Impact Evaluation of SU-L1009: Support to Improve the Sustainability of Electricity Services

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November 25, 2020

Abstract

This paper evaluates the impact of a rural electrification program benefiting indigenous and maroon communities in Suriname. Using quasi-experimental methods we find that the program increased ownership of electric durables, reduced expenditure in non-grid energy, reduced migration and increased household income and subjective welfare. However, many of the effects are not statistically significant due to lack of statistical power. Moreover, while we find positive effects on wage income, the effect on other income sources and time use is ambiguous.

JEL classifications: O12, O13, Q40, L31,

Keywords: Electricity, Rural electrification, Indigenous communities, Maroon communities, Suriname.

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1 Introduction

Access to electricity is considered an essential driver for development, both for businesses and households. Earlier literature on the macroeconomic impact of electrification is vast and shows that electrification has a positive impact on the economy (see Burke et al. (2018) for a review). Results from micro studies are more mixed. Large scale micro studies using secondary data find large positive effects of electrification on various outcomes (Dinkelman, 2011; Rud, 2012; Lipscomb et al., 2013; Kassem, 2020). However, recently, a randomized control trial in Kenya failed to detect any effects (Lee et al., 2020).

This report presents the impact evaluation of a rural electrification program implemented in Suriname between 2016 and 2019 by *Energie Bedrijven Suriname* (EBS) with funding from the Inter-American Development Bank. The objective of the program was to extend access to high quality, 24-hour per day electricity service to 5 villages in Surinamese hinterland: Powakka, Redi Doti, Pierre Kondre Kumbasi, Cassipora, and Pokigron, which were previously connected through a village generator for 6 hours a day.

Using a Difference in Difference approach, we evaluate the effect of the program on household income, time use, energy consumption, and subjective welfare.[1] For this purpose, two household surveys were conducted: baseline and follow-up, carried out on a set of treated and control villages. This analysis was complemented by using a Propensity Score Matching approach to select a sample with similar observable characteristics at baseline.

We find that the program increased ownership of electric durables and decrease expenditure in non-grid energy, although these effects are not precisely estimate. The effect on battery expenditure, however, is negative and statistically significant. Moreover, we find that treated households were less likely to move out of the village or to have household members leave the households for education or work purpose. Although we find no effect on time use, we find that the program significantly increased household income and, in particular, wage income in treated villages. The effect of the program on subjective welfare was found to be positive but

[1]Subjective welfare refers to how respondents evaluate the lives of their household and those other households in their village based on their perception of happiness an well-being. Specifically, for this study, subjective welfare was measured by showing respondents the picture of a ladder with nine rungs and asking them on which rung they would place either their household or their village considering that the “poorest in Suriname” would be placed on rung 1 and the “richest in Suriname” would be place on rung 9. Respondents were instructed to think of their wealth not only in terms of money but also in terms of “number of healthy kids, fresh air, sufficient food, access to clean drinking water, etc”.

not statistically significant.

This is the first impact evaluation study of a government electrification program in Suriname and, to our knowledge, the first study the looks at at the impact of access to reliable and consistent electricity, instead of new connections.[2]

The rest of this document is structured as follows. Section 2 describes the context. Section 3 illustrates the main features of the program and its theory of change. Section 4 presents the evaluation design and data collection. Section 5 describes the econometric approach. Section 6 presents the results. Section 7 concludes.

2 Context

Suriname is the youngest sovereign country in South America. The country covers an area of 163,820 square kilometers and has a population of approximately 567,000 people. Approximately 90% of the population lives in the coastal area. The interior of Suriname (the Hinterland), which extends to the Amazon Rainforest, is sparsely inhabited. Many of the estimated 217 villages in the interior can only be reached by boat or plane.

The electricity sector in Suriname is based on contractual arrangements between the State and public and private companies. The responsibility for the sector is assigned to the Ministry of Natural Resources (MNH). *Energiebedrijven Suriname* (EBS) is a state-owned company under the supervision of the MNH which operates under a 50-year, countrywide concession covering transmission, distribution, and commercialization of electricity. The electricity sector in Suriname consists of individual grid systems. Paramaribo and the surrounding areas are interconnected to the EPAR grid of EBS, which serves about 79% of the population.[3] Smaller grids, operated by EBS and powered by thermal generators on a 24-hour basis exist in the western part of the country and in the main towns in the coastal plain. Electricity supply in the Hinterland is under the mandate of *Dienst Electrificatie Voorziening* (DEV), which is an agency of the MNH.

Before the implementation of this project, about 130 villages had diesel generators installed, serving an estimated population of 30,000 people. About 100 of these villages were provided with diesel fuel by DEV on a monthly basis. This is still the current situation for the villages that

[2] Jeuland et al. (2021) provide a recent review of the literature on the impact of traditional electricity, which shows that no other studies have been conducted in Suriname prior to this impact evaluation.

[3] As of 2016 that some villages in the coastal area and in the interior (Marowijne District, Brokopondo District) were also connected to the EPAR grig of EBS.

did not benefit from the program object of this report. The electricity service was designed for an average time of 6 hours per day (from 5 pm to 11 pm). Rural households were not charged for the service as all the costs are absorbed by the Government of Suriname (GOS). Fuel supply was constrained due to cost and logistical reasons and in some villages it is unavailable for long periods.

3 Description of the program & theory of change

Implemented by *Energie Bedrijven Suriname* (EBS) between 2016 and 2019 with funding from Inter-American Development Bank (IDB), the program aimed at improving the sustainability of electricity services in Suriname by strengthening the institutional capacity of EBS and expanding electricity coverage in the Hinterlands. This program was aligned with the the energy policy of the GOS (Development Plan 2012-2016), which aimed at providing affordable and reliable electricity for all as well as pursuing national provision (IDB, 2013a).

This impact evaluation focuses on Component II of the project, which consisted in connecting Powakka and surrounding villages into the electricity grid and on the installation of a hybrid renewable energy systems to improve sustainability of electricity supply in Atjoni and nearby villages.

Before the project, the targeted villages received free energy through subsidized and costly thermal power, during 5-6 hours a day through off-grid diesel generator. The intervention aimed to provide them with access to better quality, 24 hours per day, electricity.

The households benefiting from the program were supposed to actively connect themselves to the electricity grid by paying for their indoor installation (the cost of connection from the pole to the house was covered by the program). Moreover, once connected, they would be subject to a new tariff scheme for the electricity they consumed (i.e. they would lose access to the subsidized electricity previously provided through the village generator). Extensive consultations conducted prior to the program indicated that households were willing to pay for 24 hours a day stable electricity, but not for the limited and unreliable service they were receiving before.

3.1 Expected results

The program was motivated by the fact that lack of access to reliable electricity services constrains households in their use of time, consumption possibilities, and economic activities. Its Monitoring and Evaluation (M&E) plan (IDB, 2013b) provided a detailed list of expected outcomes there were meant to be assessed as through a proposed impact evaluation. In this section we provide a summary of the expected results of the program as described in that document, enriched using information obtained from the Socio-environmental assessment of the program (IDB, 2013c).

Access to reliable electricity was expected to allow households to benefit from a larger set of home appliances, resulting in time savings in household chores (e.g. washing can be accomplished more quickly using a washing machine). As a results, it could increase labour market participation of women via the development of small home businesses or more time devoted to income generating activities (Dinkelman, 2011). Moreover, as an input for economic activities, access to electricity could trigger opening of new businesses or increase the potential of existing firms, hence increasing household income.

Similarly, electricity extends evening lighting hours, making it easier for children to study, do homework, or read. Similarly, it enables schools to be equipped with modern teaching equipment and information and communication technologies, especially access to the internet (Meier et al., 2010), which in turn may improve education.

Access to modern source of energy could also have a positive impact on health as it directly contributes to a reduction of respiratory illness among the rural population caused by the use of polluting sources of energy inside the house such as kerosene lamps, reducing both public and private healthcare costs.

Moreover, more economic opportunities combined with a larger set of leisure possibilities (e.g watching television), and lower crime rates (due to better visibility at night) where expected to have a positive impact on household's subjective welfare. Finally, the improved provision of services was expected to increase demographic consolidation, and hence reduce migration away from the rural villages.

Since it is expected that the impact of the program could be different by gender, we will evaluate these questions differentiating by the gender of the household's head.

It is important to note that all the impacts are not necessarily positive. Access to electricity may also have some harmful effects. For instance, in the case of education, it is possible that access to electricity may induce a time allocation that favours substitution towards leisure. Therefore, the impact of the program in terms of time use remains an empirical question. Moreover, recent evidence is casting doubt on whether expanding grid based rural electrification is a good investment since it is unlikely to yield large benefits in the short run.[4] Indeed, a randomized controlled trial conducted in Kenya failed to detect any positive effects (Lee et al., 2020).

4 Evaluation design & data

This impact evaluation relies on two quasi-experimental methodologies: the first based on a Difference-in-Difference (DD) strategy and the second based on a Propensity Score Matching (PSM) approach.

The DD strategy consists of comparing the *change* in the mean of the outcome variable(s) for the treated households with that of a selected households for comparison (the control group). In addition to this, the PSM strategy applies a matching algorithm to select a subset of treated and control household that present similar observable characteristics at baseline, to increase comparability.

To implement this strategy, a baseline survey was conducted in 2016, prior to the beginning of the program, and a follow-up survey in 2020, after the program took place. The original evaluation design, which was based primarily on the PSM approach, had planned to survey a larger sample of control households at baseline and then select those that presented observable characteristics close to the household to be treated and re-interview only this sub-sample as part of the follow up survey.

The analysis of the baseline data conducted by Mullally (2016) indicated that the application of a matching algorithm would allow to successfully select a control group with observable characteristics similar to those of the treated community, however this would come at the cost of significantly reducing sample size and limiting statistical power. In fact, power calculations suggested that, even if the program had achieved large impacts on the outcomes of interests, the likelihood to detect them with this methodology would have been low.

Therefore, it was decided to conduct the follow-up survey with the entire baseline sample,

[4] https://www.economist.com/international/2019/02/09/electricity-does-not-change-poor-lives-as-much-as-was-thought

so as to have the possibility to conduct a DD analysis on a larger sample and complement it by applying a PSM methodology as a robustness check.

4.1 Selection of the control group

While no detailed data were available at the household or village level (the national statistical authority, ABS, did not collect data at village level and was not permitted by law to share household-level data from the 2012 Census with third parties including EBS and IDB), some information on villages was available to aid in selection, including which villages had limited access to electricity from diesel generators owned by DEV, as well as predominant ethnicity and population sizes (from CBB, Centraal Bureau Burgerzaken, a department of the Interior Ministry of the Government of Suriname). Data of registered villages was additionally received from the Ministry of Regional Development.

This information available prior to data collection made it possible to describe the group of five treated villages and to choose a set of 12 control villages with similar characteristics. First, the group of treated villages included in the evaluation consists of a Maroon village, Pokigron, a single large indigenous village (Powakka), and three smaller indigenous villages (Redi Doti, Pierre Kondre Kumbasi, and Cassipora).[5] All villages could be reached by road from Paramaribo. In addition, all of these villages had limited access to electricity from DEV generators, and while none could be described as rich or urban, the beneficiary villages were far from being among the poorest or most remote population centers in rural Suriname.[6]

Therefore, the final list of villages to be surveyed included all five treated villages and twelve control villages (see figure 1). Out of the twelve control villages, two are large Maroon villages (Adjoemakondre and Moengotapoe), three large indigenous villages (Pikien Saron, Bigi Poika, and Donderskamp) and several small to medium sized indigenous villages (Tapoeripa, Kalebaskreek, Corneliskondre, Tibit Brug, Alfonsdorp) all are located near rivers, major roads, or both. Lastly, the control group included a village located on a river that is populated by both indigenous and Maroon individuals (Bigiston). One Maroon village originally included in the control group (Ricaunomoffo) was excluded from the sample because on closer examination,it

[5]Pokigron/Atjoni is usually considered as two separate villages. However, in this report we present as one entity as this is how it reported in the data.

[6]We have no information on the specific criteria used to determine whether a village could be considered "large" or "small", therefore we rely on the classification provided by Mullally (2016).

already had 24-hour per day electricity.

The decision of surveying more villages and households in the control group compared to those to be treated is commonly recommended when the identification strategy is based on matching methods. This helps to minimize potential bias, by increasing the likelihood that for each treated unit, a similar control unit in key characteristics will be available for comparison. In addition, this approach was justified by the fact that data limitation did not allow to credibly select a control group prior to the baseline survey. Therefore, the selected approach was to expand the control group and then select a subset of treatment and control households with similar observable characteristics.

4.2 Data collection

Two questionnaires were written for data collection: a community questionnaire, in which key informants were asked to provide basic information on each village (access to services, power outages, etc.) and enumerators collected prices on fuel and light sources, and a household questionnaire that was written in order to collect data on outcome indicators as well as household characteristics that would be used to construct a matched sample. The structure of both questionnaires followed the model set by the World Bank Living Standards and Measurement Surveys (LSMS). The energy consumption modules were based on O'Sullivan and Barnes (2007). The specific language of survey questions was adjusted based on input from the data collection consultants hired to collect the baseline data (Suribraz and Social Solutions), EBS, and through revisions based on the results of the pilot of each questionnaire.

4.2.1 Baseline Survey

Baseline data collection was conducted in early 2016 by a consortium of two Surinamese firms: Social Solutions and Suribraz.[7] Prior to data collection, the questionnaire was piloted in the Maroon village of Kwakoeron, involving 25 household and two community surveys. This pilot lead to significant revisions and adaptations of the surveys, although leaving their overall structure basically intact. Once the instrument were edited, enumerator training took place in Paramaribo, starting January 4th, 2016, under the supervision of both data collection firms and EBS representatives. Following the completion of training and some further adjustments to the

[7] For detailed information on baseline data collection see Mullally (2016).

survey instruments, data collection started and was completed in seven weeks. Data was collected using paper questionnaires and later entered by data entry teams who provided additional layers of quality control.

Mullally (2016) reports that numerous challenges were encountered during data collection. The main reason is that both firms did not have prior experience with data collection involving highly detailed questionnaire and large sample sizes. Indeed, both of them admitted underestimating the scope of the survey and the amount of time that should have been dedicated to training. Unfortunately, these challenges had a negative impact on data quality, which resulted in a significant number of surveys having to be rejected due to missing values or unusable responses. While the two firms conducted 839 surveys in total: 309 in villages to be treated and 530 in control villages, after the data was cleaned, only 818 households remained. Table 1 reports the number of households that participated in the baseline survey, by village.[8]

Finally, the analysis of the baseline data conducted by Mullally (2016) revealed that, although treated and control households presented differences across multiple dimensions, the application of a PSM algorithm would allow for the identification of a subsample with similar observable characteristics. However, the resulting sample size would be limited.[9]

4.2.2 Follow-up Survey

Follow-up data collection was conducted in between February and May 2020, two to three years after the treated villages were connected to the EBS grid. The survey was conducted by Social Solutions, one of the firms that was involved in the baseline survey. In order to maximize sample size and, as a consequence, statistical power, the target sample for the follow-up survey was the entire set of households included in the baseline dataset. This included 818 households, 308 in treated villages and 510 in control villages.

Prior to data collection, in December 2020, supervisor training and piloting activities were conducted, under the supervision of a team from the IDB and EBS. The survey was piloted in two villages: Kwakoeron, the same village in which piloting occurred prior to baseline data collection; and Klein Powakka, a village connected to the EBS grid in the proximity of one of the

[8]The baseline report (Mullally, 2016) further reduced the sample by dropping additional households that presented missing values in some variables, obtaining a final sample of 788 households.

[9]Mullally (2016) applied a slightly different algorithm than the one we propose in this report and obtained a matched sample size constituted of 400 households: 275 treated and 121 control.

treated villages, Powakka. As a result of piloting the survey instrument instrument was adjusted and finalized. Particular attention was paid to ensuring that the Social Solution team gained familiarity with the data collection software. In fact, unlike the baseline, which was conducted using paper questionnaires, follow-up data collection relied on the use of tablets. Specifically, the questionnaires were programmed using Survey Solutions, a software developed by the Data group at the World Bank. This allowed for real time data quality checks (e.g. by limiting the range of certain numerical variables) and for partly pre-loading data collected through the baseline survey such as the names of household members, which facilitated household identification. Although digital data collection has several advantages and dramatically increased data quality, lack of experience of the survey firm with this technology generated some challenges in the initial phase of data collection (the first set of surveys conducted did not have the correct pre-loaded data) and some inefficiency throughout the process in terms of monitoring progress and conducting quality checks.

The survey was initially carried out in person, however, after COVID-19 travel restrictions and social distancing guidelines went into force in March 2020, it was decided to continue the survey through telephone interviews. It is important to mention that, by the time COVID-19 restrictions came into effect, all treated villages and 5 control villages had already been visited, while 7 control villages had not been visited. Figure 1 illustrates which villages were interviewed in person and which over the phone.

As a consequence of this change in data collection methodology, the survey instrument had to be adjusted. Indeed, it is best practice to keep telephone-based surveys to a maximum of 45 minutes, which would not have been feasible with the original questionnaire. For this reason, it was decided to cut the time use and the subjective welfare section from the survey. Two criteria were used to establish which sections to cut: (i) ease in collection of the data over the phone and (ii) credibility of the results. Both time use and subjective welfare section were considered to be challenging to be conducted over the phone, the former because it required to speak directly with a person different from the main respondent (e.g. a child or another adult household member), the latter because it envisioned showing the picture of a ladder to the respondent, which could not be done easily over the phone. The fact that most treated households had already been surveyed implied that the remaining sample to be surveyed over the phone disproportionately represented control households. Since the pandemic could have had an effect on both time use (because of school closure and travel restrictions) and subjective welfare (by causing uncertainty, stress, and anxiety) any result related to these outcomes obtained after COVID-19 restriction were likely to be biased.

Overall, 83% of household that participated in the baseline were re-interviewed at follow-up (681 households), including both in person and telephone surveys. Common reasons for attrition were: (i) households moved outside the project area, (ii) death of household members (relevant for single member households), and (iii) impossibility to contact the household after three attempts. In very few cases households refused to participate (only two cases) or were unable to participate due to health problems (8 cases).[10] Moreover, 446 households were surveyed in-person and 235 over the phone (34.5%). Treated households were mostly surveyed in person (89%), whereas control households were equally split between in-person (51%) and phone interviews (49%).

Table 3, column (1) shows that treated households were slightly more likely to complete the survey, but the difference is small and not statistically significant. However, column (2) shows that treated households were significantly more likely to complete the survey in person, that is to be interviewed before the implementation of COVID-19 related restrictions.

4.3 Balance checks

Table 2 reports summary statistics and balance checks for households' demographic characteristics, economic activities, electricity use, and income, based on the data collected at baseline. We find that households to be treated had slightly smaller household size and were more likely to be Maroon. In terms of economic activities, households to be treated were less likely to own non-agricultural businesses, and, as a consequence had lower business profits. However, they had significantly higher profits from sales of crops. In terms of energy use, households to be treated owned and used more electric durables than households in the control group, but spent less on batteries. Finally, subjective welfare was generally higher in communities of villages to be treated.

Given the observed lack of balance across multiple observable characteristics, the baseline report produced for this study (Mullally, 2016) recommended using a PSM methodology to select

[10] More information on challenges encountered during data collection or reasons for attrition can be found in the Field Report prepared by Social Solutions.

a subsample of treated and control households with similar observable characteristics. The following sections discuss this methodological approach in details.

5 Empirical strategy

When feasible, program evaluation should be based on random assignment of treatment status. That is, the households to be treated should be randomly selected from a pre-defined population so that the remaining households could constitute a comparison group. The key benefit of randomization is that, in the absence of the program, treated and untreated households are expected to have similar observable and unobservable characteristics and, therefore, any differences in outcomes post treatment, can be directly and solely attributed to the program.

Since randomization of access to improved electrification was not feasible, as it often is the case for infrastructure programs, this impact evaluation needs to rely on quasi-experimental methods that require additional assumption to be satisfied in order for the results to be attributable to the program.

In this section, we describe the two methodologies that will be utilized for this analysis: Difference-in-Difference (DD) and Propensity Score Matching (PSM).

5.1 Difference-in-Difference

Within the DD strategy, the following equation is estimated in order to generate estimates of the average program impact:

$$y_{it} = \beta_0 + \beta_1 Treated_i + \beta_2 Post_t + \beta_3 Treated_i * Post_t + \epsilon_{it} \tag{1}$$

where y_{it} is an outcome variable of interest for household i in period t, such as income, $Treated_i$ is dummy variable equal to one if the household belongs to a treated village, $Post_t$ a period indicator taking the value one if the observation corresponds to the post-program period, and ϵ_{it} is an error term. The parameter β_1 measures the average pre-program difference in the outcome variable between treatment and control groups, while β_2 measures the time trend – or the average difference in the outcome variable in the post- versus pre-program periods for the control group. Finally, β_3 measures the average impact, or treatment effect, of the program.

The identifying assumption for β_3 to be an unbiased estimator of the causal impact of the program is that there are no systematic differences across treatment and control groups in terms of unobservable variables that affect the change in the outcome variable. This is the assumption of "parallel trends", which states that in the absence of the program the average change in the outcome variable of treatment households would have been the same as the average change of the control households.

5.2 Propensity Score Matching

Although the DD strategy only relies on the parallel trends assumptions, the credibility of the analysis is strengthened when treated and control households present similar observable characteristics at baseline. This is because it is reasonable to assume that when households present similar characteristics, they would be more likely to have experienced similar changes (e.g. growth) in the outcomes of interest. This assumption is known as Conditional Independence Assumption (CIA) and it implies that, conditional on observable characteristics, outcomes (or in our case "changes" in outcomes) are assumed to be independent of treatment status (in absence of the program).

If the CIA is satisfied, then the DD result would provide a causal estimate of the program. However, if treatment and control households present different observable characteristics at baseline such as in our sample, the results might be biased. For this reason, Mullally (2016) proposed to conduct the analysis on a sub-sample selected to present balanced observable characteristics. To achieve this goal, a *matched sample* was identified through a PSM methodology based on Rosenbaum and Rubin (1985), that is by matching treated and control household based on their estimated probability of being treated, given their observable characteristics at baseline (e.g. their *propensity score*). Conditioning on the propensity score is enough to have independence between the treatment indicator and the potential outcomes.

In this report, we follow the methodology proposed by Mullally (2016) and we estimate the propensity score through a logistic regression and conduct a single nearest neighbor matching with replacement using the liner index generated by the result of the logistic regression.[11] Figure 2 presents the distribution of the linearized propensity score for the full sample (top panel) and

[11] We run the same logistic regression proposed by Mullally (2016) with the only differences that (i) we restrict the sample to households that complete the follow-up survey and (ii) we exclude time use variables as they present missing values for a large set of households (once coded correctly).

for the matched sample (bottom panel). As Mullally (2016) we are able to obtain a matched sample with similar observable characteristics. However, given that we have to further restrict the sample to the set of households that participate in both baseline and follow up survey, the sample size available for the PSM analysis is further reduced to 337 households: 237 in treated villages and 100 in the control group.[12]

It is important to notice that, while restricting the sample to treated and control households that present similar observable characteristics at baseline might reduce the bias in our estimates (if any) and increase credibility, this sample size reduction has negative impact on statistical power. That is, our ability to reject the null hypothesis that the program had zero effect. Therefore, in this report we present both DD and PSM results.

5.3 Outcome variables

We base our analysis on the set of outcomes indicated in the baseline report (Mullally, 2016). These include: value of electric durables, energy consumption, time use (time spent studying and working by adults and children), household income, and subjective welfare (households are asked to evaluate their socioeconomic status on a nine point scale, and then to do the same for their communities as a whole). Additionally, we consider an outcome that appeared to be relevant when the socio-environmental assessment of the program was conducted(IDB, 2013c): migration. Where possible, we report heterogeneous effect by gender of the individual (e.g. for time use) or by gender of the household head.

The original M&E plan for the project (IDB, 2013b) mentioned additional outcomes such as health and crime. However, after the baseline data collection was completed and analyzed by Mullally (2016), the list of outcomes was reviewed to take into consideration the characteristics of the final survey instruments and the data. Indeed, the crime module originally thought to be included in the survey was later removed and the baseline data suggested that there was not enough variation in the health outcome to warrant further investigation on this.

[12] Mullally (2016) had obtained a matched sample of size 400 formed by 279 treated households and 121 control households.

6 Results

In this section, we discussed the estimated effect of the program on its expected outcomes presented above. We present both DD and PSM estimates. To ensure that variables expressed in monetary terms (SRD) measured during baseline and follow-up survey are comparable, we deflate follow-up survey data using the CPI index and present all results in terms of SRD of 2015.[13]

Energy: table 4 reports the effects of the program on energy access and use. First, we check whether the program worked as expected, that is treated households got connected to the EBS grid and households in the control group did not. Column 1, panel A, presents the results. While nobody was connected to the EBS in 2016, by the time of the follow-up survey 2% of the households in the control group and 99% of the households in the treatment group were connected. We confirmed that all the control group households that got connected to the EBS grid (9 households in total) moved away from the village in which they were surveyed at baseline. Panel B reports the same results for the matched sample obtained with the PSM methodology. In this subsample only 0.4% of the control households (only 1 household) was connected to the EBS grid at the time of the follow-up survey.

Second, we estimate the effect of the program on the value of electric durables owned by the household, consumption of non-grid energy, expenditure in batteries and other energy sources. Column 2, shows the effect of the program on the value of electric durables owned by the households. Both the DD and PSM methodologies show that the program had a positive effect on this outcome, although this result is not statistically significant. Column 3 shows the effect of the program on yearly consumption of non-grid electricity, expressed in kWh. Both methodologies failed to reject the null hypothesis that the effect of the program on this outcome is zero. In fact, the point estimate obtained with the DD strategy (panel A) shows an increase in consumption of non-grid electricity, whereas the estimate obtained with the PSM methodology (panel B) show a substantial decrease. Column 4 shows the effect of the program on annual expenditure on batteries. The DD estimate (panel A) shows that the program decrease household expenditure on batteries by 511 SRD. The PSM estimates (panel B) are similar but not statistically signif-

[13] Given that the baseline survey was conducted in early 2016 and the follow-up survey in early 2020, both surveys largely report values effectively realized in the previous calendar year. For this reason, we deflate the follow-up data using the CPI index for 2019, relative to 2015.

icant. Finally, column 5 reports the effect of the program on overall expenditure in non-grid energy. Both the DD and the PSM point estimates are negative and large, showing that the program decreases households expenditure on non-grid energy by 1000-1500 SRD. However, these results are not statistically significant.

Migration: to assess whether households in the treated villages were less likely to migrate out of the village we construct a dummy variable which is equal to one if one of the following conditions are satisfied: (i) the household did not complete the follow-up survey because they moved out of the survey area; (ii) the household could be traced during the follow-up data collection but other village members reported they had moved out of the village; or (iii) the household completed the follow-up survey and reported moving out of the village. Table 3, column 3, shows that, while 4.7% of households in the control group left the village in which they were located at the time of the baseline survey,treated households were 0.8 percentage points less likely to do so (a 17% decrease), however this result is not statistically significant.

Moreover, we consider whether individual household members were more likely to move out of household for either school or work purposes. However, this analysis is restricted to the households that completed the follow-up survey as we do not have information on individual household members for the households that did not. Table 3, column 4, shows that treated households are 7.9 percentage points less likely to have at least one household member that left the household for work or school purposes (a 29% decrease) compared to the control group.

While these results are suggestive of the fact that the program has increased economic opportunities within treated villages and thus decreased migration, we cannot claim that they are causal. In fact, we cannot conduct a DD analysis as we have no information on migration patterns at the time of the baseline survey, which might have been different for treated and control villages. However, we can still apply the PSM methodology (column 5). We find that treated households were 1.3 percentage points less likely to have members who migrated for school or work purposes compared to the matched control group, although this result is not statistically significant.

Time use: table 5 shows the effect of the program on numbers of hours spent working (for others) and number of hours spend working for the household business, for male and female adults, and on number of hours spent studying, for male and female children. Using the DD strategy (panel A) we cannot reject the null hypothesis that the program generated no changes in time use in treated villages. The PSM results, presents on panel B, are also not statistically significant for most of these outcomes, except for the effect on number of hours worked by men, which appears to be negative and marginally significant. It is worth noting that these results are based only on the set of households that were interviewed in-person before the COVID-19 related restrictions were implemented, hence the sample size is limited.

Income: table 6 presents the effects of the program in terms of income and number of businesses owned by the household. We consider three main income sources: business profits, crop profits, and wage income as well as total household income. Column 1 reports the results for number of businesses owned by the households. Using the DD strategy (panel A) we find that the number of businesses owned by treated households significantly increased because of the program. This result, however, is not confirmed by the PSM estimate, which is negative, although not statistically significant. Columns 2 and 3 report the effect of the program on business and crop profits, respectively. In both cases we cannot reject the hypothesis that of zero effects both for the DD and the PSM estimators. Moreover, the sign of the point estimates is not consistent across methodologies. The effects of the program on wage income and total household income are reported in columns 4 and 5. Using the DD methodology we find that both these effects are positive and statistically significant. Specifically, the program increased annual wage income by 1750 SRD and total household income by 4050 SRD (a 37.5% increase with respect to the control group mean at baseline). The PSM methodologies confirms the direction of these results but the estimated coefficients are smaller and not statistically significant.

Perceived Welfare: table 7 reports the DD and PSM results for the effect of the program on self-reported perceived welfare. This outcome was measured by showing respondents the picture of a ladder with nine rungs and asking them on which rung they would place either their household or their village considering that the "poorest in Suriname" would be placed on rung 1 and the "richest in Suriname" would be place on rung 9. Respondents were instructed to think of their wealth not only in terms of money but also in terms of "number of healthy kids, fresh air, sufficient food, access to clean drinking water, etc".

The DD results presented in table7, Panel A, shows an increase in perceived level of welfare both from the household point of view (column 1) and from the village point of view (column 2).

However, these effects are not statistically significant. The PSM results, presented in Panel B, confirm the results that the effect of the program was positive on both outcome. Moreover, the point estimate for the household level effect is almost double as large as the DD coefficient (column 1), although still not statistically significant, while that of the village level effect shows a statically significant increase in welfare of 1.4 “rungs”, a 28% increase with respect to the control group mean. However, as for time use, these outcomes are only available for the households that were surveyed in person during the follow up survey, hence the sample size is limited.

6.1 Heterogeneity

Appendix B.1 presents heterogeneous effects by gender of household head and for each of the treated village. It is important to notice that the sample size for each of the subgroups considered is limited and, therefore, this analysis does not the have the necessary statistical.

However, this analysis shows that most of the effects of the program (increase in value of electric durables, decrease in expenditure on non-grid energy, and increase in income) are lower for female headed housholds (tables 9 and 10). The effect of the program on subjective welfare, instead, is very similar for male headed and female headed households.

It is also interesting to compare the effect of the pogram on the first village connected, Powakka, which was connected in early 2017, and the last village connected, Pokigrom, which was connected in early 2018 (the remaining villages were connected in mid 2017). Having access to high quality electricity for one year longer might have increased the observable effects of the program, however we find no evidence of such pattern and, in fact, the program appears to have had stronger effects on income and subjective welfare in Pokigrom (although the difference is not statistically significant).

7 Conclusion

This paper used a rigorous quasi-experimental approach to evaluate the impact of the provision to high quality 24/7 electricity to households in rural Suriname.

The findings suggest that the program increased ownership of electric durables and decrease expenditure in non-grid energy, although these effects are not precisely estimate. The effect on battery expenditure, however, is negative and statistically significant. Moreover, we find that treated households were less likely to move out of the village or to have household members leave the households for education or work purpose. Although we find no effect on time use, we find that the program significantly increased household income and, in particular, wage income in treated villages. The effect of the program on subjective welfare was found to be positive but not statistically significant.

Further analysis should take advantage of the richness of the data to shed light changes affecting specific energy sources as well as specific economic activities. Moreover, the robustness of the DD methodology should be assessed using alternative matching techniques and controlling for observable characteristics.

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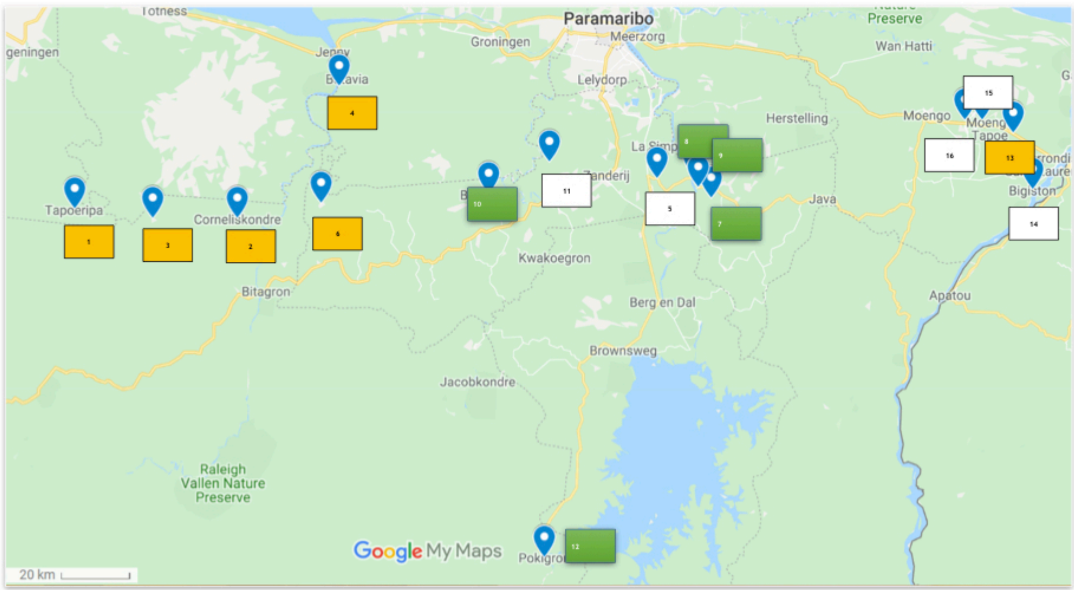
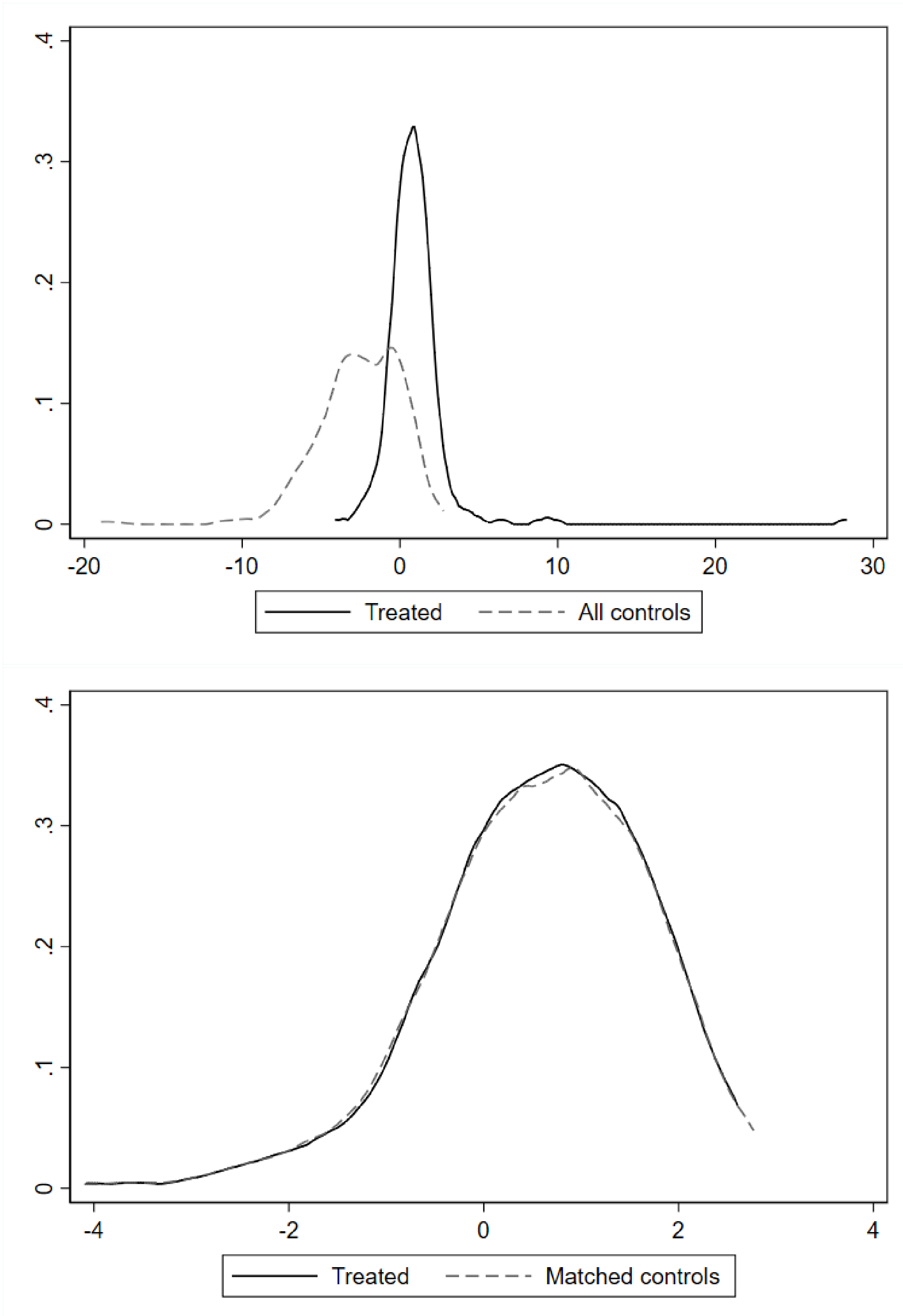
A Figures

Figure 1: Project map



Notes: Green: treated villages surveyed in person at endline; White: control villages surveyed in person at endline; Yellow: control villages survey over the phone at endline.

Figure 2: Propensity Score Distribution



B Tables

Table 1: Sample Size by Village

Control Villages	Baseline Tot	Baseline Clean	Endline	Treated Villages	Baseline Tot	Baseline Clean	Endline
Tapoeripa	11	11	10	Cassipora	21	21	18
Corneliskondre	17	17	14	Pierre Kondre Kumbasi	10	10	10
Donderskamp	55	55	47	Redi Doti	30	30	25
Kalebaskreek	38	38	33	Powakka	121	121	115
Bigi Poika	62	61	51	Pokigron	127	126	91
Tibiti Brug	13	13	9				
Pikien Saron	89	89	78				
Alfonsdorp	71	61	52				
Bigiston	60	58	40				
Moengotaope	74	68	54				
Adjoemakondre	40	39	34				
Tot	530	510	422		309	308	259

Table 2: Summary Statistics & Balance - Household Level

	Control (1)	Treated (2)	(1) vs. (2) (3)
Household size	3.83	3.54	0.29*
	(0.11)	(0.12)	(0.17)
HH head female	0.45	0.50	-0.05
	(0.02)	(0.03)	(0.04)
HH head age	54.16	55.21	-1.05
	(1.43)	(1.87)	(2.34)
HH head completed primary edu	0.58	0.64	-0.06
	(0.02)	(0.03)	(0.04)
Indigenous %	53.49	47.89	5.59
	(2.09)	(2.70)	(3.41)
Maroon %	28.75	37.82	-9.07***
	(1.98)	(2.75)	(3.33)
Owns agricultural processing business	0.03	0.00	0.03***
	(0.01)	(0.00)	(0.01)
Owns manufacturing business	0.02	0.01	0.01
	(0.01)	(0.01)	(0.01)
Owns retail business	0.03	0.03	0.00
	(0.01)	(0.01)	(0.01)
Number of HH non-ag businesses	0.19	0.10	0.09***
	(0.02)	(0.02)	(0.03)
Non-self employed laborer	0.55	0.61	-0.06
	(0.02)	(0.03)	(0.04)
Farmer	0.21	0.18	0.03
	(0.02)	(0.02)	(0.03)
Fisherman	0.22	0.03	0.19***
	(0.02)	(0.01)	(0.02)
Value of electric durables used by household, SRD	2254.54	2848.86	-594.32***
	(106.26)	(218.93)	(218.24)
Annual household energy consumption, non-grid sources, kWh	8094.48	8902.19	-807.71
	(725.84)	(1972.33)	(1795.58)
Annual expenditure on batteries, SRD	267.40	195.47	71.93**
	(19.58)	(22.67)	(30.74)
Annual expenditure on non-elect energy, SRD	4886.50	5464.74	-578.24
	(449.27)	(1162.44)	(1072.88)
Total profits from HH businesses, SRD per year	953.18	428.77	524.41*
	(194.13)	(197.05)	(293.02)
Total crop profits, SRD per year	282.17	1211.61	-929.43***
	(111.81)	(401.37)	(343.40)
Wage income, SRD per year	6126.50	5842.87	283.63
	(481.86)	(470.24)	(719.79)
Total HH income, SRD per year	9908.09	8071.32	1836.76
	(964.72)	(717.42)	(1361.02)
Welfare Household	4.04	4.08	-0.04
	(0.10)	(0.11)	(0.16)
Welfare Village	4.57	5.02	-0.45***
	(0.11)	(0.12)	(0.17)
N	510	308	818
Joint F-Stat			5.40
P-value			0.000

Notes: * $p < .10$,** $p < .05$, *** $p < .01$.

Table 3: Attrition Checks & Migration

	Follow-up survey completed		*Migration*		
	Overall	In-person	Entire HH	Individuals	Individuals (PSM)
	(1)	(2)	(3)	(4)	(5)
Treated	0.013	0.323***	-0.008	-0.079**	-0.013
	(0.027)	(0.033)	(0.014)	(0.033)	(0.062)
Mean Control	0.827	0.424	0.047	0.273	0.200
Observations	818	818	818	681	337

Notes: In column (1) the dependent variable is a dummy equal to 1 if the households completed the follow-up survey. In column (2) the depend the dependent variable is a dummy equal to 1 is the household completed the follow-up survey in-person, that is before the COVID-19 related restrictions came into effect. In column (3) the dependent variable is a dummy equal to 1 if the household moved out of village in which was living at the time of the baseline survey. In columns (4) and (5) the dependent variable is a dummy equal to 1 if at least one household member left the household for work or school purpose. In column (4) the sample is restricted to the subset of households that completed the follow-up survey. In column (5) the sample is restricted to the matched treated and control group, obtained using PSM, and PSM weights are applied. Robust standard errors in parenthesis * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 4: Energy

	EBS	Durables	Consumption NG	Batteries	Energy exp NG
	(1)	(2)	(3)	(4)	(5)
Panel A: DD					
Treated	-0.000	664.296**	-306.251	-62.414*	332.806
	(0.000)	(270.143)	(2266.271)	(32.065)	(1370.966)
Post	0.021***	-255.648	-3507.207***	453.341***	-2143.622***
	(0.007)	(159.018)	(1131.604)	(175.344)	(578.848)
Treated*Post	0.967***	184.370	722.468	-511.013**	-1047.033
	(0.010)	(332.202)	(2580.511)	(201.398)	(1445.665)
Mean Control Baseline	0.000	2329.859	8606.040	264.344	4990.066
Observations	1358	1354	1344	1352	1344
Panel B: PSM					
Treated	0.000	358.405	1133.328	-87.764**	56.803
	(0.000)	(354.331)	(1204.297)	(44.450)	(969.749)
Post	0.004	-301.975	2306.767	392.269	-125.733
	(0.004)	(387.839)	(2310.243)	(279.396)	(1210.419)
Treated*Post	0.983***	334.226	-3236.886	-427.870	-1458.611
	(0.008)	(473.084)	(2551.492)	(299.165)	(1308.695)
Mean Control Baseline	0.000	2416.034	3883.562	278.759	3205.910
Observations	674	674	670	674	670

Notes: EBS= connected to EBS grid; Durables=value of electric durables; Consumption NG = consumption of non-grid energy (kWh); Batteries = expenditure in batteries; Energy exp NG = expenditure on non-electric energy. Robust standard errors in parenthesis. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 5: Time use

	Work M	Work F	Business M	Business F	Homework M	Homework F
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: DD						
Treated	1.331**	0.551	-0.510	0.035	0.089	0.195
	(0.563)	(0.341)	(0.460)	(0.240)	(0.138)	(0.161)
Post	0.368	-0.255	-0.421	0.167	-0.090	-0.153
	(0.521)	(0.296)	(0.504)	(0.277)	(0.135)	(0.129)
Treated*Post	-0.481	0.105	0.646	-0.503	0.214	-0.004
	(0.815)	(0.499)	(0.628)	(0.343)	(0.239)	(0.195)
Mean Control Baseline	1.035	1.000	1.298	0.461	0.472	0.462
Observations	256	418	256	418	254	262
Panel B: PSM						
Treated	1.586**	0.337	0.654**	0.098	-0.058	-0.042
	(0.690)	(0.598)	(0.276)	(0.318)	(0.258)	(0.332)
Post	1.923*	-0.024	0.173	-0.427*	-0.277	-0.514
	(0.998)	(0.838)	(0.230)	(0.245)	(0.295)	(0.330)
Treated*Post	-2.041*	-0.233	-0.070	0.070	0.425	0.368
	(1.192)	(0.919)	(0.423)	(0.326)	(0.363)	(0.364)
Mean Control Baseline	0.885	1.159	0.096	0.427	0.610	0.714
Observations	172	264	172	264	160	155

Notes: Robust standard errors in parenthesis. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 6: Income

	N businesses	Business income	Crop profits	Wage income	Tot income
	(1)	(2)	(3)	(4)	(5)
Panel A: DD					
Treated	-0.106***	-421.496	1109.220**	-861.490	-2331.800*
	(0.026)	(299.441)	(495.966)	(762.054)	(1406.005)
Post	0.040	-144.758	90.215	-3523.727***	-6267.041***
	(0.028)	(297.694)	(161.309)	(615.311)	(1305.133)
Treated*Post	0.115***	376.567	-182.453	1757.110**	4054.409**
	(0.042)	(435.514)	(640.974)	(882.756)	(1657.691)
Mean Control Baseline	0.188	872.969	337.195	6734.432	10796.924
Observations	1358	1358	1358	1358	1358
Panel B: PSM					
Treated	0.042**	273.755	-189.135	357.490	890.751
	(0.021)	(273.458)	(842.590)	(927.720)	(1286.300)
Post	0.203***	537.655	-515.645	-2137.242**	-1649.322
	(0.057)	(330.832)	(807.584)	(932.811)	(1251.503)
Treated*Post	-0.051	-299.129	927.897	576.168	235.761
	(0.066)	(478.486)	(948.040)	(1134.980)	(1575.198)
Mean Control Baseline	0.034	200.844	1010.127	5366.810	6682.422
Observations	674	674	674	674	674

Notes: Robust standard errors in parenthesis. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 7: Subjective Welfare

	Individual (1)	Village (2)
Panel A: DD		
Treated	0.088	0.492**
	(0.209)	(0.227)
Post	0.625***	0.130
	(0.228)	(0.247)
Treated*Post	0.383	0.516
	(0.297)	(0.319)
Mean Control Baseline	4.074	4.657
Observations	888	886
Panel B: PSM		
Treated	-0.280	-0.302
	(0.351)	(0.379)
Post	0.316	-0.678
	(0.490)	(0.463)
Treated*Post	0.760	1.392***
	(0.529)	(0.508)
Mean Control Baseline	4.375	4.985
Observations	550	550

Notes: Robust standard errors in parenthesis. * $p < .10$, ** $p < .05$, *** $p < .01$.

B.1 Heterogeneity

Table 8: Attrition Checks & Migration

	Follow-up survey completed		*Migration*		
	Overall	In-person	Entire HH	Individuals	Individuals (PSM)
	(1)	(2)	(3)	(4)	(5)
Panel A: Gender					
Treated	0.025	0.382***	-0.010	-0.069	-0.038
	(0.036)	(0.045)	(0.017)	(0.042)	(0.082)
Treated*Female head	-0.023	-0.130**	0.001	-0.031	0.052
	(0.054)	(0.066)	(0.029)	(0.066)	(0.124)
Mean Control	0.827	0.424	0.047	0.273	0.200
Observations	818	818	818	681	337
Panel B: Village					
Pokigron	-0.105**	0.203***	0.008	0.024	0.079
	(0.043)	(0.048)	(0.023)	(0.053)	(0.075)
Cassipora	0.030	0.148	0.001	-0.106	-0.030
	(0.078)	(0.111)	(0.048)	(0.091)	(0.109)
Pierre K/Kumbasi	0.173***	0.176	-0.047***	0.027	0.168
	(0.017)	(0.157)	(0.009)	(0.147)	(0.182)
Redi Doti	0.006	0.376***	-0.014	-0.073	-0.033
	(0.070)	(0.077)	(0.034)	(0.083)	(0.098)
Powakka	0.123***	0.477***	-0.022	-0.168***	-0.092
	(0.026)	(0.035)	(0.017)	(0.036)	(0.065)
Mean Control	0.827	0.424	0.047	0.273	0.200
Observations	818	818	818	681	337

Notes: In column (1) the dependent variable is a dummy equal to 1 if the households completed the follow-up survey. In column (2) the depend the dependent variable is a dummy equal to 1 is the household completed the follow-up survey in-person, that is before the COVID-19 related restrictions came into effect. In column (3) the dependent variable is a dummy equal to 1 if the household moved out of village in which was living at the time of the baseline survey. In columns (4) and (5) the dependent variable is a dummy equal to 1 if at least one household member left the household for work or school purpose. In column (4) the sample is restricted to the subset of households that completed the follow-up survey. In column (5) the sample is restricted to the matched treated and control group, obtained using PSM, and PSM weights are applied. Robust standard errors in parenthesis * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 9: Energy

	EBS (1)	Durables (2)	Consumption NG (3)	Batteries (4)	Energy exp NG (5)
Panel A: Gender					
Treated*Post	0.981***	210.711	-432.268	-458.271	-2651.237
	(0.011)	(468.528)	(2579.627)	(319.078)	(2149.775)
Treated*Post*Female head	-0.031	-64.220	2422.421	-96.616	3341.516
	(0.020)	(666.003)	(5236.470)	(395.215)	(2874.847)
Mean Control Baseline	0.000	2329.859	8606.040	264.344	4990.066
Observations	1358	1354	1344	1352	1344
Panel B: Village					
Post*Pokigron	0.979***	1.312	-3494.899	-548.672***	-2396.323
	(0.007)	(504.586)	(5727.372)	(176.799)	(2588.849)
Post*Cassipora	0.812***	1521.729*	-6711.927	-777.340***	2473.778
	(0.089)	(864.395)	(14097.593)	(276.927)	(2176.694)
Post*Pierre K/Kumbasi	0.979***	4044.311***	7285.544	-1032.914***	1223.427
	(0.007)	(1337.652)	(4725.082)	(266.441)	(1811.915)
Post*Redi Doti	0.979***	414.570	5756.359***	393.896	3527.039***
	(0.007)	(879.742)	(2203.949)	(985.398)	(1359.623)
Post*Powakka	0.979***	-269.742	3542.709*	-592.276***	-1703.004
	(0.007)	(487.685)	(1886.309)	(176.025)	(2249.517)
Mean Control Baseline	0.000	2329.859	8606.040	264.344	4990.066
Observations	1358	1354	1344	1352	1344

Notes: EBS= connected to EBS grid; Durables=value of electric durables; Consumption NG = consumption of non-grid energy (kWh); Batteries = expenditure in batteries; Energy exp NG = expenditure on non-electric energy. Robust standard errors in parenthesis. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 10: Income

	N businesses	Business income	Crop profits	Wage income	Tot income
	(1)	(2)	(3)	(4)	(5)
Panel A: Gender					
Treated*Post	0.189***	637.857	390.386	2074.615*	6332.732**
	(0.058)	(785.972)	(952.269)	(1175.189)	(2686.544)
Treated*Post*Female head	-0.161*	-576.861	-1174.801	-757.538	-5088.292
	(0.084)	(833.519)	(1274.923)	(1778.296)	(3177.975)
Mean Control Baseline	0.188	872.969	337.195	6734.432	10796.924
Observations	1358	1358	1358	1358	1358
Panel B: Village					
Post*Pokigron	0.080	322.366	-185.050	3260.417***	5889.391***
	(0.065)	(564.304)	(169.226)	(1012.443)	(1691.352)
Post*Cassipora	0.071	-364.417	2352.444	-4913.852	-237.053
	(0.108)	(734.345)	(2006.732)	(4458.776)	(5192.456)
Post*Pierre K/Kumbasi	-0.140	151.730	882.024	6823.862***	5746.387
	(0.196)	(769.211)	(10424.658)	(2433.765)	(11535.744)
Post*Redi Doti	0.080	320.141	-1088.979	1775.315	2989.127
	(0.101)	(323.283)	(1832.938)	(1965.774)	(3559.799)
Post*Powakka	0.179***	568.927	-475.202	1161.967	3352.438*
	(0.052)	(670.377)	(836.932)	(1117.957)	(1959.956)
Mean Control Baseline	0.188	872.969	337.195	6734.432	10796.924
Observations	1358	1358	1358	1358	1358

Notes: Robust standard errors in parenthesis. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 11: Subjective Welfare

	Individual (1)	Gender (2)
Panel A: Gender		
Treated*Post	0.343	0.513
	(0.433)	(0.451)
Treated*Post*Female head	0.088	0.005
	(0.596)	(0.638)
Mean Control Baseline	4.074	4.657
Observations	888	886
Panel B: Village		
Post*Pokigron	0.174	1.175***
	(0.380)	(0.381)
Post*Cassipora	0.043	0.364
	(0.749)	(0.743)
Post*Pierre K/Kumbasi	1.210*	0.198
	(0.687)	(1.210)
Post*Redi Doti	0.914	0.239
	(0.779)	(0.679)
Post*Powakka	0.413	0.124
	(0.364)	(0.400)
Mean Control Baseline	4.074	4.657
Observations	888	886

Notes: Robust standard errors in parenthesis. * $p < .10$, ** $p < .05$, *** $p < .01$.