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Evaluating Rural Electrification Projects

Methodological Approaches

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Jörg Peters*

Evaluating Rural Electrification Projects – Methodological Approaches

Abstract

In recent years, the international community has expanded efforts in programme evaluation to improve the accountability of development projects. This paper presents approaches to implementing state-of-the-art evaluations in rural electrification projects, taking into account specific challenges that researchers face in such interventions. Furthermore, it suggests a particular approach to assess impacts before an intervention is implemented by surveying the yet non-electrified target region of the project and, in addition, an already electrified region. Besides delivering robust evidence on impacts, results from such ex-ante evaluations provide insights for the project design, thereby reducing the gap between evaluation researchers and practitioners.

JEL Classification: O12, O22, C31, C81

Keywords: Impact evaluation, ex-ante impact assessment, electricity access, rural development

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1. Introduction

Rural electrification is widely considered to be a crucial prerequisite for development and the removal of barriers hampering economic growth: Electricity potentially increases the productivity of both farm and non-farm activities, facilitates household tasks, provides an efficient and clean lighting source, and enables provision of improved social services such as education and health care. There is a consensus among practitioners and donor organisations that considerable impacts in these areas might be achieved through electrification interventions.

At the same time, the international community has increased efforts of programme evaluation in order to improve the accountability of development projects. The methodological sophistication of some of these evaluations has increased substantially, as documented in Ravallion (2008a). Ravallion (2008b), however, still criticizes the dearth of rigorous evaluation research in development policies. As a consequence, knowledge about the efficacy of approaches is limited and lessons learnt are often not capitalised beyond the individual project.

In addition to experimental approaches, the analysis of panel data collected before and after the intervention constitutes one of the most promising avenues to program evaluation. Either way, however, is difficult to implement in practice: experiments are not feasible in most situations, largely due to ethical considerations, while the collection of panel data is often precluded by time and financial constraints. Rather, researchers encounter either a situation before the intervention is implemented or after it has ended, while appropriate baseline data is lacking. Furthermore, funds for evaluation research are mostly very limited, making large or even countrywide surveys impossible. This paper discusses possibilities to derive robust insights on the impacts of electrification using cross-sectional data of limited sample size.

In the field of rural electrification, extensive studies have been conducted by the World Bank that assess the impact of electrification by comparing connected and non-connected households within the same region (EnPoGen 2003a, EnPoGen 2003b, ESMAP 2003a, and World Bank 2006). The present paper discusses the appropriateness of comparing connected and non-connected households and proposes alternative evaluation strategies. The focus is on projects that systematically provide electricity to a specific region – be it via grid extension, grid densification, or decentralised electricity. The paper examines possibilities to embed efforts for evaluating rural electrification projects in modern evaluation research as presented in Ravallion (2008a), Frondel and Schmidt (2005), Schmidt (1999), or Angrist and Kruger (1999). It proposes pragmatic options to identify the counterfactual situation, taking into account limitations and demands specific to rural electrification programmes.

In particular, the paper argues in favour of examining impacts before the electrification intervention takes place. This can be done by surveying both the yet non-electrified project region and a comparable electrified region. The already electrified region then serves for simulations of the expected behaviour of households and changes of development outcomes following electrification. Results from such *ex-ante* impact assessments deliver insights for the project design, thereby reducing the gap between evaluation researchers and practitioners. In addition, the collected data can be used for robust ex-post evaluation if the opportunity to conduct additional surveying at the end of the project cycle should arise. Drawing from electrification projects in rural Sub-Saharan Africa[1], practical examples related to the different approaches are described. Nevertheless, most of the discussions in the paper are transferable to other continents and more urbanised intervention regions.

[1] The experiences underlying this paper are largely based on a cooperation between RWI and the German-Dutch Energy Partnership *Energizing Development* (EnDev), implemented by Deutsche Gesellschaft für Technische Zusammenarbeit (GTZ), as well as on the cooperation with the joint GTZ/World Bank research project "Income Generation through Electricity and Complementary Services (INGENS)".

The paper proceeds in Section 2 by providing background information on energy consumption patterns in rural Africa. Section 3 presents a conceptual framework of how electricity potentially affects household welfare. Section 4 elaborates different strategies to identify the impact of electrification and describes ways of implementing the approach. The last section summarizes and concludes.

2. Energy Consumption Patterns in Rural Africa

In order to discuss the methodological challenges in measuring the impacts of electrification, it is helpful to understand energy consumption patterns in rural areas in developing countries and to what extent these patterns are affected by electrification.[2] Especially in Sub-Saharan Africa, energy consumption is characterized by usage of low-level energy services. In households, energy is mostly used for lighting, cooking and simple entertainment devices. In the absence of electricity, households use kerosene in hurricane lanterns or wick lamps for lighting purposes, complemented by torches and candles. The common cooking fuels are wood or charcoal. Radios are driven by dry cell batteries and, sometimes, car batteries are used to run small televisions.

If the electricity grid is available, only around 20-50% of the households in the reach of distribution lines are connected to the grid. The most important reasons for households not to connect are in-house installation costs and connection fees. Connection fees in most African countries range between 50 and 150 USD. Even the lower boundary of this range is prohibitive for many rural African households. The concrete cost of connection depends on the subsidy scheme applied by the utility, in particular in relation to electricity meters and the cabling between the meter and the

[2] Information in this section is based on household and enterprise surveys as well as various field trips in Benin, Ghana, Mozambique, Rwanda, Senegal and Uganda. See Bensch and Peters (2009), Bensch, Peters and Schraml (2009), Harsdorff and Peters (2007), Neelsen and Peters (2009), Peters (2008), Peters, Harsdorff and Ziegler (2009), Peters, Vance and Harsdorff (2009) and Peters, Sievert and Vance (2009).

low-voltage grid. As a matter of course, the total costs of connection are significantly affected by the distance the household has to bridge to reach the village distribution grid. In addition, lack of credit or savings schemes and information about savings potentials of electricity compared to traditional energy sources such as petroleum or dry cell batteries hamper households from getting connected (Peters, Harsdorff and Ziegler 2009).

Those households that connect to the grid use electricity mostly for lighting, radios and – less frequently – televisions. Electricity is almost never used for cooking purposes. This is important to highlight, since health risks related to solid cooking fuel usage as well as time spent on wood fuel collection are hardly reduced. Rather, benefits for households stem from electric lighting that is both cheaper and of higher quality than its traditional counterpart.

Additionally, households are expected to benefit from electricity-using micro-enterprises that become more productive and generate higher incomes. The economy of rural areas in Africa is dominated by agriculture, mostly for subsistence but also for income generating purposes. In the non-farming sector, most enterprises are small and serve mainly local markets. Typical firms in non-electrified villages are service and commerce enterprises such as bars, shops or hairdressers. Less frequent are manufacturing firms such as carpenters and tailors. In enterprises, non-human energy is – as with households – predominantly provided by petroleum used for lighting purposes. In the absence of electricity, small machinery run by generators is in principle available. Yet, since operating costs of generators are prohibitive in many cases, electrification enables the establishment of new enterprise types that rely on electricity, such as welding. Furthermore, schools or health stations require energy, also mostly for lighting but as well for teaching purposes or refrigerators. Their services might be improved by electrification, translating into health and educational impacts on the household level.

3. The Treatment: Access to and Use of Electricity

The ultimate objective of impact evaluation is assessing the extent to which an intervention, in the evaluation literature generally referred to as the *treatment*, affects the welfare of households. The subsequent discussion illustrates how the provision of electricity as treatment can translate into poverty reduction. Without any loss of generality, we focus on household income as the outcome, because in most cases, our methodological considerations are transferable to other potential outcomes, such as education or health indicators as well as firm performance.

The outcome Y is determined by a function f depending on an electricity service variable S and a vector X that captures relevant household characteristics:

$$Y = f(X, S) \qquad (1)$$

For example, X might consist of education and health status as well as assets and household size. There are two possible definitions of the dummy variable S: The first definition focuses on *access* availability, meaning that S equals unity if the household is located in a region that is covered by a service provider, no matter whether the household is connected or not, and S equals zero otherwise. Note that S would also equal zero for a household 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 household is connected to the electricity grid and zero if it is not. In addition, S equals one if a household disposes of a generator or a Solar Home System, be the household located in a grid-covered region or not. Therefore, defining the treatment in this sense is referred to as the *use* definition of S. Almost all impact evaluation studies on rural electrification implicitly apply this definition of the treatment. Several World Bank related publications, most prominently EnPoGen (2003a), EnPoGen (2003b), ESMAP (2003a), and World Bank (2006), determine 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.

Two evaluation problems arise from these definitions of the treatment *S*. First, if *S* indicates the actual *use* of electricity, the causation expressed in (1) – *S* is supposed to affect *Y* – also runs in the reverse direction. The household's decision to connect to the grid, *S=1*, depends on its income *Y* and a vector of additional determinants, *Z*, jointly defining the function *g*:

$$S = g(Y, Z). \qquad (2)$$

The components of the vector *X* in (1) may be included in *Z*, as well. In addition, *Z* comprises household 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: Households exhibiting a higher income are more likely to have the funds to get a connection. This mutual relationship, commonly referred to as simultaneity, counteracts the purpose of isolating the influence of household connections on income.

If *S=1* indicates *access* to electricity, the simultaneity reflected in (2) does not apply, because *S* then is no choice variable from the individual household's perspective. With respect to the decision on establishing a power grid, most rural electrification programs take into account economic potentials and ability-to-pay and, hence, typically resort to some measure of aggregate income. The individual household and its income, however, are unlikely to affect the probability that the region in which it is situated is connected.

A second evaluation problem occurs if components of *Z* are part of *X* and, in addition, unobservable. Consider the example of households 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 income 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 potential *access* to the grid an omitted variables bias might arise from community characteristics that are both part of X and Z.[3] 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 programs is to provide access to electricity. While, as a matter of course, direct benefits to those households that get connected to the grid are intended, the programs typically also aim at generating benefits for those households that do not get connected themselves. In fact, non-connected households might benefit from, for example, using electricity at neighbours, electric mills or by working more productively in a now electrified enterprise. In contrast to the application of the *use* definition, applying the *access* definition takes this into account.

4. Identification Strategies

4.1. 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 household has received the treatment and $Y_t^{S=0}$ if not. For actually treated households, the difference between these two, $G = Y_t^{S=1}$ - $Y_t^{S=0}$, is the causal impact. In this case, $Y_t^{S=0}$

[3] See Augurzky and Schmidt (2001) for an examination of community based effects in evaluation problems.

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. The two definitions of treatment variable S presented in Section 3, *access* to and *use* of electricity, pose different identification problems.

The frequency of the actual outcome $Y^{S=1}$ and the hypothetical frequency of $Y^{S=0}$ across the population of households 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. Expressing this as conditional expectations, this *mean effect of treatment on the treated* is given by:

$$M = E(Y_t^{S=1} \mid X, S=1) - E(Y_t^{S=0} \mid X, S=1) \qquad (3)$$

where the expectations operator *E(.)* denote the population average.

As is obvious, we can never observe both $Y_t^{S=1}$ and $Y_t^{S=0}$ for the same household, since it either receives the project's treatment or not. While $E(Y_t^{S=1}|X, S=1)$ can be easily estimated from a sample of treated households, $E(Y_t^{S=0}|X, S=1)$, which measures the hypothetical output of these treated households 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. The identification strategy is successful if the estimation of $E(Y_t^{S=1}|X, S=1)$ and $E(Y_t^{S=0}|X, S=1)$ gets increasingly exact with increasing sample size and becomes precise when the sample size converges towards population size.

4.2. Before-after and Difference-in-Difference Estimation

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 treated households themselves at *t-1*, the time before the implementation of the project, represent the control group. For example, the income of an electrified household is compared with its income before electrification. The identification assumption in this case would be:

$$E(Y_t^{S=0} \mid X, S=1) = E(Y_{t-1} \mid X, S=1). \qquad (4)$$

That is, one assumes that the household's income would not have changed from *t-1* to *t* if it had not received the treatment. While this assumption can be violated if external factors affecting the household's income change from *t-1* to *t*, conditions in many rural areas in Africa can be assumed to remain stable over a monitoring period of, say, five years. In this case, 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.

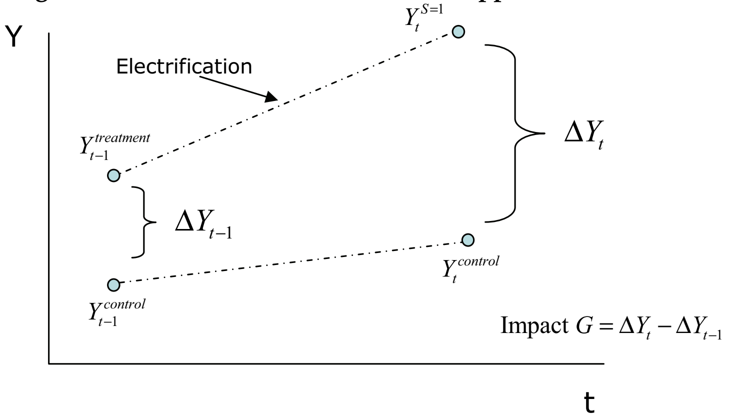
Hence, before-after comparisons can result in biased estimates of the treatment's effects if the external factors of change are not known. Since this imperfection of the method stems from the fact that it considers the treated group as its own control group, a possible alternative is to search for non-treated households in order to determine the counterfactual. This is the approach pursued under so-called **difference-in-differences-estimation** (DD),[4] which in the traditional case compares changes in the outcome variable of households that benefit from electrification to those that do not, as illustrated in Figure 1. The impact *G* is then determined as follows:

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

[4] See Frondel and Schmidt (2005), as well as Ravallion and Chen (2005).

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

Figure 1: The Difference-in-Difference Approach



Accordingly, the identification assumption is weaker than that for before-after comparisons. Under this weaker assumption, the *change* in outcomes of treated households in the hypothetical no-project-intervention scenario equals the outcome change of non-treated households in the no-project-intervention scenario:

$$E(Y_t^{S=0} - Y_{t-1}^{treatment} \mid X, S=1) = E(Y_t^{control} - Y_{t-1}^{control} \mid X, S=0) \qquad (5)$$

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

Using the different definitions of the electrification variable S presented in the previous Section 3, we encounter different identification possibilities using the DD-approach. Applying the *access* 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 nor will receive electricity coverage (control group). In order to meet the identification assumption (7), both regions have to fulfil certain conditions (see Section 4.4).

For the application of the *use* definition of *S*, surveying only the region of the project intervention is sufficient. The treatment group then would consist of those households that choose to use electricity, while the non-users constitute the comparison group. Both have to be surveyed before and after the intervention. Yet, one important disadvantage of not including a comparison group without access when using DD-estimation is that positive spillover effects from users to non-users potentially bias the results and might cause an underestimation of the impact. Furthermore, the non-connected households in the same region might react to external changes differently than the connected ones leading to a violation of assumption (7).

Both before-after comparison and DD-estimation require data from both before and after the project intervention, which can 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 2008b). The reason is that consumers 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, though, typically only covers around three to five years, including the planning phase before the actual hardware installation.

4.3. Ex-Ante Impact Assessment

While the identification assumptions related to the DD estimation are certainly the most convincing ones among non-experimental evaluation approaches, these practical restrictions cause difficulties. Predicated on a good survey design and the appropriate analytical technique, **cross-sectional comparison** of data collected at one point in time can address many of the problems related to before-after and DD-comparison. Specifically, by approximating the long-term impacts of an intervention, cross-sectional estimation alleviates the problems of limited monitoring horizons and lacking baseline data that characterize most development projects. In fact, ex-post cross-sectional comparison has been applied frequently in the evaluation literature.[5]

Here, the focus is instead on cross-sectional comparison conducted before the intervention, which we refer to as *ex-ante impact assessment*. The methodological considerations and identification assumptions 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 households simulate what would have been, had there been no electrification program for the now electrified, in the ex-ante case the already electrified households simulate the behaviour of the now to be electrified households.

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

$$E(Y_t^{S=0} \mid X, S=1) = E(Y_t^{control} \mid X, S=0) \qquad (6)$$

[5] See Becchetti and Costantino (2008), Cuong (2008), Kondo et al. (2008), McKernan (2002), Morduch (1998), and Ravallion and Wodon (1998) for applications in the development literature.

In other words, it is assumed that electrified households, if they – hypothetically – had no electricity, would behave and develop just as the non-electrified households. As in the case of DD, we need two regions to investigate the impacts of *access* to a service. If these two groups are sufficiently comparable (see Section 4.4) the identification assumption is more likely to hold and we are able to estimate the true impact *G* of access to electricity on the household.

The impact studies on electrification projects conducted by World Bank (EnPoGen 2003a, EnPoGen 2003b, ESMAP 2003a, and World Bank 2006) apply the cross-sectional approach by comparing households that *use* electricity to those that do not, both 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 households induce a downward bias if using and non-using households 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 electricity-using households and non-users may lead to strong selection, simultaneity and spillover biases.

One opportunity to improve the comparability of users and non-users of electricity is the application of **matching approaches**. For this purpose, households from the treatment group are matched to those from the comparison group with respect to specific observable characteristics that are *covariates* of the decision to connect. The crucial step is the choice 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 household heads or assets like construction material of the dwelling 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 might be accounted for. In particular, the simultaneity bias resulting from (2) can be reduced.[6]

In principle, matching approaches can even be used if only one region that has access to electricity is surveyed and connected and non-connected households are compared. Often, however, there are only few partners of sufficient comparability that can be matched. The reason is that non-connected households in the access region differ systematically from connected ones – also with respect to the matching criteria. In contrast, if both access and non-access regions are surveyed, non-connected households from the non-access region can serve as matching partners to connected households from the access region. Thereby, the probability of finding good matches is much higher.[7] In addition, this allows for investigating spillover effects by comparing non-connected households in the access region to their comparable counterparts in the non-access region.

Another possibility to deal with selection and simultaneity biases in comparing users and non-users is to find an identification variable that is correlated with the use of electricity but uncorrelated with the household's outcome variable. While such **instrumental variables** (IV) are not easy to find in general, it might even allow for identifying the causal effect without having a control region at hand. For example, Peters, Sievert, and Vance (2009) investigate the impact of electrification on the profit of firms in the electrified region. They use firm location within the agglomeration as

[6] 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.
[7] For applications of this procedure in the electrification case see Bensch, Kluve, and Peters (2009) and Peters, Vance, and Harsdorff (2009).

an instrument, which affects the probability of being connected, but not the firm's profit.[8]

The ex-ante cross-sectional set-up, i.e. surveying the target region without electricity and an already electrified region, still allows for DD-estimation after an ex-post survey. The already electrified region provides for a benchmark that enables the comparison of differences. In the same way as the region that remains non-electrified in the traditional DD-approach, this already electrified region nets out fixed individual effects and the confounding influence of changing environments. As in the traditional approach, the identification assumption requires that the average change in Y for the treated households without an intervention would have been the same as for the comparison group. The only distinction is that the comparison group in this case was already treated before the intervention started in the treatment region.

4.4. Selection of Appropriate Control Regions and Practical Implementation

Altogether, the inclusion of an electrified region in addition to the project region that is not electrified yet offers the most promising opportunities to identify impacts of electrification interventions. First, it allows investigating the *access i*nterpretation of *S,* which requires less strict assumptions than the *use* definition. Second, using and non-using households from both regions can be matched so that simultaneity and selection distortions are reduced.

Ideally, the survey covers a variety of different village types in order to control for different levels of "macro-economic", geographic conditions and other community characteristics contained in *X*. In this way, the effect of electrification *S* on outcome *Y*

[8] Note that this approach does not yield consistent impact estimates if treatment effects are heterogeneous across individuals. In this case, the IV approach rather identifies the so-called local average treatment effect. In the presented example, it answers the question of how large the treatment effect would be if the binary firm location variable increases from 0 to 1. See Augurzky and Schmidt (2001) and Angrist, Imbens, and Rubin (1996).

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, this ideal design is often not implementable due to budgetary restrictions. Researchers frequently face budgets that call for tight survey setups and target regions that often cover less than 20 villages, which may not capture enough variation of village characteristics.

Under such restricted circumstances, comparability has to be assured during the selection of the regions whose households are supposed to be compared to those in the project's target region. 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 account of 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 scrutinized: A pre-selection of potential sites can be made with the help of agents of the electricity utility on the one hand and NGOs or other institutions familiar with the countryside on the other. A subsequent extensive field visit by researchers familiar with the study's purpose and methodology is deemed mandatory for final selection. The reason is that, although a checklist of general characteristics to be fulfilled in terms of comparability is crucial, it can hardly be comprehensive. Furthermore, readily available information on the criteria mentioned above is seldom up-to-date, appropriately disaggregated, and unequivocal.

In most cases, 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 more difficult because the few electrified rural communities are often business centers or otherwise privileged areas. In the usual case, though, utilities and electrification projects follow an either virtual or physically existent priority list in accordance with national rural electrification plans (IEG 2008). This list is compiled by taking into account characteristics like road access and business potentials. Therefore, the target areas selected for an electrification project in Africa are typically not deprived areas, but are rather similar in economic terms to those regions that were connected in recent years. This, however, might not apply if political considerations outweigh socio-economic indicators in the selection of regions to be electrified.

Since a perfect selection of comparison and treatment region can hardly be assured beforehand, it is essential that researchers stay in close contact to the field work. During several ex-ante impact assessments in Africa it turned out that having junior researchers on the ground during the entire survey provides for an indispensable grasp at potential caveats. In general, field supervisors with methodological skills are extremely valuable to obtain accurate and complete quantitative data from the structured questionnaires applied in the household interviews. While these quantitative data constitute the core of the evaluation, it is important to complement them by qualitative information (see also White 2008). Also for this purpose, it is useful to have skilled supervisors on the ground to conduct semi-structured interviews with key informants such as local administration staff, NGOs, health stations, school principals and entrepreneurs. Bensch, Peters and Harsdorff (2009) outline opportunities of how the ex-ante impact assessment can be additionally used to deliver helpful insights for the project design.

5. Conclusion

A consensus exists among most practitioners and donor organisations that considerable impacts might be achieved through electrification interventions. At the same time, expectations to substantiate this assumption by robust impact evaluation have risen considerably. While indications of the impacts of electrification programmes based on profound surveys are documented in the literature, these studies typically rely on a comparison of connected and unconnected households living in the target region of an electrification project. The paper has shown that the identification assumption underlying this comparison is violated in most cases by simultaneity and selection biases. Furthermore, this approach does not account for the self-conception of most rural electrification projects, which intend to generate benefits for the whole region, not only for those households that use the service directly.

Surveying two regions – one with *access* to electricity and one without – provides for a solution to these shortcomings. This set-up allows for investigating the impacts of having – in principle – access to electricity, which requires identification assumptions that are easier to satisfy. Furthermore, connected households in the access region can be matched to comparable households in the non-access region, thereby alleviating the problems arising from selection and simultaneity. A pivotal condition for the success of this approach is a sufficient comparability of the access and non-access region. The paper suggests a rough guideline for the selection of treatment and control regions.

Concerning the strategy to identify causal impacts of the electrification treatment, the paper examines the available state of the art approaches, namely before-after, difference-in-difference (DD) and cross-sectional comparison. The DD approach is in many regards the most desirable way. Practical considerations as well as some methodological caveats, though, speak in favour of cross-sectional evaluations that encompass an access and a non-access region. In particular, the paper recommends carrying out *ex-ante impact assessment* by surveying the yet non-electrified target region of the project and an already electrified region. This still allows for an ex-post DD-comparison, although in a slightly modified way compared to the traditional DD-approach. One major advantage of this ex-ante procedure is that it provides valuable information about the project's target region. Insights about people's potential behaviour after electrification are gained and can be fed back into the design of the electrification project, which helps reducing the gap between evaluation researchers and practitioners.

To assure compliance with methodological requirements and awareness about potential pitfalls that show up during implementation as well as to maximise insights for practitioners, the paper argues that evaluation researchers should be in close contact with both project staff and the field research team. When properly executed, the cross-sectional ex-ante approach not only generates data for an impact assessment at the outset of the electrification project, but also opens opportunities for a robust ex-post evaluation and hands-on insights to be capitalised on during the project implementation.

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