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Research Project

Powering education

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Fadi HASSAN, Paolo LUCCHINO

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Fadi HASSAN[1], Paolo LUCCHINO[2]

[1] Trinity College Dublin
[2] LSE and NIESR

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Research project

Powering education

Powering Education was rolled out between September 2013 and September 2014. It consisted of a 12-month study exploring the linkage between access to clean energy sources and education, and was based on a Randomized Control Trial (RCT) model designed and managed by the project's Research Team.

Powering Education entailed the distribution of approximately 300 solar lamps to 300 students in 12 schools located in rural and off-grid communities in Kenya, and was implemented by GIVEWATTS together with our on-the-ground team.

The lamps were delivered in two batches of approximately 150 units each and with a certain time lag between them. The first batch of lamps (143) was distributed to Treatment Group students, while the remaining students, who constituted the Control Group, were scheduled to receive their lamps at the end of the pilot. The distribution of the lamps set the context for a structured assessment of their impact on the students' performance. The Research Team carried out a total of four surveys throughout the project life cycle, and was able to closely monitor any improvements in students' performance as well as any broader effects of the replacement of kerosene lanterns with solar lamps. The main variables to determine the distribution of lamps were: previous student performance, village of residence, gender, a proxy for family wealth and living conditions.

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Abstract

We run a randomized control trial in order to assess the impact of energy access on education. We distribute solar lamps to 7th grade pupils in rural Kenya and monitor their educational performance throughout the year. We do not find a statistically significant average treatment effect. Given evidence from the field and randomization at the pupil level, we claim that this is due to spillover effects. Once our identification strategy accounts for potential spillovers, we are able to find a positive and significant intention-to-treat effect as well as a positive and significant spillover effect on control students in mathematics. Moreover, we find that increasing treatment intensity by 10% raises the average grade of a class by up to 5 points. Finally, we find a statistically significant effect on savings and short time effects on study time, employment for fathers, and work at home for mothers.

Keywords: Randomized control trial, education, energy access, spillover effects.

JEL Codes: O12, I2, C93.

Corresponding Authors

Fadi Hassan
Trinity College Dublin
College Green
Dublin (Ireland)
Email: fhassan@tcd.ie

Paolo Lucchino
London School of Economics
Houghton Street
London (UK)
Email: p.lucchino@lse.ac.uk

Introduction

More than 1.3 billion people worldwide lack access to electricity and 40% of them live in Sub-Saharan Africa (IEA, 2013). This means that roughly a quarter of humanity lives without lights at home in the evening, without power at the workplace during the day, and without the possibility of reading and studying after dark. Basically, energy poverty implies that most people are strongly constrained in their standards of living.

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; however, this figure drops to under 2% in rural areas. Despite being home to 13% of the world population, Sub-Saharan Africa accounts for just 4% of global energy demand (IEA, 2014).

The electrical power grid is expanding slowly and unevenly. Governments and private actors are working to reach deeper into remote areas, but financial, political and logistical barriers have proven to be significant obstacles to overcome. Households rely heavily on kerosene spending that makes up 20-25% of their income even if the cost of equivalent useful lighting can be 150-times higher than that provided by incandescent bulbs and 600-times higher than that from compact fluorescent lights (IEA, 2014). The WHO (2014) documents that each year 4.3 million premature deaths, of which nearly 600.000 are in Africa, can be attributed to household air pollution resulting from the traditional use of solid fuels, such as fuelwood and charcoal.

Solar energy might represent a viable and effective solution to at least satisfy the minimal needs of energy access for non-polluting lighting. Solar lamps can provide light at night to households affecting their welfare on various dimensions like health, study, socializing, and relaxing time constraints.

In this project we focus on a specific angle and run a randomized control trial (RCT) in Kenya in order to assess the impact of energy access on education performance. This is a novel experiment that hopefully will increase our awareness and knowledge on this key development issue. We distribute solar lamps to pupils in off-grid areas and measure the change of grades between the treatment and the control group. We also monitor the effect that the lamp has on expenditure on alternative lighting sources and the time use of household members.

We are unable to find significant average treatment effects. On the basis of qualitative evidence we have collected and given our randomization at the pupil level, we argue that this is due to the presence of spillover effects arising from students sharing lamps for prep classes at school. Hence, we exploit variation in treatment intensity and use an identification strategy that accounts for potential spillovers. Once we account for that, we are able to find a positive and significant intention-to-treat effect such that treated students, in a class where 50% of the pupils get a lamp, experience an increase in grades of 15-20 points[1].

Moreover, the lamp affects the control students too, such that rising treatment intensity by 10% increases their grades by up to 4 points. Finally, increasing treatment intensity by 10% the average grade of a class rises by up to 5 points.

We also find that in the short run treated students experience a 6% increase in total study time, and a 17% increase in study time at home. Moreover, we document that families within treated students experience a reduction of fuel's expenditure equivalent to around 10-15% of the median weekly income. Finally, we also find evidence of a short-term effect on fathers increase their time at work during the day, reducing socializing, and mothers who decrease the working time at home during the day increasing marginally their work for pay.

Given the small size of the technological shock that our experiment provides through a solar lamp, these results are quite encouraging about the effect that more complex solar technology can have on education and on households more broadly. Moreover, to the extent that there were experimental issues related to cases of lamp appropriation by teachers and coercive sharing at the school level, these estimates are likely to be a lower bound of the true effect.

This paper is related to the literature on energy access and development as Dinkelman (2011), Rud (2012), Lipscomb (2013). These papers concern with the effects of energy access on employment, industrialization, and human development index and housing value. Our study complements this field by having a focus on education. Moreover, the type of energy access that we look at is different. These studies examine the impact of electrification, which is a big region-wide technology shock. We evaluate the effect of solar lamp provision, which is a smaller and an individual technology shock that relates to a more easily available and cheaper source of energy access. Finally, from a methodological point of view these studies exploit natural experiments while we implement an RCT, which allows for a more direct measure.

The paper speaks also to the literature of randomized control trials on education in developing and emerging countries like Glewwe (2004), Angrist (2009), Gertler (2012), and Burde (2013). Our contribution to this literature is to examine the relevance of an additional input into the education production function: lighting, a scarce resource in these contexts.

This project contributes not only to the academic debate, but is key also to policy. Electrifying rural areas in Sub-Saharan Africa is a long and costly process. Before that, this will happen many generations of students might be harmed by the lack of energy, undermining human capital accumulation in these countries. Providing a cheap and renewable source of energy, like a solar lamp, could be an effective short-term solution to the lack of electrification. Our project will shed light on this policy dimension.

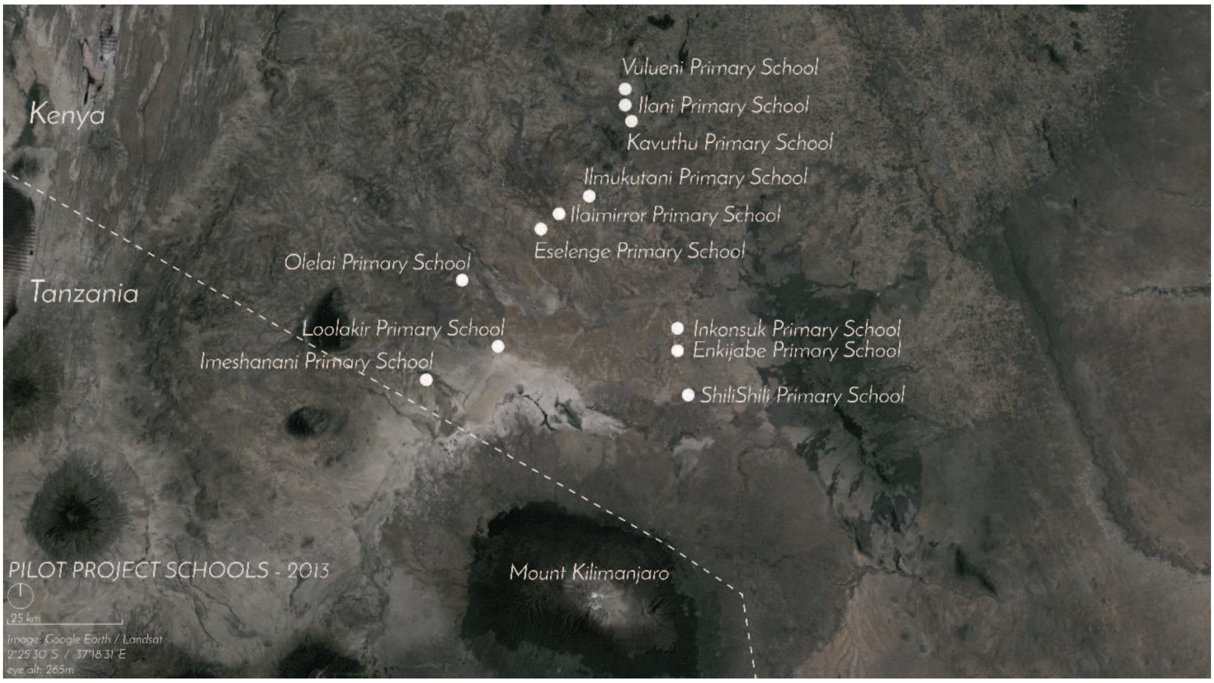
[1] These results hold only for mathematics, which is the subject prep classes mostly focus on.

The report is structured as follow: chapter 2 describes the experiment structure; chapter 3 analyzes the average treatment effect on education; chapter 4 assesses the presence of spillover effects and the effect of treatment intensity on the control group and on the average class: chapter 5 extend the analysis on the effects on households; and chapter 6 concludes.

1 Project structure and randomization

We run the RCT involving 13 classes of 7th grade students across 12 primary schools in the Loitokitok and Nzaui districts, relatively close to the Tanzanian border and Mount Kilimanjaro, as in Figure 1. We focus on schools in off-grid rural areas where household electrification is below 2.6% (KGS).

FIGURE 1 – Area of intervention



Source: own elaboration based on Google Earth / Landsat image

The project started with a baseline survey in June 2013. This step also included collecting end of term grades from school transcripts. The batch of lamps was distributed to the treatment group in September 2013 at the beginning of a new school term[2]. We then collected end of term grades for the treatment and control groups in November 2013, March 2014, and June 2014. We also run extensive surveys to students in November and March.

The baseline survey covered 341 pupils. Transcript data was collected for 286 of these by matching on student name. This constitutes our core sample, over which we conducted the randomization. We distributed a solar lamp to 143 pupils, which constitutes our treatment

[2] New academic years start in January. This implies that our sample started in 7th grade and finished in 8th grade. This contributed to attrition given that some students in our sample did not graduate to 8th grade or changed schools.

group; the remaining students, who are in the control group, were promised to receive the lamp after one year at the end of the experiment[3].

We randomize assignment to treatment at the pupil level so that within each class some students are in the treatment and some in the control group. We choose this level of randomization rather than the one across schools to maximize statistical power, given the size of our sample.

In our randomization strategy we seek balance between treatment and control groups on grades, which is our variable of interest, gender, classes, and a proxy for wealth[4]. The students in our sample live in clusters of few houses, called bomas, in rural areas without electrification; it is extremely hard, if not impossible, for students to move across bomas during the night because of wild animals and the lack of roads and illumination.

Given our sample size and the number of variables that we want to balance, we follow Bruhn and McKenzie (2009) and use a randomization method where we pick the allocation of lamps that produces the minimum statistical difference in means between control and treatment out of 10,000 draws, given a maximum t-stat of 1.5 (the so called MinMax t-stat method). Table 1 reports the values of regressing the balancing variables on treatment at the baseline survey and at the end of our project. The balance between treatment and control was well maintained throughout our study reflecting attrition at random across terms.

[3] Students in the control group received the lamp in September 2014.

[4] We construct a wealth index through a principal component analysis based on house's characteristics (e.g. type of walls, water, and toilet facilities) and a set of goods owned (e.g. radio, telephone, bicycle, etc.)

TABLE 1 – Balance between treatment and control groups

	Included in initial randomisation		**Included in initial randomisation and observed at end of project**	
	Coefficient	**p-value**	**Coefficient**	**p-value**
Mathematics	2,26	0.2	3.1	0.19
English	-0.59	0.45	-0.91	0.60
Kiswahili	-0.29	0.81	-0.91	0.55
Science	0.51	0.78	-0.2	0.93
Social Studies	-1.44	0.29	-1.75	0.33
Gender	0	1	0	0.99
Wealth index	0.02	0.77	0.02	0.82
School 1	-0.06	0.23	-0.03	0.64
School 2	0.01	0.47	0.03	0.26
School 3	0.04	0.28	0.03	0.58
School 4	0.05*	0.09	0.08*	0.08
School 5	0.02	0.7	-0.04	0.37
School 6	-0.03	0.42	0	0.97
School 7	-0.03	0.12	-0.04	0.17
School 8	0.02	0.49	0.02	0.61
School 9	0	0.99	-0.02	0.49
School 10	0.02	0.57	0.02	0.68
School 11	-0.02	0.4	-0.04	0.29
School 12	-0.04	0.22	-0.02	0.49

* Significant at the 10% level.

Source: own elaboration

2 Average treatment effect

In this section we run a series of reduced form regressions to identify the impact of treatment on educational outcomes. Given randomization, the coefficients of the regressions can have a causal interpretation.

We start our analysis by running a single difference equation with an OLS estimation for each round of grades that followed our treatment. Our basic specification is the following:

$$(1) \qquad y_{ij} = \beta_0 + \beta_1 Treatment_{ij} + Z_{ij}\gamma + \lambda_j + \varepsilon_{ij}$$

where y_{ij} is the grade of student i in class j, λ_j capture class fixed effects; and Z_{ij} is a vector of controls that includes student's age, mother's education, and number of siblings. We estimate equation (1) at the end of each term so we can monitor the effect of treatment in the short, medium, and longer run[5].

We then extend our analysis to a lagged dependent variable specification. This allows controlling for past grades that, given the cumulative process of education and learning, might influence current grades. We use grades at baseline as the lagged dependent variable of reference. Therefore, we estimate the following regression:

$$(2) \qquad y_{ijt} = \beta_0 + \beta_1 y_{ij0} + \beta_2 Treatment_{ij} + Z_{ij}\gamma + \lambda_j + \varepsilon_{ijt}$$

We run this regression also with the controls specified for the cross section estimate.

Finally, we run a first difference estimation that allows us to control for individual fixed effects. Despite the randomization, this is a useful specification as a robustness check given also missing data due to exam absenteeism over different terms. The first difference is taken with respect to grades at baseline, so all the time invariant variables between those two periods drop[6]:

$$(3) \qquad \Delta y_{ijt} = \beta_0 + \beta_1 Treatment_{ij} + \varepsilon_{ijt}$$

Table 2 summarizes the main finding of these specifications. We are unable to detect any treatment effect in the short, medium, and longer run independently from the specification used and the controls that are added[7].

[5] We present the effect on the average grade across all subjects. We have estimated the effects of each subject separately and results do not change significantly.

[6] The controls used in the other specifications are all time-invariant, so they are not included in this case.

[7] We have also run these specifications on each subject separately, but results do not change.

TABLE 2 – Average treatment effect

	Cross section						**Lagged dependent variable**						**First difference**		
	Term 1		Term 2		Term 3		Term 1		Term 2		Term 3		Term 1	Term 2	Term 3
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)			
Treatment	-0.52 (0.76)	-0.17 (0.75)	1.13 (1.06)	1.51 (0.89)	-0.43 (0.98)	0.55 (1.1)	-0.56 (0.46)	-0.34 (0.56)	1.23 (1.44)	1.52 (1.24)	-0.06 (1.01)	0.66 (1.1)	0.09 (0.61)	0.72 (1.71)	-0.06 (1.19)
Age		-0.42 (0.62)		0.2 (0.73)		0.06 (0.54)		-0.39*** (0.1)		0.08 (0.69)		-0.31 (0.3)			
Mother's education		-0.03 (1.46)		0.84 (1.97)		2.22 (1.45)		0.51 (1.13)		-0.24 (1.51)		1.06* (0.59)			
Number of siblings		0.18 (0.15)		-0.48 (0.52)		0.19 (0.22)		0.11 (0.09)		-0.52 (0.55)		0.30* (0.14)			
Grades at baseline							0.91*** (0.05)	0.92*** (0.05)	0.84*** (0.1)	0.83*** (0.11)	0.75*** (0.03)	0.73*** (0.04)			
Observations	248	224	220	196	174	157	248	224	220	197	174	157	248	220	174

***significant at the 1%level;
** significant at the 5% level;
* significant at the 10% level.

Clustered standard errors at the school level in parenthesis. The dependent variable is average grades across subjects. All specifications account for class fixed effects.

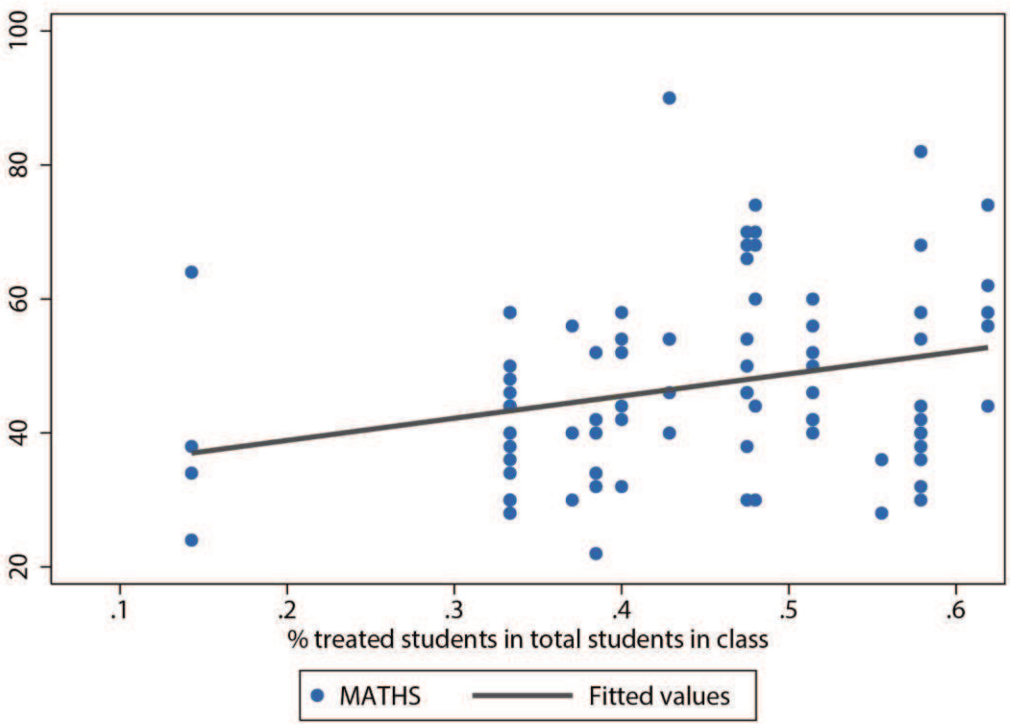
Source: own elaboration

3 Spillover effects

The lack of average treatment effect of the lamps may be due to the presence of spillover effects that we need to account for. There can be a direct spillover effects arising from students sharing lamps, as well as indirect effects coming from improved learning of treated students that then share their knowledge with control students. We are unable to disentangle these two sources of spillovers. However, given the partially non-rival nature of the good associated with treatment and the randomizations structure at pupil level, we expect spillover effects to be present. In particular, during our field visit we became aware that lamps have been shared for prep-classes and that students study in groups at school.

Figure 2 provides suggestive evidence that we should be concerned with spillovers. The graph shows that the average grades of students in the control group increases significantly with the percentage of treated students in their class. This means that on average a student with no lamp performs significantly better if she is in a class where more students have a lamp. This pattern is consistent with the presence of spillover effects.

FIGURE 2 – Control groups' grades and class treatment intensity



Source: own elaboration

The consequence of spillover effects is that the lamp affects both treated and control students. This can explain why we do not find evidence of treatment effects by directly comparing the performance of students in the two groups. However, this does not imply that lamps have no effect on grades at all; it means that our estimation strategy should account for the presence of spillovers.

Given the potential presence of spillovers, we now focus our analysis on the effects of class treatment intensity - the percentage of students in a class that was given a lamp. In order to do so, we exploit the fact we have quasi-experimental variation in the treatment intensity between classes. This arose during the process of matching survey responses and school transcripts at baselines. Starting from the full sample of 341 students, a match with transcripts was achieved only with 286 students. However, the match rate varied significantly across classes leading to a variation of treatment intensity across classes ranging between 14% and 62% Table 3. We argue that the variation in the match rate is random. As we discovered at a later stage, this was due to casual issues like major misspellings of names in the survey; the use of Baptismal names in the survey and traditional names in the transcript or vice versa; and inverting name and surname in the transcripts.

TABLE 3 - Treatment intensity variation

	Treatment intensity	Class size
Class 1	14.2%	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

Source: own elaboration

In the next section we firstly assess the presence of spillovers following the methodology of Baird et al (2014). Having found evidence of spillovers, we then focus our analysis on the effects of treatment intensity using a two-step grouped estimate as described in Donald and Lang (2007).

3.1 A quasi-randomized saturation design

In this section we measure the effect of spillover effects exploiting the fact that our project mimics a randomized saturation design as described in Baird et al. (2014), where saturation is defined as treatment intensity (the percentage of students treated within a class). This methodology allows to identify different components of the experimental effect of treatment:

spillovers on the control group, spillovers on the treated group, and treatment on the uniquely treated.

As argued above we have reasons to believe that treatment intensity variation is as good as random. This is supported by Table 4, which shows a good balance of treatment intensity respect to gender, wealth, and most grades[8]. There is imbalance for the grades in science and social studies, but this should not be of concern given that we will focus our analysis on grades in mathematics. This is because prep-classes were mainly in mathematics, so this is the subject where we expect to observe spillover effects. In fact, when we run the analysis on average grades, even if significant, it turns that most of the action comes from math while, unsurprisingly, the effect on other subjects is insignificant.

TABLE 4 – Balance of treatment intensity

Explanatory variable: treatment intensity	Coefficient	P-Value
Mathematics	19.01	0.41
English	13.04	0.64
Swahili	1.67	0.91
Science	55.61***	0.00
Social studies	-27.7*	0.07
Gender	0.06	0.88
Wealth index	-0.11	0.87

*** Significant at the 1%level;
* Significant at the 10% level.

Source: own elaboration

Contrary to the original design of Baird et al. (2014), we do not have a pure control group. Therefore, we follow an identification strategy that addresses this limitation as in McIntosh et al. (2014). Hence, our econometric model is:

$$(4) \qquad y_{ijt} = \beta Treatment_{ij} + \mu(TI_j * \delta_t) + \gamma(TI_j * Treatment_{ij} * \delta_t) + \delta_t + s_{ij} + \varepsilon_{ijt}$$

where TI_jcaptures treatment intensity in class j; δ_t is a time dummy for post-treatment period and s_{ij} are individual fixed effects.

Estimating this regression 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:

[8] A student in a class whose treatment intensity is 10% higher than another class tends to have a statistically insignificant 1.9 extra points in mathematics.

$$(5) \qquad \Delta y_{ijt} = \delta + \beta Treatment_{ij} + \mu TI_j + \gamma(TI_j * Treatment_{ij}) + \varepsilon_{ijt}$$

where μ is the saturation slope in the control group and captures spillovers in the control group; γ is the differential of the saturation slope for treated and measures the effect of changing saturation in the treated compared to control; and β is the treatment effect on the uniquely treated and captures the theoretical intention to treat effect at the point of zero saturation. The sum of the treatment on uniquely treated and of spillovers on treated constitutes the overall intention-to-treat.

The results of this regression are presented in Table 5. We can see that there is a positive and significant spillover effect on the control group. The estimates of μ are positive, significant, and large in magnitude such that a 10% increase in saturation raises math grades of the control group by 3-4.4 points. Interestingly, this spillover effect increases over time.

TABLE 5 – Spillover effects estimates

Y: Grades in Mathematics	Term 1	Term 2	Term 3
Treatment β (treatment effect at 0 saturation)	-3.6 (5.57)	19.98*** (4.87)	14.19 (11.74)
Treatment Intensity μ (saturation slope in control)	31.45** (10.19)	37.82* (18.67)	44.05* (22.12)
Treatment * Treat. Intensity γ (differential saturation slope in treatment)	5.39 (12.4)	-46.97*** (11.67)	-32.2 (26.59)
Intention to treat at saturation=0.5	14.8** (5.38)	15.4* (8.17)	20.1* (9.88)
Observations	247	220	174

*** Significant at the 1%level;
** Significant at the 5% level;
* Significant at the 10% level.

Clustered standard errors at the school level in parenthesis.

Source: own elaboration

The marginal effect of treatment intensity on the treated, which captures the spillovers on the treated $(\mu + \gamma)$ is positive and significant in term 1, but not statistically different from zero in term 2 and 3.

We find a positive and significant effect of treatment on the uniquely treated β in the medium run (Term 2). It implies that treating a student in a theoretical context of zero saturation increases his grade by 20 points. However, we do not find an effect for term 1 and 3, so further assessment is needed before a conclusive interpretation can be drawn.

Table 5 shows a positive and significant intention-to-treat effect at the saturation level of 50%. Importantly, this effect increases over time. The size of the coefficient is such that on average treated students improve their grades in mathematics by 14-20 points if they are in class where half class receives a lamp. Given the lack of a pure control group, these intention-to-treat effects are compared to an estimated counterfactual outcome, which hinges on the linear specification of our model. Given the significant magnitude of these coefficients, further analysis accounting for non linearities is needed.

3.2 Treatment intensity and average grades

In this section we focus on the impact of class treatment intensity on grades. In this case we have a dependent variable that differs across individuals (grades) and an explanatory variable of interest (treatment intensity) that is constant among all members of a class. As Moulton (1990) shows, in this type of regression models we should account for the presence of common group errors. This is particularly challenging in our setting as we have only 13 clusters. In this case, 'cluster robust' standard errors based on standard asymptotic (Liang and Zeger, 1986) are known to deliver biased estimates of the standard errors. We follow Donald and Lang (2007) who suggest a two-step estimator to deal with error components model with a small number of groups[9].

The econometric model we want to estimate is:

$$y_{ij} = \beta_0 + \beta_1 TI_j + \beta_2 Teacher_j + Z_{ij}\gamma + \alpha_j + \varepsilon_{ij} \tag{6}$$

Where $Teacher_j$ is teaching experience in class j measured by the years of teaching; Z_{ij} is a vector of individual characteristics including age, mother's education, and number of siblings; α_j is the class specific error component[10].

[9] The conditions for their procedure being valid for inference are that, given a small variance of the error component model, (i) the individual characteristics of a class converge in probability to a common average across classes, and (ii) the probability limit of class members is equal across classes. We will discuss more in details the validity of these conditions in further versions of the paper.

[10] Given that we estimate this regression at three points in time separately, we do not include the subscripts t to save on notation.

We firstly estimate the following first-stage regression by OLS:

$$(7) \qquad y_{ij} = C_j + Z_{ij}\gamma + \varepsilon_{ij}$$

where C_j is a set of class dummies that capture a covariate-adjusted class mean.

We then use the coefficients $\hat{C}_j$ estimated in (7) in the second stage[11]:

$$(8) \qquad \hat{C}_j = \beta_0 + \beta_1 TI_j + \beta_2 Teacher_j + \alpha_j$$

Table 6 presents the results of these estimations. The coefficients of class dummies for the second stage are both unweighted and weighted by class size. Given the variation of class size in the sample, our preferred specification is the one with weighted coefficients[12].

We can see that treatment intensity has a positive short-run effect with and without controlling for teaching experience. In fact an increase of treatment intensity by 10% raises the average grade of the class by 4-5 points.

These estimates are consistent with the positive intention to treat and spillover effects on the control group shown in Table 5. A class effect of treatment intensity is present also in the longer run once controlling for teaching experience and the size of the impact is not different from the short run effect. However, do not find a significant effect in term 2; hence, further analysis about longer run effects should be undertaken.

[11] We run this by weighting the coefficients by class size, by the inverse of their estimated variance, and also with unweight coefficients.

[12] We have also used the inverse of the group's variance as weights; the size of the coefficients is similar to the one with class size weights, but the variance increases slightly turning the coefficients marginally insignificant. It is not clear though whether class size or the inverse of the variance provide the appropriate weights in this context.

TABLE 6 – Treatment intensity: second stage of two-steps procedure (grouped estimates)

Y: $\hat{C}_j$ (estimated average grades)	**Weighted**						**Unweighted**					
	Term 1		Term 2		Term 3		Term 1		Term 2		Term 3	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Treatment Intensity	54.48* (24.95)	42.49* (20.61)	28.88 (22.74)	27.74 (27.11)	36.24 (21.91)	41.89* (21.47)	19.89 (28.32)	30.21 (26.19)	14.55 (22.91)	20.79 (24.16)	24.33 (24.17)	39.15 (26.02)
Teacher experience		3.28** (1.08)		1.71 (1.4)		2.31* (1.13)		3.27** (1.3)		1.48 (1.24)		2.09 (1.29)
Observations 1st stage	257	257	181	181	180	180	257	257	181	181	180	180
Groups 2nd stage	13	11	13	11	13	11	13	11	13	11	13	11

** significant at the 5% level;
* significant at the 10% level.

The dependent variable is the set of class dummies estimated in the first stage by equation (5). which capture average grades in mathematics.

Source: own elaboration

TABLE 7 – effects on study time – cross sectional estimates

	Hours of study (1)	**Hours of study (2)**	**Hours of study (1)**	**Hours of study (2)**	**Hours of study (3)**
	Term 1		Term 2		
Treatment	0.25* (0.12)	0.19 (0.14)	0.11 (0.12)	-0.06 (0.09)	0.13 (0.09)
Observations	237	242	250	252	251

*significant at the 10% level.

Clustered standard errors at the school level in parenthesis. All specifications are run with class dummies. The estimates are cross sectional OLS.
Hours of study (1) is the answer to the question: "How many hours do you study per day?".
Hours of study (2) is measured as the sum of study hours from an hourly time use table compiled by students.
Hours of study (3) is the answer to the question: "How many hours did you study yesterday?".

Source: own elaboration

4 Additional effects on time use and households

In this section, we document additional effects of the lamps on study time and other time uses by students, as well as on parents' allocation of working time, and households' savings. For this analysis we refer to data collected through extended student surveys that we carried at the end of term 1 and 2, in November 2013 and March 2014 respectively. We also use a household expenditure survey that families compiled for a week in August 2014.

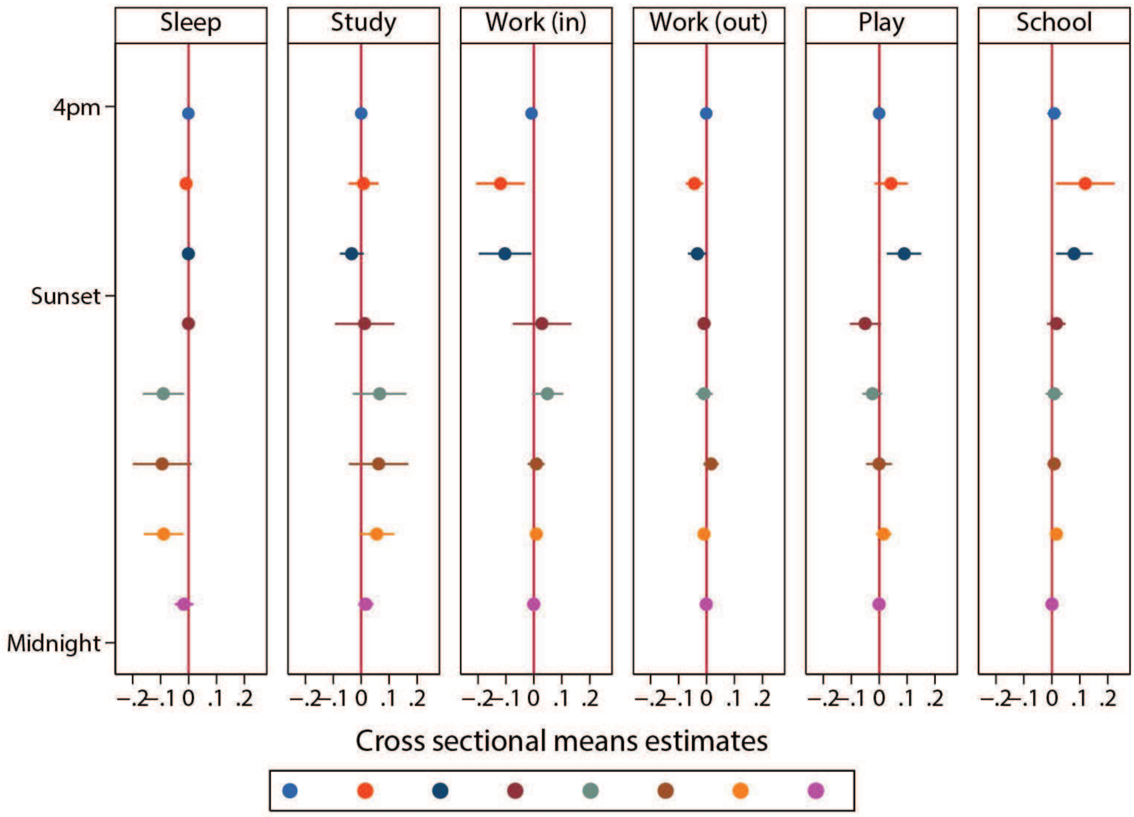
We have three different measures to capture students' study time: (i) we can use the answer to the question *"On average, how many hours per day do you study?"*; (ii) we can compute total study time through a time-use table we gave to students where they had to indicate their usual activity in each hour of the day; (iii) for term 2 only we can use the answer to the question *"How many hours did you study yesterday?"*

Table 7 reports cross sectional estimates of the treatment effect on different measures of hours of study[13]. We can see a short run significant effect on study time such that treated students tend to study 15 minutes more per day. For the average student, this is equivalent to a 6% increase in total study time, and a 17% increase in study time at home. The coefficient turns marginally insignificant if we use the measure based on the time-use table. However, if we look at Figure 3 we can see that there is an overall tendency for treated students to study more than control after sunset between 8-11 pm[14]. We are unable to find a significant effect of treatment on study time in the medium run.

[13] The regressions are run using OLS, clustered standard errors at the class school, and class dummies.

[14] Figure 3 reports cross sectional OLS estimates of different types of time-use on treatment at each hour between 4PM and midnight for Term 1.

FIGURE 3 – Treatment effect on student's time use in term 1 – Cross sectional estimates



Source: own elaboration

In Figure 3 and Table 8 we extend the analysis to other dimensions of students' time use. Looking at time use before sunset in Figure 3, we can see that in the afternoon there is a significant increase of time at school and a marginal increase of time playing[15]. Longer time at school is compensated by a decrease of time spent working at home. It is likely that the lamp allows students to spend more time at school rather than working at home, either because students can work at home more effectively at night thanks to lighting or because the lamp allows students to walk home more safely at later hours even if sunset is approaching[16]. Table 8 confirms a significant increase of time at school both in the short and medium run, such that treated students tend to spend 15-20 minutes at school more than control. Nevertheless, we do not find a significant decrease of aggregate hours worked at home during the day even if in Figure 3 we observed a decrease at specific time between 5 pm and 7 pm.

[15] From our data we are unable to see if the marginal increase in playing happens at school or not.

[16] From fieldwork experience both alternatives are plausible; some students need to walk even two hours to get to school.

TABLE 8 – Other time use effects on students – cross-sectional estimates

Explanatory variable: Treatment	Term 1	Term 2
Time at school	0.25** (0.1)	0.3* (0.16)
Play	0.06 (0.08)	-0.09 (0.06)
Sleep	-0.36** (0.08)	-0.07 (0.1)
Work (home)	-0.05 (0.1)	-0.17 (0.09)
Work (out)	-0.08 (0.06)	-0.04 (0.06)
Observations	242	252

**significant at the 5%level;
* significant at the 10% level.

Clustered standard errors at the school level in parenthesis.
All specifications are run with class dummies.
The estimates are cross sectional OLS.

Source: own elaboration

If we focus on Figure 3 looking at time use after sunset, we can see that the increase in study time highlighted previously is compensated by a decrease of sleeping time. This result is confirmed also in Table 8 where in the short run we find a significant reduction of sleeping time, such that on average treated students tend to sleep about 20-25 minutes less per day compared to the control group.

In Table 9 we assess the effects of the lamps on the time use of fathers of treated students. We document a significant short-run increase of working for pay by fathers, such that during a day they spend on average 13% more time on paid work than control. This time increase is offset by an equal reduction in the incidence of time for socializing and working at home during the day.

TABLE 9 – Time use fathers: cross-sectional estimates

Explanatory variable: Treatment	Morning	Afternoon	Evening	Morning	Afternoon	Evening
Work for pay	0.119** (0.053)	0.173*** (0.066)	0.105*** (0.032)	0.023 (0.068)	0.057 (0.07)	0.002 (0.029)
Socialising	-0.026* (0.015)	-0.104** (0.052)	-0.155*** (0.05)	0.025* (0.015)	0.036 (0.034)	-0.02 (0.054)
Work at home	-0.113** (0.049)	-0.079 (0.055)	0.05 (0.042)	-0.053 (0.067)	-0.073 (0.069)	0.018 (0.05)
Observations	227	227	227	219	217	213

***significant at the 1%level;
** significant at the 5% level;
* significant at the 10% level.

Clustered standard errors at the school level in parenthesis.
All specifications are run with class dummies.
The estimates are cross sectional OLS.

Source: own elaboration

As for time use of mothers of treated students, Table 10 shows that in the short-run they experience a significant decrease of time spent working at home, such that in the mornings they spend 11% less time at home than control. This decrease comes with a marginally insignificant increase of time spent working for pay by 7%. We can appreciate this increase better by looking at Figure 4, which shows the offsetting pattern of mother's time use between work at home and work for pay in the morning[17]. Interestingly, mothers do not compensate the time reduction working at home during the day by working more at night. We believe this is because, as reported by numerous students, the light of the lamp allows mothers to do chores more effectively in a shorter amount of time.

[17] Figure 4 reports a graphical representation of the regression coefficients presented in TABLE [9] and TABLE [10} for Term 1.

TABLE 10 – Time use mothers: cross-sectional estimate

Explanatory variable: Treatment	Morning	Afternoon	Evening	Morning	Afternoon	Evening
Work for pay	0.068 (0.047)	0.043 (0.043)	0.03 (0.02)	0.016 (0.04)	0.011 (0.047)	-0.01 (0.01)
Socializing	0 0	-0.048 (0.039)	-0.005 (0.054)	-0.008 (0.015)	-0.007 (0.007)	0.005 (0.065)
Work at home	-0.114** (0.05)	-0.048 (0.057)	-0.025 (0.057)	-0.005 (0.047)	-0.004 (0.047)	0.006 (0.065)
Observations	238	239	237	244	243	243

**significant at the 5% level.

Clustered standard errors at the school level in parenthesis.
All specifications are run with class dummies.
The estimates are cross sectional OLS.

Source: own elaboration

Nevertheless, we do not find evidence of time-use effects on mothers and fathers in the student survey we conducted after 6 months. This suggests that lamps might have only a short-term effect on this dimension. Further studies could determine what are the drivers behind this pattern and potentially how to improve their persistence.

Finally, evidence from both student surveys and household expenditure surveys show that families within treated students experience a reduction of fuel's expenditure about 60-90 Ksh ($0.66-$1) per week. This is equivalent to around 10-15% of the median weekly income in our sample. During our field visit we find that this extra saving is spent on a range of different activities and goods. For instance a household used it to build a toilet outside the house (rather than going to the bush) while others spent it on children's education.

5 Conclusions

This study presents an original experiment to assess the effect of energy access on education. Through a randomized control trial we document an overall positive effect of solar lamps on education in rural Kenya. Given our randomization design at the pupil level and the partially non-rivalry nature of lamps, we are unable to show a significant average treatment effect. However, once our identification strategy takes into account the potential presence of spillovers, we are able to find a positive and significant intention-to-treat effect and a positive and significant spillover effect on controls. Moreover, we find evidence of a positive and significant effect of treatment intensity on the average grades of a class.

We are also able to document a short run increase of study time by students, a relevant rise of savings that relaxes household's budget constraint, and short-term effect of higher employment of fathers and, marginally, of mothers.

Given the small size of the technology shock that our experiment provides, all our estimates are likely to be a lower bound of the true effect of artificial lighting on education. Moreover, experimental issues like lamp's appropriation by teachers and lamp sharing with students of different years imposed by head teachers that have been sometimes reported, are likely to bias our estimates downwards.

Further investigation on this same topic through larger samples and with different settings might help to understand better the effect of energy access on education and the mechanisms that can enhance or harm such links. Moreover, additional studies on household effects, especially on employment, would also provide a relevant and interesting line of research.

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Enel Foundation
Energy for Knowledge

Via Arno, 64
00198 Rome
(Italy)

P. (+39) 06 83054575
F. (+39) 06 64445622

info@enelfoundation.org

www.enelfoundation.org

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