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THE IMPACT OF RURAL ELECTRIFICATION ON INCOME AND EDUCATION: EVIDENCE FROM BHUTAN

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Abstract

We investigate the impact of a rural electrification program on household income and childrens schooling in rural Bhutan. Using Propensity Score Matching, we find that electrification had a statistically significant impact on non-farm income and education. Non-farm income increased by 61 percent and children gained 0.72 additional years of schooling and 9 minutes of study time per day. We do not observe significant effects on farm income. Results are consistent and robust to different matching algorithms. Our findings indicate that investments in reducing energy deficit may help improve human welfare in Bhutan.

***Keywords*:** Rural electrification; income; education; Bhutan

***JEL Codes*:** O12; O13; Q48

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1. Introduction

Despite substantial efforts targeted to ending poverty worldwide, close to 1 billion people still lived on less than $1.25 a day in 2011 (World Bank, 2015). In recent years, there has been a growing interest in understanding the role of rural electrification (RE) programs in improving welfare and poverty reduction. Providing access to electricity remains one of the critical binding constraints in spurring rural development and achieving the United Nations commitment to end poverty and provide universal energy access (UNDP, 2015). The electricity access is expected to reduce rural poverty by increasing employment opportunities and access to improved public services.

Access to electrification can potentially affect economic development through a number of channels but the most evident link is through improved productivity at the individual and household levels. The main direct benefit of electricity is clean lighting source but it can also positively contribute to farm and non-farm productivity through the improved production process and reduced cost of production (Rud, 2012; Chakravorty, Emerick, and Ravago, 2016). Access to electricity could also facilitate the start of new businesses, adoption of new technology, and mechanization of agricultural practices. Among other benefits, electricity access contributes to health improvements as households switch away from kerosene and coal to electricity (Barron and Torero, 2015); higher educational attainment (Lipscomb, Mobarak, and Barham, 2013); better food security and gender empowerment (Dinkelman, 2011; IEG, 2008). Electrification may also enhance labor supply through time savings when households switch away from firewood collection to clean source of energy (Dinkelman, 2011).[2]

Despite these benefits, however, an estimated 1.2 billion people-16% of the global population- lacked access to electricity globally in 2016 (World Energy Outlook, 2016). In this study, we evaluate the impacts of rural electrification program on household income and schooling in rural Bhutan, a landlocked country neighboring India and China. With assistance from the Asian Development Bank (ADB) and other international donors, Bhutan has made significant progress in increasing electricity coverage from 17% in 1995 to 60% by 2009 (ADB, 2010). Despite this expanded electricity coverage and the far-

[2] Electricity access might increase quality of leisure time due to increased time spent watching TV (Olken 2009).

reaching effects it may have on human welfare, empirical studies on the impact of increased electrification rate in Bhutan are rare. Against this background and context, this study aims to fill this gap in the existing literature by estimating the impacts of electricity access on income and non-income indicators of welfare in a resource-constrained setting.

Although Bhutan has substantial sources of clean and renewable hydropower energy, electrification rate has been limited and at subsistence level in the country.[3] Bhutan is a unique setting to examine the impacts of RE because mountainous terrain, scattered settlement, and low demand in the villages make the extension of grid lines difficult and capital intensive. First, the rugged terrain and scattered population densities make it difficult to realize the full benefits electrification may have on agriculture, thereby affecting farm income in unpredictable ways. Second, compared to other sources of electricity generation (coal, thermal etc.), selection bias inherent in the evaluation of infrastructure projects due to non-random project placements may be smaller because hydropower projects can only be placed near river source, which is given by nature. Our study covers the evaluation of two RE projects funded by the ADB from 2000 to 2006 in Bhutan.[4]

Evaluating impacts of large infrastructural projects such as, electricity provision suffers from the econometric challenge, as experimental data are not readily available. In the non-experimental setting, the major challenge is to address the potential selection bias. It is quite possible that characteristics of electrified households are different from non-electrified households and this may bias the impact results. Furthermore, infrastructure projects could be targeted in areas that are growing, politically important, industrialized, and close to urban centers. The non-random selection of projects is likely to bias the comparison of electrified and nonelectrified groups and would be confounded with the unobserved heterogeneity. Some of the previous studies on this topic either have relied on randomized experiments (Bernard and Torero, 2015) or instrumental variable method to deal with the fact that access to electricity is not randomly assigned (Chakravorty, Pelli, and Beyza, 2014; Dinkelman, 2011; Rud, 2012).

In the absence of a valid instrument and suitable data, we use statistical matching

[3] More than 99% of the electricity in Bhutan is generated by hydropower. Bhutan is rich in hydro resources with more than 1000 rivers and tributaries crossing favorable landscapes around the country.
[4] The two projects are Sustainable Rural Electrification Project ((SREP) and Rural Electrification and Network Expansion Project (RENEP).

techniques to estimate the plausible causal effect of electricity access on income and education. Our empirical analysis uses non-experimental data collected by the authors in 2010. We use a rich set of household and village-level variables to capture individual and village-level heterogeneity so that the decision to get electrified gets adequately captured in the propensity score matching (PSM) model.

Our findings demonstrate that rural electrification program in Bhutan led to statistically significant and economically meaningful increase on outcomes related to household welfare. Our results show that the access to electricity has a positive impact on households' non-farm income and educational outcomes of the school-aged children, although no statistically significant impact is observed on farm income. Children in electrified households are more likely to attain more years of schooling and spend more time in studying at home compared to those in non-electrified households. The results are robust to different matching estimators. Our data do not allow us to directly test for the mechanisms underlying the effects on non-farm income and years of schooling.

We make several important contributions in this study. Rural electrification rate varies from 14% in Sub-Saharan Africa to 66% in South Asia. The United Nations goal of universal access to electricity by 2030 would require an investment of 640 billion US dollars. Furthermore, the empirical evidence on the welfare gains from rural electrification is mixed. Some studies have found positive impacts on income, consumption, education (Khandkar et al., 2012; 2013), while other studies failed to observe such impacts (Bensch et al., 2011; Peters and Sievert, 2016). Therefore, it is of tremendous policy interest to understand whether electrification benefit justifies such high investment cost. In this context, findings of our study add to existing body of evidence and provide important information to policymakers on benefits of rural electrification in a setting with low electricity demand such as rural Africa. In addition to its contribution to the growing empirical literature of electrification that seeks to understand the electrification benefits in rural areas, this paper further contributes to the broader literature on infrastructure and economic development.[5]

This is also one of the handful studies in Bhutan that rigorously quantifies the

[5] These include irrigation dams in India (Duflo and Pande, 2007), railroads in the United States and India (Donaldson, 2015; Hornbeck and Donaldson, 2016), national highways in China (Faber, 2014), and rural roads in Vietnam (Mu and Van de Walle, 2011).

impacts of access to electricity on economic and non-economic outcomes. The setting of the study is unique because Bhutan has features that differ sharply from other developing countries, such as source of power generation, mountainous terrain, and low demand for clean energy. These features are important for introducing heterogeneity in impact estimates. Furthermore, authors have collected unique household- and village-level data in rural parts of Bhutan using a carefully structured household and village surveys. It is a unique aspect of this study, given the unavailability of any household survey in Bhutan at the time of the study. Finally, identifying the causal effect of electricity on income and education is an important issue in development economics; hence, we believe that we are making an important contribution to the existing empirical evidence on the impacts of rural electrification.

The remainder of this paper is organized as follows. Section 2 provides the country context and RE in Bhutan, followed by a survey of the relevant literature on the impacts of electricity provision in developing countries in Section 3. Section 4 discusses the empirical framework, and then in section 5, we present the study design and data. Section 6 presents the results and Section 7 concludes.

2. Country context and rural electrification program

Bhutan, a landlocked Himalayan country bordering India in the south and China in the north with a population of 0.7 million in 2016, is largely a mountainous country. Natural forests account for over 70% of the country's landmass. Subsistence agriculture, hydropower, and tourism are the main drivers of the national economy. Agriculture remains the dominant occupation of 63% of the population mostly in the form of subsistence farming and animal husbandry. According to the Poverty Analysis Report 2012 (PAR 2012), 70% of the population is rural and rural poverty is 16.7%, significantly higher than urban poverty (1.8%) (World Bank, 2013).

The "access to electricity for all" is an important indicator of the country's Gross National Happiness (Planning Commission, 2000). Bhutan's RE program dates back to Sixth Five Year Plan (1986-1992) when the first unit of Chhukha hydropower plant was commissioned in 1986, but lack of resources coupled with a mountainous terrain slowed the pace of the electrification. In 1995, only 20% of rural households in Bhutan were electrified. The ADB has supported Bhutan's electricity program since 1995. Other major donors include India and Japan International Cooperation Agency. Until 2009, ADB mainly supported two RE programs in Bhutan: SREP and RENEP. At the end of Ninth Five Year Plan, about 60 percent of households were connected to grid electricity in 2007 however rural electrification coverage is almost universal in 2017.

The RE program was rolled out gradually across villages. The implementing agency of the RE program, Bhutan Power Corporation (BPC), adopted a radial approach to implement RE program in the country. Villages falling within the closer radius to the power substations were electrified first, followed by the next lot. Essentially, radial distance and location of villages played a role in sequencing electrification of the villages. One may be concerned that village or household socioeconomic status may have played a role in roll out of the RE program. This seems unlikely, as no socioeconomic data prior to the 2005 Census was available[6]. The RE projects were implemented before the census results were publicly available so it seems unlikely that government may have used any village-level socio-economic or demographic indicators to expand access to electricity across villages.

Thus, the rollout of the RE program is not random even if we assume that proximity of the villages to the power stations are not correlated with the village characteristics and the outcomes analyzed in this study. Since there is not enough information on how these power stations were established, we cannot really ensure that location of the power stations with respect to the treated and control villages are random. Therefore, we use *propensity score matching* method to estimate the impact of electricity access on household income and education.

3. Empirical evidence from previous literature

Some of the recent rigorous efforts to determine attribution of RE on development outcomes in developing countries include Chakravorty et al., (2014), Dinkelman (2011), Lipscomb et. al., (2013), and Rud (2012). The first study examined impact of electrification on income, the second study on female employment and wages, the third on income, poverty, and Human Development Index, and the fourth on industrial outputs. All these

[6] Bhutan conducted the first census in 2005 and no other reliable population estimates was available prior to 2005.

studies attempted to isolate the causal impact of electrification and used an instrumental variable method to address the selection bias arising from the non-random placement of the electrification projects. Earlier literature reasoned that the relationship between infrastructure and developmental outcomes could be confounded since the project placement could target developing or socio-politically important areas.

Chakravorty et al., (2014) used district-level density of transmission cables as the instrument for household's electrification status in India and reported a significant increase on non-agricultural income due to high-quality electricity access. Using availability of groundwater as an instrument for expansion of electricity network across regions, Rud (2012) found that provision of electricity is associated with positive gains in manufacturing output in India. Dinkelman (2011) studied the labor market impacts of electricity provision in rural areas of South Africa using land gradient as the instrument for program placement. Her findings indicated an increase in female employment, fall in female's wages, and rise in male's wages. Furthermore, Lipscomb et. al. (2013) noted positive impact of electrification on Human Development Indicators, employment, salaries, and investment in education in Brazil. Another study conducted in Nicaragua by Grogan and Sadanand (2013) reported higher propensity of off-farm employment for women, but not for men. They use past population density as an instrument for current access to electricity.

Among the handful of studies on the impact of RE on developmental outcomes, we highlight the findings of studies that have used outcomes similar to ours. For example, in a recent study by Dasso and Fernandez (2015) in Peru, the authors found no effect on earnings in the double-difference model, but reported a positive impact on earning for the women in the tune of about 35 percent, but they did not find any impact on men's earnings in the fixed effect models. Similarly, the instrumental variable (IV) estimates of the effect of electrification on household income in Chakravorty et al., (2014) ranged between 86.7% and 89.8%.

Two other studies that provided the impact of electrification on income and education were conducted in Bangladesh and Viet Nam (Khandker et al., 2012 and Khandker et al., 2013). Using instrumental variable method, Khandker et al. (2012) found that the household per capita expenditure increased by 11.3 percent and overall total income rises by 21.2 percent due to electrification in Bangladesh. Boys and girls study time also increased by 22 and 12 minutes a day as a result of electrification. Likewise, Khandker et al. (2013) evaluated the impacts of electrification in Vietnam using household fixed effect model. They show that household electrification had positive impacts on total and nonfarm incomes. As a result of access to electricity at household level, total and nonfarm income rise by 28% and 27.5%, respectively in Vietnam. Household electrification also impacted educational attainment of children. Access to electricity at household level increased school enrollments by 9 percentage points for girls and 6.3 percentage points for boys. Boys' schooling increased by 0.11 years, while the impact on girls' schooling was statistically insignificant.

Rural electrification was also found to increase labor supply of men and women, schooling of boys as well as girls, household per capita income and expenditure in India (Khandker et al., 2014). This study used IV method and the instrument used was interaction of proportion of electrified households in the community and households' own socio-economic characteristics. The IV is consistent with the literature on peer pressure and demonstration effect that highlight the importance of neighbor's activity on own decisions. Rural electrification also increased labor supply of men and women and help reduce poverty in India. Van de Walle et al. (2017) used double-difference and IV method to assess the impact of household and village electrification rate on income, consumption, labor supply, and education in India. They found that household electrification had significant gains on consumption, labor supply, and schooling in rural India over 1982-1999. According to the IV method, electrification caused a consumption gain of 8.8% (0.5% per annum), representing a gain of Rs. 300.3 per person per year. Positive effects, however, were found for girls but not for boys. There was some evidence of dynamic effect of village connectivity for households without electricity themselves. Wage rates were unaffected by rural electrification and the gains in labor earnings were mainly from extra work by men.

The findings from these studies clearly reflect that empirical evidence on the impact of electricity provision is mixed. The sign and magnitude of the effects depend on the outcomes analyzed, empirical methodology, and location of the study. Given a limited number of rigorous impact evaluations of RE programs so far in a handful of countries, more efforts are needed to gather evidence in a causal framework. Moreover, the impacts may vary by the context of the country, depending on the prevailing enabling environment for the access and use of electricity for improved human welfare.

4. Empirical framework

This study uses propensity score matching (PSM) to estimate the plausibly causal impact of RE on household income and schooling. In a seminal work, Rosenbaum and Rubin (1983) proposed PSM as a method to reduce the bias in the estimation of treatment effects with observed data sets. In recent years, matching methods have become increasingly popular and widely used in the evaluation of development interventions (Becker and Ichino, 2002, Ravallion, 2008; Rauniyar et al., 2010; Kumar and Vollmer, 2013).

The basic premise in the matching technique is to generate groups of treated and control households that have similar characteristics so that comparisons can be made within these matched groups. In the event of a large number of observed characteristics, direct matching becomes infeasible and propensity score p(X) (a single-index variable) can be used (Rosenbaum and Rubin 1983). Propensity score p(X) is the estimated probability of receiving treatment given the background covariates. In this study, treated households are matched with the comparison households based on propensity score and the difference in the mean outcomes of treated and control groups is attributed to the RE program. The identifying assumption is that selection into treatment is based on time-invariant observed characteristics and these observables are adequately captured in the propensity score model. The method further assumes no selection bias based on unobserved characteristics (Dehejia and Wahba, 2002; Smith and Todd, 2005).

4.1. Average treatment effects on the treated (ATT)

Let Y_{1i} and Y_{0i} are the outcome variables for treated and control households, respectively, and $D \in \{0, 1\}$ is the indicator of treatment status. The propensity score p(X) is the conditional probability of receiving treatment given observed characteristics:

$$p(X) \equiv \Pr(D = 1 \mid X) = E(D \mid X) \qquad (1)$$

where X is the multidimensional vector of observed characteristics.

Given the propensity score p(X), the Average Treatment Effect on the Treated (ATT) can be stated as:

$$
\begin{aligned}
ATT &\equiv E\{Y_{1i} - Y_{0i} \mid D_i = 1\} \\
&= E[E\{Y_{1i} - Y_{0i} \mid D_i = 1, p(X_i)\}] \\
&= E[E\{Y_{1i} \mid D_i = 1, p(X_i)\} - E\{Y_{0i} \mid D_i = 0, p(X_i)\} \mid D_i = 1] \qquad (2)
\end{aligned}
$$

Equation (2) gives the average programme impact under the overlap and conditional independence assumption (CIA). CIA assumes that the outcomes are independent of treatment conditional on X, and these can be written as $Y_1, Y_0 \perp D \mid X$, whereas, overlap assumption implies that for each X there are both treated and control units, i.e. $0 < Pr[D=1j X] < 1$.

4.2. Matching Algorithms

This study uses four widely used matching methods to probe the robustness of the results - nearest-neighbor (NN) matching with replacement, caliper, local-linear, and kernel matching. We used nearest five neighbors, which takes the average of the closest five matched control units as the counterfactual for each treated unit. However, this approach faces the risk of bad quality if the closest neighbor is far away. This was avoided by imposing a tolerance level on the maximum propensity score distance (caliper), this is known as caliper matching. Applying this option means that an individual from the comparison group was chosen as a matching partner for a treated individual that were within the caliper (propensity range). Furthermore, to probe the robustness of our results, we employed kernel and local-linear matching. The advantage of kernel matching is that it is more efficient since this method uses all untreated units, thereby minimizes the variance of the matching estimates. We applied bootstrap method to estimate the standard errors in different matching algorithms.

The selection of bandwidth parameter is important in matching methods to reduce the bias. The bandwidth choice introduces a bias-variance tradeoff (Caliendo and Kopeinig, 2008). Larger bandwidth implies lower variance and higher bias, while smaller bandwidth implies higher variance and lower bias. The choice of bandwidth in this study is based on the prior literature on impact evaluation (IEG, 2008). Given the tradeoff between the bias and variance, we estimate ATT with bandwidth of 0.1 and 0.2 in the local linear and kernel matching method.

5. Study design and data

This study covers the evaluation of two rural electrification projects by the Asian Development Bank (ADB)-Sustainable Rural Electrification Project (henceforth, RE II) and Rural Electrification and Network Expansion (henceforth, RE III). The Royal Government of Bhutan implemented both projects between 2000 and 2006 with varying geographical coverage. The primary data for the study came from a household and a village surveys administered in 2010 in the treated and the control villages.

5.1. Sample

A mix of multi-stage purposive and probability sampling approach was undertaken to design the sampling frame. Villages that were electrified under RE II and RE III constituted the treatment sample and villages that were going to be electrified in the next phase (hereafter, RE IV) constituted the control sample. The electrification projects in the control villages were slated to commence in later parts of 2010 after data collection for our study was completed.

In stage one, of the 20 districts, 10 districts were purposively selected to achieve a geographically disparate and diverse study sample from each region. All villages in these 10 districts constituted the sampling frame for the primary data collection. The sampling frame consisted of 198 electrified and 277 non-electrified villages. In stage two, 71 electrified and 45 non-electrified villages were randomly selected from this sampling frame.[7] In stage three, 20 households were randomly drawn in each village for the household survey. Finally, the survey team was able to administer a household survey to 2,098 households residing in 126 villages. The treated sample had 1,304 households, while the control sample had 794 households.

5.2. Data

The primary data collection was conducted by a local survey company based in the capital of Bhutan. The survey company was given the list of the treated and the control villages by the study team. The survey company randomly identified 20 households in each

[7] Electrified villages were oversampled.

village and the survey was administered to the head of the household after taking their written consent.[8] The survey covers a wide variety of socioeconomic information including electrification status, demographic characteristics, education background, occupation, and employment status; household characteristics including land holding, irrigation, and livestock, income generating activities, information on micro-enterprises.

The dependent variables in this study were income of the household (farm and nonfarm) and literacy, years of schooling, and study time at home (in minutes per day) for school going children who were 7-18 years old at the time of the survey. The treatment variable was binary reflecting the electrification status of the households. Since the treatment is at the village level, household's electrification status was determined by the treatment status of the village. All households in the treated village were identified as electrified, while households in the control village were identified as nonelectrified. The RE program in Bhutan mandated to electrify villages as well as each household in the electrified villages. The program covered the cost of household's connection to village grid. Our survey data also confirmed that each household in the electrified village had access to electricity, meaning 100% compliance rate. Therefore, our results should be interpreted as ATT rather than intention to treat (ITT).

The explanatory variables used in the PSM model include several household and village level variables. Household-level variables included in the model were (a) *Human capital assets* – household size, age of the head of the household, whether head of household is literate, number of literates in the house, gender of the head of household, marital status of the head of household, and religion of the head of household; (b) *Physical assets* – Household's holdings of land, main source of drinking water, type of house, whether household owns cows, bulls, poultry, and horse. Village-level variables used in the study were the level of isolation of the village, as measured by the distance from the village to *dzongkhag* or district headquarter (*dzongkhag* is the lowest tier of local administration) and population of the village.

Table 1 shows the summary statistics of the outcome variables and the explanatory variables used in the propensity score estimation. Columns 2 and 3 present means for the households with access to electricity and for those without access, respectively, and the last

[8] Every third or fifth households were selected for the survey through random-walk technique.

column (col 4), reports the statistical difference between electrified and nonelectrified households. About 62% of the sample households were electrified and they were generally better off than the non-electrified households in terms of income and education of children. This is not surprising, because economic and educational opportunities may have improved with the access to electricity. The sample had an average household size of 4.36 members, with 71% of households headed by male members.

Survey data revealed that the literacy rate of head of the household was considerably low (25%) and about 73% of them were married with an average age of 50 years. The comparison of the second column with the third column in panel B reveals that households with access to electricity and without are not similar on a number of dimensions, indicating that the control sample may not be a valid comparison group and thus supports the use of propensity score matching to make the treated and control sample comparable.

Column (4) reports the statistical difference between the treated and the control groups. The income levels and educational outcomes are higher in electrified households than the non-electrified households but the difference in farm-income is statistically insignificant. Although there exists a difference in outcomes, yet it is unclear at this stage how much of these differences originate from the selection process and how much of this observed difference can be attributed to electrification. Regarding the variables that are included in the propensity score model, the majority of the variables differ across the treatment status. For example, the difference-in-means in gender, age, and marital status of the households are statistically significant in column (4), indicating that non-electrified households do not constitute a valid counterfactual group and are not fully comparable to the electrified households.

Although electrified households have higher levels of benefits compared to non-electrified households, assertions cannot be made that the incremental benefits are entirely due to electrification without establishing the causal association. The difference-in-means findings are reported largely to serve as a comparison to the propensity-based matching.

[Insert table 1]

6. Results and discussions

6.1. Propensity score estimation

The propensity score (p-score) model is estimated using a probit model. In the propensity score model, variables that are likely to affect electrification and outcomes are included. Furthermore, variables that are unaffected by the treatment were also included in the estimation of propensity scores. In addition to household-level variables, we also include village-level variables because they are likely to affect the participation in the rural electrification program.[9]

The CIA and the requirements of affecting both the decision to have access to electricity and the outcomes guided the selection of explanatory variables. In an ideal scenario, pre-intervention data should have been used to estimate the p-score model. Lack of pre-intervention data led us to use post-intervention variables that are likely to affect the electrification status and is not affected by the electrification intervention. These variables were household size; gender, religion, marital status and literacy of the household head; total number of literate members in the household; cultivable land area; access to potable water; housing structure; ownership of livestock; village population and distance to *dzongkhag* headquarter from the village.

Table 2 shows individual coefficients of the probit model. The household size, gender, marital status and literacy of the head of the household do not play a significant role in explaining the access to electrification. As expected, distance to district headquarters has a negative effect on the probability of electrification. Village population has a positive effect on the probability of obtaining a connection to the grid, though the coefficient is very small and close to zero. By contrast, household size and access to tap water have a negative effect; however, the coefficients are statistically insignificant. This is not problematic because the empirical specifications include many correlated variables and the purpose of the estimation is to calculate the propensity score and not to model an underlying selection mechanism.

[9] Any discrete choice model, either a probit or logit can be used to estimate the propensity score and selection of model is not a critical issue. Model choice is more important in the case of multiple treatments (Lechner, 2001).

[Insert Table2]

Figure 1 depicts the distribution of propensity scores for electrified and non-electrified households. Distribution suggests that electrified households have slightly higher probability mass at higher levels of the propensity scores (greater than 0.6), while non-electrified households have a higher probability mass at lower levels of the propensity score (lower than 0.6). This indicates that based on the set of observed characteristics included in propensity scores estimation, electrified households are slightly different from un-electrified households. Thus, there should be a potential gain from using matching estimators compared to multivariate regressions.

[Insert figure 1]

Common support: In order to obtain credible matching estimates, only those comparison and treatment observations whose propensity scores fall within the region of common support were included.[10] While implementing the common support criteria, treatment observations whose propensity score is higher than the maximum or less than the minimum propensity score of comparison observations were dropped from the sample.

Figure 2 shows that there is enough overlap between electrified and non-electrified households to make robust comparisons. Imposing the common support criterion results in the elimination of 20 electrified households (1.56% of the total electrified sample), and none from un-electrified households. Of the total sample 1,304 electrified and 794 non-electrified households, we excluded 25 electrified and 33 non-electrified household observations from the analysis because of missing values. In the remaining 2040 households, 20 were off-support and had also to be dropped from the analysis.

[Insert Figure 2]

Balance test: To assess the quality of the matching, a 'balancing test' of the characteristics of the matched samples was performed. If CIA is valid, then all the Xs should be "*balanced*" across the treated and matched untreated groups. The analysis implemented three balancing tests commonly employed in the matching literature (Caliendo and Kopeinig, 2008). *First*, we examined t-tests for the difference in covariate

[10] The commonly implemented method, Min-Max method was used to ensure the common support. The Min-Max method involves the comparison of the minimum and maximum propensity scores of treatment and comparison observations. All observations in the treatment (comparison) group whose propensity score is smaller than the minimum and larger than the maximum of the propensity scores in the comparison (treatment) group were discarded.

means between the matched treatment and comparison samples. *Second*, as proposed by Rosenbaum and Rubin (1983), standardized difference before and after matching was analyzed. If the covariates are balanced, there should be a reduction in the standardized bias. *Third*, pseudo-R-squared of the propensity score model after matching should be low since systematic differences in the distribution of covariates between the treated and matched untreated groups are wiped out. Results from Table 3 suggest that there are no significant differences in means for most of the variables, and all covariates are balanced post matching except livestock ownership.

[Insert Table 3]

The results using measures of pseudo R-squared and standardized difference are presented in Table 4. The pseudo R-squared generated in the matched sample (0.003) is much lower than the pseudo R-squared generated prior to matching. Finally, examining the median standardized difference before and after matching, we find that standardized bias was lower after matching (9.53 vs 2.56), and was never above a value of 5, which is well within acceptable bounds (Smith and Todd, 2005). To sum up, the matched sample passed all the three different balancing tests implying that matched comparison households were good counterfactual for the treated households.

[Insert Table 4]

6.2. Ordinary least square and propensity score matching results

Table 5 shows the results from ordinary least square method. All the columns include household as well as village controls. Results show that access to electricity had significantly positive impacts on all the outcomes except farm-income. Non-farm income was 50% higher in electrified household compared to non-electrified households (column 2). The estimates (columns 3-5) suggest that electrification had substantial impacts on education outcomes. Literacy rate was 3 percentage points higher in electrified households. Due to access to electricity, children were able to spend more time studying at home: the difference is about 10 minutes (column 5); and years of schooling was higher by 0.55 years for children living in electrified households. Since Table 5 presents the potentially biased OLS results; we turn to Table 6 that reports the estimates from the PSM analysis.

[Insert Table 5]

Table 6 contains the PSM results that have dealt with all of the observables affecting programme assignment and outcomes. Findings suggest that household with access to electricity have higher levels of income and better educational outcomes. A disaggregated analysis reveals that electrification had a significant impact only on non-farm income. Non-farm income was 62% (column 7) to 76% (column 4) higher in electrified compared to non-electrified households and this difference was statistically significant at 1% level of significance. The impact was highest under 0.1 local linear matching (LLM) and lowest under 0.2 kernel matching.

[Insert Table 6]

Farm-income was higher in electrified households but the impact was imprecisely estimated and insignificant at the conventional level of significance (column 4). Intuitively, the impacts of rural electrification on farm-income can be mixed. On the one hand, the impacts on farm-income may be muted or even negative if people switch out of agriculture into non-agricultural activities as a result of electrification. On the other hand, farm-income may go up for farmers that continue to be engaged in agriculture due to increased mechanization of agricultural practices, use of capital-intensive technology, and improvements in agricultural productivity. However, these channels may not have led to significant impacts on farm-income because mechanization on small-scale subsistence farming on scattered and fragmented land is quite difficult and unsustainable. Furthermore, access to electricity may affect non-farm through increased productivity, start of new micro-enterprise undertaking, and home-based small businesses. Due to data limitation, we are unable to provide empirical evidence on any of these channels in this study.[11]

Our estimates are comparable to findings in the previous literature. For example, previous studies on impact evaluation of rural electrification found that access to electricity increased nonfarm income by 56% to 90% in Bangladesh (Khandekar et al., 2012) and by 70% in Viet Nam (Khandekar et al., 2013). Both studies failed to identify the channel through which non-farm income witnessed improvements among electrified households.

[11] Interestingly, during focus group discussions (FGDs), many participants claimed that their income from weaving had more than doubled after they received electricity, and electrification had increased their income potential by facilitating microenterprise businesses. Many FGDs participants also reported that increased non-farm income was associated with other micro-enterprise activities, in addition to weaving. Increased poultry production in Bhutan's southern districts was cited as an example.

Another study conducted in India estimated the impact of electricity quality on household income to be in the order of 86% to 90% (Chakravorty et. al., 2014). A back of the envelope calculation further indicates a sizable effect of electrification on non-farm income. For example, the electrification rate in 1995 was 20% and if we optimistically assume that electrification rate has increased by 30% between 1995-2009, then a 62% increase in non-farm income would imply that non-farm income increased by 18.6 percent over this period due to increased access to electricity.[12] Furthermore, non-farm income in this study accounts for only 29% of total household income in electrified and 21% in non-electrified households.

Results associated with the impact of access to electricity on education of children appear in Table 6. The estimates suggest that access to electricity significantly improves literacy, years of schooling and study time at home in electrified households. The impact on years of schooling varies from 0.55 years (column 3) to 0.72 years (column 7). It is highest in kernel (0.2) matching, suggesting that electrification contributes to 0.72 additional years of schooling for school going children, which is an increase of 21% at average schooling of 3.48 years for the whole sample. Table 6 also shows that children's study time at home increases by 9-12 minutes per day, implying an increase of about 16% since the average study time in the sampled households is 75 minutes per day. These results are similar to the finding reported in the Bangladesh and Vietnam by Khandekar et al. (2012, 2013).

The positive educational outcomes due to electricity found in this study have various explanations. Although it is difficult to conclusively pin down the pathways, several hypotheses consistent with the results emerge in our analysis. These hypotheses are not mutually exclusive; each has a part in the overall results. The most compelling explanation is the increased evening study time at home for children due to availability of high-quality bright light as a result of electricity. Children experience less strain on their eyes and their efficiency and productivity increase when they study under a bright light from electric bulbs compared to a dim flickering candles or kerosene lamp. Children from poorer families benefit the most from electricity, as they faced no other option than to study

[12] It should be noted that a 62% increase in non-farm income in our study might not translate into substantial income effect in absolute term because of low average income in rural Bhutan.

under kerosene lamps because of prohibitive costs to the households.

The failure of teachers to take up posts in remote locations and frequent absenteeism from such postings is a major problem in many developing countries, and Bhutan is not an exception. Electrification can be instrumental in coping with such shortage of teachers and can improve teaching quality and continued education by making rural positions more attractive to teachers (IEG, 2008). Participants of the focused group discussions stated that teachers preferred to stay in electrified villages because they did not need to commute daily from their original residences. Higher accommodation costs in electrified villages support this assertion. More importantly, villages are able to recruit and retain better-qualified, experienced teachers in electrified villages compared to non-electrified ones. Further, teachers are happy to stay in electrified villages and they can prepare their teaching lesson plans at night. Other reported benefits from electricity access include increased awareness and knowledge, use of mass media to supplement normal classroom teaching, improved student performance in vocational schools and flexibility in teaching in evening hours (IEG, 2008).

6.3. Sensitivity analysis with Rosenbaum bounds

The PSM analysis adjusts for selection bias from the observed factors and any selection bias emanating from unobserved factors still remains a concern. In order to check the extent of unobserved bias, we use Rosenbaum (2002) bounds to estimate how large the effect of a hypothetically unobserved confounding factor would have to be to overturn our ATT estimate in Table 6. Table 7 presents the results of the Rosenbaum bounds sensitivity analysis.[13] We conduct the sensitivity analysis only for the outcomes on which electrification had statistically significant ATT impacts as shown in Table 6. Given that the estimated treatment effect is positive, the lower bounds under the assumption that the true treatment effect has been underestimated are not important (Becker and Caliendo, 2007) and therefore not reported in this paper.

In a randomized experiment, randomization of the treatment ensures that the value of gamma equals 1. Since values of critical values (Γ) is unknown, we try several values of Γ to elicit the critical value at which our findings would change. In particular, the

[13] Rosenbaum bounds were estimated using *rbounds* command in Stata 14.

maximum level Γ where inference about the ATT effects would start to be overturned is set to 1.5 with increments of 0.1. Results in Table 7 show that the critical value that will overturn the statistically significant ATT effects varies from 1.2 to 1.5 for different outcomes. The positive effect on log of non-farm income would not disappear due to unobserved selection bias unless treated and control households differ by 50% in terms of the unobserved covariate. The ATT effect on literacy is insensitive to selection bias from unobserved variables. The critical p-values for years of schooling and study time at home are 1.4 and 1.2, respectively. This suggests that if the odds of being in the treated group are 1.4 and 1.2 times higher because of unobserved covariates, our findings for years of schooling and study time at home would change. The general conclusion is that while it appears that access to electricity had positive treatment impacts, the results are sensitive to bias due to unobservables for some outcomes.

[Insert Table 7]

7. Conclusion and Policy Implications

Many researchers have sought to link electricity access with economic development and poverty reduction, but the evidence base for this link remains limited and mixed (IEG, 2008). This study contributes to filling this knowledge gap by exploring the plausibly causal impact of access to electricity on income and schooling in rural Bhutan.

Results based on matching method indicate that access to electricity improved non-farm income and educational outcomes. The impact on non-farm income due to electricity can be as high as 76%. Children in electrified households gain an additional 0.72 years of schooling and spend more time studying in the evening. Taken together, this study showed that rural electrification has played an important role in improving the quality of life of households in rural Bhutan.

Our study has a few limitations. It is important to recognize that our cross-sectional analysis has potential shortcomings. The study only had access to post-intervention data collected in 2010, whereas treatment occurred between 2000 and 2006. It is possible that some of the variables included in the matching model may have been affected by electrification, thereby biasing our results. Furthermore, it is also possible that some households in the control villages are also privately electrified through solar panels, generators, or other sources of off-grid source. However, this is not a major concern as it would bias the estimates towards zero, and the results shown in Table 6 may reflect lower bound of the estimated impacts. Being connected to the electric grid does not always translate to having access to electricity throughout the day. The access to electricity variable used in this study neither captures number of hours households have electricity nor the quality of lighting. Additionally, household’s decision to get access to electricity may generate spillover effects and our study is unable to capture the spatial dependence and heterogeneity due to spillover effects. All these factors are likely to bias our estimates of the impact of electricity access.

The findings of this study have key policy implications. The use of electricity for income-generating activities in Bhutan has been very limited, but the potential to increase household income is quite high. While rural electrification is necessary, but may not be sufficient condition for expanding income opportunities. This requires substantial investments in complementary infrastructure, including access to roads, market development, irrigation systems, skills development, and services. Under the current scenario, the demand for electricity is likely to remain below lifeline block in Bhutan in short- to medium-term for most of the households. Integrated infrastructure development can create substantial multiplier effects, thus promoting and stimulating growth in the local economy. In order to spur rural development, RE program needs to be associated with mechanisms to provide credit for electricity using technologies (power tools, mills, sewing machines, lamps).

Furthermore, the study provides a sound basis for increasing investment in electrification in order to improve educational outcomes. According to the findings of this study, electricity increased years of schooling by 0.72 years. For the sake of discussion, we can estimate the labor market impacts of an increase in years of schooling due to electrification. The evidence on wage returns to education in developing countries shows that one additional year of schooling adds approximately 10% to a person’s income (Psacharopoulos and Patrinos, 2004). Using this estimate of returns to education, we estimate that access to electricity can increase wage by 7.4% by enhancing educational attainment in Bhutan in the long run. Greater study time at home in electrified households is another channel that can affect years of schooling and wage in Bhutan. Therefore, in a country with 53% adult literacy rate and secondary school participation rate of 54%, electricity provision can significantly improve educational attainment in Bhutan. Furthermore, with the current percentage of internet users at 25% in Bhutan and the government's emphasis on the introduction of information technology in the schools, access to electricity will continue to remain an important policy intervention.

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Table 1. Descriptive statistics of outcomes and matching variables

Variables	Whole sample	Household with electricity	Household without electricity	Difference (2)-(3)
	(1)	(2)	(3)	(4)
PANEL A				
Economic Outcomes				
Annual farm income (log)	5.56	5.59	5.53	0.06
Annual non-farm income (log)	3.60	3.81	3.27	0.54***
Education Outcomes (7-18 years old)				
Literacy	0.85	0.88	0.80	0.08***
Years of schooling	3.18	3.48	2.64	0.84***
Study time at home (minutes per day)	72.57	75.75	65.32	10.43***
PANEL B				
Matching variables				
Household size	4.36	4.42	4.33	0.09
Gender of head of the household (Male=1)	71.21	0.69	0.73	-0.04**
Age of household head	49.74	49.47	49.72	-0.25
Whether household head is literate (yes=1)	0.25	0.21	0.27	-0.06***
Total number of literates in the household	1.63	1.45	1.74	-0.29***
Marital status of household head	0.73	0.71	0.75	-0.04**
Access to tap water	0.56	0.55	0.57	-0.02
Amount of land (acres)	3.39	3.76	3.24	0.52
House structure (brick=1)	0.71	0.63	0.75	-0.12***
Whether owns livestock (yes=1)	0.87	0.86	0.89	-0.03***
Religion of household head	0.7	0.65	0.72	-0.07***
Total population of the village	305.03	295.36	308.9	-13.54
Distance from Dzongkhag (km)	47.4	52.29	43.66	8.63***
Observations	2098	1304	794	

Notes: *, **, *** denote significance at the 10%, 5%, 1% level, respectively. Dzongkhag is the district headquarter.

Table 2: Probit estimates of household's access to electricity

Household characteristics	Coefficient	Standard error
	(1)	(2)
Household human capital assets		
Household size	-0.024	0.057
Square of household size	-0.005	0.005
Gender of head of household	0.126	0.078
Age of head of household	-0.019***	0.007
Square of age of head of household	0.000***	0.000
Marital status of head of household	0.096	0.080
Literacy status of head of household	0.035	0.082
Total no of literates	0.150***	0.029
Religion (1= Buddhist)	0.050	0.071
Household physical assets		
Main source of drinking water (1 = tap water)	-0.060	0.060
Type of house (1 = brick)	0.332***	0.70
Amount of agricultural land	-0.112***	0.022
Square of amount of agricultural land	0.002***	0.001
Own cow (1 = yes)	-0.075	0.131
Own bull (1 = yes)	0.022	0.133
Own horse (1 = yes)	-0.479***	0.134
Own poultry (1 = yes)	-0.141	0.107
Village-level variables		
Distance to district headquarter	-0.008***	0.001
Population of the village	0.000*	0.000
Wald chi-square	171.11	
p-value	0.000	
McFadden's Pseudo R-square	0.0635	
No of observations	2040	

Notes: Outcome variable is an indicator of households' electrification status.

*** p<0.01, ** p<0.05, * p<0.1

Table 3: Post-matching means of the variables

Household characteristics	Household with electricity	Household without electricity	% Bias reduction	t-stat
	(1)	(2)		(3)
Household size	4.32	4.29	67.6	0.34
Square of household size	22.70	22.84	89.1	-0.17
Gender of head of household	0.73	0.72	65.1	0.78
Age of head of household	49.64	50.25	-142.7	-0.92
Square of age of head of household	2745.2	2801.4	11.3	-0.87
Marital status of head of household	0.75	0.73	64.9	0.71
Literacy status of head of household	0.27	0.27	93.5	0.23
Total no of literates	1.72	1.69	89.6	0.50
Religion (1= Buddhist)	0.72	0.72	99.3	1.44
Access to tap water)	0.57	0.55	-37.5	1.02
Type of house (1 = brick)	0.75	0.75	96.2	-0.26
Amount of agricultural land	2.85	2.96	78.8	-0.77
Square of amount of agricultural land	18.55	24.43	96.8	-1.01
Own livestock (1 = yes)	0.87	0.84	33.9	1.89
Distance to district headquarter	43.71	43.89	98.0	-0.14
Population of the village	308.01	300.17	42.1	0.73

Notes: *, **, *** denote significance at the 10%, 5%, 1% level, respectively. Matched samples are constructed using nearest neighbor with replacement and common support.

Table 4: Absolute bias, pseudo-R^2, LR

	Pseudo-R^2	LR χ^2	$p > \chi^2$	Standardized bias
	(1)	(2)	(3)	(4)
Unmatched	0.060	160.39	0.000	9.53
Matched	0.003	9.60	0.887	2.56

Table 5: OLS-impacts on household income and children's schooling

	Economic outcomes		Educational outcomes		
	Log farm income	Log non-farm income	Literacy	Years of schooling	Study time at home
	(1)	(2)	(3)	(4)	(5)
Access to electricity	0.204	0.495***	0.030*	0.546***	10.17***
	(0.189)	(0.179)	(0.015)	(0.113)	(03.478)
Household controls	yes	yes	yes	yes	yes
Village controls	yes	yes	yes	yes	yes

Notes: Income is expressed in log form. Educational outcomes are for 7-18 years old children in the household. Economic outcomes and study time are at household level. Household control variables are household size, gender, religion, marital status and literacy of the household head, total number of adult literate members in the household, cultivable land area, access to potable water, housing structure, ownership of livestock; and village control variables are village population and distance to *dzongkhag* headquarter from the village.

*, **, *** denote significance at the 10%, 5%, 1% level, respectively.

Table 6: ATT effects of household electrification on income and schooling

	Matching methods						
	Nearest neighbor	Caliper		Local linear		Kernel	
	(5)	d=0.01	d=0.001	bw=0.1	bw=0.2	bw=0.1	bw=0.2
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Economic outcomes							
Log farm income	0.225	0.221	0.119	0.296	0.261	0.163	0.073
	(0.256)	(0.227)	(0.239)	(0.251)	(0.247)	(0.181)	(0.178)
Log non-farm income	0.680***	0.736***	0.689***	0.763***	0.758***	0.672***	0.612***
	(0.211)	(0.205)	(0.179)	(0.199)	(0.193)	(0.197)	(0.179)
Educational outcomes							
Literacy	0.029*	0.027*	0.016	0.027*	0.025*	0.038*	0.051***
	(0.017)	(0.014)	(0.018)	(0.017)	(0.013)	(0.015)	(0.018)
Years of schooling	0.589***	0.602***	0.553***	0.606***	0.595***	0.657***	0.720***
	(0.133)	(0.118)	(0.142)	(0.144)	(0.115)	(0.105)	(0.126)
Study time at home (minutes per day)	10.11***	9.05**	12.05***	11.48***	11.07***	9.91***	9.35***
	(4.82)	(4.35)	(4.43)	(3.68)	(3.78)	(3.57)	(3.19)

Notes: Bootstrapped standard errors are shown in parenthesis. Kernel uses normal density. Nearest neighbor done with replacement with five neighbors. Educational outcomes are for 7-18 years old children in the household. Economic outcomes and study time are at household level.
*, **, *** denote significance at the 10%, 5%, 1% level, respectively.

Table 7. Rosenbaum bound sensitivity analysis test for hidden bias

	p-critical			
Gamma (Γ)	Log (non-farm income)	Literacy	Years of schooling	Study time at home
1	< 0.0001	< 0.0001	< 0.0001	0.004
1.1	< 0.0001	< 0.0001	< 0.0001	0.05
1.2	< 0.0001	< 0.0001	0.0002	0.231
1.3	0.002	< 0.0001	0.010	0.539
1.4	0.044	< 0.0001	0.109	0.808
1.5	0.254	< 0.0001	0.406	0.944

*Note*s: Study time at home is in minutes. Gamma is the log odds of differential assignment due to unobserved factors. Rosenbaum bounds are reported only for the significant ATT estimates in Table 6.

Figure 1. Distribution of propensity scores for electrified and non-electrified households

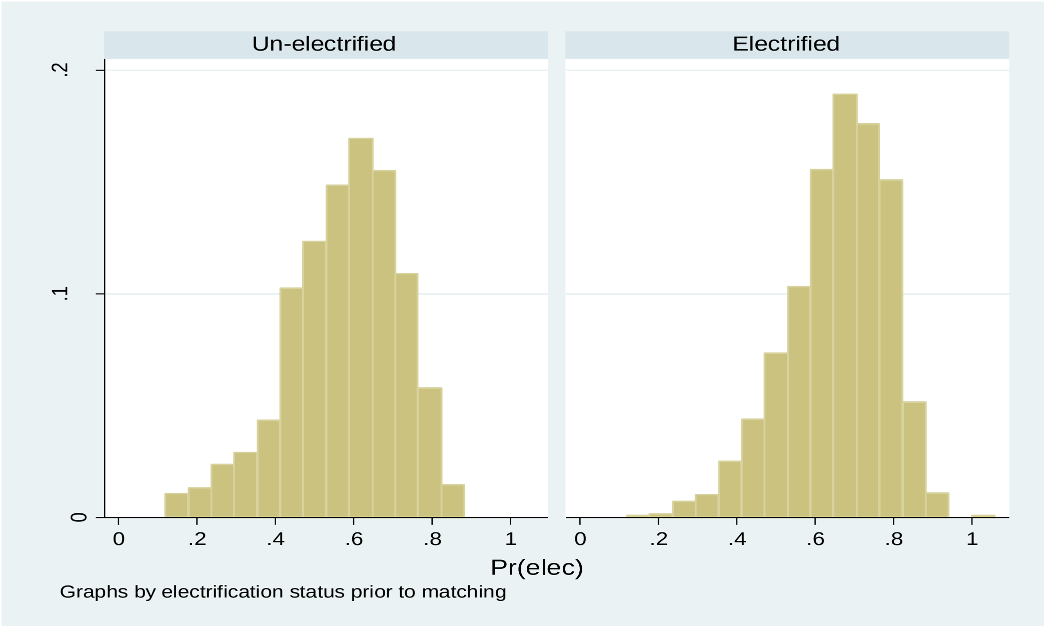

Figure 2. Overlap and common support