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EVALUATING RURAL ELECTRIFICATION PROJECTS: METHODOLOGICAL APPROACHES

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Abstract

In recent years, the international community has expanded efforts in program 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. It suggests an 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 deliver insights for the project design, thereby reducing the gap between evaluation researchers and practitioners.

Keywords: Impact evaluation, ex-ante impact assessment, electricity access, rural development.
JEL classification: O12, O22, C31, C81.

Introduction

Rural electrification is widely considered to be a crucial prerequisite for development and to remove 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 organizations that considerable impacts in these areas are achieved through electrification interventions.

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At the same time, the international community has increased efforts of program 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 capitalized beyond the project. In the field of rural electrification, extensive studies have been conducted by the World Bank (EnPoGen 2003a, EnPoGen 2003b, ESMAP 2003a, and World Bank 2006) that assess the impact of electrification by comparing connected and non-connected households within the same region.

The present paper discusses the appropriateness of comparing connected and non-connected households and alternatively proposes to compare households living in a region that is principally covered by an electricity service to those who live in comparable non-covered regions. 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) or Angrist and Kruger (1999). It proposes pragmatic options to identify the counterfactual situation, taking into account limitations and demands specific to rural electrification programs.

Besides experimental approaches, the analysis of panel data collected before and after the intervention constitutes one of the optimal approaches to program evaluation. Both methods, however, are difficult to implement in practice: experiments are not feasible in most situations due largely to ethical considerations, while the collection of panel data is often precluded by time and financial constraints. Researchers rather encounter either a situation before the intervention is implemented or after it has ended with lacking appropriate baseline data. Furthermore, funds for evaluation research are mostly very limited, making large or even country-wide surveys impossible. Therefore, beyond the desirable case of having panel data at hand, this paper offers possibilities to derive robust insights on the impacts of electrification using cross-sectional data of limited sample size.

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 simulates 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

[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)".

examples related to the different approaches are described. Still, most of the discussions in the paper are transferable to other continents and more urbanized intervention regions.

The paper proceeds in Section 1 by providing background information on energy consumption patterns in rural Africa. Section 2 presents a conceptual framework of how electricity potentially affects household welfare. Section 3 elaborates different strategies to identify the impact of electrification and describes ways of implementing the approach. The last section summarizes and concludes.

1. 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]

In general, energy use in rural areas in developing countries can be broken down into household, agricultural, and small-scale rural industry subsectors and services. 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 connect 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 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).

[2] Information in this section is based on household and enterprise surveys as well as project reports from 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), Harsdorff, Vance and Peters (2009), Neelsen and Peters (2009), Peters (2008), and Sievert, Peters and Vance (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 frequently, manufacturing firms such as carpenters and tailors can be found. 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.

2. The Treatment: Access to and Use of Electricity

The ultimate objective of impact evaluation is assessing the extent to which an intervention affects the welfare of households. In the evaluation literature, the intervention is generally referred to as the treatment. The subsequent discussion illustrates how the provision of electricity as treatment can translate into poverty reduction along the Millennium Development Goals (MDGs). For the sake of clarity, the focus is on household income as the outcome. In most cases, the methodological considerations are transferable to other potential outcomes, such as education or health indicators as well as firm performance.

The outcome variable income Y is determined by a function f depending on an electricity service variable S and a vector X_Y that captures other relevant household characteristics. For example, X_Y might consist of education and health status as well as assets and household size.

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

The definition and interpretation of the electrification variable S is crucial for the purposes of this paper. There are two possible definitions. The first would be to interpret S as "access", meaning that S equals one if the household is in a region that is principally covered by a service provider and S equals zero otherwise. According to this understanding, S would equal one for a household situated in a grid-covered region, no matter whether the household is connected or not. On the other hand, S would equal zero for a household in a non-grid-covered region, even if it disposes of 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 one if the household is connected to the electricity grid and zero if it is not. Equivalently, 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" interpretation of S. Almost all impact evaluation studies on rural electrification implicitly apply this interpretation 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 is interpreted as "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, i.e. that S takes the value 1, depends on its income Y and a vector of additional determinants, X_S, jointly defining the function g:

$$S = g(Y, X_S) \qquad (2)$$

The components of the vector X_Y in (1) are principally as well included in X_S. In addition, X_S comprises other individual household 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 relation, commonly referred to as simultaneity, counteracts the purpose of isolating the influence of household connections on income.

Taking the second interpretation of the treatment, S, as "access" to electricity, the simultaneity reflected in (2) does not apply. Most rural electrification programs take into account economic potentials and ability-to-pay and, hence, income is one indirect criterion to decide which region to electrify next. However, they typically resort to some measure of aggregate income. The individual household and its income are therefore unlikely to affect the probability that the region in which it is situated is connected. Hence, S in the sense of access to electricity is not a choice variable from the individual household's perspective.

A second problem in the impact analysis occurs if components of X_S are part of X_Y 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*. 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 to generate benefits for those households that do not get connected themselves. In fact, households that are not directly connected to the grid might benefit from spillovers, e.g. by using electricity at neighbours, electric mills or by working more productively in a now electrified enterprise. In contrast to the "use" definition, the "access" definition takes this 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. Following Frondel and Schmidt (2005) and Ravallion and Chen (2005), we denote the post-treatment output by *Y* if the household hypothetically had not received treatment and by *Y+G* for the case that it has received treatment. *G* is therefore the gain exclusively attributable to the project and reflects the causal impact. In the following, several strategies to identify this causal impact are presented, taking into account the particularities of electrification projects. The two interpretations of the treatment variable *S* presented in section 2, access to and use of electricity, pose different problems for identifying *G*.

The frequency of outcome *Y* and *Y+G* across the population of households depends on a set of characteristics X_S (the index *S* is left out in the following). 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:

$$M = E(Y + G \mid X, S = 1) - E(Y \mid X, S = 1) \qquad (3)$$

where *E(.)* denotes the expected values.

As is obvious, we can never observe both *Y* and *Y+G* for the same household, since it either receives the project's treatment or not. While *E(Y+G|X,S=1)* can be easily estimated from a sample of treated households, *E(Y|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 $E(Y|X,S=1)$ with something than can be obtained by estimation from an existent dataset. In practice, this is only possible by finding a comparison or control group that serves to simulate the counterfactual situation for the treatment group.

3.2 Before-after and difference-in-difference estimation

A frequently pursued approach is the before-after comparison, where $E(Y|X,S=1)$ is replaced by $E(Y_{t-1}|X,S=1)$, 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:

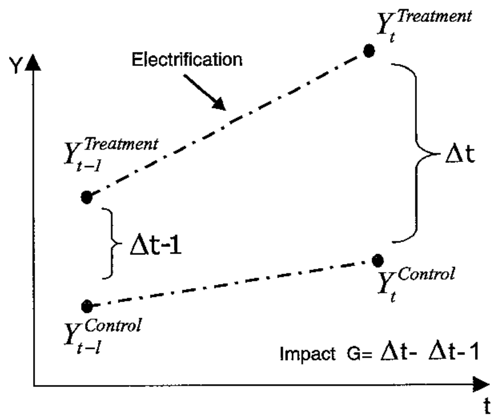
$$E(Y \mid X, S=1) = E(Y_{t-1} \mid X, S=1) \qquad (4)$$

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 factors of change are not known. Since this imperfection of the method stems from the fact that it considers the treated group as the control group itself, one solution is to include non-treated households in order to determine the counterfactual. This is the approach pursued under so-called difference-in-difference-estimation (DD),[3] 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. DD controls for changing external factors affecting the household's outcome variable.

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

Figure 1
The Difference-in-Difference Approach



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. Accordingly, the identification assumption is weaker than for before-after comparison. 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:

$$E(Y - Y_{t-1} \mid X, S = 1) = E(Y - Y_{t-1} \mid X, S = 0) \qquad (5)$$

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 (5) is by nature not observable, while the latter can easily be estimated from a control group sample.

Using the different definitions of the electrification variable *S* presented in the above section, we encounter different identification possibilities using the DD-approach. Applying the "access" interpretation of electrification, 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 (5), both regions have to fulfil certain conditions (see Section 3.4).

For the "use" interpretation 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 control group. Both have to be surveyed before and after the intervention. Yet, one important disadvantage of not including a control group without access using DD-estimation is that positive spillover effects from users to non-users, as described in section 2, 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 (5).

Both before-after comparison and DD-estimation require disposing of 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.

3.3 Ex-ante impact assessment

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.

Ex-post cross-sectional comparison has been applied frequently in the evaluation literature.[4] In this paper, 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 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 households simulate what would have been had there be 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.

[4] 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 formalized terms, the identification assumption for cross-sectional comparison is:

$$E(Y \mid X, S=1) = E(Y \mid X, S=0) \qquad (6)$$

In other words, it is assumed that electrified households, if they—hypothetically—had no electricity, would behave and develop as the non-electrified do. As in the case of DD, we need two regions to investigate the impacts of "access" to a service. Given a sufficient comparability of these two groups (see Section 3.4), the identification assumption holds 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), the identification assumption (6) does not hold in most cases and leads to an upward bias in the impact assessment. On the other hand, as in the case of DD, 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. Furthermore, in order to meet assumption (6), we have to exclude the existence of omitted 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 households that use electricity and those who do not leads to strong selection, simultaneity, and spillover biases in most cases.

One opportunity to improve the comparability of users and non-users of electricity and thereby to meet the identification assumption is the application of matching approaches. For this purpose, the households from one group are matched to those from the other group according to specific observable characteristics that are covariates of the decision to connect ($S=1$). The crucial step is the identification of appropriate covariates to base the matching on. These covariates are required to influence the decision to connect, but must not be responsive to the intervention. Therefore, the pre-intervention outcome Y_{t-1} is an appropriate covariate. Yet, in the case of cross-sectional comparison, data on pre-intervention variables is not available. In this case, variables can be chosen as covariates that can be assumed to influence the decision to connect, but can be supposed to be not affected by electrification in the short to medium term. Examples might be the education of household heads or assets like construction material of the dwelling and size of buildings.

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.[5]

[5] 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.

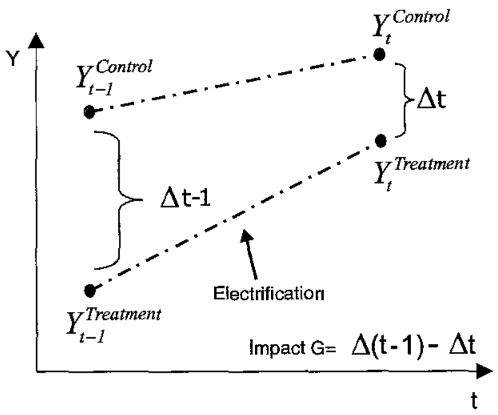
Matching approaches can also be used if only one region that has access to electricity is surveyed. Often, however, it turns out that only few partners of sufficient comparability can be matched. The reason is that non-connected households in the access region differ systematically from connected ones—also 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.[6] In addition, this allows for investigating indirect spillover effects of electrification 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 is not correlated with the household's outcome variable. While such instrumental variables are not easy to find in general, it might even allow 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 an instrument, which affects the probability of being connected, but not the firm's profit.

At first glance applying a cross-sectional comparison before the project implementation does not allow for ex-post evaluation using the DD-approach. The reason is that in the traditional case, DD estimation requires two regions without electricity, one of which is going to receive the intervention. The ex-ante cross-sectional comparison, in contrast, calls for surveying the target region without electricity and an already electrified region. However, as illustrated in Figure 2, this set-up 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.

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

Figure 2
The Difference-in-Difference Using an Already Treated Control Group



Note: The control group already has access to electricity in *t-1* while the treatment group expects electrification between *t-1* and *t*.

3.4 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" interpretation 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" 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, 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 harder 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 control 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.

4. Conclusions

A consensus exists among most practitioners and donor organizations that considerable impacts are 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 programs 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, advocate 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 valuable information about the project's target region and people's potential behaviour after electrification are gained and can be fed back into the design of the electrification project. This helps reducing the gap between evaluation researchers and practitioners, the latter of whom are frequently not convinced of sufficient direct benefits of rigorous evaluation surveys for their immediate day-to-day work.

To assure that the proposed method is applicable with reasonable expenditures and to maximise insights for practitioners both in and beyond the project, a mixed methods approach is recommended using quantitative and qualitative information as proposed by White (2008) and Rao and Woolcock (2003). The paper argues that evaluation researchers should be in close contact with both project staff and the field research team. Having junior researchers on the ground during the entire survey assures optimal compliance with methodological requirements and awareness about potential pitfalls that show up during implementation. When properly implemented, the cross-sectional ex-ante approach not only generates data for an impact assessment at the outset of the project, but also opens opportunities for a robust ex-post evaluation and hands-on insights to be capitalized during implementation of the electrification project.

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