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Impacts of Rural Electrification in Rwanda

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ABSTRACT

Impacts of Rural Electrification in Rwanda*

Rural electrification is believed to contribute to the achievement of the MDG. In this paper, we investigate electrification impacts on different indicators. We use household data that we collected in Rwanda in villages with and without electricity access. We account for self-selection and regional differences by using households from the electrified villages to estimate the probability to connect for all households – including those in the non-electrified villages. Based on these probabilities we identify counterfactual households and find robust evidence for positive effects on lighting usage. Effects on income and children's home studying become insignificant if regional differences are accounted for.

JEL Classification: O12, O13, O18, O22

Keywords: rural electrification, energy access, impact evaluation, matching, difference-in-difference

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1. Introduction

Electrification is widely believed to contribute to the achievement of the Millennium Development Goals (MDGs), based on the assumption that sustainable access to modern energy services fosters economic and social development, and leads to improvements in the quality of life. Yet, particularly in rural Sub-Saharan Africa electrification rates are still low, as only 11 % of the population use electricity. In rural Rwanda, the electrification rate is even considerably lower at 1.3 % (UNDP/WHO 2009). As part of the efforts to achieve the MDGs it is among the national policy priorities of most countries to improve access to electricity. The national target for Rwanda, for example, is to augment the overall electrification rate to 30 % by 2020 – six times the rate in 2005. The international donor community joins these efforts and has increased its support to the energy sector in general and electrification projects in particular (IEG 2008). As part of these international endeavours, the Dutch-German Energy Partnership *Energising Development* (EnDev) envisages the sustainable provision of access to modern energy for 6.1 million people in 17 developing countries. For this purpose, *EnDev*, which is implemented by *Deutsche Gesellschaft für Internationale Zusammenarbeit* (German International Cooperation, GIZ, formerly known as GTZ), supports the development of solar and hydro power schemes, and electricity grid extension as well as the dissemination of improved cooking stoves (GTZ 2010).

Against this background of increasing interest in rural electrification, it is crucial to obtain a more solid basis of empirical knowledge about its relevance to different dimensions of poverty. While ESMAP (2003), KHANDKER ET AL. (2009a), and KHANDKER ET AL. (2009b), for example, provide evidence on poverty impacts induced by electrification programmes in Asian countries, empirical findings hardly exist for Africa.[1] The *EnDev* programme in particular has dedicated itself to monitor outcomes, that is, the number of connected people, and to evaluate the socio-economic impacts induced by the electricity access. Therefore, a couple of target region surveys have been conducted under the *EnDev* umbrella.

For the analysis in this paper we use household data collected for the *EnDev* rural electrification project in Rwanda, called *Private Sector Participation in Micro-hydro Power Supply for Rural Development* (PSP Hydro). One main goal of the survey was to assess before project implementation the impacts that can be expected from the installation of the micro-hydro mini-grids. For this purpose, not only households in the yet non-electrified project areas were surveyed, but also households in comparable non-project regions that already have access to electricity.[2] The outcomes we consider are lighting usage, children's study time at home, energy expenditures, and income.

To identify impacts on these outcomes, we first estimate based on the subsample of already electrified villages a probit model with the connection status of households as dependent variable. The estimated model is then employed to predict connection probabilities for all households in the sample, including those from the yet non-electrified project villages. By means of these probabilities we identify counterfactual households using different matching algorithms. Unlike ordinary cross-sectional research designs, the advantage of our approach is that we can recruit the comparison group from a region without access to electricity. This is likely to substantially reduce selection biases, as the identified households simply could not self-select into the treatment, thus

[1] IEG (2008) as one of the few exceptions provides some evidence for Ghana. PETERS AND VANCE (2011) analyze the effect of electrification on fertility in Côte d'Ivoire. PETERS, VANCE, AND HARSDORFF (2011) and NEELSEN AND PETERS (2011) examine the usage of electricity and its impacts in micro-enterprises. Likewise, RUD (2010) and DINKELMAN (2008) look more at the productive effects of electrification.
[2] Note that the paper does not evaluate the GIZ intervention, although it uses data collected for this project. The evaluation is still underway, as the intervention is being rolled out.

bringing them close to the counterfactual situation of the electrified households. In addition, we apply a difference-in-difference approach in order to account for potentially remaining distorting dissimilarities between the electrified and the non-electrified region.

The remainder of the paper is organized as follows. Section 2 presents the country background and the *PSP Hydro* project. Section 3 focuses on the design of the underlying survey and gives descriptive statistics on the surveyed sites and electrification impact indicators. The fourth section presents the impact analysis using different propensity score matching algorithms. Section 5 concludes.

2. Country and project background

Rwanda is a country located at the heart of the African continent, with a current (2010) population of about 10.5 million people. Given its small territorial size, Rwanda is the most densely populated country in continental Africa. It is a rural country with approximately 90 per cent of the population engaged in agriculture, mainly subsistence farming. Rwanda has few natural resources and its main exports are coffee and tea. Even though the current annual GDP per capita reaches only around USD 900, the country – averaging growth rates of 4.9 per cent per annum since 2000 – has made substantial progress over recent years in stabilizing and rehabilitating the economy to pre-genocide, i.e. pre-1994, levels. Progress has also been observed in areas such as access to education and health as well as gender equality. The latest specific Rwanda National Human Development Report (UNDP 2007) points out that agriculture, demography, and income distribution pose major problems on a sustained growth path. Moreover, Rwanda, like the majority of Sub-Saharan Countries, faces a serious lack of electricity supply, which is part of a general energy shortage.

While around 25 % of Rwandan urban households are connected to the electricity grid, only 1.3 % had access to some form of electricity in rural areas in 2005 (UNDP/WHO 2009). The per capita electricity consumption is one of the lowest in the world and concentrated in the main cities: The capital Kigali alone accounts for more than 70 % of the total national low-voltage electricity consumption. Investments in new generation or network capacities have been very limited in the past, such that energy sector reform advanced only slowly. Apart from imports from neighbouring countries, supply mainly consists of outdated hydroelectric power stations, thermal power stations acquired in 2004 making up as much as half the available national electricity generation of 71 MW in 2010 (MININFRA 2010). Before 1994, less than 20 micro-hydro power plants existed with capacities of around 50 to 100 kW. In 2008, only one of them was operational (SHER 2008). Adding to these supply constraints, the hilly and land-locked character of the country makes the provision of energy to rural areas difficult and expensive.

The Government of Rwanda defined several objectives and targets in order to tackle the persistent problem of rural energy poverty in the country, including increased access to grid electricity (MINECOFIN 2000). As a consequence, a variety of activities in cooperation with the international community has addressed these problems. Most recently, the *Electricity Access Roll Out Programme*, financed predominantly by the Rwandan Government, World Bank, and the Netherlands, has the ambitious objective to attain a national electrification rate of 16 % by 2014. As regards electricity generation, the exploitation of large methane gas deposits in Lake Kivu has recently started. The extraction is technically challenging, but has potentials to multiply installed generation capacity in the country and even allow for electricity exports.

Compared to these large programs, the *PSP Hydro* project is a small-scale effort to tackle energy poverty. Being implemented by GIZ since mid 2006, it is one of the earlier interventions in the sector. In light of a formerly inexistent private electricity generation sector and favourable geographic and climatic conditions for micro-hydro power in the country, *PSP Hydro* aims at developing a private sector for micro-hydro based power generation. The electricity shall either be fed into mini-grids at the village level or into the national electricity grid. For this purpose, subsequent to a tendering process five private Rwandan entrepreneurs have been supported financially and technically in setting up business plans for the investment in the power plants and mini-grids, as well as their installation and operation. Like all other *EnDev* projects, *PSP Hydro* is obliged to report regularly the number of newly served people and to monitor impacts (GTZ 2010).

3. Research design and data base

3.1 Survey design and site selection

We collected the household data used in this paper during the preparation phase of the *PSP Hydro* project. In designing the survey we took two main purposes into account: First, to provide for baseline data to be used in an ex-post evaluation of impacts. Second, to assess *before* project implementation the impacts that can be expected from the installation of the micro-hydro mini-grids.[3] In order to fulfil the second purpose, we did not only survey the yet non-electrified regions that will be served by the *PSP Hydro* mini-grids, but we also surveyed comparable, already electrified villages. Given the limited number of non-electrified sites to be electrified by *PSP Hydro* and, hence, to be surveyed, it is not possible to properly account for regional differences in an eco-

[3] For more information on *PSP Hydro*, the overall evaluation scheme and further project related objectives see BENSCH AND PETERS (2010).

nometric model or through covariates in a matching approach – simply because variation in these village level criteria is too low in our data set. In order to address this from the outset of the study, we put much effort into a proper sampling of electrified comparison sites. One of the authors worked on the ground to prepare and implement the study together with a local Rwandan NGO.

Before selecting the electrified villages, we first seek to get a clearer understanding of the selection process of *PSP Hydro* sites and the prevailing socio-economic conditions there. While it is frequently the case in sparsely electrified rural areas that villages chosen to get connected are economically prosperous or enjoy particular political influences, the *PSP Hydro* sites had been selected mainly based on technical considerations. Those sites were included in the project that exhibited promising conditions for micro-hydro power production and where project developers believed to have the highest implementation capacities. In fact, during the field trips the *PSP Hydro* sites turned out to be neither particularly remote nor particularly promising from an economic perspective. These observations fit into what CHANDRA AND THOMPSON (2000) refer to as an *exogenous event*: In principle, the placement of infrastructure like roads or electricity is in most cases endogenous from an evaluative perspective, because it is based on reasons that are correlated with the outcome indicators of the evaluation such as economic growth or income in the region. Sometimes, however, it can be considered as an exogenous event, because the infrastructure is placed for other reasons not related to the evaluation outcomes. Infrastructure construction is then ascribed a quasi-experimental character as done by CHANDRA AND THOMPSON (2000) for interstate highways that connect larger U.S. cities and thereby happen to cross non-metropolitan counties.[4]

These findings on the non-electrified sites were then taken into account in selecting appropriate electrified villages for comparison. In a first step, we compiled a shortlist of electrified sites based on information available at the headquarter of the national electricity utility *EWSA* (called *Electrogaz* at the time of the survey*)*. Together with local experts we identified some 15 electrified villages and clusters of villages that were comparable to the non-electrified *PSP Hydro* sites in terms of the following criteria: population, geographical location, distance to Kigali, the general agricultural structure in the respective region, and distance to the next tarmac roads. In a following step, we conducted field trips to check for additional comparability criteria that could only be verified on the ground: frequency of and distance to local and regional markets, cultivated cash crops, village structure, and individual particularities that could endanger the comparability to the non-electrified *PSP Hydro* sites. The electrified villages should furthermore have disposed of electricity access for at least four years. During the field trips, the electrified villages were as well tested for the exogenous event idea in order to maintain comparability also in this regard. The exogenous events we exploited in our study were the following: the electrified site happens to be located between one of the four larger hydro power plants in the country and Ruhengeri, the fourth largest city in Rwanda. In other cases, the electrified village was only connected because it was located close to a rural hospital or a monastery in need of electricity.[5]

Eventually, a sample of seven sites turned out to qualify for the study and were surveyed in 2007

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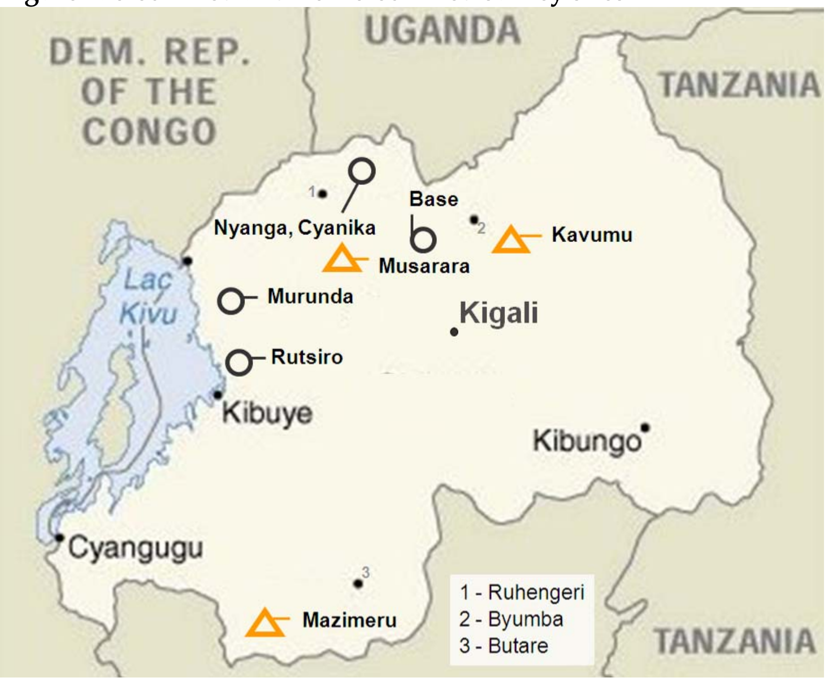
[4] In addition to CHANDRA AND THOMPSON (2000), see DATTA (2010), DUFLO AND PANDE (2004), and MICHAELS (2007) for examples of how to identify impacts of infrastructure by using some sort of *exogenous event* approach.

[5] At least for the hospital, one might again suspect it was put there for particular and, hence, distorting locational factors. However, lower-level public infrastructure in Rwanda is in general typically located in the middle of the respective catchment area without being regionally close to a rural center or the like.

and 2008, each site comprising four to ten agglomerations within an area of roughly 15 to 30 sq km (see Figure 1). Using simple random sampling, 537 households were interviewed in seven sites using structured questionnaires; 265 in the three non-electrified regions and 272 in four electrified regions. Among the latter, 129 households were found to be connected to the grid at the time of the survey. The target population of the survey consisted of households that live in a 50 meter corridor around the planned or existing grid in the non-electrified and electrified villages, respectively. We thereby only sampled households that already have or will have actual access to the electricity grids and excluded dispersed households living in the hills around the agglomerations, simply because they cannot be connected by the existing or future grid.

Figure 1: electrified and non-electrified survey sites



Red triangles represent PSP Hydro *sites that are not yet electrified, but will be over the coming years. Blue circles are sites already provided with electricity. The site at Murunda actually is part of* PSP Hydro*. The micro hydro plant is supposed to feed into the grid after rehabilitation. The site is categorized in the electrified group in this paper, since it already disposed of electricity access before the intervention.*

In addition to the structured household questionnaires that provide the data for the quantitative analysis, we obtained qualitative information from a range of key informants like local chiefs, NGOs, or project staff that were visited for semi-structured interviews.

3.2 Socio-economic conditions in the surveyed regions

In this section we provide information based on the survey that describes the composition and socio-economic characteristics of the households (see Table 1). All electrified and non-electrified sites are located in the middle longitudinal corridor of Rwanda. They exhibit comparable geographical and climatic characteristics, e.g. concerning rainfall and topography, the two decisive characteristics for micro hydro power. Although Rwanda is densely populated, people in rural areas live much dispersed on hilly terrain. This dispersion was traditionally bolstered by rules that forbade changing residence without government approval. This is also one reason for the relatively low incidence of migration. Only 13 percent of the surveyed households, for instance, report that any household member has ever been migrating[6].

The genocide in 1994 that devastated the country's human, physical and social capital was followed by a long, difficult but promising process of recovery. Nevertheless, consequences are still evident – for example in the fact that 25 percent of surveyed households lack either the father or the mother. This is also the reason why 22 percent of the households are headed by a female (Table 1). Almost half the survey population is younger than 15 years. Family sizes are in general relatively small with an average of 5.4 household members. Subsistence farming is ubiquitous: 84 percent of households do possess fields.

Moreover, Table 1 provides descriptive statistics grouping the households into those living in an

[6] Household members who left the household due to marriages are not considered in this figure.

electrified village and those that live in a non-electrified one, i.e. "access" and "non-access" sites, respectively. The terminology of an "access site" refers to the fact that being an electrified village implies that access to electricity exists in the village, and in principle can be used by everyone living in the village. The decision to actually connect to the electricity grid existent in the village is then made at the household level. The left panel of Table 1 therefore looks at the surveyed households in the villages with access, distinguishing between households that did connect and households that did not. Clearly, households in non-access villages cannot be connected to the electricity grid.[7]

In Table 1 it can also be assessed to what extent the comparability of village level characteristics described above translates into comparability at the household level. Although the comparison sites have been carefully chosen, the table suggests that access and non-access sites do not seem to be fully comparable in the aggregate. The t-statistic values presented in the table (column 5) show that the tests for differences-in-means between the 272 access villages (column 1) and the 259 non-access villages (column 4) are significant for most of the characteristics. However, at least part of the observed differences may be induced by the electrification access. In a first crude analytical step, we therefore additionally account for the heterogeneity between connected and non-connected households. The respective values that are also given in the table (column 2 and 3) indicate that most of the observed differences between access and non-access sites seem to be driven by connected households. Yet, at this stage, it cannot be determined how much of these differences stem from selection processes within the access villages and how much the

[7] Table 1 shows that a total of 6 households in non-access villages reported to be "connected". In fact, these six households already dispose of an electricity source in the form of an individual generator. For the sake of consistency, they are excluded from the summary statistics presented in the table.

electrification intervention contributed to these differences. We will scrutinize this question in the following impact assessment section.

Table 1: Descriptive statistics on survey population, differentiated by electricity access and connection status

	Total	Access villages connected	Access villages non-connected	Non-access villages	Test statistic
N households	272			265	
N connected		129		6	
N not connected			143	259	
Household variables:					
HH Size	5.2	5.5	4.9	5.6	$\|t\| = 2.08$**
Female household head	0.22	0.16	0.27	0.19	$X^2(1) = 0.62$
Education years father	8.2	9.8	6.7	5.9	$\|t\| = 5.53$***
Education years mother	6.5	8.5	4.8	4.5	$\|t\| = 5.69$***
Any HH member migrated	0.16	0.13	0.20	0.13	$X^2(1) = 1.30$
Housing variables:					
Household owns the house	0.84	0.84	0.85	0.92	$X^2(1) = 8.21$***
Floors are cemented	0.49	0.78	0.22	0.33	$X^2(1) = 13.77$***
Stone or brick wall	0.33	0.55	0.12	0.12	$X^2(1) = 35.17$***
Glass window	0.52	0.77	0.22	0.23	$X^2(1) = 47.20$***
Ownership of fields	0.79	0.78	0.81	0.89	$X^2(1) = 10.35$***
Employment variables:					
Main occupation of HH head is subsistence farming	0.42	0.28	0.54	0.65	$X^2(1) = 29.08$***
Father occupied in public service	0.26	0.38	0.13	0.13	$X^2(1) = 9.97$***
Mother occupied in public service	0.17	0.34	0.02	0.06	$X^2(1) = 16.29$***
Financial variables:					
Bank account ownership	0.59	0.80	0.41	0.43	$X^2(1) = 13.67$***
Household ever took out a loan	0.33	0.48	0.20	0.30	$X^2(1) = 0.84$

Note: ***,** and * indicate significance levels of 1%, 5% and 10%, respectively.

Connected households have disposed of their electricity connection for an average of 5.5 years, with a median of three years. The median price they paid for the connection including in-house installations amounted to 110,000 FRw (200 USD). For 91 percent of them, lighting is considered as the main advantage of electricity. Households traditionally use so-called *agatadowas*, traditional kerosene lamps made of used tins, and hurricane lanterns. Candles rather act as a backup lighting source in connected households in case of power outages. Torches are not frequently used either, since people only rarely leave their home after night has fallen. Connected households, on the other side, use fluorescent tubes, incandescent light bulbs, and energy saving compact fluorescent light bulbs on average to the same extent (per household 0.5 of each on average), while the latter two are more popular for in-house lighting. On average 2 to 2.5 bulbs light the households that mainly consist of a single building with different chambers. The use of energy saving bulbs has been strongly supported by the government in recent years and has increased since the implementation of the surveys to the detriment of incandescent light bulbs.

Among the connected households, 8 percent developed any novel activity potentially in need of electricity, such as commerce, milling, welding or sewing. Sewing, a typical home business activity, however, primarily remains an activity that is mechanically powered – even in electrified regions. Only eleven households at all own a sewing machine. Concerning other electrical appliances, only radios and mobile phones can be found in the majority of households in electrified villages. Television sets are present in less than 27 percent of connected households. Yet, more than every second connected household uses electrical appliances beyond lighting, radios and mobile phones. In the non-electrified villages, hardly any electrical appliance is used.

4. Impact Assessment

4.1 Research questions and indicators

The conceptual framework underlying this research is straightforward and in principle based on the results chain[8] of many electrification interventions: Electricity is newly provided to a region and a certain share of households gets connected to the grid, which is translated into poverty reduction via different channels. While for those households that make use of electricity this might happen directly, also the non-connected might benefit from spillover effects or improved social services. DfID (2002) and UN (2005), for example, establish linkages between electricity and most MDGs. Not all of these hypotheses can be investigated with the household level data we have at hand.

In this paper, we focus on impacts on the directly connected households. Four indicators are examined: (i) lighting hours, (ii) time that children use lighting for studying at home, (iii) energy expenditures per household member, and (iv) income per working-age household member. The rationale behind these indicators is as follows: As many impacts related to electrification are based on the usage of modern lighting, *lighting hours* is the first outcome that we examine. While most electrification experts and practitioners might consider this as a trivial question and take effects on this indicator for granted, from an evaluative point of view it is worth verifying if the service is actually taken up: This take up is a necessary condition for many MDG relevant impacts, as they relate to the increased usage of lighting. Furthermore, going beyond the narrow view on the MDG, cheap access to high quality lighting constitutes a major change in the life of rural households with potential long-term effects on many economic and social dimensions.[9] Without being able to investigate this potential transition in all aspects, we dedicate some effort to scrutinize the intermediate indicator of lighting usage. *Lighting hours* are measured by summing up the amount

[8] The *theory of change* of a development project is typically represented in a results chain that links the intervention's inputs and activities to its outputs and impacts.
[9] See FOUQUET AND PEARSON (2006) for considerations on the long-term psychological effects of improved artificial lighting usage.

of light consumed per day over all lighting devices.[10] In a second step, we use the *kids studying at home* indicator as an intermediate measure to approximate the transmission channel to ultimate educational impacts. We analyze the daily home studying time of primary school children only, since secondary education in Rwanda is commonly provided at boarding schools.

Third, in order to examine the extent to which electricity usage has materialized already in monetary terms, we look at *energy expenditures* as an indicator for increased disposable income after having paid the energy bill. Electricity is much more efficient as an energy source and, hence, cost savings are likely. On the other hand, one might expect increasing energy usage due to new appliances like television or increased lighting usage. Therefore, we inspect to what extent households effectively save money. In order to account for different household sizes and compositions, we standardize monthly energy expenditures values by dividing them by the number of "adult equivalents" in the household. The fourth indicator, *income*, is investigated, because it is frequently argued that electrification leads to increases in productivity or creation of new activities. In this case, we standardize the indicator by relating it to the number of working-age adults in the household and take the annual value.[11]

Descriptive statistics on these selected indicators are depicted in Table 2. The observation made for the socio-economic and demographic characteristics presented in the previous section holds for these variables as well: While differences between access and non-access villages are pronounced, these seem to be driven mainly by the connected households.

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[10] This indicator thereby is a conservative one, since we do not account for the improved lighting quality. This could also be done by summing up the lumen hours consumed.

[11] Both *energy expenditures* and *income* are expressed in terms of the local currency, Rwandan Francs (FRw), with a per US dollar exchange rate of 568.75 in 2009, 550 in 2008, and 585 in 2007.

Table 2: Descriptive statistics of indicators concerning potential electrification impacts

	Access villages			Non-access villages	|t-statistic| difference in means access vs. non-access
	total	connected	non-connected		
N	272	129	143	259	
Lighting hours per day	13.2	24.5	2.9	3.6	8.02***
Lumen hours per day	8,865	18,630	57	63	9.40***
Kids studying home (in hours)	0.84	1.12	0.56	0.69	1.53
Energy expenditures per adult equivalent (in FRw)	1610	2190	1150	790	4.57***
Income per work-age adult (in 1000 FRw)	366.6	562.5	196.8	162.3	4.31***

Note: ***,** and * indicate significance levels of 1%, 5% and 10%, respectively.

4.2 Identification strategy

From an impact evaluation perspective, our survey design serves to identify the impacts of the treatment *electrification* via two principal strategies. The first and obvious one is the comparison of household indicators before and after the electrification. For this purpose, the data collected in the *PSP Hydro* project villages serves as a baseline that needs to be complemented by a follow up survey capturing the socio-economic conditions after the electrification intervention. The second strategy is what we refer to as *ex-ante impact assessment*: By comparing households in the already electrified non-project regions (*access regions*) to those in the yet non-electrified project villages (*non-access regions*), impacts of electrification can be evaluated using cross-sectional methods. The results of this second strategy are presented in this paper.

In doing so, it has to be taken into account that service interventions in general are difficult to evaluate. Endogeneity can arise both at the level of villages and at the level of households. At the household level the evaluator encounters potential self-selection processes, due to which simply comparing outcomes of participants and non-participants may suffer from substantial biases (see

FRONDEL AND SCHMIDT 2005; RAVALLION 2008). As elaborated in PETERS (2009), this applies as well to the case of electrification interventions, where it is the choice of the individual household whether it connects to the grid or not. Households that decide to connect may do so for reasons that are potentially unobservable to the researcher and that, at the same time, affect the outcomes of interest. Such self-selection effects can be expected to substantially bias a cross-sectional impact evaluation that simply compares connected to non-connected households in the access region. In particular, using such a cross-sectional comparison, impacts on income as a major poverty indicator can hardly be evaluated. The reason is that households with higher income are more likely to raise the funds for connecting to the grid. This simultaneity of income and connection status implies that it cannot be disentangled if a household has a higher income because it is connected, or if it is connected because it has a higher income (see PETERS 2009).

To address these *selection-into-treatment* processes, we use the household data from both the access and the non-access regions: This survey set-up enables the comparison of connected households from the already electrified access region to *comparable* households from the non-electrified non-access region. That is, we use the information from the access villages on which types of household did connect and which types did not to identify among all households in the non-access villages those that are most likely to connect once access is provided. By thus including households from the non-access region we increase the probability of identifying the right counterfactual: households in the non-access region that properly resemble the connected households of the access region in every aspect but the fact that they have not received the intervention. Those households have not had the opportunity to self-select into the treatment. Thereby, selection and simultaneity biases can be eliminated to an arguably large extent. The fact that these households are located in non-access regions also guarantees that they have not benefited from spillover effects. If comparison households are selected from the same villages in which also the electrified households are located, such spillover effects – if they exist – would lead to an underestimation of impacts.

We implement this identification of comparable households by, firstly, estimating a probit model using observations from the access region only. The probit model regresses the connection status of a household on a number of covariates. Including households from the non-access region here does not make sense, since households in this region do not have the possibility to get connected. Instead, secondly, the coefficients from this probit model are used to predict the probability to get connected for each household in both the access and non-access region. These *propensity scores* are then used in the third step to implement different *matching approaches*, i.e. to determine a set of non-connected households from the non-access region that is matched to connected households such that two balanced and thereby equivalent groups are constructed. Electrification impacts can then be measured using differences in outcomes between the two groups.

The covariates to be included in the probit model have to fulfil some conditions: First, matching builds on the so-called *conditional independence assumption* (CIA): The outcome variables must be independent of the treatment (in our case grid connection of the household) conditional on the propensity score, i.e. the observed covariates. This assumption requires in our cross-sectional set-up that the covariates are *non-responsive* to the connection status (ROSENBAUM 1984; HARDING 2003). Furthermore, only covariates should be included that affect both the decision to connect and the outcome variable (SCHMIDT AND AUGURZKY 2001; CALIENDO AND KOPEINIG 2005). In the optimal case, one has pre-intervention observations at hand, for example household income at the time of the grid connection. Lacking these, we utilize variables that we observe after the intervention, but for which we assume that they, firstly, affected the decision to connect and that they, secondly, are not affected by the electrification intervention.

In our data the following variables meet the requirements of affecting both the decision to connect and the impact indicators as well as being non-responsive to the treatment: the household head's education in years of schooling and a dummy variable that indicates whether the head is male or female. Furthermore, the number of buildings the household inhabits and the number of rooms as well as a dummy variable on whether the floors are cemented are included in the probit model. All these covariates are intended to capture the pre-electrification income – in particular the housing variables are important as relatively inelastic wealth indicators. In addition, years of education also represent self-selection processes driven by information asymmetries. The number of buildings and rooms pick up the lighting demand also at the time of the connection.

The estimated propensity scores from the probit model are used in two ways to identify comparable households. First, we stratify the subsample from the non-access villages into those that are likely to get connected, once the grid is available, and those that are likely not to get connected (Section 4.4). The *hypothetically connected* households are then compared to the actually connected households. Second, we employ state-of-the-art propensity score matching algorithms to individually assign comparable connected and non-connected households to each other (Section 4.5).

These matching approaches address the self-selection processes described on the outset of this section. In addition, the placement of the electrification projects might be non-random and village differences can affect the outcome variables. While we addressed this in the careful selection of sites to be included in the study, we additionally account for potentially remaining regional dissimilarities between the electrified and the non-electrified regions by applying a difference-in-difference estimator.[12] For this purpose, the sample is divided into four groups: In the access region we have the connected and the non-connected households; in the non-access region the hypothetically connected and the hypothetically non-connected households.

As with ordinary difference-in-difference approaches, in which one has over-time data at hand, we consider the two groups in the access region as the counterpart to the *after* situation and the two groups in the non-access region as the *before* situation. While the idea in over-time data is to difference out secular changes over time that have nothing to do with the intervention under evaluation, our purpose is to sort out regional differences that are not related to the electrification status. The impact estimation based on the single difference between connected households and the hypothetically connected households gives us a comparison that is reasonably unbiased with regard to self-selection processes at the household level. Regional dissimilarities might remain that are not induced by the electrification status. Therefore, we additionally subtract the difference between the non-connected households in the access region and the hypothetically non-connected households in the non-access region. The underlying assumption is that if regional dissimilarities between the two regions exist, they become manifest in this second difference between the non-connected households in both regions and can thus be differenced out. A bias would only remain if the different characteristics of villages – for example their wealth status – would imply better impact potentials after electrification. For example, it might be the case that wealthier households increase their lighting demand stronger than poorer households in the wake of electrification.

[12] See Ravallion (2008) for a general description of cross-sectional difference-in-difference approaches. Applications can be found in JACOBY (2002), KONDO ET AL. (2008), MORDUCH (1998), and PITT AND KHANDKER (1998).

While it cannot be ruled out completely, this effect can be expected to be quite small.

4.3 Propensity score estimation

Table 3 depicts the results of the estimated probit model with the connection status as dependent variable and using households from the access region only. As can be seen, most variables have statistically significant coefficients at the 1 % level and the fit reflected by the Pseudo R2 is fairly high at 0.36.

The coefficients from this model are used to predict the probability to connect, also referred to as *propensity score*. These propensity scores are used to stratify the non-access households into those that are likely to connect and those that are not likely to connect once electricity is available. We assume that those households that exhibit a propensity score larger than 0.5 belong to the former group. We refer to them as the *hypothetically connected*. The quality of this prediction can be examined by means of the estimated propensity scores of effectively connected and non-connected households in the access region. In fact, we correctly predict the decision to connect for 78 % of households who decided to connect and for 75 % of households who decided not to connect.

Table 3: Probit regression of connection status on decision-to-connect determinants

Covariate	Coefficient	Standard error	p-value
Years of education of HH head	0.058***	0.224	0.01
Female HH head	-0.151	0.245	0.54
Floors are cemented	1.322***	0.195	0.00
Number of buildings	0.416***	0.108	0.00
Number of rooms	0.219***	0.074	0.00
Pseudo R2	0.363		
Likelihood ratio test statistic (X^2)	130.93***		

Note: ***,** and * indicate significance levels of 1%, 5% and 10%, respectively.

4.4 Stratified matching

We use this stratification to obtain a comparison group for the connected households that is more appropriate than the non-connected households from the access region. Indeed, comparing connected households to the hypothetically connected households from the non-access region we see, as shown in Table 4 that the difference in means decreases substantially for all outcome variables and becomes insignificant for *kids studying at home* and *energy expenditures* per capita. Lighting hours remain – in line with intuition – strongly significant. Also the difference on *income* is still significant at the 5 % level.

Table 4: Comparing connected and hypothetically connected households

	Access region		Non-access region	|t-statistic| for test on difference in means…	
Outcome Indicator	**Connected HH**	**Not connected HH (access region)**	**Hypothetically connected HH (non-access region)**	**connected vs. non-connected**	**connected vs. hypothetically connected**
Lighting hours per day	20.4	2.9	6.89	13.4***	7.5***
Kids studying at home (in hours)	1.12	0.56	1.08	4.1***	0.2
Energy expenditures per adult equivalent (in FRw)	2192	1169	1462	3.1***	1.6
Total HH income per work age adult (in 1000 FRw)	564.2	191.5	392.5	5.6***	2.0**

Note: The six households (HH) in the non-access regions who own a generator do all exhibit a propensity score larger than 0.5 and belong to the hypothetically connected group. ***,** and * indicate significance levels of 1%, 5% and 10%, respectively.

We found in Section 3 – in line with methodological expectations – that the comparison of connected and non-connected households in the access region is not appropriate due to strong differences in socio-economic characteristics. Likewise, the question now is to what extent the comparability of the groups has improved by identifying the hypothetically connected households.

As proposed by ROSENBAUM AND RUBIN (1985) we scrutinize this by looking at the differences in means on the covariates between the connected and the hypothetically connected households. As can be seen in Table 5, the difference between the groups to be compared becomes substantially smaller if the non-connected households from the access region are replaced by the hypothetically connected ones from the non-access region. In the case of the cemented floors covariate the sign of the difference even turns around and is now significantly negative.

Table 5: Balancing between connected and hypothetically connected households

Covariate	Connected vs. non connected HH in access region	|t-statistic| on difference in means	Connected vs. hypothetically connected HH	|t-statistic| on difference in means
Years of education of HH head	3.5	6.5***	0.7	1.1
Female HH head	-0.12	2.4**	-0.05	1.0
Floors are cemented	0.55	10.8***	-0.18	3.6***
Number of buildings	0.74	6.6***	0.10	0.7
Number of rooms	0.88	5.2***	-0.04	0.2

Note: ***,** and * indicate significance levels of 1%, 5% and 10%, respectively.

In addition, a further matching quality indicator is applied as proposed by SIANESI (2004): The probit model regressing the connection status on covariates is estimated first with all households and then with the matched ones only. By comparing the Pseudo-R2 before and after, we can see if any systematic difference in the distribution of covariates between connected and non-connected households remains. The Pseudo-R2 will fall after matching if a balance improvement is expected.[13] Running the probit model again using this time the connected and hypothetically connected households only gives us a Pseudo-R2 of 0.08 compared to 0.36 using the households from the access region only (see Table 3). While the substantial decrease is supporting the success

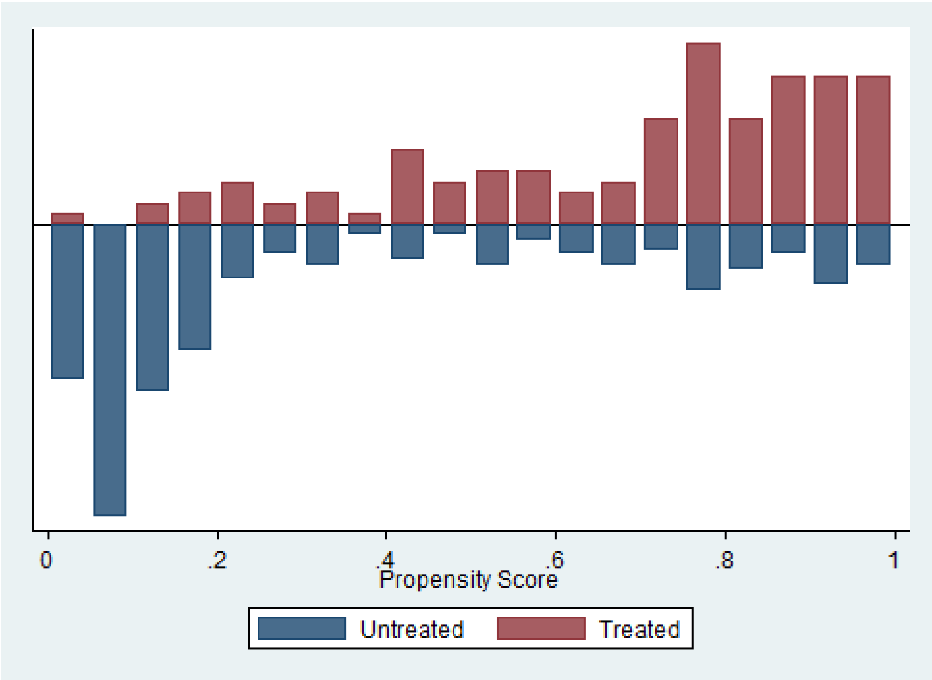
[13] These matching quality tests are also used in BECERRIL AND ABDULAI (2010) and PETERS, VANCE, AND HARSDORFF (2011).

of the matching procedure, the respective Chi Squared statistic still shows a joint significant influence of the covariates. In sum, the prediction of hypothetically connected households seems to yield a more reasonable comparison group, although slight discrepancies remain as indicated by the non-balancing in one covariate and the explanatory power of the post-matching sample.

Further examination of the propensity scores in Figure 2 shows that this persisting imbalance of the two groups stems from an unequal distribution of the propensity scores in the two groups. While a common support between the groups is given across the full range of the estimated score, a number of connected *treated* households exhibits very high propensity scores while only few non-access *untreated* households do so. For the calculation of the treatment effect depicted in Table 4 all non-access households with a propensity score larger than 0.5 are drawn on – equally weighted irrespective of their individual propensity score.

Therefore, in Section 4.5 we make more detailed use of the information contained in the propensity score by individually assigning matching partners, or using weights accordingly. An additional benefit compared to the stratification approach is that the comparability is improved by also including non-access households with propensity scores below 0.5. Although the predictive quality of our probit model is quite satisfactory, still 20 % of connected households exhibit propensity scores below this benchmark.

Figure 2: Propensity score distribution of connected households and non-access households



Note: Treated refers to the connected households in the access region, while the untreated households are the non-connected ones in the non-access region.

4.5 Nearest neighbour matching

In the following, we use the estimated propensity scores from the probit model in Table 3 to match connected households individually to the hypothetically connected households, our control households. We start by matching the *nearest neighbour without replacement* (NN). For each connected household, the NN algorithm picks a control household that has the closest propensity score for comparison. This is done as long as the connected household is in the area of common support where the propensity scores of the control households overlap with those of the connected ones. Accordingly, connected households that exhibit propensity scores higher than the highest control propensity score or lower than the lowest control propensity score are dropped[14]. Hypothetically connected households act as control households only for a single connected

[14] The results are robust to the employment without common support.

household (without replacement).

Table 6: Treatment effects using propensity score matching (connected vs. control households)[15]

Outcome Indicator	Treatment effect	|t-statistic| for test on treatment effect	Number of observations treated	untreated
Lighting hours per day	13.8	9.22***	122	399
Kids studying at home (in hours)	0.31	2.20**	74	201
Energy expenditures per adult equivalent (in FRw)	626.4	1.48	102	382
Total household income per work age adult (in 1000 FRw)	204.2	2.58***	122	399

Note: Only observations on support are counted. The sample for kids studying at home comprises only households that have children at primary school age. ***,** and * indicate significance levels of 1%, 5% and 10%, respectively.

The matching results are depicted in Table 6. *Lighting hours* are substantially higher in connected households with the difference being significant at the 1 % level. The magnitude of the difference amounting to more than 13 hours becomes clear when looking at the average levels of lighting consumption among connected and control households, which are at around 25 and 3 hours, respectively (see Table 2 in Section 3). For the *kids studying at home* indicator we find a small difference: Primary school children in connected households study around 20 minutes more per day than children in control households. Energy expenditures are higher in connected households, also after matching. While the difference of around 700 FCFA is at best marginally significant given levels of 14 %, respectively, the coefficient is of notable magnitude taking into account the average weekly per adult equivalent energy expenditure in the region of 1250 FCFA. Apparently, take-up of new services and more intensive lighting usage overcompensates the efficiency increase

[15] As a robustness check, we also applied a Kernel Matching algorithm. The results do not change in levels. The difference in the *kids studying at home* indicator is only significant at the 12% level only, while the *energy expenditures* indicator becomes significant at 6%. All other significance levels remain constant.

after the switch from traditional sources to electricity.

The *income* indicator also shows a considerable difference between the matched groups: While the average income is 276 000 FCFA, the matched difference is at around 200 000 FCFA and statistically significant using both matching algorithms. Given the observation that connected households do not use many electric appliances or machines that can be used for income generating productive purposes, at least the magnitude of this difference comes as a surprise. It raises concerns about remaining differences between the access and the non-access region that are not removed by matching and, therefore, bias our impact estimates. It is therefore essential to look into the success of the matching approaches, i.e. to check to what extent the balancing has improved after matching.

For this purpose and again in line with ROSENBAUM AND RUBIN (1985), we first look at the differences in means of the included covariates for connected and control households. Concretely, we look at the respective *t*-statistics for the unmatched sample and the matched one using the NN algorithm. In Table 7, we therefore show the difference in means differentiated by the respective outcome variable. With the exception of *years of education* the difference is non-significant in the matched sample.

In addition, we compare the Pseudo-R2 as described in Section 4.2 (see also SIANESI 2004). For all outcome variables matching decreases the Pseudo-R2 considerably. The Chi squared statistic testing the joint significance of all covariates is significant at the 1 % level before matching and becomes insignificant after matching. This indicates that there is a systematic difference between the groups of connected and non-connected households that disappears if the matched non-connected households are taken to from the new comparison group.

Table 7: Balancing on covariates: t-statistics for test on difference in means between treatment and comparison group

	Lighting hours and income		Kids studying at home		Energy expenditures	
Covariate	**Unmatched**	**Matched**	**Unmatched**	**Matched**	**Unmatched**	**Matched**
Years of education of HH head	8.56***	1.66*	6.89***	2.18**	7.28***	0.63
Female HH head	1.48	0.18	-2.41**	-1.13	-1.00	0.37
Floors are cemented	10.88***	-0.01	8.89***	0.81	9.72***	0.01
Number of buildings	7.19***	1.36	5.48***	1.11	6.29***	0.65
Number of rooms	5.52***	-1.05	5.18***	0.58	5.28***	-1.27
Pseudo R2	0.24***	0.02	0.30***	0.04	0.22***	0.01

Note: Although the matching process is the same in principle for all outcome variables, the test statistics and Pseudo R2 are different because of different numbers of observations due to missing values or not applicable items. Only *Lighting hours* and *income* are based on the same number of observations. We conducted t-tests on the differences in means for the individual covariates and Chi squared tests for joint significance. ***,** and * indicate significance levels of 1%, 5% and 10%, respectively.

These standard matching quality indicators underpin the success of our matching approach. However, as could be seen in Table 1 some potentially relevant wealth and occupation indicators that have not been included as covariates and, hence, are not included in the balancing check in Table 7, were significantly different in the two regions before matching. It is straightforward that they could also affect the outcomes under investigation, most importantly *energy expenditures* and *income*, to a lesser extent also *lighting hours* and *kids studying at home*.[16] While one might expect that these variables become as well more balanced by the matching procedure, any remaining dissimilarity between these two groups could lead to biases in our estimations, at least for the outcome indicators *income* and *energy expenditures*. Therefore, Table 8 shows the *t*-statistics on

[16] For robustness checks we also included these variables in our propensity score matching approach as covariates. The results do not change substantially: Direction of signs, level, and significance remain constant.

differences in means for those variables in Table 1 that were structurally different before the matching process.

Table 8: Balancing in other variables not included as covariates

	Lighting hours and income		Kids studying at home		Energy expenditures	
Covariate	**Unmatched**	**Matched**	**Unmatched**	**Matched**	**Unmatched**	**Matched**
HH owns the house	-1.63*	-1.11	0.49	0.56	-0.89	-0.87
Stone or brick wall	11.30***	4.54***	10.40***	5.31***	10.13***	3.93***
Ownership of fields	-2.55**	-2.46**	-0.43	-0.24	-1.55	-1.82*
HH head mainly subsistence farmer	-6.66***	-1.37	-5.38***	-2.42**	-5.3***	-0.30
Father occupied in public service	5.76***	0.68	4.63***	1.40	5.64***	0.81
Mother occupied in public service	9.55***	4.32***	5.93***	2.71***	9.32***	4.10***
Bank account ownership	7.85***	1.05	6.72***	1.30	7.04***	0.51

Note: Although the matching process is in principle the same for all outcome variables, the test statistics are different because of different numbers of observations due to missing values or not applicable items. Only Lighting hours and income are based on the same number of observations. ***,** and * indicate significance levels of 1%, 5% and 10%, respectively

For some of the variables that have not been included as covariates the balancing improves substantially. It is suspicious, though, that irregularities remain, in particular for the construction material of the walls, indicated by the *stone or brick wall* dummy. Apparently, the share of households that has mud walls in the non-access region is too high so that the matching algorithms do not find enough households with stone or brick walls as matching partners to the connected households in the access region with stone or brick walls. The same applies for the *mother occupied in public service* variable. The difference cannot be overcome by the matching approaches to a satisfying extent. This provides some indication for the suspicion that general differences in terms of wealth and income between the two regions remain, although the balancing clearly improves for virtually all other wealth indicators and non-responsive income indicators. To account for this, we complement our empirical analysis using a difference-in-difference approach in Section 4.6.

4.6. Difference-in-difference estimation

Using the results from Section 4.4, we stratify our sample into four groups: (i) connected and (ii) non-connected households, both in the access region; (iii) hypothetically connected and (iv) hypothetically non-connected households, both in the non-access region. As with the ordinary over time difference-in-difference approach, we implement the approach by simply regressing our outcome variable on three dummies: First, one for the access region, second, one for whether the household is *either* connected *or* hypothetically connected, and, third, one for whether the household is connected (technically the interaction of the former two). While the first two dummies take out the regional effect and the selection-bias effect, the third dummy gives us the residual effect net of both regional differences and selection effects.

Table 9: Treatment effects using difference-in-difference (DD) estimation[17]

Outcome Indicator	Treatment effect	|t-statistic| for DD estimator	Number of observations
Lighting hours per day	13.4	8.58***	542
Kids studying at home (in hours)	-0.03	0.18	287
Energy expenditures (in FRW)	193.3	0.39	504
Total HH income per work age adult (in 1000 FRW)	65.1	0.63	542

Note: Sample for kids studying at home comprises only households that have children at primary school age. ***,** and * indicate significance levels of 1%, 5% and 10%, respectively. Standard errors underlying the presented *t*-statistics are robust to the clustering of observations at the village level.

[17] The DD-coefficient for lighting hours remains significant at the 1% level if control variables like education or various wealth proxies are included in the regression. Likewise, the DD-coefficients for the three other outcome indicators remain non-significant if we account for these control variables. Furthermore, the results remain constant if observations from the only village in the south of the country, Mazimeru, are dropped.

As can be seen in Table 9, the results on *lighting hours* from the matching approaches can be confirmed in a striking way: The level of the treatment effect remains virtually constant and significant at the 1% level. The *kids studying at home* and the *energy expenditures* indicators – formerly significant at weaker levels – decrease substantially and become clearly non-significant. The same applies for the treatment effect on *income*, which decreases from a significant difference of around 200,000 FRW to a non-significant difference of some 65,000 FRW. It seems that the difference-in-difference method succeeds in removing regional differences that appear to have induced the significant results on the *income* variable in the single-difference estimations. The strong service take-up in electrified households as evidenced by the significant impacts on lighting hours prove to be robust also when we account for regional differences.

5. Conclusion

This paper examines the effect of electrification status on lighting usage, the time children dedicate to studying at home, energy expenditures, and income using cross-sectional data from rural Rwanda. The household data was collected in villages with access to the grid of at least four years duration on the one hand and in villages without access to grid electricity on the other hand. These villages were selected according to specific comparability criteria and in line with exogenous project placement considerations to avoid endogeneity problems that are typical in the evaluation of infrastructure interventions. The inspection of socio-economic living conditions reveals that, nevertheless, differences between the two types of villages exist that are, however, mostly driven by the connected households. We therefore apply a propensity score method in order to identify households in the non-access regions that are likely to connect to an electricity grid – if it were available. Referring to these households as the *hypothetically connected households* and comparing them to the already connected households we approximate a more proper counterfactual situation and respond to distorting effects of selection into treatment processes that can be expected to be rather strong in electrification interventions. Since suspicions about regional differences between the access and the non-access region remain that might also affect our outcome variables – particularly energy expenditures and income – we additionally apply a difference-in-difference approach that accounts for such regional differences.

The impact indicators we investigate are hours of lighting usage, the time primary school children dedicate to studying at home, energy expenditures, and income per working-age adult. We find strong and significant effects on lighting hours that are robust across all methods, thereby confirming that the service is actually used by the households. We also obtain small positive effects on the *kids studying at home* indicator, which are significant at reasonable levels for the matching approaches. As soon as we account for regional differences, the effect decreases and becomes insignificant. In terms of *energy expenditures* one could theoretically expect connected households to pay less for energy sources; in contrast, though, we find a higher energy bill among the connected households. Again, the significance on this result disappears if the difference-in-difference approach is applied. This might nevertheless indicate that increased energy consumption due to new appliances like television outweighs the efficiency increasing effect for lighting devices. Also for *income* the matching approaches show a significant difference between connected households and their matched non-connected counterparts. In line with expectations – the households in the access region hardly use electricity for income generating activities – the difference turns out to be non-significant if regional dissimilarities are taken into account by the difference-in-difference approach.

Altogether, the non-robustness for effects on *kids studying at home*, *energy expenditures* and *income* outcomes as soon as regional differences are accounted for increases the credibility of the positive effect that can be detected for *lighting hours* – robustly across all methods. While the lack of evidence for impacts on the higher aggregated indicators might sound disappointing, the importance of the substantial take up of electric lighting must not be underestimated. Although the linkage to MDG relevant indicators is certainly not always visible in the short run, electric lighting can be expected to change life in newly electrified communities profoundly and sustainably. As qualitative communication with many villagers in different African countries has shown, it is first and foremost lighting that forms the major appeal of electricity for rural people. Referring to the historical development in Europe, FOUQUET AND PEARSON (2006) might not exaggerate in stating that the improvements in access to high quality lighting "may have changed the way we think about and sense the world – less dependent on the sun and moon, less afraid of the dark and distancing ourselves from the communal fire". They furthermore claim that "our ability to live and work in a well-illuminated environment has radically transformed the economy and society of industrialized countries". These impacts have to be taken into account in the evaluation of electrification policies and, hence, in further research. This might include studies that examine the long-term effects of lighting access as well as the willingness-to-pay of rural people for electricity, thereby gauging the *true* value that non-access people assign to modern energy. This interpretation makes electricity an end in itself, i.e. a direct development indicator, rather than a means to achieve other development indicators like the MDG. This is also reflected in the new *Multidimensional Poverty Index* (MPI) proposed by UNDP that builds up on the famous *Human Development Index* (HDI): One out of ten dimensions of poverty (or: "deprivation") in the MPI is lack of electricity (UNDP 2010).

Methodologically, the analysis in this paper contributes to the development of cross-sectional evaluations of electricity take-up and impacts – also before the project starts. For this purpose, though, two regions with and without access to electricity have to be included in order to address selection-into-treatment processes at the household level. In doing so, potential regional differences have to be taken into account: Either by a large sample of villages to account for village level variation using econometric tools or by a proper selection of villages. In addition, it might be necessary to apply a double difference approach if substantial regional differences between the treatment and control group remain in spite of a proper village selection. These regional differences can be netted out by the double difference estimation.

The prediction of connection probabilities used in Section 4.3 seems to be quite successful. While not in the focus of this paper, this method could as well be used to approximate not only impacts but also electricity consumption in areas that do not dispose of electricity access yet. This information is of high relevance for the dimensioning of the power plants and grids to be established. The grid's and plant's dimension, in turn, is a major cost determinant in mini-grid electrification projects.

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