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Abstract

This study analyses the effects of rural electrification (RE) on paid and unpaid work in rural households in Ethiopia. I use a panel data set collected by the Central Statistics Agency of Ethiopia in coordination with the Living Standards Measurement Study Integrated Surveys of Agriculture from the World Bank for a wide arrange of household and community conditions for 2011-2016 across four regional states of Ethiopia; Amhara, Oromia, Tigray, Southern Nations, Nationalities and Peoples Region (SNNP). I use a Difference in Differences method with matching, using a Kernel Propensity Score Matching technique. The results show that RE in the short term significantly increases temporary employment. In other aspects, the results suggest that RE, on its own, does not lead to significant changes in domestic and permanent labour, as well as agricultural and nonagricultural self-employment. The literature suggests the short time span in which such impact is measured as a potential reason. The study contributes to the growing body of literature on impact assessment of non-randomly allocated public services, specifically RE, by proposing an approach to address selection bias.

Keywords: Rural electrification, difference in difference, propensity score matching, Ethiopia, time allocation.

Table of Contents

1. Introduction 1

2. Illuminating the Path to Rural Electrification: Approaches to Establishing Mechanisms of Change 5

3. Ethiopia's Electrification context......7

4. Data and methodology......10

4.1 Data......10

4.2 Electricity indicator and treatment variable......12

4.3 Variables of interest for this study......13

4.4 Descriptive statistics......14

4.5 Empirical strategy 17

4.5.1 Difference in Differences (DiD)...... 18

4.5.2 Propensity score matching with difference in differences...... 20

5. Results......22

5.1 Effects on domestic labour......22

5.2 Effects on self-employed labour......23

5.3 Effects on casual salaried labour 26

6. Lessons......29

7. References: 31

8. Appendix......36

1. Introduction

Even though electricity has been a subject of study for more than 200 years, it was at the beginning of the last century that its use was expanded around developed countries and it is now considered a human right (Forrester, 2016; Tully, 2006). The Universal Declaration of Human Rights states that “Everyone has the right to a standard of living adequate for the health and well-being of himself and of his family, including food, clothing, housing, and medical care and necessary social services…[]”(UN General Assembly, 1948). The United Nations characterizes adequate housing as a place where, among several other conditions, “safe drinking water, adequate sanitation, energy for cooking, heating, lighting” (United Nations Habitat, 2009) exist. Therefore, access to electricity is a derived universal human right.

Sustainable Development Goal (SDG) number 7 (affordable and clean energy) recognizes as its first target, universal, affordable, and reliable electricity access (United Nations, 2022). Infrastructure that facilitates access to electricity has positive and significant effects on several SDGs. Dinkelman (2011) found that connection to energy increases female labour as it frees them from household tasks that would otherwise take plenty of time in South Africa (SDG no. 1 of no poverty, no. 5 of gender equality, no. 10 of reduced inequalities). Furthermore, they found a significant increase in self-employment attributed to the decrease in production costs and time (SDG no. 8 of decent work and economic growth). Candelise et al. (2021) show that electricity also has a positive impact on food security (SDG no. 2) through two mechanisms. First, it increases productivity and improves conservation methods. Secondly, it diversifies economic activities which later on positively affect income.

In 2017, according to Moore et al. (2020), 800 million people were not connected to any source of electricity. In 2000 25% of the population in Sub-Saharan Africa had access to electricity. Ethiopia was one of the lowest countries with only 12% of the population having access to such service. 20 years later, the proportion increased by a factor of 4.5 to 51%(World Bank, 2022a). Ethiopia’s population in 2020 was 115 million, meaning that two years ago, almost 60 million Ethiopians were still not connected to the main grid or any other kind of isolated generator. Furthermore, there is a clear disparity between access to electricity in rural and urban communities. Whereas 85% of the population residing in urban areas benefit from the service (World Bank, 2022c), only 39% do so in rural communities(World Bank, 2022b).

Rural electrification expansion in the region of Sub-Saharan Africa faces logistic and budgetary challenges (Khodayar, 2017; Lee et al., 2020b) that test the economic power that rural households have to afford it (Bernard & Torero, 2011; Lee et al., 2020a; Mugisha et al., 2021) . According to Bernard and Torero (2015), the prices of electrification to the main line in Ethiopia are covered by the households and this floated between 300-450 Ethiopian birr (ETB). Furthermore, the service reliability, once it exists, is more than often faulty (Onishi, 2015).

Rural electricity spread takes place in already inhabited areas of Ethiopia, contrary to other countries where it is done prior to any developments. Literature documents that areas with higher economic growth potential and rich households are more likely to be connected to the grid (Bernard & Torero, 2011; Lee et al., 2020a; Moore et al., 2020). As explained in the chapters that follow, the case of Ethiopia is no different. Therefore, electrification spread cannot be considered or assessed as random. Studies around electrification yield heterogeneous results coming from these selection bias problems (Bernard & Torero, 2011; Lee et al., 2020a; Moore et al., 2020) Bernard (2012) attributes the lack of empirical reliable evidence about the effects of electrification to contradictions, resulting from the absence of robustness, when assessing groups with and without electricity.

In the particular case of Ethiopia, there are few papers where the social and economic effects of rural electrification and formally discussed (Bernard & Torero, 2011, 2015; Fried & Lagakos, 2021; Moneke, 2020). Fried and Lagakos (2021) looks into migration as an effect of RE and found significant and positive effects on non-agricultural activities, irrigation rates and. Moneke (2020) analyses welfare as a response to RE, he finds little effect when looking at electrification on its own but larger effects when studying it alongside road infrastructure. Bernard and Torero (2015) talk about the social interaction effects of RE, they find that households' decisions to connect to the grid is significantly influenced by their neighbor's electrification status. My study contributes to this literature by adding more information on the way intrahousehold dynamics are changed with electrification in rural aeras of the country.

This study assesses the impact of rural electrification (RE) on the time households allocate to paid and unpaid labour in some regions in Ethiopia. By comprehending how the distribution of time changes in relation to RE, it is possible to gain a deeper understanding of households' behavior and their response to the adoption of a public service. I leverage panel data from 2011 to 2016 to use a difference in differences (DiD) approach and a combination of DiD with propensity score matching (PSM). Namely, the paper studies the fluctuations in salaried and domestic labour as well as self-employed agricultural and nonagricultural work. I find that electricity, by itself, does not lead to significant improvements in any outcomes measured but only in temporary employment where I find a significant impact after two years. Studies like this might be important to take into consideration when assessing the impact of RE as it offers an approach on how to deal with the non-random allocation of a public service such as electricity. Also, it serves to show how electricity should be accompanied by other development initiatives if meaningful effects are expected to be found.

My work resembles, to some extent, the one by Bernard and Torero (2015). We both look into time allocation in response to electrification in rural areas of Ethiopia. Also, both studies propose a way for how selection bias can be circumvented. They fix the heterogeneous allocation of electricity by giving random discount vouchers that gave discounts on-grid connections and found no effects on time allocation in the short term in Ethiopia. Their study employs a more robust design than mine. On the other hand, there are some important differences to note. Firstly, they calculate the average treatment effect (ATE) using a randomized control trial (RCT). My study estimates an average treatment effect on the treated (ATT) using a difference in differences with matching approach. Secondly, unlike their work which considers small towns, my analysis solely focuses on rural communities. Thirdly, Bernard and Torero (2015) carry out their analysis in 8 villages in Southern Ethiopia (sample size of around 628 households). I look at 285 enumeration areas across 5 regions spread around the country[1] (sample size of 2,647 households).

The paper is composed of six chapters. In Chapter 2, I examine recent literature on rural electrification to establish the mechanisms of change. Chapter 3 provides an overview of the Ethiopian context, including the identification of selection bias constraints. Chapter 4 presents the data and an according methodological approach. In Chapter 4, I discuss the variables of interest for my study and how I derived an electricity indicator (the treatment variable). Followed by this, I discuss the two samples analyzed in my study. Thereafter, I propose a set of models to address selection bias in the case of Ethiopia, while acknowledging limitations resulting from a lack of pretreatment data and reduced sample size. I begin by describing the weaker models and conclude with an approach that consolidates their strengths. Chapter 5 showcases and discusses the results

[1] See section 4.1 on Data description.

for the five variables of interest in both the short and mid-run, presenting coefficients from various models while accounting for potential biases. Finally, Chapter 6 presents lessons derived from the study.

2. Illuminating the Path to Rural Electrification: Approaches to Establishing Mechanisms of Change

To this day, a variety of papers formally discuss the impact of RE in low-income countries. These papers offer valuable insights into the mechanisms of change that can be derived from the impact of electrification on household time allocation. I start this chapter by explaining how RE directly impacts domestic tasks and self-employment through access to technology and resulting increase in time availability. Thereafter, I explain how in the context of my analysis, RE indirectly affects salaried labour through the increment of labour supply as a result of time release and directly through the gain of new knowledge allowing households to secure more stable jobs.

Firstly, without electricity, households spend a considerable share of their time on household chores such as fetching water, collecting wood, cleaning, and cooking. Household chores as well as childcare are tasks where women take the leading role in Ethiopia (Kebede et al., 2013). Dinkelman (2011) shows through a study carried out in South Africa, that rural RE increases female labour within 5 years as it frees, or reduces the time, from household tasks. Therefore, increasing household's overall income. Indeed, Khandker et al. (2013) and Perdana (2019) argue that electrification increases electric home appliance purchases which will result in a decrease in time spent on household activities. Consistent with this, Moore et al. (2020) and Lee et al. (2020b) found a significant increment in the use of electric hardware for household tasks as a result of electrification in the mid-term for low and middle-income countries. Access to electrification can help boost production through access to more efficient technologies and diversification of economic activities, shifting away from mainly agriculture (Bernard, 2012; Candelise et al., 2021). Accordingly, Fried and Lagakos (2021) found that electrification results in higher non-agricultural business activities as well as the use of technology in agriculture in rural Ethiopia. Barron and Torero (2014) find increases on self employment and non agricultural work in the mid-term.

Secondly, I focus on solely on the effects of RE on its beneficiaries and do not investigate the community or general equilibrium effects that could also impact salaried labour[2]. Although the dataset I use contains community-level information, it is not sufficient to determine whether a community is fully electrified. Therefore, I decided not to use this information. I argue that RE has

[2] I elaborate on this limitation in Section 5.3.

indirect impacts on salaried labor, as households gain more time for paid work by reducing the time spent on other tasks. As a result, the analysis only looks at changes in salaried labor supply from a household perspective. It is only logical then, to expect to see significant changes in salaried labour if there are significant changes in domestic labour and self-employment. From this that I first look into these and afterwards into salaried labour. Furthermore, the introduction of RE may enhance the skill set of individuals, thereby providing them with access to higher-paying and more secure employment opportunities. Additionally, electrification may incentivize households to increase their work hours in order to offset the additional expenses associated with this service.

3. Ethiopia's Electrification context

Ethiopia is a landlocked country larger than Austria, Germany, and France combined and also the second most populated country in Sub-Saharan Africa. According to the International Trade Administration (2022), Ethiopia has the potential to increase its energy-generating capacity by 13 times its current production. More than 90% of Ethiopia’s energy comes from hydropower coming from 9 important dams (Moneke, 2020; World Bank, 2022d).

After restructuring measurements, the Ethiopian Electric Power Corporation (EEPCo), a state-owned organization, was founded in 1996 (Ethiopian Electric Power, nd). In 2013, it further restructured and split into two; the Ethiopian Electric Power (EEP) and the Ethiopian Electric Utility (EEU). On the one hand, the EEU is mainly in charge of tasks related to the distribution of energy, grid expansion and sales (Bernard & Torero, 2015; Ethiopian Electric Utility, 2022). On the other, the EEP takes the lead on energy production as well as research and development of new technologies (Ethiopian Electric Power, nd).

Over the past years, Ethiopia has implemented several electrification programs with the hope to achieve universal electrification by 2025 (Ministry of Water Irrigation & Energy, 2019; World Bank, 2018b). The most important are 1) The national electrification program (NEP), 2) The renewable electrification program, and 3) The Ethiopian electrification program.

The first of the above-mentioned is a program ran and funded by the Ethiopian Government, through the EEU, but it also receives help from organizations like the World Bank (WB). Initiated in 2017, the NEP consists of two rolled-out phased interventions. Whereas one entails expanding the grid to 65% of the population, the other one aims to electrify households with off-grid equipment. As per the report of the Ministry of Water Irrigation & Energy (2019)these project gives priority to areas with economic-growth potential.

One of today's most promising and ambitious electrification programs is the renewable electrification program. Ethiopia is a country rich in natural resources, and the government has been leveraging these to increase the country's overall access to electrification without jeopardizing future generations. This program involves a series of implementations going from cleaner sources of electrification to promoting energy efficiency and reducing consumption. As previously mentioned, Ethiopia’s clean energy-generating potential is considerable. The country has the potential to produce 45,000 MW from hydropower, 10,000 MW from the wind, 5,000 from geothermal energy and 5 Kw h/m^2 / day from solar panels as reported by Mengistu et al. (2015). To put it in perspective, 60,000 MW would be enough to power a country like Germany for the whole year[3]. On the counterpart, the reserves of fossil fuels are also highly considerable (Mondal et al., 2017). To this day, Ethiopia is building one of the largest hydropower-generating stations in the world. The Great Ethiopian Renaissance Dam has been in construction since 2011, when finished it will be the largest hydropower station in Africa. Such high renewable energy potential could meet not only Ethiopia's but also neighboring countries' energy demands (Tiruye et al., 2021). Even though access to more efficient technologies is getting progressively cheaper, programs like this face the challenge of an increment in the total electrification costs (Mondal et al., 2018).

Lastly, the Ethiopian electrification plan is a collaborative effort between the WB and the Ethiopian government which focuses mainly on expanding electrification to rural areas of Ethiopia. This program is a variant of the NEP. The program funds off-grid renewable technologies such as solar panels and mini-grids. Furthermore, the program provides support in strengthening the sector's capacity which entails training in a wide variety of important-related tasks such as financial management, technical and commercial skills, accountability and governance, among others (World Bank, 2018a).

Overall, in recent decades, Ethiopia has seen an important rise in access to electrification through its plan efforts. A vast majority of the investments pushing electricity generating infrastructure were done in recent years to the point that the country has seen it increase by more than 10 times (Bernard & Torero, 2015; Kruger et al., 2019; Moneke, 2020). Despite going from 12% of the population in 2000 to 51% in 2019, Ethiopia continues to have Africa's second-highest electricity connectivity deficit (World Bank, 2018; World Bank, 2022b). Nonetheless, the current availability of information poses a fitting setting for this study as there is baseline data prior to these

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[3] $60,000 \text{ MW} = 60 \text{ GW}$

$1 \text{ GW} = 1 \text{GW} \cdot \text{hr}$

$60 \text{ GW} \cdot 24\text{hrs} = 1{,}440 \text{ GW} \cdot \text{hr} = 1.4 \text{ TW} \cdot \text{hr} \rightarrow$ Energy in one day

$1.4\, TW \cdot hr\ (365) = 511\, TW \cdot hr\ in\ one\ year$

According to Statista. (2023). *Electricity net consumption in Germany from 2000 to 2021*. Statista Retrieved March 20th from https://www.statista.com/statistics/383650/consumption-of-electricity-in-germany/ Statista (2023), the net consumption of electricity in 2021 in Germany was 511.59 TW ·hr developments. In the case at hand, this means that there is information on households before and after electricity was made available and adopted.

4. Data and methodology

This chapter begins by giving an overview of the dataset utilized for this study. Thereafter I explain the reasoning behind the electricity indicator and allocation of treatment and control groups. Following, I discuss the variables of interest for my study and the way they were derived from the dataset as well as some descriptive statistics showcasing differences at baseline between electrified and unelectrified households. Lastly, the empirical strategy as well as the motivation behind it is explained.

4.1 Data

The analysis is carried out using data from the Central Statistical Agency of Ethiopia (CSAE). Particularly, the research uses the Living Standards Measurement Survey (LSMS) from 3 different rounds that took place at the end of one year and the beginning of the following one [2011-2012, 2013-2014, 2015-2016].

The first wave of the survey focused only on rural areas and small towns of the country. The following two opened the scope for large cities to be included as well. Since 82% of the interviewed households in small towns were already electrified by the time they were first visited versus almost 9% in rural areas, small towns are not considered in the study. The same argument applies to large cities in the following rounds of data collection. After dropping all observations from small towns and large cities, the sample size remains considerable. 2,647 households from 290 enumeration areas (EAs) in rural zones were constantly followed throughout the 3 waves.

EAs were chosen based on a stratified random selection method. Firstly, the country is divided into strata, in this case, the regions of Ethiopia. Although the surveys were carried out throughout most provinces of the country, regions with the largest populations were given priority and therefore their representativeness was ensured through the allocation of sufficient EAs. In other words, based on a "probability proportional to size of the total EAs in each region" (Central Statistics Agency of Ethiopia (CSA), 2020). Namely, the regions of Amhara; Oromiya; Southern Nations, Nationalities and People's Region (SNNP); and Tigray. The rest of the regions do not count with enough sample size to be representative and therefore, as advised by the CSAE, were concentrated in an "other regions" category (See figure 4.1).

Secondly, households were randomly selected whitin the EAs, 12 for each EA. The first 10 were randomly selected amongst the Agricultural Sample Survey households (AgSS). These are households linked to agricultural activities. The remaining 2 were randomly selected amongst households unrelated to agricultural activities. In some cases, the documentation of the survey argues that only one or no households complied with this condition. Hence, more agricultural households had to be chosen to ensure equal numbers of households between EAs. Nonetheless, observations, where there was considerable missing data, had to be ruled out. Leaving this study with some EAs containing less than 10 households (around 7% of the total EAs). This replicated cross-sectional data of household (also individual) and community-level data sheds light on a different set of characteristics related and not to agricultural activities.

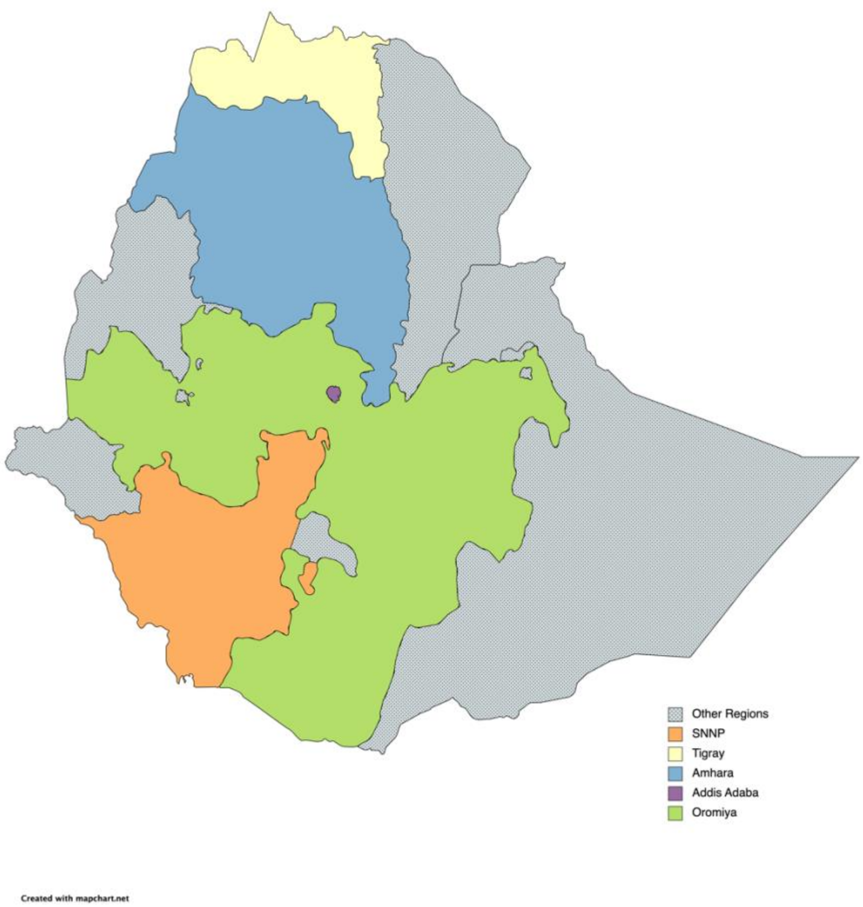

Figure 4.1 Map of Ethiopia

The study employed five different questionnaires to collect data from the households and communities in the sample. The household questionnaire was given to all households in the sample and covered various sociodemographic characteristics, including education, health, food security, expenditure, and time use. A community questionnaire was given to selected members of the community to gather information on the socioeconomic conditions in the selected EAs. Additionally, three agricultural questionnaires were administered to randomly selected households from the AgSS. These included the post-planting, post-harvesting, and livestock questionnaires, which aimed to collect information on agricultural practices and production in the area.

4.2 Electricity indicator and treatment variable

The data provided by the Central Statistical Agency of Ethiopia does not record a binary variable where households show to have or not electricity. Nonetheless, there is a variable from which such an indicator was derived. In the household questionary, interviewees are required to report the "main source of light". There were different possible answers to this question; from electricity meter to firewood. Households were deemed as "electrified" if they reported one of the following:

- Electricity meter – Private.
- Electricity meter – Shared.
- Electricity from generator.

Everything else was considered as "not electrified".

Followed by this, two treatment variables are generated to determine the effect on the short and mid-term. Therefore, two different sample sizes, one for the short and another one for the mid-term analysis. Mainly, the observation considers three subgroups within the panel sample to make such variables. The subgroup that did not get electricity throughout the three waves is selected to be the pure control group for both the short and mid-term groups. The subgroups that did not have electricity in the first round but reported it in the following are assigned to the treatment group in the short-term. These subgroups are; the one that had no electricity in the first round but had in the following two and also the subgroup that had no electricity in neither the first nor the third round but had in the second round. Since the short-term effects are evaluated from 2011 to 2013, thus not considering 2016, these two subgroups can be assigned to the treatment group[4]. The subgroup that had no electricity in the first period but did so in the following two was assigned to the treatment group in the mid-term assessment. See Table 4.2.1

[4] See Table 8.1 in the appendix for further explanation.

Table 4.2.1 Electricity indicator

		Short-term treatment group	
	Control group		Mid-term treatment group
Year	000	010	011
2011	2156	31	122
2013	2156	31	122
2016	2156	31	122
Total	6468	93	366

4.3 Variables of interest for this study

The thesis focuses on five indicators to assess the impact of RE on time allocation in Ethiopia. Namely, how it affects yearly working salaried hours jobs, domestic labour, and self-employed labour. For the purposes of this study, I define the previous concepts as follows. Salaried labour is divided into casual and permanent labour. Permanent job refers to first (main) and secondary job where the employer is not a member of the household and also without considering temporary labour. Casual labour considers only what is referred to in the survey as "other temporary/casual labour".Domestic labour hours comprise hours for water and firewood collection as well as household agricultural activities. Self-employed labour is divided into agricultural and nonagricultural. Both activities comprehend the amount of hours spent by the household to produce any good or service to sell.

Taking inspiration from authors evaluating the impact of electrification (Chhay & Yamazaki, 2021; Dinkelman, 2011; Khandker et al., 2013; Lee et al., 2020b) the study considered to following variables as covariates; household head (female or male), adult-sex ratio, dependency ratio, land (measured in hectares), household head scholarly level, livestock (measured in TLU), household size and distance to the nearest market.

The dependency ratio was estimated as the quotient of inhabitants of the household aging between 18-65 years old and the total household size. The same logic was applied when estimating the adult sex ratio, meaning that household members 18 or older were considered as adults. Given that scholarly was not measured in years but rather as the highest grade obtained[5], I decided to create a basic education dummy. The former indicated 1 when the household head had completed the 4th

[5] From these it was also not clear how to translate them into years.

grade and 0 otherwise. Even though there was data available about the distance of households to different locations, I decided to only keep the distance to the nearest market. I found that, in most cases, the distance to the nearest market was comparable to the distance to the nearest small town, urban area, or phone. The questionnaire includes questions about the number and types of animals owned by households. To simplify the analysis, all animals were combined into one variable using the concept of tropical livestock units (TLUs)[6]. This method allows for the standardization of different animal types based on their equivalent value in terms of TLU, enabling a more straightforward and efficient analysis of the data (Ahmed & Mesfin, 2017; Cecchi et al., 2010; Eurostat, 2023).

4.4 Descriptive statistics

Tables 4.4.1, 4.4.2, and 4.4.3 present descriptive statistics of the variables discussed above. For all these, I considered the full baseline panel sample. In other words, I do not only consider treatment and control observations[7]. The software I use to carry out the analysis in this study and showcase the tables (Stata) does not take into consideration observations where there are missing values on variables. From this that sample sizes differ among variables.

Tables 4.4.1 shows means, sample sizes, and standard deviations of the covariates and outcome variables used in this analysis. Table 4.4.1 presents descriptive statistics of variables at baseline (2011) for the full sample size. On average, over three-quarters of the households are male-headed; the adult sex and dependency ratio are almost equal at 0.5. Whereas the former means that for every female on the household, there are two males, the latter means that for every member of the household unable to provide, there are two members who can. In addition, the educational levels among the head are low: 20% of them reported having completed coursework equivalent to the 4th grade. Table 4.4.1 also shows the values of other important covariates that speak to the wealth of the household but also its proximity to the nearest place likely to be electrified. In most cases, as mentioned before, it was found that the distance to markets was the same as the one to small urban areas. From this, the study assumes markets are in such places and therefore only considers this variable in the analysis. On average, households are quite far from the nearest market (almost 13

[6] According to Eurostat (2023) 1 TLU “is the grazing equivalent of one adult dairy cow producing 3 000 kg of milk annually, without additional concentrated foodstuffs”.

[7] The reasoning behind this is that my intention at this point is to showcase difference between electrified and nonelectrified households. Also in order to make the section less extensive as would have been showing differences for control and treatment units for both samples.

Km). The data shows that, on average, households noted to own a little over 1 hectare of land and 4.2 TLU.

Table 4.4.1 Descriptive Statistics at baseline (2011)

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	N	mean	sd	N	mean	sd
Annual casual labour hours				2,647	109.6	481.2
Annual permanet labour hours				2,647	143.8	597.3
Annual domestic labour hours				2,647	1,206	2,067
Annual self-employment in farming activities				2,647	2,663	2,865
Annual self-employment in non-farming activities				2,647	846.5	1,561
Household size	2,647	4.947	2.317			
Dependency ratio	2,647	0.545	0.235			
Female-headed households	2,631	0.235	0.424			
4th grade completed (hh head)	2,647	0.203	0.402			
Land (Ha)	2,277	1.151	1.836			
Distance to the nearest market (Km)	1,446	12.90	11.29			
Accumulated livestock units	2,051	4.206	6.745			
Adult Sex ratio (female/male)	2,621	0.558	0.244			

Variables do not consider weights at this point

Differences within households with and without electricity at baseline are presented in Table 4.4.2. There are significant differences when comparing these two sets of households; households with electricity are significantly closer to the market (almost 2.5 Km) and have a higher household head rate of education (17% higher). These findings are similar to the ones from Dinkelman (2011). On the other hand, households without electricity showed to have a significantly higher amount of TLU (almost 1.5 more). The former is consistent with the annual self-employed in farming activities as these are also shown to be significantly higher in households without electricity in Table 4.4.3. Different from what Dinkelman (2011) reports related to household social demographics, Table 4.4.2 shows no significant differences between electrified and unelectrified households.

Table 4.4.2 Difference in covariates between households with and without electricity at baseline (2011)

VARIABLES	(1) Electrified	(2) Mean	(3) Not electrified	(4) Mean	(5) Mean difference
Household size	262	4.847	2385	4.958	-0.111
Dependency rate	262	0.530	2385	0.546	-0.016
4^{th} grade completed (hh head)	262	0.363	2385	0.186	0.177***
Land (Ha)	201	0.988	2076	1.167	-0.179
Distance to the nearest market (Km)	140	10.771	1306	13.129	-2.358**
Accumulated Livestock Units	180	2.868	1871	4.335	-1.467***
Adult sex ratio (female/male)	261	0.566	2360	0.558	0.009
Female-headed households	262	0.23	2369	0.235	-0.002

Table 4.4.2 shows differences in means for households with and without electricity. Variables measured in 2011.
P-values significant at *** p<0.01, ** p<0.05, * p<0.1.

Table 4.4.3 Difference in outcome variables between households with and without electricity at baseline (2011)

VARIABLES	(1) Electrified	(2) Mean	(3) Not electrified	(4) Mean	(5) Mean difference
Annual casual labour hours	262	99.634	2385	110.715	-11.082
Annual permanent labour hours	262	441.420	2385	111.051	330.369***
Annual domestic labour hours	262	953.876	2385	1233.440	-279.564**
Annual self-employment in farming activities hours	262	1983.145	2385	2737.489	-754.344***
Annual self-employment in non-farming activities hours	262	1437.344	2385	781.570	655.774***

Table 4.4.3 shows differences in means of variables of interest for households with and without electrification. Measured in 2011.P-values significant at*** p<0.01, ** p<0.05, * p<0.1

As mentioned before, there are 5 main outcome variables in this analysis that speak to how households in rural areas of Ethiopia allocate their time related to paid and unpaid work. Table 4.3.3 shows the differences in means of these 5 between households with and without electricity at baseline (2011). Note that there are significant differences in four out of the five. On average, households with electricity showed to have a higher amount of permanent hours and self-employed in non-farming activities. Furthermore, they presented a lower amount of hours allocated to domestic labor and self-employed farming activities. Differences presented in the last two rows could be attributed to what Candelise et al. (2021) mention about electricity expanding labour possibilities and therefore moving from agriculture which is the principal source of income in the country (Bryan et al., 2009). Moreover, the significant difference in domestic labour could be in line with what Perdana (2019) comments. He said that electrified households are, in theory, more likely to purchase electrical devices which would reduce the time of domestic tasks such as cleaning and cooking.

4.5 Empirical strategy

As previously discussed, assessing the impact of rural RE is a complex task as its spread is not random, much less in the case of Ethiopia, for this that most researchers rely on quasi-experimental methods to solve this selection bias[8]. In the Ethiopian context, two issues arise when measuring the effect of RE. Mainly the endogenous adoption and allocation of infrastructure [9].

To measure the impact of RE on household labour hours I propose different models to mitigate selection bias and add robustness to the results. The impact is measured in the short and mid-term, 2011-2013 and 2011-2016 respectively. Namely, the study entails analyzing the data using; 1) a difference in differences (DiD) model without weights and covariates, 2) a DiD only with covariates, and 3) a combination between PSM and DD. For the interests of this study, DiD-PSM results are the ones from which I draw conclusions and base my final analysis in the results section. The intention of showing the results from the other designs previously discussed is to add robustness to my results.

In this case, the intention is to estimate the effect of RE in households, therefore the study is focused on making an analysis that is internally valid. In other words, calculate the impact on a particular stratum of the population of Ethiopia, only those who are treated. Hence, the paper estimates the average treatment effect on the treated (ATT). This has reasons of interest but also practical ones. On the one hand, studies like this are more relevant in marginalized areas than on urbs and have a larger impact on proper public policy planning and implementation. On the other,

[8] Refer to the appendix for further explanation on how other authors address this issue.
[9] Refer to the appendix for a more elaborate explanation.

ATT can assist in reducing the selection bias where treatment allocation is not randomized (Matthias & Eberl, 2020).

4.5.1 Difference in Differences (DiD)

"Maybe 2022 will be the year where the new difference-in-differences (DiD) literature has matured enough that we now don't need to learn about a new paper that questions the old ways of doing things every few days."

-Mckenzie (2022)

(Lead Economist, Development Research Group, World Bank)

For the past years, DiD designs have gained a lot of popularity among researchers that study the causal effects of policy programs around the world (Callaway & Sant'Anna, 2021; Goodman-Bacon, 2021; Sant'Anna & Zhao, 2020). DiD is a tool utilized to calculate the causal effect of an intervention. To put it in simple words, DiD looks at the difference over time in outcomes between two groups. At baseline, one group does not receive any treatment (control group) and the other one does (treatment group). Afterwards, data is collected again and the difference in average outcomes between treatment (Y_t) and control (Y_c) groups at times n and n+1 is considered the average treatment effect on the treated (ATT). DiD, at its canonical form, looks at $(Y_{t,n+1} - Y_{t,n+1}) - (Y_{c,n} - Y_{c,n})$. DiD methods are often used in settings where randomized control trials (RCT) cannot be done for either budgetary or ethical reasons (Wing et al., 2018). For the case at hand, access to electricity cannot be considered random for the reasons mentioned at the beginning of this section.

The reasoning behind this method is that either one of the two groups had they received the treatment, would have seen its outcome trend variate in the same way. In other words, in the absence of treatment, the outcome for both groups would have followed the same trend (Gertler et al., 2016; Roth et al., 2022). The former refers to the parallel trends assumption (PTA). Taking this into consideration, by estimating the differences before and after for both groups the DiD approach aims to delimit the causal effect of the program being assessed.

The PTA is a crucial assumption for DiD estimands to show a causal effect. Although "fundamentally untestable"(Sant'Anna & Zhao, 2020), researchers have found ways to argue the validity of this assumption or ways to work around it under certain circumstances (Bilinski & Hatfield, 2018; Harmon, 2022; Rambachan & Roth, 2019, 2023; Sant'Anna & Zhao, 2020). Other assumptions to take into consideration are the stable unit treatment value assumption (SUTVA) and excludability assumption. The first one states that the potential outcome of an intervention on a unit does not affect the outcome of another unit (Abdia et al., 2017; Angrist et al., 1996). The second one indicates that treatment allocation bears no effect on outcomes but only through its impact on treatment status (Kimmel et al., 2021).

The DiD estimand can be calculated within an Ordinary Least Squares (OLS) regression where covariates and fixed effects can also be taken into consideration. The study will initially work with the following DiD specification and gradually will be complementing it to increase precision:

$$Y_{i,t} = \alpha + \beta \cdot treat_i + \gamma \cdot post_t + \delta \cdot (treat_i \cdot post_t) + \varepsilon_{i,t} \qquad \text{Equation (1)}$$

Where Y denotes the outcome variable measured at time t (0 or 1) for group i (treatment or control). Treat is a dummy variable that distinguishes between treated and control units, 1 and 0 respectively. Post is a dummy variable that takes the value of 1 after treatment has been implemented and 0 otherwise. Note that depending on the term being evaluated this could be 2011(0) and 2013 (1), or 2011(0) and 2016 (1). δ measures the interaction term between the last two, this is also the DiD coefficient or estimand. In other words, it captures the difference in the impact of treated and control units before and after treatment was given. ε accounts for the error term.This is the simplest impact evaluation method in my study. By itself, it is already controlling for time-invariant unobservables. However, by considering baseline and endline characteristics (covariables) one can also control for observable selection bias. Therefore, covariates are added to equation (1).

$$Y_{i,t} = \alpha + \beta \cdot treat_i + \gamma \cdot post_t + \delta \cdot (treat_i \cdot post_t) + \theta \cdot X_{i,t} + \varepsilon_{i,t} \qquad \text{Equation (2)}$$

Where X represents a vector of covariates.

4.5.2 Propensity score matching with difference in differences

The PTA from DiD is a difficult assumption to meet in the case of the study and test for (Kahn-Lang & Lang, 2020; Khandker et al., 2009; Sant'Anna & Zhao, 2020). Since the study does not count on pre-baseline information to look at outcome trends, a graphical analysis is not possible. Furthermore, when comparing treatment and control units purely at baseline, there appear to be significant differences in the covariates considered[10]. The DiD with covariates is still not controlling for observables that are time-variant. This could be a major source of bias affecting the results.

In order to address these limitations, the work proposes a method of DiD using a sample matched at baseline. Using DiD on the matched sample can help overcome selection bias problems as well as issues with the parallel trends (Daw & Hatfield, 2018; Khandker et al., 2009; Wing et al., 2018). I use propensity score matching (PSM)[11] as a matching method to narrow down the sample utilized in my analysis.

The basic idea is to run a DiD analysis on the subsample matched from PSM at baseline. Therefore, reducing the difference between treatment and control units. In that sense, leveraging the strengths of both methods to reach more consistent results.

As mentioned at the beginning of this section, this are the preferred results as they are the least affected by selection bias. The intention for presenting the other results is for completeness only.

Nonetheless, this approach is still far from perfect as it does not adequately consider differences between regions. Given the political and socioeconomic variations within Ethiopia[12], varied effects between regions are to be expected. Consequently, a matching technique using regional dummies can still match families from various locations, which could lead to unreal results. A study that focuses on subpopulations would have been preferred for this reason. In other words, one in which households are paired according to the area. Thus, the common support check and the propensity score computation should be carried out separately for each group. In situations like the one from this study, this course of action is suggested (Caliendo & Kopeinig, 2008).

[10] For both the short and mid-term samples, there are statistically significant differences in dependency ratio, household size and basic education levels.

[11] See appendix for more information on PSM and the way it is used in the context of this study.

[12] See appendix about the Ethiopian context for further information.

However, due to the very limited control sample size among the regions, such an analysis could not be carried out. Therefore, results must be interpreted carefully as they do not represent a region in particular nor the average effect for the population in general. Results represent an average within the treated population, also known as ATT.

5. Results

This chapter is divided into 3 sections, according to the labour classifications I discuss in section 4.3. I first look into the effects RE on domestic and self-employed labour, which I divided into agricultural and non-agricultural self-employment. Following, I study the impact on salaried labour, which I disaggregated into casual and permanent employment.

Five tables are presented, one for every outcome variable, showcasing the effects on the short and the mid-term (this is 2 and 5 years). Next to each set of results, I present the minimum detectable effect (MDE) for each outcome variable derived from my power calculations[13]. Since I look into the short and mid-term impact, two sample sizes are employed[14] and therefore two MDEs are calculated for each outcome variable. Standard errors (SE) are clustered at an EA level since I cannot rule out autocorrelation among households in the same EA

5.1 Effects on domestic labour

Firstly, I look into the changes in time allocated to household tasks after households were electrified. The assumption is that after electrification, households increase their uptake of electric appliances (Khandker et al., 2013; Moore et al., 2020) and would then decrease their time spent on domestic chores such as fetching water, cleaning or cooking (Khandker et al., 2013; Perdana, 2019). Similar to Bernard and Torero (2015), I find no significant effects in either the short or the mid-term (see Table 5.1.1). There are some potential explanations behind these results. The first one is that study is underpowered to detect significant effects as the ones shown in Table 5.1.1, assuming such effects exist. The second is that the assumption stated above might be too strong of an assumption since the poorest part of the population of Ethiopia, according to World Bank (2018a), resides in rural areas. Continuing with this line of reasoning, it is possible that households, when provided with electricity, may not necessarily prioritize purchasing electric home appliances to aid with domestic chores. Instead, they may opt to buy leisure electronic devices such as radios or televisions. For instance, in Vietnam, Khandker et al. (2013) discovered that ownership of rice cookers, which rank as the third most utilized electrical appliance, increased from 21% to 51%

[13] Refer to the appendix for further explanation on power calculations.
[14] Discussed in section 4.2.

over a three-year period. In contrast, ownership of televisions rose from 48% to 74% during the same time frame. Moore et al. (2020) argue that the impact of time-reducing chores is "uncertain". When looking at the reported ownership of tasks-related (Stoves, refrigerator, water pump, etc) and leisure-oriented (televisions, radios, satellite dish) appliances I find unexpected figures. Whereas the number of appliances destinated to leisure activities decreased, the number of appliances for domestic work decreased in 2013 and then increased in 2016[15]. Perhaps, significant effects could be found after a longer period of time and looking at a bigger sample size to increase statistical power.

Table 5.1.1 Impact of rural electrification on domestic labour yearly hours

MODELS	DiD	DiD-Cov	DiD-PSM	MDE (Effect size %)
Short-term				356 (29.5%)
ATT	79.01	-153.5	41.88	
SE	(149.1)	(332.7)	(265.8)	
Observations	4,618	2,030	1,768	
Mid-term				407.4 (33.8%)
ATT	86.02	62.15	2.365	
SE	(183.5)	(407.6)	(307.9)	
Observations	4,556	2,330	1,698	

Table 5.1.1 shows the short and mid-term ATT of RE on domestic labour using 3 approaches. Clustered SE in parentheses. Coefficients are significant at *** p<0.01, ** p<0.05, * p<0.1.

5.2 Effects on self-employed labour

When it comes to self-employment, the data allows me to disaggregate the variable based on whether individuals are self-employed in the agricultural or non-agricultural sectors. Barron and Torero (2014) and Dinkelman (2011) see that RE has a positive impact on female labour in El Salvador and South Africa, respectively. It is anticipated that RE will promote economic diversification (Candelise et al., 2021), as housewives are able to leverage low-investment tools to

[15] For leisure activities, ownership of electro-domestics decreased by 34% between 2011 and 2016. Appliances for household chores decreased by 54% and then increased by 67%.

pursue new small business opportunities, such as "food preparation, ironing, and washing clothes" (Barron & Torero, 2014, p. 5). Thus, deviating household's time spent from only agricultural activities and at the same time increasing the overall time in nonagricultural activities.

Accordingly, it is observed that there is a decline in the time spent on agricultural self-employment and a corresponding increase in non-agricultural self-employment in both the short and mid-term, although insignificant in every case. Despite the expected direction of the coefficient, the matching and DiD approach utilized as the primary estimator fails to demonstrate a significant impact in both cases.

For the two variables considered in this model, DiD with PSM shows to be underpowered to find significant results as in both cases the coefficients fall under the MDE. It is important to mention that in these cases, effects are more than likely to be present in the long run as there is a clear accordance from the theory to the output given by the tables. In that sense, the results coming from Tables 5.2.1 and 5.2.2 could be indicating that households are taking more time to adjust to changes. For instance, it might take them a while to see the actual benefits of such a service, and also in order to take advantage of it, they might need to resort to financial services to invest in this. Thus, a larger sample size would be preferred in order to detect significant effects. When assessing agricultural self-employment, even though the size of the estimated MDEs is relatively small according to statistical norms, their economic significance is notable. Specifically, these MDEs represent a reduction of 1.4 hours per day in the short term and 1.6 hours per day in the mid-term. Similarly for the case of nonagricultural self-employment where an effect of the size of the MDE would entail an increase of 0.9 hours per day in the short-term and 1 hour per day in the mid-term. Although the original plan for this study was to include a set of dummy variables indicating whether households were starting or continuing to operate a business, this idea had to be abandoned because very few households reported doing so compared to the rest of the sample. Censoring and truncation techniques could be of value in a case like this in order to zoom in on these cases. Nonetheless, for the purposes of my study, this is considered a limitation and therefore also an invitation for the readers to look further into this.

Table 5.2.1 Impact of rural electrification on self-employed agricultural yearly hours

MODELS	DiD	DiD-Cov	DiD-PSM	MDE (Effect size %)
Short-term				525.1 (19.7%)
ATT	-125.4	-665.9	-242.4	
SE	(255.8)	(548.2)	(451.6)	
Observations	4,618	2,030	1,768	
Mid-term				587.9 (22%)
ATT	367.1	338.2	-458.9	
SE	(346.2)	(572.1)	(551.3)	
Observations	4,556	2,330	1,698	

Table 5.2.1 shows the short and mid-term ATT of RE on self-employed labour for agricultural activities using 3 approaches. Clustered SE in parentheses. Coefficients are significant at *** p<0.01, ** p<0.05, * p<0.1.

Table 5.2.2 Impact of rural electrification on self-employed nonagricultural yearly hours

MODELS	DiD	DiD-Cov	DiD-PSM	MDE (Effect size %)
Short-term				335 (39.6%)
ATT	207.4	485.5	229.8	
SE	(216.8)	(405.4)	(300.8)	
Observations	4,618	2,030	1,768	
Mid-term				392 (46.3%)
ATT	304.6	415.0	301.6	
SE	(220.9)	(407.1)	(201.2)	
Observations	4,556	2,330	1,698	

Table 5.2.2 shows the short and mid-term ATT of RE on self-employed labour for none-agricultural activities using 3 approaches. Clustered SE in parentheses. Coefficients are significant at *** p<0.01, ** p<0.05, * p<0.1.

5.3 Effects on casual salaried labour

Unlike other studies that consider a village "electrified" if just one household reports having it (Fried & Lagakos, 2021), my study only looks at the impact of RE on individual household beneficiaries and not the community as a whole. This poses another limitation to my study, as I cannot speak to the broader effects coming from the community or general equilibrium effects. However, I can examine the impact on salaried labor as a result of the increased availability of household members to join the workforce, due to the liberation of time in households. From this that I first started looking at domestic and self-employment hours to determine if there is a decrease in the time allocated to these activities that could enable a change in salaried labour hours.

Casual labour

I first look into the effect of RE on casual labour. In the context of this study, casual labour is defined as the hours households reported working on any casual or temporary job. Despite not being able to find significant effects in the previous outcome variables, households showed a statistically significant, though moderate, increase in hours destined for casual labour after being electrified (See Table 5.3.1). This result suggests that the supply for temporary jobs increased in the short term. Although not shown to be statistically different from 0, the coefficients resulting from the other models can help in the interpretation of the results. The results appear to be robust as the coefficients consistently have the same sign across all estimation methods[16]. However, this was not the case for mid-term impact.

Statistical power in this case does not appear to be the problem as the coefficient, in absolute terms, is larger than the MDE. Therefore, considering the actual size of the MDE (less than 15 min per day), I would be inclined towards speculating about the inexistence of such an effect. Also, the negative sign could be explained by the other labour effects not measured in my study. I cannot rule out the possibility of temporary jobs becoming permanent and therefore no longer measured under this variable but in the permanent labour outcome variable.

[16] However, it is important to acknowledge that other factors may influence the reliability of the estimates and to interpret the results with caution.

Table 5.3.1 Impact of rural electrification on casual labour yearly hours

MODELS	DiD	DiD-Cov	DiD-PSM	MDE (Effect size %)
Short-term				107 (97.6%)
ATT	58.39	81.40	166.2**	
SE	(57.70)	(101.0)	(64.97)	
Observations	4,618	2,030	1,768	
Mid-term				83 (75.7%)
ATT	144.9	156.9	-116.8	
SE	(100.29)	(162.8)	(96.11)	
Observations	4,556	2,330	1,698	

Table 5.3.1 shows the short and mid-term ATT of RE on casual labour using 3 approaches. Clustered SE in parentheses. Coefficients are significant at *** p<0.01, ** p<0.05, * p<0.1

Permanent labour

Secondly, I look into the impact RE has on salaried labour. For the purpose of this study, salaried labour entails the sum of what members of households reported to be their first (main) job and a second job where the employer was not a member of the household and also not a temporary job. The DiD-PSM estimator does not show any significant results nor it suggests an economically meaningful change after 2 or 5 years (See Table 5.3.2). This result is also not entirely surprising for reasons. Foremost, I do not see significant changes in my self-employment or domestic labour variables which would suggest a change in permanent labour. Secondly, effects like this could be present after a longer period. Chhay and Yamazaki (2021) were able to find significant effects after ten years. Also, they are clever enough to disaggregate employment into different sectors, therefore able to single out results in a better way. My salaried labour variable captures an overarching effect as the sample size and distribution of my data would have not allowed me to do it as well[17]. Lastly, given the sample size I work with, my study is powered to detect effects of 209

[17] I am only able to make the distinction between agricultural and nonagricultural self-employment.

and 245 hours for the short and mid-term, respectively. In both cases, these are considerably larger than any of the coefficients shown in Table 5.3.2, meaning that the study is well underpowered to detect effects, were these to exist. Nonetheless, given the size of the MDEs, I would be more inclined toward speculating about the absence of any effect during a span of 5 years. Drawing from the insights of Dinkelman (2011) it is also plausible that electrified households chose to establish micro businesses, rather than engaging in salaried employment, as an alternative means of generating income. This is merely speculation as I, as explained in 5.2, do not measure this.

Table 5.3.2 Impact of rural electrification on permanent labour yearly hours

MODELS	DiD	DiD-Cov	DiD-PSM	MDE (Effect size %)
Short-term				209.2 (145.5%)
ATT	122.0	59.27	-34.15	
SE	(103.0)	(102.9)	(86.49)	
Observations	4,618	2,030	1,768	
Mid-term				244.7 (170.2%)
ATT	43.66	66.92	56.75	
SE	(111.4)	(118.0)	(165.5)	
Observations	4,556	2,330	1,698	

Table 5.3.2 shows the short and mid-term ATT of RE on permanent labour using 3 approaches. Clustered SE in parentheses. Coefficients are significant at *** p<0.01, ** p<0.05, * p<0.1.

6. Lessons

This paper studies the effect of electrification on household time allocation for paid and unpaid labour in rural areas of Ethiopia. Namely, the effect that it has on salaried, domestic, and self-employed (agricultural and nonagricultural) labour hours in a year. Overall I find no significant effects in either the short or the mid-term. With the exception of temporary labour in the short term where I find a minimal, but significant, increase. There are some possible explanations behind the insignificance of my results. Firstly, statistical power and possible presence of unobservables within my design. Secondly, the time-lapse in which this analysis is carried out is perhaps not as long as needed for households to adapt to this transition and start showing changes in household dynamics. Lastly, focusing only on beneficiaries of electrification and not considering community-level indicators or general equilibrium effects is also a potential limitation to my study.

Studies like this are relevant as they add to ongoing literature assessing RE but also because of their public policy relevance. Firstly, when assessing infrastructure developments where the spread is everything but random and data is scarce, there is no silver bullet. Authors make use of different approaches to deal with the selection bias E.g (Instrumental variables, regression discontinuity, DiD, PSM). I use panel data from 2011 to 2016 to conduct an analysis that entails an array of DiD and DiD with PSM. "Combining empirical approaches and data sources, each with their own strengths and weaknesses, is also potentially useful for dealing with the multiple biases that make it challenging to identify the effects" Dinkelman (2011). Secondly, electrification on its own should be assessed as an enabler or a catalyst rather than a driver of change. Lee et al. (2020a) argue that grid expansion initiatives might not yield meaningful results if these are not accompanied by other development programs facilitating improved access to markets or households capacity to purchase electrodomestics, or if they don't target "regions that already benefit from complementary factors" (pp. 125). Further studies looking at interaction effects are recommended. For instance, Moneke (2020) finds little effect of electrification on welfare but a larger one when analyzing it with other developments such as road infrastructure.

Furthermore, it might also be relevant to study what Callaway and Sant'Anna (2021) call "group-time average treatment effect" (GTATE) to assess the impact of a policy that is disaggregated over time, such as rural electrification. In other words, the average treatment effect for a given group at a given time. Recent literature proposes a method to assess programs that occurred over staggered phases (Callaway & Sant'Anna, 2021; Goodman-Bacon, 2021; Sun & Abraham, 2021). In this sense, we find ourselves in a situation similar to what Callaway and Sant'Anna (2021) discuss in their paper. Treatment effects with a fluctuation of treatment timings, potentially dissimilar treatment effects and where the parallel trend assumption is consistent after controlling for observed covariates. Dif-Dif in its canonical form does not take this into consideration; therefore, no reliable causal effects can be taken (Goodman-Bacon, 2021). Furthermore, Goodman-Bacon (2021) and Sun and Abraham (2021) noted that just adding a post and lagged dummy controlling for the period a unit first received treatment (also referred to as event-study dummies) was not enough either to get reliable coefficients of treatment effects. Callaway and Sant'Anna (2021) propose a method in DiD where such heterogeneities are enforced into the model and allow for interpretable and strong coefficients to be determined. As a result, this allows the study to measure the 1) Group average treatment effect (GATE) ,2)Time average treatment effect (TATE), and 3) the GTATE.

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8. Appendix

Table 8.1 Electrification indicator

Electrified (E)	2011	2013	2016	Observations	%			
000				2156	81%	Control		
010		E		31	1%			Short-term treatment
011		E	E	122	5%		Mid-term treatment	
001			E	76	3%			
100	E			27	1%			
101	E		E	9	0%			
110	E	E		14	1%			
111	E	E	E	212	8%			
	Total			2647	100%			

Power calculations

A power analysis was conducted using the 'power twomeans' command in Stata to estimate the minimum detectable effect (MDE) given the sample size. The MDE is the smallest effect a study is able to find given its conditions (e.g., sample size, expected power, significance level,statandar deviation, among others). Two sets of MDE's are calculated since the study accounts for short and mid-term effects and therefore, two different sample sizes are used. Considering a sample of 2,309 and 2,278 households for short and mid-term effects respectively, 80% of power, and a significance criterion of 0.05; results indicated the following MDE's for each of the outcome variables.

Mid-term effect Power calculations

Variabe	MDE	Mean	Effect size in%
Casual labour	82.99	109.62	75.7%
Permanent labour	244.7	143.75	170.2%
Domestic labor	407.4	1205.77	33.78%
Self-employ farm	587.9	2662.82	22.07%
Self-employ nofarm	392.4	846.48	46.35%

Short-term effect Power calculations

Variabe	MDE	Mean	Effect size in%
Casual labour	107	109.62	97.61%
Permanent labour	209.2	143.75	145.5%
Domestic labuor	356	1205.77	29.5%
Self-employ farm	525.1	2662.82	19.72%
Self-employ nofarm	335	846.48	39.58%

Making use of the conventions proposed by Cohen (1992) (i.e., MDE<20% small, MDE<50% medium, MDE<80% large), the results suggest that my design should be able to capture medium-very large effects only for casual and permanent labour in both the short and middle-term, assuming there are any. For the remaining three variables, the power calculations indicate that small-medium effect sizes can be captured given the considerations mentioned above, should there be any effect to find.

Ways on how other authors deal with selection bias when assessing RE

Burlig and Preonas (2016) use a regression discontinuity and a difference in differences design to estimate the mid-term impact of India's national electrification program between 2005 and 2011.
Dinkelman (2011) study the effects of RE on community-level employment in South Africa between 1996 and 2001 utilizing an instrumental variable approach. Similar to the former, Ribeiro et al. (2021) use an instrumental variable design as well as a difference in differences to estimate the effect of RE on individual-level time allocation in daily activities in Brazil between 2000 and 2010. Similar to the approach followed by this paper, Khandker et al. (2013) use propensity score matching combined with a difference in differences design. They run a difference in differences analysis on the matched observations to estimate the individual and household-level effect of RE on welfare in Vietnam between 2002 and 2005. Chhay and Yamazaki (2021) use an inverse probability of treatment weighting regression adjustment method. Later on, they combine this with a difference in differences method to leverage the panel data set from 1998 to 2008 in order to estimate the effect of RE on the employment arrangement in Cambodia.
Bernard and Torero (2015) and Lee et al. (2020b) are among the few authors using a randomized experiment to assess the impact of RE utilizing similar approaches in Ethiopia and Kenya, respectively. Lee et al. (2020b) randomly picked clusters of households to be offered the chance

to connect to the grid at a subsidized cost. Bernard and Torero (2015) fix the heterogeneous allocation of electricity by giving random discount vouchers that offered discounts on grid connections to study social interaction effects as a response to RE in Ethiopia.

Selection bias in the Ethiopian context:

Two identification issues that arise when measuring the effect of RE are the endogenous adoption and allocation of infrastructure in Ethiopia. As reported by (Khandker et al., 2013), wealthier households are more likely to connect to the grid once electricity is made available to the community. Households are responsible for paying for the installation costs of electricity, also known as last mile costs, these include; drop-down cables from the pole as well as purchasing and installation of poles to the last one connected if needed (Bernard & Torero, 2011). These prices, as mentioned before, range from 300-450 ETB (Bernard & Torero, 2015).

On the other hand, areas with higher growth potential are likely to be prioritized over the rest (MWIE, 2018). Furthermore, regions aligned with the then-current regime were also more likely to be electrified (Moneke, 2020). Moneke (2020) reports that electrification expansion, during its first stages, prioritized “politically demanded locations” (p.22) and regions that were near the grid going from these places and the power plants. From 1991, when the Tigray’s people liberation front (TPLF) overthrew President Mengistu Haile Mariam, until 2018, the TPLF ruled over the country. Not surprisingly, the data from the LSMS for the 3 waves shows a considerable difference between the region of Tigray and the rest of the regions out of the rural households sampled (see Table 9.2)

Table 8.2 Electricity distribution among regions

	Electricity indicator (2011)			Electricity indicator (2016)		
Region	No	Yes	Total	No	Yes	Total
	206	40	246	186	60	246
Tigray	84%	16%	100%	76%	24%	100%
	565	52	617	547	70	617
Amhara	92%	8%	100%	89%	11%	100%
	473	30	503	442	61	503
Oromia	94%	6%	100%	88%	12%	100%
	732	56	788	701	87	788
SNNP	93%	7%	100%	89%	11%	100%
	409	84	493	352	141	493
Other regions	83%	17%	100%	71%	29%	100%
	2385	262	2647	2228	419	2647
Total	90%	10%	100%	84%	16%	100%

Propensity Score Matching

Although a less popular approach, PSM can be a useful tool to deal with selection bias issues in some cases[18]. The underlying idea of PSM is that if one can find a group of non-treated units with similar probabilities of being treated, then the difference in outcomes between these two groups can be attributed to the intervention (Austin, 2008; Caliendo & Kopeinig, 2008; Khandker et al., 2009).

In practice, PSM first finds a similar "clon" for a treated unit in the control group and then makes the analysis on the two samples. Considering a wide array of observable characteristics, the first step estimates the probability of being assigned to treatment. In order to avoid the curse of dimensionality[19], this method bundles all characteristics into one vector (X) to calculate such probability. By assigning probabilities to each observation and using them as weights, the researcher can balance the sample (Rosenbaum & Rubin, 1983). To calculate the propensity score, linear probability models such as logit or probit are most often used. According to Caliendo and Kopeinig (2008), in cases where the probability is calculated on a dichotomic variable, logit and probit models generally come up with similar results. This approach comes with two, no belittled, assumptions. The conditional independence and the common support assumption.

The first one, also known as the unconfoundedness assumption, states that outcomes (Y) of an intervention are orthogonal to the treatment assignment (T) given a set of observable characteristics (X).

$$X|T \perp Y \qquad \text{Equation (3)}$$

This is a rather strong assumption as it considers the vector X to bundle all potential characteristics affecting treatment assignment. As a result, this methodology is popular in programs or interventions where selection criteria are clearly stated. For instance, student loans in Mexico.

[18] Such as situations where no panel data is available but only crossectional data. However, it can also be the case that it is the only way but it the researcher has reason to believe there are variables not being considered, it can lead to misleading conclusions.

[19] The curse of dimensionality is the situation where adding a new characteristic to the matching criteria, exponentially decreases the likeliness of finding an according comparison group.

Eligibility is dependent on a wide range of requirements that speak to the socio-economical status of the applicant and their academic achievements [20]. Therefore narrowing down the potential treated population to distinguished students within a certain age range and with the economic means to pay back a loan (proof of collateral).

The second assumption, also known as the overlap condition (Khandker et al., 2009), establishes that individuals with the same X characteristics have a larger than 0 probability of being allocated to either the treatment or control group (Caliendo & Kopeinig, 2008). In simpler terms, this assumption makes sure that treated units have "clones" in the control group. The larger these two groups are the more predictive value the model has (Khandker et al., 2009).

Furthermore, PSM, just as DiD, also considers the SUTVA as part of its analysis.

The paper first matches[21] observations at baseline (2011) on a set of covariates, fixed effects and outcome variables. Balancing tests were done after matching to evaluate the balancing properties. The results showed that the sample used for matching in the mid-term analysis is properly balanced. In other words, no significant differences were found between treatment and control units after matching. When running psmatch after psmatch2, no significant differences where shown after matching between treatment and control groups. Although, due to the small amount of households electrified overall in the study, the units assigned to control group are much more than those assigned to treatment. This could, potentially, lead to bias estimates of the treatment effect since the variability in the control group might be much larger.

[20] See the manual of FIDERH or CONACYT

[21] Since the control sample is substantially large, Kernel matching is the preferred matching estimator in order to get more accurate estimates

Balancing tests for treatment and control groups after matching

Table 8.3 Short-term sample				
Variable	Mean (Matched)		t-test	
	Trated	Control	t	p>|t|
Distance to market	9.7407	11.281	-0.97	0.333
Basic education	0.3333	0.27603	0.64	0.522
Household size	5.1852	5.2138	-0.07	0.942
Field size	0.9332	0.9864	-0.22	0.825
Dependacny ratio	0.54091	0.54978	-0.22	0.824
Region	13.37	13.547	-0.07	0.944
Livestock	3.7424	3.9581	-0.22	0.829
Female	0.54043	0.5454	0.11	0.91
Household head	0.24074	0.2353	0.07	0.948
Casual labout	88.593	90.004	-0.02	0.985
Permanent labour	237.74	199.29	0.27	0.788
Domestic Labour	1199.8	1291.8	-0.3	0.767
Self employed agricultural	2474.8	2566.9	-0.2	0.841
Self employed nonagricultural	832	749.92	0.31	0.756

Table 8.4 Mid-term sample				
Variable	Mean (Matched)		t-test	
	Trated	Control	t	p>|t|
Distance to market	9.9211	11.47	-0.77	0.443
Basic education	0.34	0.26796	0.69	0.489
Household size	5.289	5.3332	-0.09	0.927
Field size	1.003	1.0764	-0.21	0.836
Dependacny ratio	0.5737	0.57447	-0.02	0.987
Region	13.763	12.643	0.37	0.71
Livestock	3.7916	4.1288	-0.26	0.794
Female	0.57061	0.564	0.12	0.902
Household head	0.2368	0.2293	0.08	0.939
Casual labout	80.737	76.002	0.05	0.957
Permanent labour	168.37	144.77	0.18	0.856
Domestic Labour	1243.9	1264	-0.05	0.958
Self employed agricultural	2222.3	2567	-0.62	0.534
Self employed nonagricultural	790.95	733.24	0.18	0.857