THE EFFECTS OF ELECTRIFICATION ON EMPLOYMENT IN RURAL PERU*

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Abstract

We examine the effects of a rural electrification program on employment in Peru. Exploiting the roll-out of the program across districts over time, we adopt differences-in-differences and fixed-effects strategies to estimate the impact of electrification on labor market outcomes. Our preferred specification suggests that, among males, the program increases hours of work and diminishes the likelihood of having a second occupation. Among females, the treatment raises earnings and these gains seem to be driven by a shift towards non-agricultural jobs. Then, we construct a measure of treatment intensity and show that each additional electrification project increases the magnitude of the estimated impacts (in absolute terms).

Keywords: Electrification, employment, Peru
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1 INTRODUCTION

Access to services such as water, electricity and telecommunications seems to be key to generate welfare improvements in rural areas. Many governments in developing countries allocate large amounts of money to finance infrastructure projects as an attempt to boost economic development in poor areas. These efforts have led to a recent increase in the studies which empirically examine the link between access to public infrastructure and the well-being of poor families.

In 2006, 39 percent of rural households in Peru had access to electricity. The government rapidly increased electricity coverage in the last decade through the Rural Electrification Program (knowns as PER, for its name in Spanish). Since these families largely depend on the labor market to meet their consumption plans, in this study we focus on the effects of electrification on employment. In particular, we exploit the timing of the implementation of the program to shed light on the labor market consequences of rural electrification. In the absence of an experimental design, our approach consists of using two identification strategies: Differences-in-Differences and Fixed-Effects. For the former, we use seven repeated cross-sections of the Peruvian national household survey during the period 2006-2012. For the latter, we utilize a unique household panel data set collected between 2007 and 2010.

Electricity provision can affect employment through different channels. First, it can be thought of as a technological shock that improves household production. Second, it implies a larger time endowment because everyone can work during the night (and not only during the day). Third, it could promote the start of new businesses by allowing households to produce goods and services that require appliances. Fourth, it might increase the time spent watching TV (Olken 2009). Since the theoretical prediction of access to electricity on labor supply is ambiguous, we empirically examine the total effect of electrification on several employment outcomes.

We document the following impacts of the program on labor market outcomes. Our preferred specification suggests that men in treated areas reallocate their time devoted to work as follows: they work harder (2.5 additional hours per week) in their main occupation but are less likely to have a second job (a reduction of 6 percentage points). Among women, our estimates indicate that treatment increases earnings and hourly wages by around 30 percent. Additional evidence suggests that these gains are driven by a lower probability of working in agriculture. Since male earnings were unaffected, these estimates imply that the program reduced the gender wage gap in treated districts. We then construct a measure of treatment intensity and show that the impacts of the program are larger in areas where more projects were concluded. These findings represent our contribution to the literature that examines the role of electricity provision on labor markets in developing countries.

The rest of the paper proceeds as follows. Section 2 reviews the related literature. Section 3 describes the program. Section 4 outlines the empirical strategies. Section 5 gives the details of our data. Section 6 displays our results. Section 7 offers concluding remarks.

2 Literature Review

A growing literature examines the consequences of providing the poor with public infrastructure and services such as roads, water, electricity, phones, and internet. This empirical literature has documented that such public projects have positive effects on the well-being of rural families. Overall, these investments in public infrastructure and services allow the poor to have access to markets, and information. In the next paragraphs we discuss with further detail the contributions and limitations of the existing literature. First, we review studies that provide evidence from Peru on the association between electricity, phones and mobile phones on labor supply, agricultural production, and consumption. We conclude this section with a discussion of previous studies which look at the impact of electrification on employment, industrialization, and development.

A couple of observational studies provide evidence on the association between access to electricity and work decisions in rural Peru. Escobal (2005) makes a first attempt to document correlations between access to infrastructures and labor income in rural areas. Using matching techniques, he finds that access to electricity and roads is positively associated with: i) total hours of work, ii) the share of time spent in non-agricultural activities, and iii) the likelihood of working in wage activities. Thus, infrastructure provision not only affects the level of hours of work but also allows rural households to have more diversified income portfolios.

Torero et al. (2007) analyze how the privatization of electricity companies affects the quality of the service and its impact on hours of work.

That is, they compare households whose service is provided by a private firm versus families whose supplier is a public firm. Based on matching estimates, their results indicate that rural households under private provision of electricity have more opportunities to work in non-farm activities, and as a result, the share of time in non-farm activities increases.

More recently, Beuermann (2011) estimates the impact of providing phones to isolated rural villages. This intervention was carried out by the Peruvian Fund for Investments in Telecommunications (FITEL, for its name in Spanish) during 2001 and 2004. The author uses village-level panel data and exploits differences in the timing of the intervention across villages to identify the impact of phones on agricultural profitability and child labor. His results suggest that access to phones increases agricultural profitability by 20 percent, and reduces agricultural child labor by 9 percentage points.

Beuermann et al. (2012) study the impact of mobile phone coverage on consumption and the value of assets. They construct patterns of coverage at the village-level during 2001 through 2007. After matching this information to household outcomes, they find that access to mobile phones increases consumption by 7.5 percent, and the value of assets by 13.5 percent.

The studies which use data from Peru indicate that infrastructure projects have positive effects on the well-being of rural families. The first two papers rely on matching techniques while the others use panel data to recover their parameters of interest. Our empirical work will add to this literature by examining how access to electricity affects employment and earnings in rural areas of Peru using exogenous variation in the placement of an electrification program that took place during 2006 and 2010. Next, we consider three studies that are closely related to ours.

First, Dinkelman (2011) estimates the impact of electricity provision in rural areas of South Africa during 1990-2007. He takes advantage of the roll-out of the electrification program in South Africa, done by ESKOM, the electricity utility. Between 1993 and 2003, about US $1.4 billion was spent on household electrification and about 470,000 households were electrified. The community-level selection of the program was not random so the author relies on two empirical strategies using data at the community level. First, he uses land gradient as an instrument for program placement and then estimates the impact of electrification on employment. Second, the author adopts a fixed-effects estimator to remove time-invariant unobservable that may jointly affect program placement and employment. Based on both techniques, he finds evidence on significant effects of household electrification on employment. His results indicate that electrification leads to an increase in female employment in both the extensive and intensive margin. He also shows that women's wages fall while male earnings go up. The main weakness of the study is that there is systematic migration of people from non-treated areas to electrified communities. These migration patterns inflate the reported estimates and also affect the interpretation of the results. In spite of this limitation, this paper clearly shows that infrastructure has important effects on labor markets in developing countries.

Second, Rud (2012) evaluate the link between electricity provision and industrialization in India during the period 1965-1984. To overcome endogeneity concerns, he uses the start of the Green Revolution, an agricultural technology intense in irrigation introduced in the 60s as a natural experiment. In particular, he predicts electricity expansion using (initial) groundwater availability in the 60s. The author documents the existence of a first-stage and then uses IV to estimate the impact of electricity provision on industrial output. His results suggest that an increase in electrification provision is associated with an increase in manufacturing output. He also finds that places with higher access to electricity have more factories and output among small firms.

Third, Libscomb et al. (2013) analyze the effects of electrification on the Human Development Index (HDI) using county-level data from Brazil. The authors also rely on geographic characteristics to adopt an IV approach. More specifically, they simulate how the electricity grid would have evolved if its expansion had only take into account geographic cost considerations (water flow and river gradient), ignoring demand-side considerations. Then, they use this forecast as an instrument for actual program placement. The validity of their strategy relies on the assumption that cost-side determinants can be fully separated from demand-side concerns. Namely, they assume that costs are not related to demand levels for electricity. Their results indicate that electricity provision is associated with higher levels of HDI. Moreover, their analysis suggests that migration is unlikely to account for the large magnitude of development gains observed. They also estimate large, positive effects of electrification on employment, salaries, and investments in education, but not health.

These three papers use geographic characteristics as instruments for program placement. That is, they assume that land gradient, groundwater availability and river gradient do not have direct effects on employment, industrialization and development, respectively. For instance, Dinkelman (2011) assumes that land gradient do not affect the likelihood of engaging in labor-intensive activities such as agriculture. Alternatively, Rud (2012) rules out that manufacturing output can grow faster simply because of access to groundwater and not because of electrification. These limitations are unavoidable given the difficulty in solving the endogenous nature of infrastructure investments. In this paper, we also have to deal with the non-random allocation of the program. More specifically, we use two empirical strategies, DD and FE to examine the employment response to electrification in rural Peru. Our paper add to this literature by using individual-level data instead of aggregate data. Also, our data have detailed information on labor outcomes that allow us to identify the channels through which electrification affects employment in rural areas of Peru.

3 THE PROGRAM

In 1993, the Peruvian Ministry of Energy and Mines launched the Rural Electrification Program (PER, for its name in Spanish) as an attempt to foster social and economic development in rural areas. The implementation of the program began after the enactment of the "Act of Rural Electrification". According to this Act, the main objective of the PER is to provide rural families with electricity, with the support of the private sector, public institutions, and local governments.

The PER was not randomly assigned across districts. Instead, the Ministry used the following set of variables to identify eligible districts:

- Lower index of rural electrification (percentage of households with electricity)
- Higher poverty rate (percentage of households whose consumption is below the poverty line)
- Lower proportion of the estimated subsidy per connection
- Lower cost per connection
- Higher use of renewable energy

Based on these dimensions, they defined the roll-out of the program. However, the program did not expand according to these criteria because every time new data was released (or new authorities got elected to work for the Ministry) the initial plans were changed. In fact, the Ministry divided the PER in two sub-periods: 1993-2004 and 2005-2010. The first sub-period was based on variables constructed using the census of 1993. The second sub-period used data from three sources: census 1993, census 2005, and the poverty map of 2007.

In this paper, we focus on the projects that were concluded in the period 2006-2010 for two reasons. First, most of the projects were concluded in this period (due to rapid economic growth and high political stability). Second, the Institute of National Statistics (INEI for its name in Spanish) collected a unique household panel data set during this same period.

We present the evolution of electricity coverage in Figure 1. We estimate it using data from the ENAHO, including only rural districts [1]. In only seven years, it jumped from 39 percent to 68 percent. The figure shows that access to electricity in rural areas roughly grew by 5 percentage points per year in the study period. The program aims to provide electricity to 95 percent of rural households by 2021.

So far, 628 electrification projects have been concluded throughout rural Peru. To illustrate, we present the timing of the program in Figure 2. The distribution of the projects according to its type is as follows: i) 55 transmission lines with a length of 2,872 kilometers; ii) 299 rural electrical systems whose total length is 20,000 kilometers); iii) 268 hydropower plants; iv) 4 projects that included the installation of 1,523 solar panels; and v) 2 wind-turbines. The total cost of these projects is US$ 657.5 million.

4 Empirical Strategies

As mentioned in the previous section, the PER did not have an experimental design. Therefore, any difference in outcomes between treated and non-treated households would be a biased estimate of the effect of the program. To deal with unobserved confounders, we rely on two non-experimental methods to estimate the impact of PER on employment. First, we utilize repeated cross-sections and adopt a Differences-in-Differences (DD) approach. Second, we use household panel data

[1] The data consists of seven annual cross sections of the ENAHO.

and apply a Fixed Effects (FE) estimation. In both cases, non-treated and treated households would be referred to as the control and treatment group, respectively (bear in mind that the treatment is defined at the district level). Each approach has its identifying assumptions and data requirements which we describe with further detail in the next subsections. Both methods could be biased if there is systematic migration across areas. For example, migration would lead to "contamination", which refers to the fact that some individuals from the control group may receive the treatment and vice-versa. Given that FE not only captures district-level unobservable characteristics but also individual-level heterogeneity, we refer to it as our preferred specification.

4.1 DIFFERENCES-IN-DIFFERENCES (DD)

The DD approach exploits the roll-out of the program to remove *permanent* differences across control and treated districts. That is, the DD estimator solves the problem of endogenous program placement under the assumption that the selection bias is additive and does not vary over time.

To fix ideas, let $i = 1, 2, ..., N$, $j = 1, 2, ...M$, and $t = 1, 2, ..., T$ denote, individuals, districts and years, respectively. This setup lead us to estimate the following equation:

$$y_{ijt} = \alpha_j + \alpha_t + \alpha_1 d_{ijt} + \sum_{t=2}^{T} \beta_t (d_{ijt} * \alpha_t) + X'_{ijt}\gamma + \mu_{ijt} \quad (1)$$

where y_{ijt} is the outcome variable (participation, hours of work, and so on); α_j and α_t denote district and year fixed effects; d_{ijt} indicates that the individual lives in a district where at least one project has been concluded by year t; X_{ijt} is a vector of characteristics (such as maternal language, education, and so on), and β_t are the parameters of interest. The error term is denoted by μ_{ijt} and is allowed to be correlated across individuals within districts.

Though the data from electrification projects are defined at the district level, our treatment indicator varies at the individual level because not all individuals were treated in the same year. This within-district variation allows us to include district fixed effects which, among other things, capture geographic characteristics that are relevant to determine the cost of each project (and therefore the placement of the program). The crucial assumption to obtain consistent estimates is that, in the absence of the treatment, employment trends would have been the same in treated and control districts. Based on the criteria used by the Ministry (see section 3), this assumption sounds plausible.

We should note that we fail to identify which households are actually beneficiaries of the program. Therefore, our results should be interpreted as "Intent-To-Treat" (ITT) estimates. In other words, we estimate the (overall) effect of providing electricity at the district level instead of measuring the impact of actually receiving it. Moreover, note that by construction the ITT estimate is year-specific.

Around one third of treated districts received more than one electrification project in the study period. Thus, we are able to construct a measure of "treatment intensity" among treated districts that varies over time. More formally, we also run:

$$y_{ijt} = \alpha_j + \alpha_t + \phi * Intensity_{jt} + X^{'}_{ijt}\gamma + \nu_{ijt} \tag{2}$$

where $Intensity$ measures the *cumulative* number of concluded projects in district j by year t and all the other variables are defined as in equation (1) [2]. The coefficient ϕ measures the marginal impact of concluding one additional project, conditional on being exposed to the program. The identifying assumption of equation (2) is that our measure of intensity is uncorrelated with the error term once we control for district and year fixed effects, and individual characteristics.

4.2 FIXED-EFFECTS (FE)

Rather than using repeated cross-sections, the FE approach requires to follow the *same* individuals over time. The advantage of having panel data is that we can estimate the following equation:

$$y_{ijt} = \lambda_i + \lambda_j + \lambda_t + \rho e_{ijt} + \eta_{ijt} \tag{3}$$

where y_{ijt} is the dependent variable, λ_j and λ_t denote district and year fixed effects, e_{ijt} indicates that the individual lives in a district where at least one project has been concluded by year t, and η_{ijt} is the error term, which is allowed to be correlated within individuals across periods. The main difference with respect to equations (1) and (2), is the inclusion of an individual fixed effect denoted by λ_i, which captures

[2] In other words, our measure of intensity is not the number of projects concluded each year. Instead, is the cumulative number of projects concluded by year (i.e. it is increasing over time).

unobserved individual heterogeneity. The price of doing so is that we cannot include individual controls that do not vary over time such as district/year of birth, maternal language, sex, and education. Recall that FE is our preferred specification because it is robust to the inclusion of individual fixed effects.

Hence, the key assumption to apply a FE strategy is that unobserved confounders are time-invariant[3]. Given that program placement was determined by district characteristics that were measured at some point of time (which allows us to think of them as time-invariant variables), the identifying assumption of the FE approach seems to be reasonable.

Finally, we run the FE analogous of equation (2). That is:

$$y_{ijt} = \lambda_i + \lambda_j + \lambda_t + \pi * Intensity_{ijt} + \varepsilon_{ijt} \tag{4}$$

where all variables are defined in a similar fashion. The interpretation of the parameter π is the same of ϕ from equation (2): the effect of concluding one additional electrification project, conditional on being exposed to the treatment.

5 DATA

Our primary data source is the Encuesta Nacional de Hogares (henceforth, ENAHO) conducted by the INEI on a yearly basis. This survey includes comprehensive information at both the household and individual level. The ENAHO is representative at the national, urban and rural level.

For the DD approach, we pool repeated cross-sections for the period 2006-2012. For the FE strategy, we use a unique household panel data set for the period 2007-2010. The sample size of the former is much larger than the latter. In both cases, we focus on rural districts.

The dependent variables are taken from the ENAHO employment record (in which all individuals over 14 are interviewed). We construct nine labor market outcomes for the empirical analysis: i) participation; ii) employment; iii) hours of work; iv) log earnings; v) log hourly wages; vi) whether the individual has two jobs; vii) whether he is a wage-

[3]We distinguish the individual fixed effect from the district fixed effect only to illustrate the main difference with respect to the DD approach. However, in practice, they are not distinguishable because individuals in our panel do not migrate. Therefore, the district of residence is captured in the individual fixed effect.

earner; viii) whether he works in agriculture; and ix) whether he is self-employed (see the Appendix for further details).

The vector of controls used in the DD approach is taken from the ENAHO education record. It includes characteristics such as maternal language, sex, age, and education level (for more details see the Appendix). We also include an indicator variable for the presence of children below 5 in the household, taken from the demographics record.

The list of electrification projects that were concluded between 2006 and 2010 was taken from administrative records of the Ministry of Energy and Mines. In our study period, 567 projects (out of 628) were concluded in 412 rural districts. For each project, we observe the year of conclusion, and the treated districts (101 projects include more than one district). However, we are unable to distinguish between types of projects (e.g., transmission lines versus hydropower plants).

Treated districts are defined as follows. We say that a district is treated by year t if at least one project had been concluded in that year or earlier. In addition, our measure of treatment intensity is the cumulative number of concluded projects by year. For instance, if a district has one project per year, from 2007 to 2010, its intensity measure would be as follows: 1,2,3,4 (see Figure 3 to check how intensity varies by area). Then, we use unique district identifiers, year of interview and year of conclusion of each project to match both data sets. After this matching, our final DD and FE samples include 246,735 and 12,964 individuals, respectively.

Table 1 shows the evolution of electricity coverage by area (treated and non-treated). In both samples, we note that initial coverage was lower in treated areas than in control districts, which is consistent with the criteria used by the Ministry. When looking at the DD sample, we see that treated areas experienced a rapid growth in access to electricity: coverage went from 26 percent in 2006 to 67 percent in 2012. A similar expansion in access to electricity is observed when we use the panel sample. Control areas also experienced an increase in access to electricity during the study period, which supports the common-trend assumption needed to correctly estimate the impact of the program.

To show that the program led to higher electricity coverage, we regress (actual) access to electricity on the treatment variable, and on our measure of intensity. Table 2 reports the corresponding estimates for both samples. Columns 1 and 3 document the positive and highly significant impact of the treatment on access to electricity: coverage in treated districts is roughly 8 percentage points higher than in control areas. Similarly, columns 2 and 4 indicate that, conditional on being exposed to the program, each additional electrification project increases coverage by 2.5 and 3.5 percentage points in the DD and panel sample, respectively. Taken together, these results suggest that our ITT and intensity variables are strongly correlated to actual access to electricity.

6 RESULTS

6.1 DD RESULTS

The DD results for the whole sample are shown in Table 3. Panel A displays the ITT estimates for each year on the nine labor market outcomes. In general, the point estimates are very small and not significantly different from zero. Only the projects that were concluded in 2010 appear to have a modest impact on the probability of having two jobs and the likelihood of being wage-earner (see columns 6 and 7, respectively). The coefficient in column 6 indicates that treated individuals are less likely to have two jobs than people living in non-treated areas. In column 7, we see that the treatment slightly increases the likelihood of being wage-earner by 0.8 percentage points. In Panel B, we present the estimated impact of concluding one additional electrification project, conditional on being exposed to the program. Our findings suggest that treatment intensity only has a significant effect on hours of work and the probability of working in agriculture.

Table 4 reports the estimated effects of electrification only for men. Our results indicate that the program does not affect participation or employment rates (columns 1 and 2). We do find a positive but small (and barely significant) effect on weekly hours of work: projects that were concluded in 2009 increases labor supply by 0.8 hours. Columns 6, 7, and 9 suggest that providing electricity reduces the likelihood of having two jobs, and increases the probability of being wage-earner and self-employed, respectively. Though the estimates are highly significant, they are small in magnitude. Panel B shows that participation and employment rates decrease at a very low pace as we add electrification projects.

In Table 5, we present the ITT estimates for the female sample. In the top half, we see that electrification projects have no impact on labor market outcomes except for self-employment. Column 9 shows that projects concluded in 2008 reduce the likelihood of being self-employed by 1.7 percentage points. The bottom half of Table 5 exhibits that hours of work and the likelihood of working in agriculture change as we conclude additional projects. Our measure of intensity suggests that labor supply goes up by 0.2 hours of work with each additional project while the probability of working in agriculture goes down by 0.6 percentage points as intensity increases.

Youth employment (workers between 14-25 years old) is pervasive in rural areas and, therefore, is relevant to have a closer look at it. Panel A in Table 6 presents ITT estimates of the program among the youth. These results show that young workers are not affected by the program in most dimensions. Columns 7 and 8 indicate that the treatment increases the probability of being wage-earner (projects that were concluded in 2007) and decreases the likelihood of working in agriculture (projects that were concluded in 2008), respectively. When looking at Panel B, we do not find significant effects which may suggest that youth employment is less sensitive to treatment intensity than adult employment.

In short, the DD estimates imply that the program have little impact on labor market outcomes. First, the PER reduces the likelihood of having a second occupation and slightly rises the chances of being wage-earner in the sample as a whole. Second, the treatment seems to increase male's hours of work but the point estimate is small. Third, electrification projects tend to decrease the probability of being self-employed among women. Fourth, the probability of being wage-earner goes up and the chances of working in agriculture go down among young people in treated districts. Now, we turn to discuss the results of our preferred specification.

6.2 FE RESULTS

Table 7 reports the estimated effects of the PER for the whole sample. Panel A displays the ITT estimates and Panel B shows the estimates of treatment intensity. The ITT estimates suggest that the program did not have a significant impact on participation and employment rates, earnings, the likelihood of being wage-earner nor the probability of working in agriculture. However, providing electricity increases hours of work, reduces the likelihood of having two jobs, and rises the chances of being self-employed (see columns 3, 6, and 9, respectively). More specifically, weekly hours of work go up by almost two hours; the probability of having two jobs decreases by 3 percentage points; and individuals who live in treated areas are more likely to be self-employed by more than 3 percentage points. Qualitatively, these effects indicate that individuals who benefit from the program choose to work harder as independent workers but are less likely to have two jobs. Panel B of Table 7 shows that the impact on hours of work and the likelihood of having two jobs is larger (in absolute terms) as we add electrification projects. Conditional on being exposed to the program, each additional project increases labor supply by 0.5 hours of work and decreases the likelihood of having two jobs by roughly 2 percentage points. These results suggest that the effects of electrification on these outcomes are increasing on the number of projects that each district received.

The point estimates for the male sample are presented in Table 8. Panel A reports statistically significant impacts on hours of work, the likelihood of having two jobs and the probability of working in agriculture. The coefficient in column 3 suggests that providing electricity increases male labor supply by 2.6 hours of work (per week). A negative effect on the likelihood of having two jobs is shown in column 6: treated male individuals are 6 percentage points less likely to have two jobs than men in the control group. These two coefficients are twice larger than those for the sample as a whole. Finally, the point estimate in column 8 reveals that the program increased the probability of working in agriculture by 5 percentage points. The bottom half of Table 8 indicates that each additional project has a positive impact on male's hours of work. This increase leads to a mechanical reduction of hourly wages of 8 percent with each new project (given that earnings remain unchanged). Column 6 shows that increase in treatment intensity also reduces the likelihood of having two jobs by 2.4 percentage points.

In Table 9, we focus on the effects of electrification on female labor market outcomes. If we look at Panel A, we notice interesting differences with respect to men. First, employment rates go up by 3.5 percentage points in treated districts. Second, providing electricity increases earnings and hourly wages by around 35 percent. Third, the treatment diminishes the likelihood of working in agriculture by almost 4 percentage points (which is consistent with the findings of Escobal, 2005 and Torero et al. 2007). Fourth, women in treated areas are more likely to be self-employed than their counterparts in the control group. The estimated coefficients in Panel B suggest that an additional electrification project increases female earnings by 10 percent and reduces the probability of having two jobs by 1.3 percentage points. Our results then indicate that women in treated districts with, for instance, two projects earn 40 percent more than women in control areas. These gains in earnings appear to be very large and should have a positive impact on the gender wage gap given that male earnings do not response to the treatment nor to its intensity.

Table 10 shows the estimated impacts of electrification on youth employment (workers age 14-25). In the top half, we find no statistically significant effects except for the likelihood of having two jobs. The point estimate in column 6 suggests that treatment diminishes the probability of working in two jobs by 5.5 percentage points. The bottom half of Table 10 reveals that treatment intensity have a positive impact on earnings (10 percent) and a negative effect on the likelihood of having two jobs (1.3 percentage points).

To sum up, our panel data estimates suggest that, in the sample as a whole, the treatment increases hours of work, diminishes the likelihood of having two jobs and rises the probability of being self-employed. These results may indicate that rural electrification increases the benefits of labor specialization (i.e. it is less attractive to have more than one job) and encourages the start of new businesses (i.e. people are more likely to be self-employed). However, the estimated coefficients are modest in magnitude. When we split the sample by sex, our results show that the program have positive impacts on male's hours of work and female's earnings. Among the youth, the program reduces the likelihood of working in two jobs. Finally, the analysis of treatment intensity reveals that each additional project reinforces such impacts.

7 CONCLUDING REMARKS

Governments in developing countries devote a significant share of their resources to invest in public infrastructure. In fact, providing access to basic services such as water sanitation, electricity, and telecommunications is one of the top priorities in their policy agendas.

In this paper, we examine the labor market consequences of providing electrification to rural districts using individual-level data from Peru. To deal with the endogenous placement of the electrification program, we adopt differences-in-differences and fixed-effects strategies. We therefore assume that unobserved confounders are time-invariant at the district and individual level, respectively.

Both estimation strategies yield qualitatively similar estimates. Our ITT estimates suggest that the treatment increases hours of work and decreases the likelihood of having more than one job among males. One potential explanation for this result is that the electrification program raises the benefits of specializing and therefore makes it less attractive to have a second occupation. It seems as if electricity increases available time to work and economies of scale increase the payoff of focusing on one occupation instead of having two. Among women, we document that earnings are higher in treated areas than in control districts. Interestingly, these gains appear to be driven by a lower probability of working in agriculture. Taken together, these pieces of evidence suggest that the program reduced the gender wage gap among beneficiaries. Moreover, our measure of treatment intensity indicates that the size of these effects (for both men and women) increases as more electrification projects are concluded.

Let us recall two caveats of our empirical analysis. First, the validity of our strategies rests on the assumption that selection bias takes the form of unobserved time-invariant characteristics. Second, we fail to account for potential systematic migration that could bias our results. For instance, our estimated increase in hours of work could be driven by migration of hard-working men from control areas to treated districts.

Future work should look at the impact of electrification on educational outcomes. Access to electricity could be thought of as shock to school quality which, in turn, may affect the demand for education. Also, electrification could affect health outcomes through better equipments at health centers or less demand for in-door polluting technologies (e.g. replacing wood/candles for electrical appliances/lightbulbs). Future research on such outcomes would be important to understand the overall effect of electrification on the lives of the rural poor.

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Figure 1: Electricity coverage in rural Peru 2006-2012

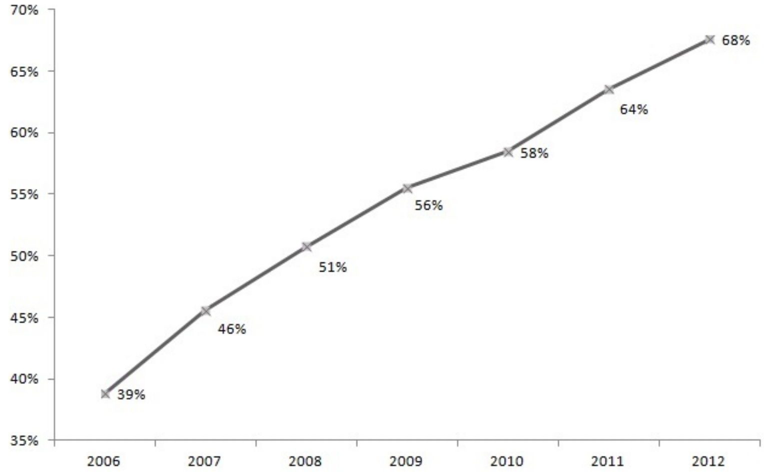

Figure 2: Timing of the Electrification Program 2006-2010

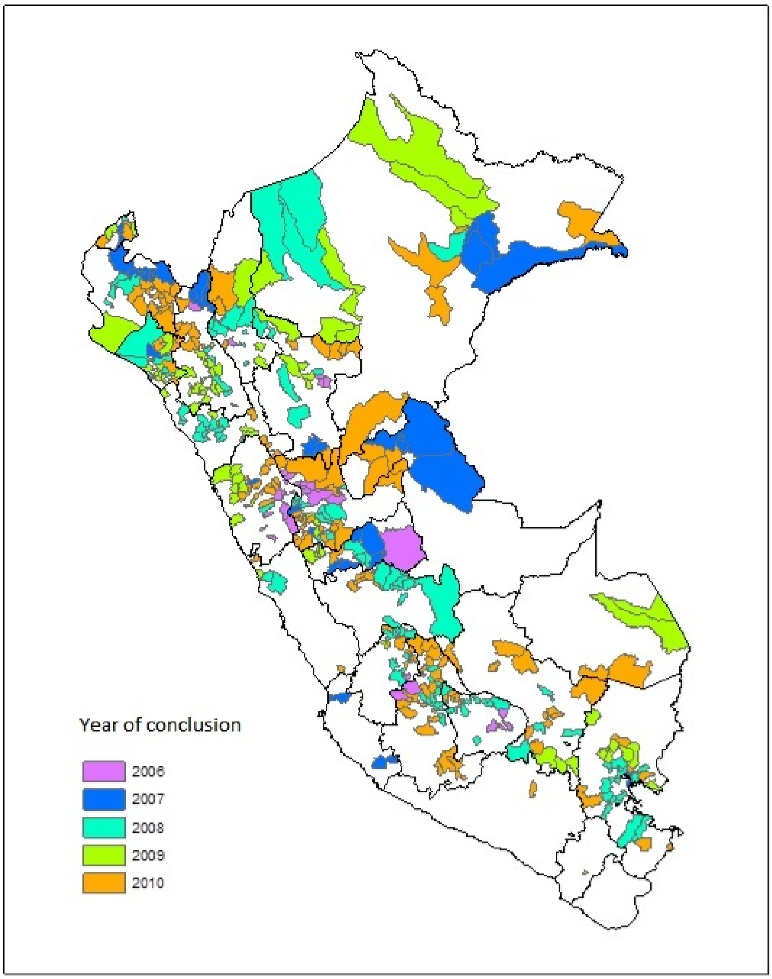

Figure 3: Treatment Intensity by year 2010

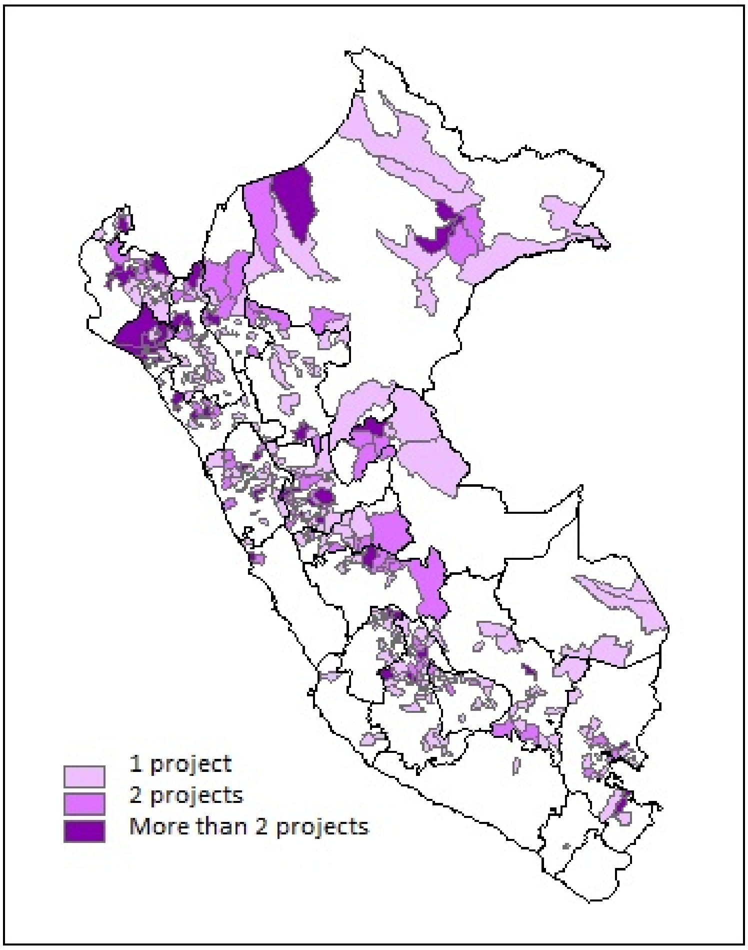

Table 1: Estimated Electricity Coverage in Rural Peru using ENAHO samples.

	2006	2007	2008	2009	2010	2011	2012
DD Sample:							
Treated areas	26%	38%	43%	52%	54%	62%	67%
Non-treated areas	45%	54%	60%	60%	63%	66%	68%
Panel Sample:							
Treated areas	-	44%	50%	58%	65%	-	-
Non-treated areas	-	53%	55%	56%	60%	-	-

Table 2: The Effects of the PER on Access to Electricity

	(1)	(2)	(3)	(4)
	DD Sample		Panel Sample	
ITT	0.0819***		0.0750***	
	(0.0169)		(0.0093)	
Intensity		0.0247***		0.0354***
		(0.0091)		(0.0034)
Observations	246,735	145,655	12,964	7,247
R-squared	0.4641	0.3946	0.0751	0.1470
Number of individuals			3,980	2,234

Note: Clustered standard errors are shown in parenthesis. Each column is a separate regression. The dependent variable is equal to 1 if the household has electricity, and 0 otherwise. All regressions control for district and year fixed effects. Columns 1 and 3 include all rural families, both control and treated households. Columns 2 and 4 only include individuals who were exposed to, at least, one electrification project in their district.

Table 3: Differences-in-Differences Estimates (DD). ITT and Intensity. Whole Sample

	(1) Participation	(2) Employed	(3) Hours	(4) Log earnings	(5) Log hourly wage	(6) Has 2 jobs	(7) Wage Earner	(8) Agriculture	(9) Self-employed
Panel A:									
ITT	-0.0057*	-0.0114***	-0.7593***	0.4278***	0.4958***	0.0294***	0.0006	-0.0023	0.0032
	(0.0033)	(0.0040)	(0.1875)	(0.0272)	(0.0250)	(0.0046)	(0.0028)	(0.0055)	(0.0032)
ITT*2007	-0.0047	-0.0102	0.1094	-0.0095	-0.0137	-0.0084	0.0111	-0.0044	-0.0070
	(0.0099)	(0.0124)	(0.5696)	(0.0760)	(0.0721)	(0.0103)	(0.0077)	(0.0128)	(0.0084)
ITT*2008	-0.0029	-0.0035	-0.2188	-0.0538	-0.0345	-0.0011	0.0085	-0.0004	-0.0081
	(0.0059)	(0.0072)	(0.3673)	(0.0505)	(0.0483)	(0.0075)	(0.0063)	(0.0089)	(0.0057)
ITT*2009	0.0032	0.0029	0.1467	0.0201	-0.0038	-0.0106	0.0056	-0.0021	-0.0046
	(0.0055)	(0.0064)	(0.3356)	(0.0435)	(0.0426)	(0.0068)	(0.0055)	(0.0089)	(0.0059)
ITT*2010	0.0073	0.0082	0.2792	-0.0129	-0.0187	-0.0149**	0.0080*	0.0025	0.0068
	(0.0050)	(0.0060)	(0.3118)	(0.0389)	(0.0384)	(0.0061)	(0.0047)	(0.0072)	(0.0047)
Observations	246,735	246,735	246,735	73,579	71,467	246,735	197,596	246,735	246,735
R-squared	0.0877	0.3928	0.2731	0.3292	0.2825	0.0852	0.1752	0.2897	0.3272
Panel B:									
Intensity	-0.0004	-0.0005	0.1711**	0.0032	-0.0130	-0.0003	-0.0002	-0.0044*	0.0002
	(0.0014)	(0.0014)	(0.0710)	(0.0135)	(0.0122)	(0.0029)	(0.0014)	(0.0023)	(0.0017)
Observations	121,541	121,541	121,541	35,430	34,334	121,541	97,653	121,541	121,541
R-squared	0.0902	0.3958	0.2670	0.3058	0.2585	0.0854	0.1578	0.2853	0.3394

Note: Standard errors clustered at the district level are shown in parenthesis. Each column is a separate regression. All dependent variables are binary except for hours, log (weekly) earnings, and hourly wages. Individuals who do not work have zero (weekly) hours of work. Earnings and wages are only computed for those with positive values of income. All regressions control for presence of children below 5 in the household, maternal language, age, education, sex, district fixed effects, and year fixed effects. Panel A includes all rural families, both control and treated households. Panel B only includes individuals who were exposed to, at least, one electrification project in their district.

Table 4: Differences-in-Differences Estimates (DD). ITT and Intensity. Only Men

	(1) Participation	(2) Employed	(3) Hours	(4) Log earnings	(5) Log hourly wage	(6) Has 2 jobs	(7) Wage Earner	(8) Agriculture	(9) Self-employed
Panel A:									
ITT	-0.0079**	-0.0165***	-1.1988***	0.4216***	0.4878***	0.0250***	-0.0039	-0.0120*	-0.0074*
	(0.0031)	(0.0044)	(0.2309)	(0.0314)	(0.0299)	(0.0061)	(0.0049)	(0.0067)	(0.0045)
ITT*2007	-0.0034	-0.0114	0.3997	-0.0019	-0.0049	-0.0199	0.0179	0.0087	0.0015
	(0.0096)	(0.0121)	(0.7042)	(0.0864)	(0.0819)	(0.0139)	(0.0134)	(0.0145)	(0.0123)
ITT*2008	-0.0022	0.0080	0.1573	-0.0329	-0.0067	0.0048	0.0186*	0.0031	0.0018
	(0.0066)	(0.0084)	(0.4872)	(0.0572)	(0.0561)	(0.0098)	(0.0109)	(0.0110)	(0.0079)
ITT*2009	-0.0015	0.0051	0.7668*	0.0185	-0.0178	-0.0101	0.0095	-0.0014	-0.0065
	(0.0056)	(0.0074)	(0.4477)	(0.0503)	(0.0497)	(0.0090)	(0.0092)	(0.0103)	(0.0078)
ITT*2010	0.0051	0.0099	0.4679	0.0015	-0.0091	-0.0208**	0.0167**	0.0138	0.0152**
	(0.0052)	(0.0070)	(0.4041)	(0.0458)	(0.0453)	(0.0081)	(0.0083)	(0.0090)	(0.0072)
Observations	125,098	125,098	125,098	49,346	47,721	125,098	92,766	125,098	125,098
R-squared	0.0649	0.4907	0.3307	0.3340	0.3027	0.0966	0.2196	0.3308	0.4619
Panel B:									
Intensity	-0.0017*	-0.0033***	0.1013	-0.0041	-0.0113	-0.0025	0.0001	-0.0025	-0.0025*
	(0.0009)	(0.0012)	(0.0685)	(0.0126)	(0.0103)	(0.0035)	(0.0018)	(0.0030)	(0.0014)
Observations	61,883	61,883	61,883	23,959	23,091	61,883	46,032	61,883	61,883
R-squared	0.0658	0.4927	0.3249	0.3022	0.2744	0.0978	0.2004	0.3298	0.4756

Note: Standard errors clustered at the district level are shown in parenthesis. Each column is a separate regression. All dependent variables are binary except for hours, log (weekly) earnings, and hourly wages. Individuals who do not work have zero (weekly) hours of work. Earnings and wages are only computed for those with positive values of income. All regressions control for presence of children below 5 in the household, individuals' maternal language, age, and education, and district and year fixed effects. In this table, we restrict the sample to men. Panel A includes all rural families, both control and treated households. Panel B only includes individuals who were exposed to, at least, one electrification project in their district.

Table 5: Differences-in-Differences Estimates (DD). ITT and Intensity. Only Women

	(1) Participation	(2) Employed	(3) Hours	(4) Log earnings	(5) Log hourly wage	(6) Has 2 jobs	(7) Wage Earner	(8) Agriculture	(9) Self-employed
Panel A:									
ITT	-0.0028	-0.0058	-0.2862	0.4310***	0.5004***	0.0344***	0.0041	0.0087	0.0161***
	(0.0052)	(0.0059)	(0.2410)	(0.0436)	(0.0423)	(0.0048)	(0.0029)	(0.0063)	(0.0049)
ITT*2007	-0.0041	-0.0054	-0.0215	0.0080	0.0027	0.0039	0.0113	-0.0165	-0.0137
	(0.0143)	(0.0177)	(0.6386)	(0.1399)	(0.1302)	(0.0111)	(0.0080)	(0.0165)	(0.0132)
ITT*2008	-0.0011	-0.0119	-0.4313	-0.0876	-0.0865	-0.0066	0.0039	-0.0023	-0.0169**
	(0.0094)	(0.0108)	(0.4645)	(0.0855)	(0.0802)	(0.0085)	(0.0061)	(0.0109)	(0.0083)
ITT*2009	0.0092	0.0024	-0.4277	0.0187	0.0176	-0.0107	0.0057	-0.0014	-0.0024
	(0.0088)	(0.0095)	(0.4077)	(0.0756)	(0.0733)	(0.0080)	(0.0060)	(0.0112)	(0.0086)
ITT*2010	0.0101	0.0074	0.1362	-0.0386	-0.0516	-0.0086	0.0027	-0.0081	-0.0004
	(0.0073)	(0.0084)	(0.3681)	(0.0647)	(0.0652)	(0.0070)	(0.0049)	(0.0085)	(0.0071)
Observations	121,637	121,637	121,637	24,233	23,746	121,637	104,830	121,637	121,637
R-squared	0.1078	0.3098	0.2158	0.3193	0.2729	0.0715	0.1240	0.2630	0.1779
Panel B:									
Intensity	0.0008	0.0021	0.2345**	0.0137	-0.0162	0.0020	-0.0006	-0.0064***	0.0030
	(0.0022)	(0.0023)	(0.0995)	(0.0173)	(0.0207)	(0.0025)	(0.0014)	(0.0020)	(0.0026)
Observations	59,658	59,658	59,658	11,471	11,243	59,658	51,621	59,658	59,658
R-squared	0.1067	0.3115	0.2073	0.2893	0.2360	0.0649	0.1055	0.2530	0.1812

Note: Standard errors clustered at the district level are shown in parenthesis. Each column is a separate regression. All dependent variables are binary except for hours, log (weekly) earnings, and hourly wages. Individuals who do not work have zero (weekly) hours of work. Earnings and wages are only computed for those with positive values of income. All regressions control for presence of children below 5 in the household, individuals' maternal language, age, and education, and district and year fixed effects. In this table, we restrict the sample to women. Panel A includes all rural families, both control and treated households. Panel B only includes individuals who were exposed to, at least, one electrification project in their district.

Table 6: Differences-in-Differences Estimates (DD). ITT and Intensity. Only Youth

	(1) Participation	(2) Employed	(3) Hours	(4) Log earnings	(5) Log hourly wage	(6) Has 2 jobs	(7) Wage Earner	(8) Agriculture	(9) Self-employed
Panel A:									
ITT	-0.0118	0.0008	0.3301	0.5614***	0.5725***	0.0271***	0.0331***	0.0022	0.0040
	(0.0092)	(0.0096)	(0.3802)	(0.0544)	(0.0422)	(0.0074)	(0.0065)	(0.0101)	(0.0050)
ITT*2007	-0.0007	-0.0081	1.3732	0.0772	0.0128	0.0082	0.0319*	-0.0231	-0.0123
	(0.0299)	(0.0327)	(1.2273)	(0.1296)	(0.1129)	(0.0195)	(0.0177)	(0.0282)	(0.0140)
ITT*2008	-0.0144	-0.0235	-0.4709	0.0702	0.0223	0.0067	0.0113	-0.0395**	-0.0062
	(0.0189)	(0.0196)	(0.7765)	(0.1042)	(0.0931)	(0.0126)	(0.0141)	(0.0198)	(0.0097)
ITT*2009	0.0065	0.0004	-0.6801	-0.0650	-0.0269	0.0156	-0.0054	-0.0204	0.0034
	(0.0169)	(0.0172)	(0.6913)	(0.0977)	(0.0724)	(0.0115)	(0.0127)	(0.0189)	(0.0093)
ITT*2010	-0.0027	-0.0030	-0.3031	-0.0954	-0.0963	-0.0095	-0.0085	0.0049	0.0092
	(0.0151)	(0.0158)	(0.6260)	(0.0842)	(0.0684)	(0.0107)	(0.0120)	(0.0152)	(0.0081)
Observations	53,947	53,947	53,947	11,174	11,069	53,947	50,407	53,947	53,947
R-squared	0.1386	0.1415	0.1397	0.3042	0.3301	0.0737	0.0831	0.1870	0.1080
Panel B:									
Intensity	-0.0034	-0.0015	0.0121	0.0122	-0.0034	0.0038	-0.0010	-0.0005	0.0002
	(0.0025)	(0.0027)	(0.1212)	(0.0187)	(0.0117)	(0.0031)	(0.0031)	(0.0037)	(0.0015)
Observations	27,453	27,453	27,453	5,853	5,799	27,453	25,632	27,453	27,453
R-squared	0.1305	0.1348	0.1300	0.2733	0.2981	0.0584	0.0768	0.1538	0.1020

Note: Standard errors clustered at the district level are shown in parenthesis. Each column is a separate regression. All dependent variables are binary except for hours, log (weekly) earnings, and hourly wages. Individuals who do not work have zero (weekly) hours of work. Earnings and wages are only computed for those with positive values of income. All regressions control for presence of children below 5 in the household, individuals' maternal language, age, and education, and district and year fixed effects. In this table, we restrict the sample to individuals between 14 and 25 years old. Panel A includes all rural families, both control and treated households. Panel B only includes individuals who were exposed to, at least, one electrification project in their district.

Table 7: Panel Data Estimates (FE). ITT and Intensity. Whole Sample

	(1) Participation	(2) Employed	(3) Hours	(4) Log earnings	(5) Log hourly wage	(6) Has 2 jobs	(7) Wage Earner	(8) Agriculture	(9) Self-employed
Panel A:									
ITT	0.0088	0.0184	1.7000***	0.0550	0.0466	-0.0303**	-0.0030	0.0067	0.0311***
	(0.0109)	(0.0113)	(0.6154)	(0.0671)	(0.0715)	(0.0128)	(0.0108)	(0.0136)	(0.0109)
Observations	12,964	12,964	12,964	5,348	5,184	12,964	12,964	12,964	12,964
R-squared	0.0002	0.0004	0.0020	0.0241	0.0370	0.0029	0.0017	0.0015	0.0010
Individuals	3,980	3,980	3,980	2,291	2,277	3,980	3,980	3,980	3,980
Panel B:									
Intensity	0.0020	0.0044	0.5100**	0.0223	-0.0030	-0.0188***	-0.0028	-0.0004	0.0028
	(0.0037)	(0.0039)	(0.2108)	(0.0365)	(0.0356)	(0.0045)	(0.0034)	(0.0046)	(0.0039)
Observations	7,246	7,246	7,246	2,996	2,901	7,246	7,246	7,246	7,246
R-squared	0.0001	0.0007	0.0021	0.0336	0.0438	0.0064	0.0012	0.0029	0.0013
Individuals	2,234	2,234	2,234	1,302	1,296	2,234	2,234	2,234	2,234

Note: Standard errors clustered at the individual level are shown in parenthesis. Each column is a separate regression. All dependent variables are binary except for hours, log (weekly) earnings, and hourly wages. Individuals who do not work have zero (weekly) hours of work. Earnings and wages are only computed for those with positive values of income. All regressions control for year fixed effects. Panel A includes all rural families, both control and treated households. Panel B only includes individuals who were exposed to, at least, one electrification project in their district.

Table 8: Panel Data Estimates (FE). ITT and Intensity. Only Men

	(1) Participation	(2) Employed	(3) Hours	(4) Log earnings	(5) Log hourly wage	(6) Has 2 jobs	(7) Wage Earner	(8) Agriculture	(9) Self-employed
Panel A:									
ITT	-0.0072	0.0053	2.6145***	-0.0754	-0.0897	-0.0591***	-0.0197	0.0516***	0.0173
	(0.0126)	(0.0132)	(0.8854)	(0.0738)	(0.0809)	(0.0196)	(0.0173)	(0.0185)	(0.0142)
Observations	6,668	6,668	6,668	3,572	3,438	6,668	6,668	6,668	6,668
R-squared	0.0007	0.0018	0.0036	0.0229	0.0353	0.0043	0.0023	0.0046	0.0006
Individuals	2,052	2,052	2,052	1,410	1,402	2,052	2,052	2,052	2,052
Panel B:									
Intensity	0.0049	0.0064	1.0979***	-0.0504	-0.0804*	-0.0242***	-0.0025	0.0028	-0.0015
	(0.0048)	(0.0049)	(0.2905)	(0.0408)	(0.0420)	(0.0068)	(0.0053)	(0.0064)	(0.0047)
Observations	3,762	3,762	3,762	2,034	1,956	3,762	3,762	3,762	3,762
R-squared	0.0024	0.0043	0.0071	0.0248	0.0357	0.0061	0.0009	0.0038	0.0012
Individuals	1,156	1,156	1,156	815	812	1,156	1,156	1,156	1,156

Note: Standard errors clustered at the individual level are shown in parenthesis. Each column is a separate regression. All dependent variables are binary except for hours, log (weekly) earnings, and hourly wages. Individuals who do not work have zero (weekly) hours of work. Earnings and wages are only computed for those with positive values of income. All regressions control for year fixed effects. In this table, we restrict the sample to only men. Panel A includes all rural families, both control and treated households. Panel B only includes individuals who were exposed to, at least, one electrification project in their district.

Table 9: Panel Data Estimates (FE). ITT and Intensity. Only Women

	(1) Participation	(2) Employed	(3) Hours	(4) Log earnings	(5) Log hourly wage	(6) Has 2 jobs	(7) Wage Earner	(8) Agriculture	(9) Self-employed
Panel A:									
ITT	0.0282	0.0358*	0.8686	0.3595**	0.3582**	0.0014	0.0143	-0.0385*	0.0459***
	(0.0179)	(0.0184)	(0.8504)	(0.1422)	(0.1433)	(0.0161)	(0.0127)	(0.0198)	(0.0167)
Observations	6,296	6,296	6,296	1,776	1,746	6,296	6,296	6,296	6,296
R-squared	0.0016	0.0014	0.0012	0.0363	0.0513	0.0035	0.0018	0.0017	0.0024
Individuals	1,938	1,938	1,938	883	876	1,938	1,938	1,938	1,938
Panel B:									
Intensity	-0.0007	0.0027	-0.0627	0.0996*	0.0726	-0.0135**	-0.0033	-0.0032	0.0072
	(0.0056)	(0.0062)	(0.2628)	(0.0577)	(0.0478)	(0.0061)	(0.0042)	(0.0067)	(0.0060)
Observations	3,484	3,484	3,484	962	945	3,484	3,484	3,484	3,484
R-squared	0.0007	0.0006	0.0007	0.0705	0.0821	0.0098	0.0028	0.0025	0.0043
Individuals	1,086	1,086	1,086	489	485	1,086	1,086	1,086	1,086

Note: Standard errors clustered at the individual level are shown in parenthesis. Each column is a separate regression. All dependent variables are binary except for hours, log (weekly) earnings, and hourly wages. Individuals who do not work have zero (weekly) hours of work. Earnings and wages are only computed for those with positive values of income. All regressions control for year fixed effects. In this table, we restrict the sample to only women. Panel A includes all rural families, both control and treated households. Panel B only includes individuals who were exposed to, at least, one electrification project in their district.

Table 10: Panel Data Estimates (FE). ITT and Intensity. Only Youth

	(1) Participation	(2) Employed	(3) Hours	(4) Log earnings	(5) Log hourly wage	(6) Has 2 jobs	(7) Wage Earner	(8) Agriculture	(9) Self-employed
Panel A:									
ITT	-0.0371	-0.0276	1.7417	0.0629	0.1601	-0.0555***	0.0195	0.0376	-0.0047
	(0.0268)	(0.0274)	(1.2081)	(0.2377)	(0.2421)	(0.0211)	(0.0217)	(0.0279)	(0.0153)
Observations	4,257	4,257	4,257	930	923	4,257	4,257	4,257	4,257
R-squared	0.0048	0.0048	0.0073	0.0770	0.1124	0.0091	0.0146	0.0145	0.0087
Individuals	1,666	1,666	1,666	616	613	1,666	1,666	1,666	1,666
Panel B:									
Intensity	-0.0007	0.0027	-0.0627	0.0996*	0.0726	-0.0135**	-0.0033	-0.0032	0.0072
	(0.0056)	(0.0062)	(0.2628)	(0.0577)	(0.0478)	(0.0061)	(0.0042)	(0.0067)	(0.0060)
Observations	3,484	3,484	3,484	962	945	3,484	3,484	3,484	3,484
R-squared	0.0007	0.0006	0.0007	0.0705	0.0821	0.0098	0.0028	0.0025	0.0043
Individuals	1,086	1,086	1,086	489	485	1,086	1,086	1,086	1,086

Note: Standard errors clustered at the individual level are shown in parenthesis. Each column is a separate regression. All dependent variables are binary except for hours, log (weekly) earnings, and hourly wages. Individuals who do not work have zero (weekly) hours of work. Earnings and wages are only computed for those with positive values of income. All regressions control for year fixed effects. In this table, we restrict the sample to individuals between 14 and 25 years old. Panel A includes all rural families, both control and treated households. Panel B only includes individuals who were exposed to, at least, one electrification project in their district.

Appendix

- Abbreviations:
  - PER: Rural Electrification Program
  - ENAHO: Encuesta Nacional de Hogares (Peruvian National Household Survey)
  - DD: Differences-in-Differences
  - FE:Fixed Effects
  - ITT: Intent to Treat
- Dependent variables:
  - Participation: equal to 1 if the individual is in the labor force, and equal to 0 otherwise
  - Employed: equal to 1 if the individual is in employed, and equal to 0 otherwise
  - Hours of work: Equal to the hours of work in the week previous to the interview. It is zero for those who do not work.
  - Log Earnings: weekly earnings are computed only for those who reported positive values of income (dependent and independent workers). In-kind payments are ignored.
  - Log hourly wages: only computed for those who reported positive values of income and non-zero hours of work.
  - Has 2 jobs: equal to 1 if the individual has two jobs, and equal to 0 otherwise
  - Wage-Earner: equal to 1 if the individual is a wage-earner , and equal to 0 otherwise
  - Agriculture: equal to 1 if the individual works in agriculture and equal to 0 otherwise
  - Self-employed: equal to 1 if the individual is self-employed, and equal to 0 otherwise
- Control variables:
  - Children: equal to 1 if there are children below 5 in the household, and 0 otherwise
  - Maternal language: equal to 1 if the individual speaks Quechua, and 0 otherwise
  - Sex: equal to 1 if the individual is male, and 0 otherwise
  - Age: age of the individual at the year of interview (measured in years)
  - Education level dummies (no education, incomplete primary, complete primary, incomplete secondary, and so on
  - District and year fixed effects