index	0.02	0.79	0.02	0.82
School 1	-0.06	0.22	-0.03	0.64
School 2	0.01	0.49	0.03	0.26
School 3	0.04	0.22	0.03	0.58
School 4	0.04	0.17	0.08*	0.08
School 5	0.02	0.70	-0.04	0.37
School 6	-0.03	0.54	0.00	0.97
School 7	-0.03	0.13	-0.04	0.17
School 8	0.02	0.48	0.02	0.61
School 9	0.02	0.60	-0.02	0.49
School 10	0.00	0.99	0.02	0.68
School 11	-0.02	0.40	-0.04	0.29
School 12	-0.04	0.22	-0.02	0.49
Additional variables				
Hours of study	0.11	0.79	0.16	0.76
Missed days of schools (previous month)	0.06	0.78	0.5*	0.07
Source of studying light: wood/candle	0.00	0.97	0.05	0.55
Source of studying light: kerosene	-0.04	0.69	-0.05	0.60
Mother's education	0.04	0.44	0.06	0.38

The table shows balancing tests at the beginning and at the end of our experiment on
a series of key characteristics. ***. **, *, significant at the 1%level, 5% level, and 10%
levels, respectively.

Table 2: Attrition by term

Y: Missing the exam (attrition)	Term 1	Term 2	Term 3
Treatment interacted with:			
Grades at baseline	-0.002	-0.002	-0.002
	(0.14)	(0.12)	(0.23)
Gender	0.02	-0.1	-0.09
	(0.77)	(0.16)	(0.25)
Age	0.02	0.01	0.02*
	(0.15)	(0.35)	(0.09)
Number of children in household	0.02	0.003	-0.03**
	(0.19)	(0.79)	(0.02)
Mothers' education	-0.04	0.05	0.02
	(0.44)	(0.86)	(0.71)
Fathers' education	-0.014	0.01	0.01
	(0.75)	(0.64)	(0.77)
Wealth index	0.01	-0.02	-0.01
	(0.46)	(0.11)	(0.48)
Joint F-stat	1.21	1.63	1.91*

The table shows the results of a fully interacted model where attrition in each term is regressed on treatment interacted with each of the variables listed above. ***significant at the 1%level; ** significant at the 5% level; * significant at the 1% level. Clustered standard errors at the school level. P-values in parentheses.

Table 3: Treatment intensity variation across classes

	Treatment intensity	Class size
Class 1	14.20%	7
Class 2	33.3%	18
Class 3	33.3%	36
Class 4	37.0%	27
Class 5	38.4%	13
Class 6	40.0%	15
Class 7	42.8%	28
Class 8	47.5%	40
Class 9	48.0%	25
Class 10	51.4%	35
Class 11	55.5%	9
Class 12	57.8%	38
Class 13	61.9%	21

The Table shows the treatment intensity and the number of students of the classes in our sample.

Table 4: Balance between matched and unmatched students' baseline characteristics

Explanatory variable: unmatched student	Coefficient	p-value
Hours of study	-0.35	0.48
Missed school days	-0.16	0.55
Number of people in household	0.31	0.63
Wealth index	0.06	0.12
Source of study light: kerosene	0.02	0.79
Mother education	0.05	0.41
Interviewer 1	0.03	0.56
Interviewer 2	-0.05	0.14
Interviewer 3	0.01	0.69
Interviewer 4	0.06	0.20
Interviewer 5	-0.03	0.57
Interviewer 6	-0.03	0.37
Interviewer 7	0.02	0.70
Interviewer 8	0.03	0.51
Interviewer 9	-0.01	0.72
Interviewer 10	-0.03	0.37

The Table shows the results of a cross-section regression at the pupil level of each of the individual characteristic on dummy equal to one if the student name has been matched between the baseline survey and transcripts, and zero otherwise. ***significant at the 1%level; * significant at the 10% level.

Table 5: Balance of treatment intensity across classes, average values per class at baseline

Explanatory variable: treatment intensity	Coefficient	p-value
Mathematics	-0.13	0.94
English	1.04	0.51
Swahili	-0.30	0.75
Science	3.59***	0.00
Social studies	-1.13	0.25
Gender	0.23	0.54
Wealth index	-0.97	0.17
Teacher experience	-6.5	0.54
Teacher education	-0.17	0.57
Hours of study	-0.79	0.72
Number of children in household	1.04	0.40
Source of study light: kerosene	0.33	0.37
Mothers' education	-1.69	0.32
Observations	13	

The Table shows the results of a cross-section regression at the class level of each of the class average characteristic listed above on treatment intensity. ***significant at the 1%level; * significant at the 10% level.

Table 6: Spillover estimates with class saturation, pooled sample

Y: Grades in Mathematics

β, treatment on uniquely treated	0.37
	(0.25)
	[0.35]
	{0.21}
μ, saturation slope on control	2.21
	(0.026)**
	[0.09]*
	{0.06}*
γ, differential saturation slope on treatment	-1.03
	(0.18)
	[0.26]
	{0.82}
Intention-to-treat:	
- Min saturation (16.6%)	0.57
	(0.06)*
	[0.06]*
	{0.08}*
- Average saturation (43%)	0.88
	(0.028)**
	[0.06]*
	{0.10}*
- Max saturation (62%)	1.1
	(0.035)**
	[0.047]**
	{0.09}*
Spillover effects:	
Marginal effect of 10% higher treatment intensity on *control* students	0.22
	(0.026)**
	[0.09]*
	{0.07}*
Marginal effect of 10% higher treatment intensity on *treated* students	0.11
	(0.18)
	[0.21]
	{0.22}
Observations	641

The Table reports the results of specification 2 for the pooled sample of students across terms. ***significant at the 1%level; ** significant at the 5% level; * significant at the 1% level. *P*-values from clustered adjusted standard errors at the school level in parentheses (), *p*-values from wild-bootstraps in brackets[], *p*-values from randomization inference in braces {}. The dependent variable is the standardized grade in mathematics.

Table 7: Spillover estimates with class saturation, Term 1

Y: Grades in Mathematics

β, treatment on uniquely treated	-0.39
	(0.30)
	[0.42]
	{0.68}
μ, saturation slope on control	2.32
	(0.00)***
	[0.02]**
	{0.04}**
γ, differential saturation slope on treatment	0.71
	(0.39)
	[0.24]
	{0.20}

Intention-to-treat:

- Min saturation (16.6%)	0.10
	(0.70)
	[0.50]
	{0.34}
- Average saturation (43%)	0.90
	(0.016)**
	[0.052]*
	{0.06}*
- Max saturation (62%)	1.4
	(0.00)***
	[0.032]**
	{0.04}**

Spillover effects:

Marginal effect of 10% higher treatment intensity on *control* students	0.23
	(0.00)***
	[0.02]**
	{0.04}**
Marginal effect of 10% higher treatment intensity on *treated* students	0.30
	(0.016)**
	[0.014]**
	{0.01}***

Observations	33	247

The Table reports the results of specification 2 for the sample of students from term 1 only. ***significant at the 1%level; ** significant at the 5% level; * significant at the 1% level. *P*-values from clustered adjusted standard errors at the school level in parentheses (), *p*-values from wild-bootstraps in brackets[], *p*-values from randomization inference in braces {}. The dependent variable is the standardized grade in mathematics.

Table 8: Spillovers with class and geographical saturation, pooled sample

Y: Grades in mathematics

β, treatment on uniquely treated	0.52
	(0.23)
	[0.30]
	{0.15}
μ, class saturation slope on control	1.86
	(0.067)*
	[0.046]**
	{0.09}*
γ, differential class saturation slope on treatment	-1.33
	(0.27)
	[0.33]
	{0.87}
σ, geo saturation slope on control	0.47
	(0.25)
	[0.28]
	{0.26}
ϕ, geo saturation slope on treatment	-0.05
	(0.9)
	[0.9]
	{0.57}

Intention-to-treat:	
Average class saturation (43%)	0.9
& Average geo saturation (37%)	(0.00)***
	[0.00]***
	{0.087}*
Spillover effects:	
Marginal effect of 10% higher	0.18
class treatment intensity on *control* students	(0.067)*
	[0.046]**
	{0.087}*
Marginal effect of 10% higher	0.04
geo treatment intensity on *control* students	(0.25)
	[0.28]
	{0.087}*
Marginal effect of 10% higher	0.05
class treatment intensity on *treated* students	(0.63)
	[0.69]
	{0.087}*
Marginal effect of 10% higher	0.04
geo treatment intensity on *treated* students	(0.35)
	[0.37]
Observations	521

The Table reports the results of specification 3 for the pooled sample of students across terms. ***significant at the 1%level; ** significant at the 5% level; * significant at the 1% level. *P*-Values from clustered adjusted standard errors at the school level in parentheses (), *p*-values from wild-bootstraps in brackets[], *p*-values from randomisation inference in braces {}. The dependent variable is the standardized grade in mathematics.

Table 9: Spillover estimates, effects on study habits

	Intention to treat at average saturation (43%)	Spillovers on control, marginal effect of a 10% increase in saturation
Dependent variable:		
Study with others	0.45	0.10
	(0.01)**	(0.01)***
	[0.02]**	[0.02]**
Study with others at school after sunset	0.31	0.05
	(0.07)*	(0.19)
	[0.04]**	[0.11]
Study with others at school before sunset	-0.00	0.03
	(1.00)	(0.58)
	[0.52]	[0.28]
Study with others at home after sunset	0.16	0.03
	(0.11)	(0.19)
	[0.13]	[0.19]
Study with others at home before sunset	0.01	0.00
	(0.35)	(.)
	[0.38]	[0.45]
Observations:	254	254

The Table reports the results, for a set of dependent variables on study habits, of i) intention to treat in class with average saturation and ii) spillover effects. The specification is a cross-section that takes the form of $Study\ Habit_{ij} = \alpha + \beta\ Treatment_{ij} + \mu\ TI_j + \gamma(TI_j\ *\ Treatment_{ij}) + \epsilon_{ij}$. P-values from clustered adjusted standard errors at the school level in parentheses () and p-values from randomization inference in brackets[]. * $p<0.1$, ** $p<0.01$, ***$p<0.001$.