Productive Use of Energy – PRODUSE
Measuring Impacts of Electrification on Small and Micro-Enterprises in Sub-Saharan Africa

Methodology

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ESMAP Energy Sector Management Assistance Program

THE AFRICA ELECTRIFICATION INITIATIVE
L'INITIATIVE D'ELECTRIFICATION DE L'AFRIQUE

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Methodology

By Jörg Peters and Colin Vance[17]

1. Potential Linkages between Electricity and Income Generation

The productive or income-generating use of electricity is seen as a promising way to contribute to poverty reduction. Micro-enterprises are frequently considered to be one of the main beneficiaries of electrification and, in turn, an important contributor to income generation – above all in rural communities. The empirical relationship between energy input and firm productivity, however, is mainly based on anecdotal evidence (Meadows 2003). This is particularly true for Sub-Saharan Africa where the relation has not yet been systematically investigated (see *Chapter 2* for a systematic literature review).

Theoretically, several transmission processes are possible through which improved electricity access might translate into higher income. This section briefly reviews these potential pathways, which, as displayed in *Figure 4*., are categorised by refrigeration, communication, motive power and lighting. Motive power is arguably the most important trigger for the generation of cash income given that mechanisation and automation typically allows achieving higher outputs at constant inputs.

The usage of electrical energy for lighting has several advantages compared to lighting with traditional fuels like kerosene lamps. Most importantly, electricity for lighting – unlike kerosene or petroleum – is delivered directly to the firm, is of higher quality and is much cheaper. This might render extended operating hours profitable. Furthermore, electric lights, fans, radio or television might attract additional customers.

In addition, firms can become more productive from electricity-based information and communication technologies, which reduce information costs. Electricity also facilitates the preservation of goods via refrigeration devices and, thereby, might bring down the costs of such goods.

All these effects lead directly or indirectly to higher productivity in the sense that less input is needed to produce the same output. This increased productivity might either lead to higher profits for the firm owner or higher incomes for workers. Electricity usage, thus, ultimately leads to income generation in the form of higher firm owner's income, higher employment or higher wages. The following country studies examine employment and wages as well as many intermediate outcomes such as lighting or machinery usage. The focus, however, is on owner's income and in particular on profits as the major firm performance indicator.

2. The Treatment: Availability and Connection

The ultimate objective of this study is assessing the extent to which electricity affects income generation in micro-enterprises. In general terms, the evaluation literature refers to an intervention as the *treatment*. This section presents the different interpretations of electrification as a treatment. Furthermore, it illustrates the relation between the outcome variable *firm profits*[18] and electrification as well as other input factors such as labour and capital.

Firm performance can be described by a standard production function f that determines firm's profits Y as a function of electricity S and a vector of additional determinants X. This vector includes the complementary factors capital and labour.

$$Y = f(X,S) \tag{1}$$

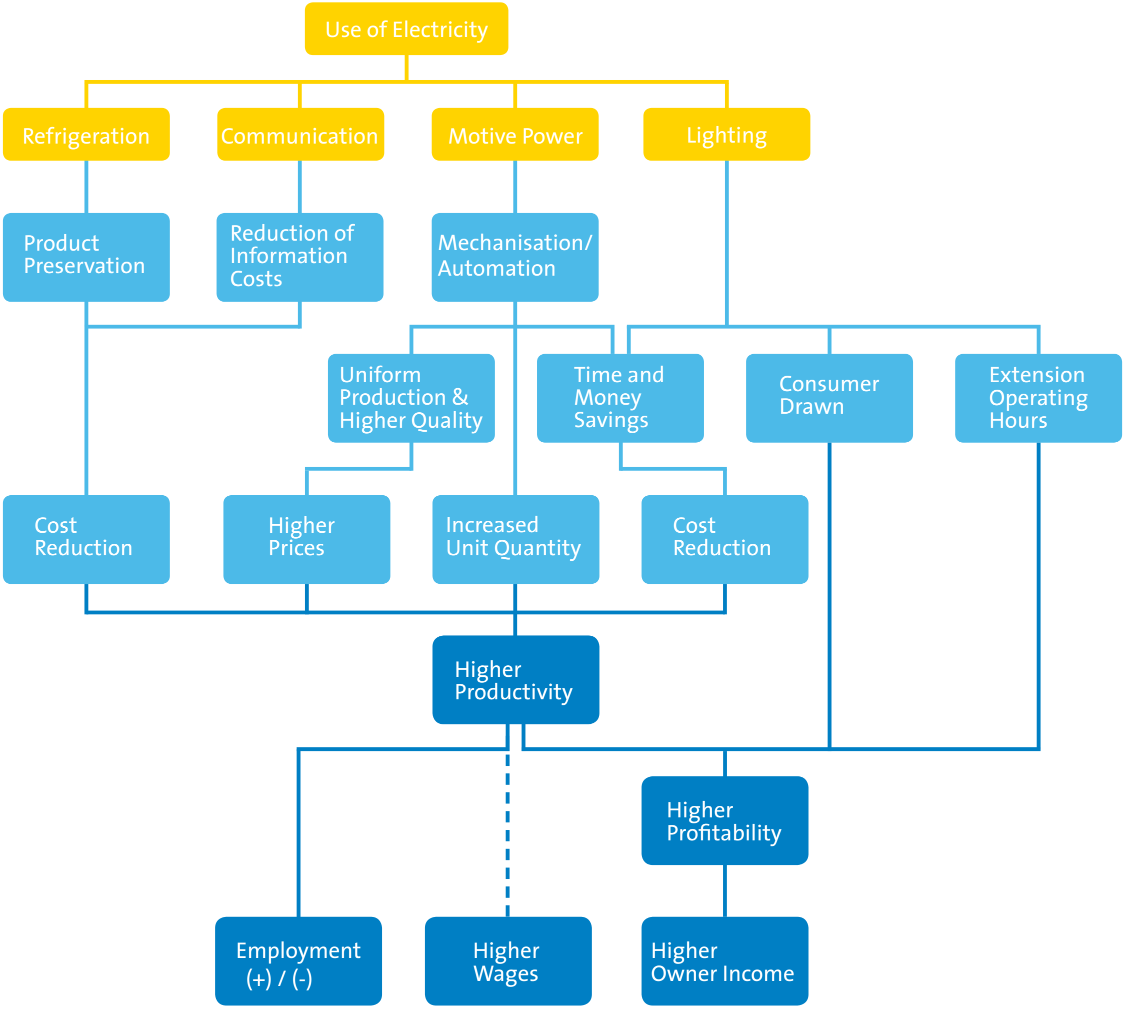
Given the complementarity of these factors, an increase in, for example, energy infrastructure leads to higher productivity of the other factors.[19]

17) The authors are grateful for valuable comments and suggestions by Benjamin Attigah, Nadja Kabierski-Chakrabarti and Kilian Reiche.

18) The discussion in this section is essentially transferable to other outcome variables such as employment or wages.

19) See Straub (2008) for presentation of the general case of infrastructure in a production function.

Figure 4: Pathways from Electricity to Income Generation



There are two possible definitions of the dummy variable S: the first definition focuses on principal *availability* of electricity, meaning that *S* equals unity if the firm is located in a region that is covered by an electricity service provider, no matter whether the firm is connected or not, and *S* equals zero otherwise. Note that *S* would also equal zero for a firm in a non-grid-covered region, even if it possesses an alternative electricity source such as a generator.

Second, one might also be interested in the effect of directly receiving the service. In this case, *S* equals unity if the firm is connected to the electricity grid and zero if it is not. In addition, S equals one if a firm disposes of a generator or a SHS, be the firm located in a grid-covered region or not. Therefore, defining the treatment in this sense is referred to as the *connection* definition of *S*. Almost all impact evaluation studies on rural electrification implicitly apply this definition of the treatment. Until recently most evaluations of electrification programmes determined impacts by comparing households or firms that are connected to the electricity grid to those that are located in the same grid-covered region but that are not connected.[20]

20) Table 1 summarises advantages and disadvantages of different approaches to identify impacts."

Two evaluation problems arise from these definitions of the treatment *S*. First, if *S* indicates the actual *connection* to the electricity service, the causation expressed in (1) of S affecting *Y* may also run in the reverse direction. Firms that perform better are more inclined to connect to the electricity grid. Therefore, we face a system of simultaneous equations consisting of (1) and

$$S = g(Y,Z) \tag{2}$$

In other words, a firm's decision pertaining to the connection to the grid depends on output and a vector of additional determinants *Z*, which may include the components of the vector *X* in (1). In addition, Z comprises firm-specific characteristics such as distance to the distribution grid or personal relations to the electricity utility's staff. The main intuition behind (2) is straightforward: firms exhibiting a higher profit are more likely to have the funds to get a connection. This mutual relationship between the treatment and the outcome, commonly referred to as simultaneity, complicates the task of isolating the influence of firm connections on income.

If *S=1* indicates *availability* of electricity, the simultaneity reflected in (2) does not apply on the firm level (because S is then not a choice variable from the individual firm's perspective) but on community level. With respect to the decision on establishing a power grid, most rural electrification programmes take into account economic potential and ability-to-pay and, hence, typically resort to some measure of aggregate income. Thus, better-off communities are more likely to be electrified. At the same time, it can be expected that electrification also affects the income of the community.

A second evaluation problem occurs if components of *Z* are part of *X* and, in addition, unobservable. Consider the example of entrepreneurs that are more motivated or risk-taking. Because of these character traits, they might be more inclined to get a grid connection. At the same time, these generally unobservable characteristics certainly affect the outcome variable profit *Y*. Hence, differences in *Y* would be assigned to the connection *S* according to equation (1), even though they are in fact due to these unobservable differences in characteristics. This is commonly referred to as omitted variables or selection bias.

If *S=1* designates *availability* of the grid, an omitted variables bias might arise from community characteristics that are both part of *X* and *Z*.[21] One might imagine that, for example, smart local politicians affect the business environment and, hence, the individual income in a village. At the same time these politicians might be able to affect the probability that the national grid is extended to the village.

The self-conception of most rural electrification programmes is to provide access to electricity for the whole community, not only individual households or firms. While, as a matter of course, direct benefits to the firms that get connected to the grid are intended, the programmes typically also aim at generating benefits for those firms that do not get connected themselves. In fact, non-connected firms might benefit from, for example, using electricity at their neighbours or other firms. Likewise, they might suffer because connected firms now have a competitive advantage and attract more customers. In contrast to the application of the *connection* definition, applying the *availability* definition takes such spillover effects into account.

3. Identification Strategies

3.1. The Identification Problem

To determine the *true* effect of *S* on *Y* requires comparing the outcome variable after having received the treatment to the counterfactual situation of not having received it. In general, we denote the post-treatment outcome by $Y_t^{S=1}$ if the firm has received the treatment and $Y_t^{S=0}$ if not. For actually treated firms, the difference between these two, $G = Y_t^{S=1} - Y_t^{S=0}$, is the causal impact. In this case, $Y_t^{S=0}$ is the hypothetical counterfactual situation. In the following, several strategies to identify this causal impact are presented, taking into account the particularities of electrification projects. While for the present report we only have cross-sectional data to

21) See Augurzky and Schmidt (2001) for an examination of community-based effects in evaluation problems.

hand, the conducted surveys can be used as a baseline for subsequent evaluation of longer-term effects in future years. Therefore, we do not limit the presentation to the cross-sectional identification strategy but also introduce before-after techniques.

The frequency of outcome $Y_t^{S=0}$ and $Y_t^{S=1}$ across the population of firms depends on a set of characteristics X. One main interest in an impact analysis is on the average individual outcome change resulting from the project intervention. This *mean effect* of treatment on the treated is expressed in the following manner:

$$M = E(Y_t^{S=1}|X, S=1) - E(Y_t^{S=0}|X, S=1) \tag{5}$$

where E(.) denotes the expected values.

As is obvious, we can never observe both $Y_t^{S=1}$ and $Y_t^{S=0}$ for the same firm, since it is either targeted by the project or not. While $E(Y_t^{S=1}|X, S=1)$ can be easily estimated from a sample of treated firms, $E(Y_t^{S=0}|X, S=1)$, which measures the hypothetical output of these treated firms had they not been treated, is not observable. This is what Frondel and Schmidt (2005) refer to as the core of the evaluation problem. To solve this, we have to formulate *identification assumptions* that allow replacing the unobservable and, hence, not estimable $E(Y_t^{S=0}|X, S=1)$ with something that can be obtained by estimation from an existent dataset. In practice, this is only possible by finding a comparison group that serves to simulate the counterfactual situation for the treatment group.

3.2. Cross-Sectional Impact Evaluation

In this study, we use cross-sectional comparison to identify the impacts of electrification. In the PRODUSE case study from Benin, this is done before the intervention is implemented, which we refer to as *ex-ante impact assessment*. The methodological considerations and identification assumption are equal for ex-ante and ex-post cross-sectional evaluation. For both approaches, the intuition is that one group simulates the behaviour of the other: while in the ex-post case, the non-electrified firms simulate what would have been had there been no electrification programme for the now electrified, in the ex-ante case the already electrified firms simulate the behaviour of the now to be electrified firms.

In formalised terms, the identification assumption for cross-sectional comparison is:

$$E(Y_t^{S=1}|X, S=1) - E(Y_t^{S=0}|X, S=0) \tag{8}$$

In other words, it is assumed that electrified firms, if they – hypothetically – had no electricity, would behave and develop as the non-electrified do. We need two regions to investigate the impacts of *availability* of a service. These two groups have to be sufficiently comparable (see *Section 4.1*) to fulfil the identification assumption.

The *connection* treatment can be investigated without a comparison region. This is what the PRODUSE Ghana study and a couple of World Bank studies do (EnPoGen 2003a, EnPoGen 2003b, ESMAP 2003a, World Bank 2006. See also Peters and Vance 2011). In this case, firms that are connected to the grid are compared to those that are not connected, with both being located in the same region. Due to the simultaneity reflected in (2), validity of the identification assumption (8) is highly questionable and leads to an upward bias in the impact assessment. Furthermore, spillover effects positively affecting the outcome variable of non-using firms induce a downward bias if connected and non-connected firms in the same region are compared. Lastly, assumption (8) must not be undermined by unobservable variables that affect selection into treatment and the counterfactual no-treatment outcome at the same time. In total, investigating only one region and examining the difference between connected and non-connected firms requires strong assumptions to rule out selection, simultaneity and spillover biases.

One opportunity to improve the comparability of connected and non-connected firms is the application of **matching approaches** (see, for example Khandker et al. 2009a, 2009b).” . For this purpose, firms from the access region are matched to those from the non-access region with respect to specific observable characteristics that are *covariates* of the decision to connect. Covariates are variables that affect the decision to connect. The matching procedure is used to form a new and more comparable comparison group. The crucial step is the identification of appropriate covariates, which are required to influence the decision to connect but must not be responsive to the intervention. In this sense, the pre-intervention outcome Y_{t-1} is an appropriate covariate. Yet, in the case of cross-sectional comparisons, data on pre-intervention variables is frequently not available. In this case, variables such as the education of firm owners or assets like construction material and size of buildings can be chosen as covariates, as they can be assumed to influence the decision to connect but are not affected by electrification in the short to medium term. By basing the matching approach on such covariates, unobservable factors that are associated with the pre-intervention variables can be accounted for. In particular, the simultaneity bias resulting from (2) can be reduced.[22]

In principle, matching approaches can be used if only one region that has access to electricity is surveyed and connected and non-connected firms are compared. However, due to the strong selection into treatment process (most of the better-off firms are connected, most of the poorer firms are not) this is likely to yield bad matches (see, for example, Bensch, Kluve and Peters 2011 and Peters, Vance and Harsdorff 2011). As a consequence, there are only few partners of sufficient comparability that can be matched. In contrast, if both access and non-access regions are surveyed, non-connected firms from the non-access region can serve as matching partners to connected firms from the access region. Thereby, the probability of finding good matches is much higher. In the Benin study we apply a matching technique to form a new comparison group out of the non-access firms.[23]

Another possibility to deal with selection and simultaneity biases in comparing connected and non-connected firms is to find an identification variable that is correlated with the connection variable but uncorrelated with the firm's output variable. While such **instrumental variables** (IV) in general are not easy to find, it might even allow for identifying the causal effect without having a control region at hand. In the Ghana case study, we use firm location within the agglomeration as an instrument, which affects the probability of being connected but not the firm's profit.

Altogether, predicated on a good survey design and the appropriate analytical technique, cross-sectional data offers possibilities to identify the causal effect of electrification.[24] Since it approximates the long-term impacts of an intervention, cross-sectional estimation alleviates the problems of limited monitoring horizons. It cures the curse of lacking baseline data in many development projects and it can additionally be used for ex-ante impact assessments as done in this report.

3.3. Evaluation Strategies after an Ex-Post Survey

In addition to the immediate investigation of electrification impacts, the surveys conducted for this report were designed to deliver baseline data for robust ex-post evaluation. Therefore, this section presents possibilities to use the data once post-intervention surveys will have been conducted.

A frequently pursued approach is the **before-after comparison**, where $E(Y_t^{S=0}|X, S=1)$ is replaced by $E(Y_{t-1}^{S=0}|X, S=1)$, i.e. the expected income of treated firms themselves at $t-1$, the time before the implementation of the project, represent the comparison group.[25] For example, the profit of an electrified firm is compared with its profit before electrification. The identification assumption in this case would be:

$$E(Y_t^{S=0}|X, S=1) = E(Y_{t-1}|X, S=1) \qquad (6)$$

22) See, for example, Angrist and Krueger (1999), Caliendo and Kopeinig (2008) and Dehejia and Wahba (2002) for a description of how to effectively match observations.

23) For another application of this procedure see Bensch, Kluve and Peters (2011).

24) Ex-post cross-sectional comparison has been applied frequently in the evaluation literature. See, for example, Becchetti and Costantino (2008), Cuong (2008), Khandker et al. (2009a), Kondo et al. (2008), McKernan (2002), Morduch (1998) and Ravallion and Wodon (1998).

25) Khandker et al. (2009b) use household data from Vietnam before and after electrification to assess the impacts on income, expenditures and educational outcomes.

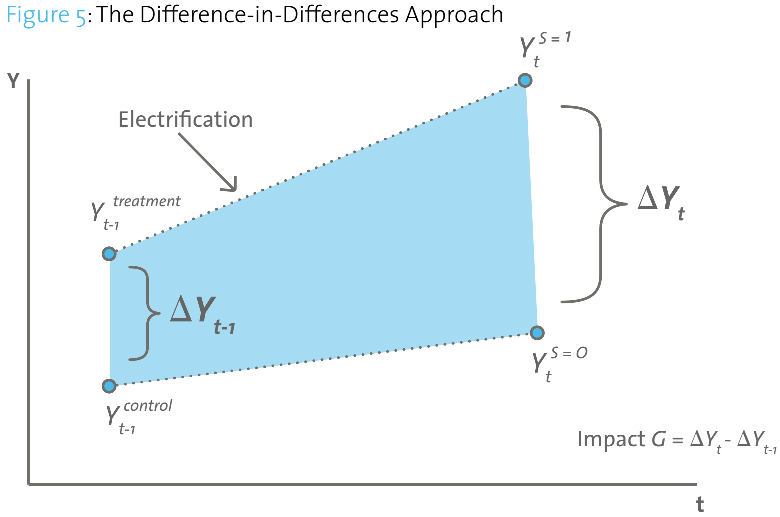
That is, one assumes that the firm's profit would not have changed from *t-1* to *t* if it had not received the treatment. This assumption is clearly violated if external factors affecting the firm's profit change from *t-1* to *t*. If conditions in the treatment region instead remain stable, the simple before-after comparison can be a valid identification approach. Yet, in the planning phase of the project the researcher does not know if the environment will change – and if it does, the change might not even be observable.

Hence, before-after comparisons often result in biased estimates of the treatment's effects if the external factors of change are not known. Since this imperfection of the method stems from the fact that it considers the treated group as its own control group, a possible alternative is to search for non-treated firms in order to determine the counterfactual. This is the approach pursued under so-called **difference-in-differences-estimation** (DD),[26] which in the traditional case compares changes in the outcome variable of firms that benefit from electrification to those that do not, as illustrated in *Figure 1*. The impact G is then determined as follows:

$$G = \underbrace{(Y_t^{S=1} - Y_t^{S=0})}_{\Delta Y_t} - \underbrace{(Y_{t-1}^{treatment} - Y_{t-1}^{control})}_{\Delta Y_{t-1}}$$

DD controls for changing external factors affecting the firm's profit variable. Furthermore, unobserved heterogeneity between firms that is constant over time is automatically accounted for by calculating the differences in outcomes for both treated and non-treated firms. Entrepreneurial spirit might be one example for this unobserved, time constant heterogeneity.

Figure 5: The Difference-in-Differences Approach



Accordingly, the identification assumption is weaker than that for before-after comparisons: the *change* in profits of treated firms in the hypothetical no-project-intervention scenario must equal the profit change of non-treated firms in the no-project-intervention scenario:

$$E(Y_t^{S=0} - Y_{t-1}^{treatment} | X, S=1) = E(Y_t^{S=0} - Y_{t-1}^{control} | X, S=0) \qquad (7)$$

In other words, the assumption is that in the absence of the intervention, the average change in *Y* for the treated firms would have been the same as for non-treated firms. It is to be kept in mind that the first expression in (7) is by nature not observable, while $E(Y_t^{S=0} - Y_{t-1}^{control} | X, S=0)$ can easily be estimated from a comparison group sample.

26) See Frondel and Schmidt (2005) as well as Ravallion and Chen (2005).

Using the different definitions of the electrification variable *S* presented in the previous *Section 2*, we encounter different identification possibilities using the DD-approach. Applying the *availability* definition of *S*, we require two regions that have to be surveyed before and after a project intervention: one that is not yet covered by an electricity provider but that will receive access to the service soon (treatment group) and another that neither has received nor will receive electricity coverage (comparison group). In order to meet the identification assumption (7), both regions have to fulfil certain conditions (see *Section 4*).

For the application of the *connection* definition of *S*, surveying only the region of the project intervention is sufficient under specific conditions. The treatment group then would consist of those firms that choose to use electricity directly, while the non-connected constitute the comparison group. Both have to be surveyed before and after the intervention. Yet, the assumption in (7) would be violated if the non-connected firms react to external changes, in particular to the treatment, differently than the connected ones. Moreover, spill-over effects within the grid-connected region have to be excluded, which might not be warranted in many cases. For example, non-connected firms might benefit from additional orders of connected firms or they might suffer from the loss of consumers who switch to the connected firms.

Table 9: Advantages and Disadvantages of Different Impact Evaluation Approaches

	Key Requirements/Assumptions	Advantages	Disadvantages
Before-after comparison	The observed change in the outcome variable for a firm has to be solely assigned to the electrification intervention.	No control group of firms not having benefited from the intervention is needed.	Very strong assumption considering that secular changes (e.g. changing energy prices, general economic growth) as well as effects of other development projects that may be present in the project area cannot be accounted for. Participants must already be identified at baseline.
Cross-sectional comparison	Control firms in control regions are comparable to the firms benefiting from the electrification intervention in terms of socio-economic and market access conditions.	Can be implemented when the intervention is already ongoing and no baseline data is available. Can be implemented before the intervention allowing for ex-ante assessment of the target region and likely impacts. The inclusion of control sites allows the capturing of secular changes.	Both the identification of genuinely comparable control regions and counterfactual firms can be difficult in practise.
Difference-in-difference estimation	The growth trends in outcome variables are the same for intervention and control group. Alternatively, unobserved characteristics are time-invariant between the two groups. Requires a baseline survey and, hence, that the target region of the project remains the same between baseline and follow-up survey.	Most robust non-experimental approach because unobserved heterogeneity between connected and non-connected firms is accounted for as long as it is time-invariant.	Requires two surveys in both the treatment and the control group (cost). Participants must already be identified at baseline and the same enterprises must be interviewed aga

As a matter of course, both the before-after comparison and the DD-estimation require data from both before and after the project intervention, which can, however, often not be fulfilled in practical evaluation scenarios. Many projects do not carry out adequate baseline studies at the time of the planning phase prior to the project's implementation. Furthermore, evaluation practitioners frequently overlook that ex-post surveys should be conducted only after sufficient time has elapsed since the beginning of the intervention, particularly in infrastructure projects (Ravallion and Chen 2005, Ravallion 2009). The reason is that consumers – be they households or firms – need time to adapt to the new situation after electrification. ESMAP (2003b), for example, notes that educational impacts can be observed ten years after the electrification intervention at the earliest. The monitoring phase, however, typically only covers around three to five years, including the planning phase before the actual hardware installation.

The ex-ante cross-sectional set-up that we apply in Benin, i.e. surveying the target region without electricity and an already electrified region, also allows for DD-estimation after an ex-post survey. The already electrified region provides for a benchmark that enables the comparison of differences. Principally, this already electrified region nets out fixed individual effects and the confounding influence of changing environments – in the same way as the region that remains non-electrified in the traditional DD-approach does. As in the traditional approach, the identification assumption requires that the average change in *Y* for the treated firms without an intervention would have been the same as for the comparison group. This breaks down to an additional assumption for this modified DD-estimation: the already electrified comparison group has to behave with respect to the change in *Y* as if it was not electrified. This additional assumption would be violated if a region is not only shifted to higher income level but also to a different growth level.

4. Study Implementation

4.1. Selection of Appropriate Control Regions

The studies in this PRODUSE report are based on cross-sectional data and try to compare firms from electrified regions to firms from non-electrified regions. In doing so, the survey covers ideally a variety of different village types in order to control for different levels of community characteristics or geographic conditions. In this way, the effect of electrification *S* on outcome *Y* can be disentangled from other observable effects like access to transportation, climatic conditions, soil quality or business opportunities (Kondo et al. 2008, Ravallion and Wodon 1998). However, in the present case, this ideal design is not implementable due to budgetary and other project restrictions. Instead, we were only able to design survey setups that cover 8 villages at most, which may not capture enough variation of village characteristics.

Under such restricted circumstances, comparability of electrified and non-electrified communities has to be assured during the selection of these communities. Village level parameters like size, demography, political importance and access to roads, transport services or telecommunication have to be checked in both regions. Most importantly, the business environment has to be similar. This can be ensured by taking into account local market conditions, the availability of cash crops, infrastructure, etc. Generally speaking, differences in local characteristics between the treatment and the control region that also influence the outcome variable Y have to be reduced as far as possible. For this purpose, the considered regions should be carefully scrutinised, based on information from the utility but not least also by an extensive field visit by researchers familiar with the study's purpose and methodology. The reason is that, although a checklist of general characteristics to be fulfilled in terms of comparability is crucial, it can hardly be comprehensive.

In most cases, such regions exhibiting sufficiently comparable conditions to the project's target region are available. Rural Africa, in particular, is only sparsely electrified, so that comparable non-electrified regions should be available abundantly. Finding comparable already electrified areas for the ex-ante cross-sectional analysis is harder because the few electrified rural communities are often business centres or otherwise privileged areas. In the Ghana case study, we were not able to identify an appropriate control region. The project and, hence, survey region is in peri-urban areas that are already grid covered. Comparable non-access regions do not exist in Ghana. The Benin approach succeeded in finding a comparison group. The two areas seem to be gradually comparable. In the Uganda case study it turned out that the selected electrified and non-electrified regions were not satisfactorily comparable, partly because the researchers were not sufficiently involved in the selection process.

4.2 Survey Approach

The studies are mainly based on a structured questionnaire covering virtually all areas of firm activity. In addition, availability and usage of infrastructure, Business Development Services and credits are addressed. With regards to the main variables used in the analysis – firm profits, capital, labour and energy – the idea was to relieve the respondent from aggregative calculation work as much as possible. Instead, the questionnaire tries to grasp these fields in enough detail to allow for the accurate derivation of variables not directly elicited. It is an ongoing discussion of whether to address, for example profits directly ('How much does your firm earn?') or by asking for the sold items and costs ('How much of good X and how much of good Y do you sell?'). While some survey practitioners argue in favour of eliciting profits or sales by a direct question, others opt for calculating profits from income and expenditure figures.[27] The justification for this latter approach is that – in addition to the ad-hoc calculation argument – many respondents feel uncomfortable about revealing their income due to tax-related concerns or to an aversion of exposing details about their financial situation. With the applied questionnaire, it can be decided on a case-to-case basis which approach is to be chosen.

Quantitative questionnaires are frequently criticised for being prone to misreporting errors. In particular, for financial questions it is suspected that respondents purposely understate their income or the value of assets. In general, critics claim that interviewed firms or households involuntarily give wrong answers – simply because they do not know the exact answer and can only provide estimates. The validity of these arguments is certainly beyond discussion. Resorting only to qualitative judgements as an alternative is, however, also not warranted. The reason is that misreporting on profits, for example, only poses problems if the extent of the error in the answers is correlated with the electrification treatment (Ravallion 2008). In other words, only if we suspect that firms that are more likely to misreport are also more (or less) inclined to get connected does misreporting induce a systematic bias across the sample. While distorting effects of misreporting – as a classical unobservable variable – via correlations with the treatment variable can never be ruled out, a priori it seems plausible to assume that there is no correlation between inaccurate answers and the decision to connect.

Quantitative data collected with the structured questionnaire is complemented by qualitative information gathered by open interviews with key informants and resource persons in the survey areas. Such additional insights are indispensable to understand the business environment in the villages and to complement the quantitative data. These qualitative interviews were conducted by junior researchers that were on the ground during the whole field work. Being familiar with the methodological issues outlined in this chapter, they acted as field supervisors at the same time. These junior researchers were not only responsible for the quality control, including consistency and completeness checks, they also reported methodological problems and pitfalls back to the central study team.

27) See De Mel, McKenzie and Woodruff (2009) and Daniels (2001).

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