Powering Education 2, Enel Report*

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1 Introduction

This project focuses on the link between access to light and educational outcomes. This is a follow-up project that builds on Hassan and Lucchino (2015) who showed a positive intention to treat and spillover effects of solar lamps on grades in mathematics. The current project scales up the original one by extending the sample size and it changes the randomisation structure. In this project we rely on a randomised saturation design, where treatment is firstly randomised across schools, such that we assign some schools to a pure control group, some to pure treatment group and some to have a mixed combination of treated and control students. Moreover, compared to the original study, which was implemented in the Amboseli region, this project is located in the Gucha South district, a lush Western region strongly reliant on agriculture. In this way, we test for the external validity of our previous study and see if the results hold in a different setting.

Our results confirm a positive and significant treatment effect, such that students that receive a lamp see their grades in mathematics increase by about four points. The lamp leads to longer study time at home, especially in the evenings, and less time devoted to chores; both these effects are likely facilitate an increase in grades. We also find a positive spillover effect of about 2 points on the grades of students that did not receive the lamp, but are in a class where 50% of pupils did. However, this spillover effect is not statistically different from zero. We find spillover effects on study time, such that non-treated students in mixed classes increase their study time at home in the early morning. Possibly, this may result from a competition effect that such students feel in relation to those that received the lamp, which leads

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them to compensate by studying more during the morning when daylight is available. Nevertheless, this extra effort is not enough to translate into significantly higher grades.

The results of the paper provides key insights on the external validity of the effects of solar lamps. The experimental context in Gucha South is different from the one in Amboseli where Hassan and Lucchino focuses on (2015). In Amboseli, schools used to organise study sessions at the end of classes in the late afternoon, which generated great opportunities for spillover effects to take place. In the current context, the space for interaction between treated and control students is more limited as students tend to walk straight home after classes and study mostly alone. From a policy point of view, this suggests that in order maximise the spillover effects and to enhance the cost-effectiveness of a solar lamps intervention for education, it is key to encourage schools to create interaction opportunities were treated and control students can interact more is key to spillovers.

The overall message from the two studies is that solar lamps can be an effective and significant tool to improve educational outcomes in off-grid areas. However, solar lamps should not be seen as a substitute of electrification, but as a short-term practical solution to limit the drawbacks on human capital accumulation coming from the lack of electricity. Corporates and public entities involved in the long process of electrification should consider whether off-grid solutions such as solar lamps may offer an immediate first step in the longer journey of energy development.

2 Project context

The project aims to understand and quantify the educational impact determined by access to modern forms of lighting compared to traditional fuels, such as kerosene or fire. This requires identifying a target area exhibiting both low penetration of the electricity grid and limited presence of off-grid energy providers.

The project was implemented in partnership with Givewatts, a non-profit NGO providing clean energy to school children through schools and other institutions. Drawing on their local knowledge, we identified the Kisii County as a candidate region for the project. Givewatts further agreed to not carry out their own operations in the target area for the duration of the project.

We use existing data to cross-validate this recommendation and further define our target area. The Kisii county is divided into 5 districts, and within 3 of these (Gucha, Gucha South and Masaba) more than 95% of the population is reported as lacking access to electricity in the Kenya Population and Housing Census 2009.[1] We complement this information with satellite night light data, which offers a more up-to-date snapshot of energy access in the region as well as accurate measurement of light intensity for areas as small as 1 square kilometre. We identify Gucha South as the district with the lowest current levels of electricity assess. We select Gucha South as the target district for the project.

A striking feature of the project area is its geographical and socio-economic homogeneity. Dwellings tend to be constructed with similar materials and techniques, and have broadly the same size. These are typically built within the family plot of land, which is also invariably cultivated. The most common local amenity is the primary school. Families source their basic goods from local 'shopping centres', which amount to little more than a handful of shops/stalls selling basic goods (e.g. vegetables, soap, kerosene) and services (e.g. mobile charging). Figure **??** shows the landscape of the region and illustrates its homogeneity.

3 Data collection and randomisation procedure

Given the size of our sample and the budget constraint we could not run a baseline survey, however we could rely on administrative data on grades, access to electricity, and overall economic activity in order to assign treatment at the school level. In order to identify the geolocation of schools, as well as the type od school and its size, we use the Kenyan Ministry of Education's School Mapping Database. In order to ensure some homogeneity of schools' characteristics, we limit our sample to public, mixed-gender, day-only schools, which are the majority of schools. We conducted a preliminary field visit in April 2015 to those schools and 84 out of 85 agreed to sign an Engagement Letter expressing their willingness to participate, regardless of their future treatment assignment. On this occasion, we also collected information on the number of students in Standard 7 and the number of 'streams' (classes) in each school. Then, using satellite night data we focus our sample on schools in area without access to electricity, so we drop 3 schools out of 85.[2].

In order to proxy for schools' quality, we use pupils' grades on national examinations. Specifically, Standard 8 students complete a national exam

[1]This is the latest official statistical source.

[2]Specifically, we drop schools located within a 1 square kilometre grid unit with a light intensity score higher than zero (i.e. the lowest level)

at the end of the school year and obtain a Kenya Certificate of Primary Education (KCPE). Individual-level KCPE data is made available by the Ministry of Education, from which we calculate the mean grade by school in Mathematics, English and KiKiswahili for the 2014 exam. We successfully match these data for 74 out of the 85 targeted schools using the Kenya National Examinations Council (KNEC) unique identifier. We suspect failure to match 11 schools is due to the KNEC code having changed between the year of the School Mapping (2007) and 2014.

Finally, we calculate distances from the school to the closest road and to population centres as proxies for access to markets and overall levels of economic activity. Information on main roads was drawn from Bing Maps, while data on local roads was sources from the Kenya Board of Roads. The latter also offers information on the surface type (earth, gravel and premix). Data on population centers was sourced from Virtual Kenya portal. We calculated distances to:

- Main Urban Center: Kisii town. Population around 200,000.
- Townships: This is for the main towns in the Area: Kisii (200,000) and Migori (50,000).
- Trading centers: Are bigger than Market centers, located in medium sized towns and tend to have lights.
- Market Centers: Smaller and located in areas without lights.
- Distance to any of the above.

We end up with 72 eligible schools with full data of the original 85 schools invited to participate to the project. However, power calculations suggested that including more than 60 schools did not deliver gains in statistical precision and we could not justify the cost of using the full sample. Therefore, we draw a random subset of 60 schools out of the 72 with complete data.

Our randomisation process need to take into account several dimensions: i) the statistical power associated to the different features of a randomised saturation design, ii) distance between schools to avoid cross-cluster interference, and iii) treatment balance over key observable characteristics. We apply a re-randomisation method inspired by the MinMax t-stat method of Bruhn and Mckenzie (2009). Specifically, we use a simulation approach to select the draw that offers the best trade-off between statistical power, schools' distance by treatment type, and treatment balance. For each random draw, following Baird et al. (2014), we calculate i) the minimum detectable effects, ii) the pooled treatment effect, iii) the pooled spillover on control, iv) the intention to treat effect at 50% saturation, v) the intention to treat effect at 100% saturation , and vi) the slope effect between the two treatment saturations. Additionally, for each draw, we also calculate the smallest geographical distance between schools of different treatment types, Finally, adapting the approach in Bruhn and Mckenzie (2009), we calculate a summary measure covariates' balance defined as the lowest p-value of the F-test of the regression on the categorical variable for the 3 treatment types across all variables selected for balancing, basically we ensure that the means across treatment types are the same. The balancing variables we focus on include distances to population centres, the number of streams and class size, average 2014 grades in Mathematics, English and KiKiswahili, and dummy indicators for each of the 9 administrative sub-locations within Gucha South. The latter is intended to guarantee a homogenous distribution of treatment across the region.

We simulate 100,000 random draws and select the chosen draw as follows. Firstly, we restrict our considerations to the draws that assign no more than 800 lamps to ensure we work within our budget. Secondly, we further restrict our focus on the draws where the smallest distance between any two schools of different type is at least 850m. This identifies 235 draws. Further, we filter the draws to keep only those with a minimum p-value across the balancing variables of no more than 0.2 identifies. All this process identifies twelve potential draws. Among these, we select the draw which offers the lowest minimum detectible effect. However, as we estimate a minimum detectable effects for five different dimensions, over which there may be a different ranking of draws, we must decide which margin to give most weight to. We notice how for all MDEs except the slope effect, the variation in power across the candidate draws is small (typically less than 0.5 points on a scale of 100). Instead, there is a greater heterogeneity in power over the slope margin. We therefore opt to select the draw offering the lowest effect along this margin.

This procedure allows us to randomly select the schools with no-treatment, full treatment and partial treatments. Then, students in schools with partial treatment saturation were randomly assigned to receive a lamp using a public lottery stratified by gender. Finally, we distribute a total number of 784 lamps across our sample.

4 Balance, attrition, and compliance

Table 1 shows that treatment intensity is well balanced across grades in the last KCPE certificate[3] as well as class size and main geographical characteristics where the school are located. In fact, schools' distance from a main road, town, market, hospital and a bank are well balanced across treatment intensity.

Table 2 reports the balance of treatment status across students. Treatment was randomly assigned through a lottery stratified by gender. The results of the regressions, which used saturation weights to account for the representativity of the full sample across treatment intensity, show that treatment was balanced across grades at baseline and gender. For a randomly selected subsample of students we have an additional set of information that was obtained through an extensive survey.[4] Table 2 reports balancing outcome also for this subset of interviewed students and we find balance also on parents' education, for the number of people in the household, number of siblings aged 5-18. Nevertheless, balance is not robust over wealth status and it depends on the type of index we rely on. If we use a wealth index based only on houses' characteristics (i.e. type of walls and toilets), there is balance across treatment. However, once we add ownership of goods like bicycles, motorcycles, TV, and radio, the index score for treated students is about 30% higher than for the untreated. The survey was taken 8 months after the lamps' distribution. We cannot rule out that treatment might have affected the purchase of these objects, whereas it is unlikely that treatment affected the more structural characteristics of the house like walls and toilets. In this case the lack of balance would be less worrisome. However, in order to mitigate this concern, we control for wealth status in the econometric analysis.

In our experimental setting attrition takes the form of missing the final exam. About 27% of the students in our sample and 20% of the students interviewed were subject to attrition In Table 3 we can see that attrition is independent from treatment. Attrition tends to be associated with lower grades, but the coefficients, even if significant, are very small in magnitude. If we look at household characteristics for the subsample that were interviewed, we find that attrition is unrelated to parents' education, number of people in the household, number of siblings, and wealth. Therefore, based on these results, we conclude that attrition is unlikely to drive our results.

As far as compliance is concerned, the experiment delivered good results.

[3]KCPE is the leaving certificate for primary education in Kenya.

[4]The subsample is about 55% of the full sample. The sample was randomly selected, stratifying by school, gender, treatment status and baseline attainment in maths.

About 90% of respondents reported that the lamp was working well or only with minor problems; in 10% of the cases the lamp has been reported to be broken. This condition is not statistically associated to past grades or household characteristics. In more than 94% of cases, the solar charge of the lamp was sufficient for the activities households wanted to use it for. In terms of lamps' appropriation, more than 90% of students said that the lamp stayed at home during the night; in four cases it stayed at school; in three cases at some other students' house; and for the remaining 10% of cases in some other unreported location. Finally, 98% of respondents used the lamp mostly for studying; whereas, 38% of respondents declared to use the lamp also for working at home.

5 Identification strategy

Our identification strategy hinges on the methodology of Baird et al. (2014). It follows the randomised saturation design of the experiment and it focuses on both treatment and spillover effects. Firstly, we estimate the pooled effect running:

$$Y_{ic} = \beta_0 + \beta_1 T_{ic} + \beta_2 S_{ic} + \beta_3 X_{ic} + \epsilon_{ic} \tag{1}$$

where Y_{ic} is the dependent variable of interest (grades or study time) of student i in cluster c; T_{ic} is a dummy for treatment; S_{ic} is a dummy for control students in a non-zero saturation cluster; X_{ic} are control variables, which include variables used for stratification and baseline outcome variables; finally, ϵ_{ic} is the error term, which is assumed to have both a cluster and individual component such that $\epsilon_{ic} = \nu_c + \omega_{ic}$.

If we run equation 1 with saturation weights, β_1 captures the intention to treat effect and β_2 captures spillover on non-treated. Whereas if we run the regression without saturation weights we can obtain the total causal effect (TCE), which is the cluster-level difference between treated and pure control clusters. The TCE is basically a weighted average of treatment and spillover effect, where the weights are given by the probability of treatment and control: $TCE = \frac{\mu}{1-\psi}\beta_1 + \frac{1-\mu-\psi}{1-\psi}\beta_2$, where μ is the probability of treatment and ψ is the probability of being a pure control.

The OLS estimate of this coefficient is going to be consistent and unbiased under the assumptions that there is no cross-cluster interference in outcome, so that Y_{ic} is independent from treatment status of an individual in another cluster. We believe that our setting satisfies assumption one because pupils' interaction across schools is very limited, only 5% of students in our sample declared to study with students from other schools. Moreover, our randomisation structure ensures that there is enough distance between schools with different saturation levels, which mitigates our concerns about this assumption. Statistical inference rests on a stratified inference assumption, such that the outcome of an individual is independent of the identity of the other individuals assigned to treatment. This is an agnostic assumption which can be justified in the absence of detailed information on the network structure. However, it is reasonable to assume that the network structure can influence how spillover effects can materialise and it can bias our results on spillovers.

As we will see in the next section, we find a positive, but not significant, evidence of spillover on non-treated. In this case it is possible to use within-cluster controls as contrafactual, so we can pool together pure control students and control students in partially treated classes estimating the following specification:

$$Y_{ic} = \beta_0 + \beta_1 T_{ic} + \beta_2 X_{ic} + \epsilon_{ic} \tag{2}$$

where β_1 provides the intention to treat effect.

Finally, we can also determine how treatment and spillover effects vary with treatment intensity, by measuring individual and spillover effect at each non-zero saturation. In order to do so we run the following specification:

$$Y_{ic} = \beta_0 + \sum_{\pi \setminus \{0\}} \beta_{1\pi}\ T_{ic} * \mathbf{1}\{\pi_c = \pi\} + \sum_{\pi \setminus \{0\}} \beta_{2\pi}\ S_{ic} * \mathbf{1}\{\pi_c = \pi\} + \beta_3\ X_{ic} + \epsilon_{ic} \tag{3}$$

which allows to identify intention to treat, spillover of non-treated and TCE for different levels of saturation, such that $I\hat{T}T_\pi = \hat{\beta}_{1\pi}$, $S\hat{N}T_\pi = \hat{\beta}_{2\pi}$, and $T\hat{C}E_\pi = \pi\hat{\beta}_{1\pi} + (1-\pi)\beta_{2\pi}$, $\forall \pi \in \Pi \setminus \{0\}$. The hypothesis test $\beta_{1\pi_j} = \beta_{1\pi_k}$ identifies whether ITT differs by saturation, thereby testing for a spillover on the treated.

6 Treatment and spillover effects: preliminary results

In this section we discuss the results of our experiment by looking at the effects on both grades and time-use by students. In the first case we can rely on our full sample; whereas, in the second case the analysis can focus only on the subsample that we interviewed.

6.1 Effects on grades

Table 4 looks at the pooled effect on grades by different subjects. We run two types of specification, one controlling for gender only, which is the variable on which we stratify, and one controlling also for grades at baseline. The latter is our preferred specification, because education is a cumulative process and it is important to control for the level students start from. We find a positive intention to treat and spillover effects of the lamp across all grades. However, only the treatment effect in mathematics is statistically significant, when we control for initial grades, such that treatment increases grades in math by 4 points. This is consistent with the results of Hassan and Lucchino (2015). If we look at the total causal effect, we find a positive and significant difference between treated and pure control clusters of 3.4 points in mathematics.

The lack of significant spillover effects is not in line with our previous findings in Hassan and Lucchino (2015). This should not be too surprising, because the geographical and cultural settings of the two experiments are different. Actually, these heterogenous results shed some light on the external validity of spillover effects and on what is needed to make them materialise. In this setting most students (about 59% of respondents) study alone. Of the remaining 41%, only 36% of them study with treated students. So only about 15% of our overall sample can be subject to spillovers through lamp sharing and only half of those are in classes with mixed treatment status. Moreover, in this experiment teachers did not get involved in the organisation of afternoon study groups, whereas in Amboseli it was more common to organise study sessions after the end of classes. In the current context, the space for interaction between treated and control students is more limited; indeed fieldwork experience revealed that students walk straight home after the classes, which was not the case in Amboseli. From a policy point of view, all this suggests that in order to enhance the effectiveness of solar lamps on educational outcomes in a cost-effective way by exploiting spillover effects, it is key to involve schools for the organisation of post-lectures study groups and the creation of interaction opportunities between treated and control students.

Given the lack of significant spillover evidence, we can use within-cluster controls as contrafactual and run specification 2. Table **??** shows that the treatment effect on mathematics is confirmed and turns statistically more significant. Moreover, with this specification we also find a significant effect for english, whereas the effect on Kiswahili is positive and just marginally insignificant at standard critical values.

Finally, Table 6 analyses treatment effects at different saturation bins by running specification (3). We run two types of specifications, one including spillovers on non-treated and one without. This changes the control group of reference. In the first case the control group is given by students in pure control classes. Across all subjects the coefficients of treatment at 50% saturation is not statistically different from the one at 100% saturation. The results show that treatment is marginally insignificant at any saturation level. This does not invalidate our results for the pooled effect as power for the two subsamples is reduced respect to the pooled estimation. In the second specification we drop the variable on spillovers for non-treated. In this way the control sample include both control students at pure control and control students at 50% saturation. This translates into a gain in significance on the coefficients for treatment as power increases. The coefficients for the second specification have a lower magnitude, but this is due to the fact that the control group now include also control students from treated class who tend to have higher grades than students in pure control; hence the average grades of the control group is going to be higher and the average difference of the treated students lower.

6.2 Effects on students' time allocation and family usage

We run extended interviews with a subsample of students asking details on the activities that they undertake during the day. We estimate the effect that lamps have on time use focusing our econometric specification on the pooled effect, as we do not have sufficient power for the other specifications. Table 7 shows that the lamp has a positive and significant treatment effect on study time, such that treated pupils study about 35 minutes per day more than students in the pure control group. Moreover, treatment also leads to a significant reduction of time spent on chores. This is consistent with the findings of Hassan and Lucchino (2015) and it is likely due to the fact that thanks to the lamp's illumination, chores can be done more quickly in the evenings. We find evidence of a positive spillover effect on non-treated students who reduce the time spent with family and playing.

Table 8 analyses study time more in detail. We find that the lamp has effects on study time at home rather than at school. This is consistent with our interpretation of the lack of spillover effects, as studying at school tend to be the major source of spillovers in this type of interventions (Hassan and Lucchino, 2015). Moreover, we find spillover effects on study time at home on non-treated students. If we look at study time in different periods of the day we find that treated students increase their study time both in the morning and in the evenings where the effect is stronger. Nevertheless, for non-treated students in mixed classes, study time increases significantly only in the mornings. This is not surprising given that control students do not have access to the lamps' illumination in the evenings. It is possible that there is a competition effect on control students who decide to devote more study time in the mornings to compensate for the fact that they did not receive the lamp.

7 Conclusions

We run an extensive experiment in a randomised saturation design setting where we distribute solar lamps to 7th grade pupils in rural Western Kenya. We find a positive and significant effect on treated students who increased their grades in mathematics by 4 points compared to pupils who did not receive the lamp. Moreover, we find a positive but not significant effect also on students that did not receive the lamp but that are in a class where 50% of classmates did.

We also find evidence of longer study time for treated students who on average study 40 minutes more during a day, especially in the evenings. We see that there are spillover effects on study time such that control students in mixed classes increase their study time early in the morning, possibly because of some competition effect they feel in relation to treated students.

The results of this study confirm the positive effect that lamps have on education that were highlighted also in Hassan and Lucchino (2015). It is very important that such result is confirmed also in a different setting as it increases the external validation of the impact of solar lighting on education. Moreover, the lack of significant spillover effects in this context suggests that the involvement of schools is key in order to maximise the educational returns of solar lighting. Encouraging and facilitating afternoon study groups is crucial to generate spillovers also on control students. Our overall assessment is that the two studies confirm that solar lamps can be an effective and significant tool to improve educational outcomes of pupils in off-grid areas. However, solar lamps should not be seen as substitute for electrification, but as a short-term practical solution to limit the drawbacks on human capital accumulation coming from the lack of electricity.

Figures

Figure 1: Gucha South

Tables

Table 1: Balance of treatment intensity across schools

Explanatory variable: treatment intensity	Coefficient	p-value
Mathematics	-2.94	0.23
English	-2.46	0.27
Kiswahili	-2.23	0.33
Class size	-7.9	0.25
Distance to main road	-1.1	0.30
Distance to town	0.83	0.48
Distance to market center	0.76	0.19
Distance to hospital	-0.83	0.16
Distance to bank	0.60	0.23

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

Table 2: Balance of treatment across students

Explanatory variable: treatment	Coefficient	p-value
Mathematics	-0.62	0.72
English	1.52	0.35
Kiswahili	0.54	0.72
Gender	0.02	0.32
Mother's education	0.02	0.86
Father's education	0.03	0.69
N. of people in the household	-0.07	0.60
N. of siblings aged 5-18	-0.17	0.22
Wealth Index 1	0.04	0.66
Wealth Index 2	0.38***	0.00

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

Table 3: Attrition

Dependent variable: Missing final exam	Coefficient	p-value
Treatment	-0.01	0.67
Mathematics	-0.003***	0.00
English	-0.005***	0.00
Kiswahili	-0.005***	0.00
Gender	0.02	0.20
Mother's education	0.006	0.49
Father's education	-0.01	0.18
N. of people in the household	0.00	0.99
N. of siblings aged 5-18	0.001	0.8
Wealth Index 1	0.003	0.72
Wealth Index 2	-0.006	0.62

***significant at the 1%level.

Table 4: Intention to treat and spillovers - Pooled effect on grades

	Mathematics		English		Kiswahili	
	(1)	(2)	(1)	(2)	(1)	(2)
Treatment	3.7	4.08*	3.15	2.62	2.02	1.92
	(0.13)	(0.055)	(0.12)	(0.11)	(0.30)	(0.27)
Spillover non-treated	2.14	2.38	0.65	0.72	-0.55	-0.88
	(0.41)	(0.32)	(0.72)	(0.66)	(0.73)	(0.57)
Gender	-2.33***	-1.07*	0.88	0.54	1.00	0.61
	(0.00)	(0.051)	(0.15)	(0.34)	(0.14)	(0.38)
Grades at baseline		0.38***		0.38***		0.28***
		(0.00)		(0.00)		(0.00)
Total causal effect	3.24	3.43*	2.60	2.03	1.70	1.38
	(0.15)	(0.08)	(0.17)	(0.18)	(0.35)	(0.38)
Observations	1642	1543	1636	1539	1636	1539

***significant at the 1%level; ** significant at the 5% level; * significant at the 1% level. P-Values from clustered standard errors at the school level in parenthesis.

Table 5: Intention to treat and spillovers - Within cluster countrafactual

	Mathematics		English		Kiswahili	
	(1)	(2)	(1)	(2)	(1)	(2)
Treatment	2.93	3.25**	2.93*	3.14**	2.20	2.34
	(0.11)	(0.03)	(0.08)	(0.045)	(0.19)	(0.13)
Gender	-2.37***	-.98*	0.9	1.6***	0.98	1.6***
	(0.00)	(0.058)	(0.15)	(0.00)	(0.16)	(0.01)
Grades at baseline		0.38***		0.21***		0.20***
		(0.00)		(0.00)		(0.00)
Observations	1613	1543	1609	1539	1609	1539

***significant at the 1%level; ** significant at the 5% level; * significant at the 1% level. P-Values from clustered standard errors at the school level in parenthesis.

Table 6: Intention to treat and spillovers - Different treatment intensity

	Mathematics		**English**		**Kiswahili**	
	(1)	(2)	(1)	(2)	(1)	(2)
Treatment at 50%	4.31	3.48*	2.57	2.31*	1.36	1.57
	(0.14)	(0.10)	(0.18)	(0.10)	(0.40)	(0.19)
Treatment at 100%	3.83	2.99	4.32	4.05	2.98	3.19
	(0.15)	(0.26)	(0.12)	(0.12)	(0.30)	(0.25)
Spillover on non-treated	2.38		0. 75		-0.61	
	0.32		(0.68)		(0.69)	
Gender	-1.07**	-0.98*	1.55***	1.58***	1.61***	1.59***
	(0.05)	(0.06)	(0.00)	(0.00)	(0.01)	(0.01)
Grades at baseline	0.38***	0.38***	0.21***	0.21***	0.20	0.19***
	(0-00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	1543	1543	1539	1539	1539	1539

***significant at the 1%level; ** significant at the 5% level; * significant at the 1% level. P-Values from clustered standard errors at the school level in parenthesis.

Table 7: Intention to treat and spillovers - Pooled effect on time use

	Study	**Chores**	**Playing**	**Family**
Treatment	0.59**	-0.32***	-0.1	-0.14
	(0.04)	(0.00)	(0.28)	(0.14)
Spillover non-treated	0.33	-0,15	-0.26*	-0.22**
	(0.62)	(0.24)	(0.06)	(0.05)
Gender	0.07	0.27	-0.06	0.01
	(0.76)	(0.00)	(0.17)	(0.81)
Observations	858	858	858	858

***significant at the 1%level; ** significant at the 5% level; * significant at the 1% level. P-Values from clustered standard errors at the school level in parenthesis.

Table 8: Intention to treat and spillovers - Pooled effect on study time

	Study at home	**Study at school**	**Study 5AM-12PM**	**Study 12PM-6PM**	**Study 6PM-12AM**
Treatment	0.67***	-0.06	0.18**	0.07	0.40**
	(0.00)	(0.83)	(0.01)	(0.60)	(0.04)
Spillover non-treated	0.48**	-0.16	0.15**	0.03	0.25
	(0.03)	(0.76)	(0.01)	(0.90)	(0.22)
Gender	0.02	0.02	-0.06*	0.09	0.04
	(0.86)	(0.89)	(0.07)	(0.32)	(0.65)
Observations	858	858	858	858	

***significant at the 1%level; ** significant at the 5% level; * significant at the 1% level. P-Values from clustered standard errors at the school level in parenthesis.