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MARCO CAPRARO BRANCHER

SHEDDING LIGHT INTO RURAL ELECTRIFICATION AND HEALTH:
EVIDENCE FROM BRAZIL

São Paulo
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Shedding Light into Rural Electrification and Health: Evidence from Brazil

Dissertação apresentada à Escola de Economia de São Paulo da Fundação Getulio Vargas como requisito para obtenção do título de Mestre em Economia de Empresas

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Orientador: Prof. Dr. André Portela Fernandes de Souza

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To my parents Paola and Luigi, my life partner Martina and my brothers and sister Nicolas, Enrico and Giulia.

Health is the obvious starting point
for an inquiry into wellbeing.

Angus Deaton

Rural electrification can have structural
impacts on rural communities.

Douglas Barnes

Watching a coast as it slips by the ship is
like thinking about an enigma. There it is
before you, smiling, frowning, inviting,
grand, mean, insipid, or savage, and
always mute with an air of whispering,
"Come and find out".

Joseph Conrad

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ABSTRACT

This paper investigates the impacts of electricity on health outcomes in Brazilian rural areas. An instrumental variable difference-in-differences approach was addopted, exploring a federal electrification program as source of exogenous variation in the probability of beign connected to the grid. Additionally, the municipality land gradient was also explored as source of exogenous variation, since the electrification program eligibility rule was not sharp. The main findings are in line with the international literature. Electrification reduces the incidence and mortality rate of some respiratory diseases with relevant gender-heterogeneity. Moreover, it has negative impacts on cancer mortality rate. A result that was not expected is that the electrification did not had any effect on intestinal infections.

Key-words: Electrification, Health, Development.

RESUMO

Este artigo investiga os impactos da eletrificação em indicadores de saúde em áreas rurais brasileiras. A estratégia de identificação se baseia em uma abordagem baseada no uso de variáveis instrumentais e um modelo de diferenças-em-diferenças. Foi, portanto, explorado um programa federal de eletrificação como fonte de variação exógena na probabilidade do domicílio estar conectado à rede elétrica. Além disso, a declividade média do solo também foi explorada como fonte de variação exógena, uma vez que a regra de elegibilidade do programa de eletrificação não foi o único critério adotado para a seleção dos domicílios a serem conectados. As principais conclusões do estudo estão em linha com a literatura internacional. A eletrificação reduz a incidência e a taxa de mortalidade de algumas doenças respiratórias, com significativa heterogeidade de gênero. Além disso, foram encontrados impactos negativos na taxa de mortalidade causada por câncer. Por fim, um resultado inesperado foi a ausência de impactos sobre infecções intestinais.

Palavras-chaves: Eletrificação, Saúde, Desenvolvimento.

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1 Introduction

Although access to electricity is not a sufficient condition for the promotion of economic development, it is certainly a necessary condition. It is thus alarming that, as of 2017, more than a billion people around the round still did not have access to electricity.[1] Most of the population not yet connected to the electricity grid lives in rural areas, where providing electricity is a major challenge, given the low population density and low average income and consumption, factors that lead to economic and financial unfeasibility of electrification projects. Despite the challenges, the need to promote access to electricity for these populations is becoming clearer, in view of the empirical evidence found in several studies.

Access to electricity provides improvements in the quality of life of affected populations. Today, electricity is the most efficient form of illumination. In addition, as the international literature points out, it can increase the number of hours worked, as well as the hours dedicated to study. Through electricity, using appliances that promote new possibilities of communication, entertainment, and heating becomes possible.[2] Electricity facilitates access to water for irrigation and food, allowing food refrigeration, generating positive impacts on health indicators, and possibly increasing productivity in agricultural production. However, despite the general benefits, some studies have also found indirect negative impacts of electrification in specific contexts, usually in the form of second-order effects.[3]

With the emergence of electrification programs around the world, the economic literature, especially the economic-development literature, has begun to study the impacts of access to electricity in several aspects of human development. Most of the studies analyze the impacts of electrification on the labor market or education,[4] as well as a few studies focusing on the effects on health.

Although some studies have found negative impacts, the empirical evidence generally comprises positive impacts. However, the quality of this evidence requires emphasis. As put by Ravallion (2007), many studies have methodological problems that imply the lack of a distinction between correlation and causality. Hence, some recent studies have adopted more robust econometric strategies and procedures to identify the real causal impacts of electrification.[5]

Specifically, few studies exist on electrification impacts in Brazil. Following the internati-

[1] See IEA (2017).

[2] See Arraiz e Calero (2015).

[3] Kloos et al. (2012), for example, has studied a community that, after the electrification process, started pumping water from a stream whose water was unfit for consumption.

[4] See Barnes e Foley (2004), Modi et al. (2005), Khandker, Barnes e Samad (2013), Dinkelman (2011), Bensch, Kluve e Peters (2011) , Khandker, Barnes e Samad (2012), Khandker et al. (2012), Arraiz e Calero (2015), Peters, Vance e Harsdorff (2011), Salmon e Tanguy (2016)

[5] Some of these studies are Khandker, Barnes e Samad (2013), Chakravorty, Pelli e Marchand (2014), Grogan (2016) and Dinkelman (2011).

onal trend, the most relevant studies in the area emphasize the relation of access to electricity with increases in productivity[6] and time devoted to studies.[7] In relation to health, apart from specialized public health research,[8] few econometric studies contribute in an empirical way to the existing literature.

Although the literature on economic development and impact evaluation has advanced in the identification of electrification effects, some issues are important to mention. Little is known about the causal mechanisms that explain such relationships, since the available data usually do not allow certainty about the explanatory dynamics and how they occur, even for the more study-saturated areas. For example, in the case of impacts on education,[9] the literature usually points to a positive impact of electrification on the number of hours of study per child. However, in cases where the arrival of electricity causes an increase in the number of households with television, the final effect on study hours is not clear. One direction may lead to a positive direct impact of electrification on hours of study due to an increase in nighttime luminosity; but the indirect impact of electrification on study hours by the time spent watching television is negative. In addition, access to new technologies, such as television and computers, can improve student performance, despite reductions in study time.[10] This is a relevant problem, as understanding the effects and transmission channels becomes imperative for a real understanding of electrification program effects.

Therefore, based on the lack of studies on electrification impacts on health and the lack of causal evidence on this topic, this paper attempts to bring new evidence to this literature by assessing the impacts of access to electric power in Brazilian households on health outcomes. The aim is to bring new evidence to the literature on rural electrification impacts on health, using the Brazilian case to provide a general overview of this causal relation. Therefore, specific analyses for each finding should not be expected. That purpose would require a future study comprising extended and combined quantitative-qualitative research.

The Brazilian case is of special interest for two major reasons. The first concerns the number of households without access to electricity. Data from the 2000 census shows that more than 10 million people did not have electricity at that time, when more than 80% of the rural population in Brazil was not connected to the electric power grid. The second reason relates to the rapid grid growth in the country and the connection expansion that followed. In fact, data from the 2010 census shows that the number of people not connected had dramatically dropped to 2.8 million.

This improvement was due to a federal program known as *"Luz Para Todos"* (hereafter LpT) which aimed to bring electric power to all to all unconnected municipalities. As detailed

[6] See Lipscomb, Mobarak e Barham (2013).
[7] See Portela, Carraro e Ribeiro (2017) and Grogan (2016).
[8] See Kloos et al. (2012) and Diette et al. (2012).
[9] See Arraiz e Calero (2015).
[10] See Walle et al. (2013).

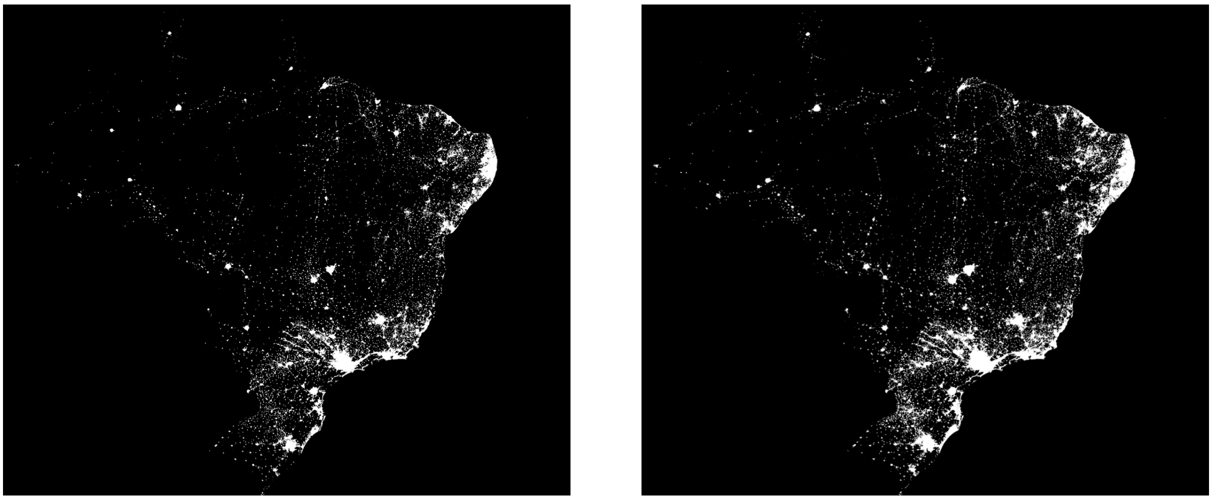

Figure 1 – Nighttime Lights in 2000 (left) and 2010 (right)
Source: NOOA/NCEI

below, this program provides an exogenous shock to household access to electricity, enabling exploration of the program design to avoid potential endogeneity relations when evaluating the impacts of rural electrification on health outcomes. The connection to the grid expansion appears in Figure 1, representing the average nighttime light emission in 2000 and 2010 in Brazil (pictures from the U.S. National Oceanic and Atmospheric Administration / National Centers for Environmental Information - NOOA/NCEI).

Following a combination of identification strategies in the work of Dinkelman (2011), Rachter (2014) and Portela, Carraro e Ribeiro (2017), this study therefore overcomes the endogeneity problem by exploring the eligibility rules for the LpT program and a grid expansion cost-related variable that creates an instrumental variable for electricity access. Combining the instrumental variable with municipality panel data for 2000 and 2010 from the Brazilian demographic census enables estimation of the causal effect of access to electricity on fertility rates, share of underweight births, mortality and hospitalization rates by disease groups, age, and gender.

The empirical evidence resulting from this study indicates that rural electrification has some important causal impacts on heatlh outcomes. Being eligible increases by almost 25 percentage points the probability of being connected in 2010. Among the complier municipalities, for each average land gradient degree, the probability of being connected decreases by 0.9 percentage points. One land gradient standard deviation is equal to 2.8 degrees, so the effect magnitude is relevant as the increase in one land gradient standard deviation reduces the access to electricity probability by 2.5 percentage points.

The results for fertility rate and birth weight share point to the direction of no electrification impact evidence among compliers. The mortality rate analyses indicate that the electrification cause a reduction on the following diseases mortality: neoplasms, endocrine, nutritional and

metabolic diseases and, as expected, a reduction in mortality rates due to respiratory diseases (as emphysema). According to the usual findings in the literature, some impact was also expected on intestinal infections, especially for children. However, no evidence of such effect was found. For hospitalization rates, no evidence of impact for respiratory system diseases was found. Restricting the attention to a group of intestinal infections, weak evidence of an increase in hospitalization rates was found for female children of less than one. Similar to the finding on mortality rates, a reduction was also found in the admission of neoplasm adult patients, with some gender-effect heterogeneity. Moreover, there is strong evidence suggesting that the electrification increases the number of Rotavirus vaccine doses applied. This is an important way to prevent severe diarrhea in children, a common cause of death in developing countries.

Following this introduction, the present study is organized into eight sections. Section 2 describes the main studies on the role of access to electricity in developing countries. Section 3 introduces the LpT program and its eligibility rules. Section 4 presents the study's data sources. Section 5 describes the identification strategy adopted. Section 6 presents the results. Section 7 describes robustness check. Finally, Section 8 concludes.

2 Related Literature

This section systematizes the main studies on electrification impacts, giving priority to studies which use econometric methods for impact evaluation. Therefore, the following literature organization is based first on study area and second on the econometric approach used to overcome the endogeneity problem. The vast majority of these studies evaluate the impacts of electrification organized around four major themes: economic activity, labor market, education, and health.

Among the papers that evaluate the impacts on economic activity are works on industrialization,[1] the creation of new companies and turnover variation,[2] productivity increases,[3] and the decision between consumption and investment[4]. The articles that identify the impacts of electrification on the labor market are generally focused on impacts on labor supply,[5] intrafamily time allocation,[6] and income.[7] Papers on the impact on hours devoted to study,[8] increase in school performance,[9] and increase in years of schooling,[10] and enrollment rate[11]schooling address the relationship between electrification and education. Finally, in the health field, the identification of two impacts divides the studies into two groups, the impact of electrification on respiratory diseases,[12] and the impact on fertility rates.[13]

While the first impact links to replacing lighting generated by the combustion of fossil fuels with electric lighting, as well as the replacement of traditional ovens by modern ones with lower harmful-gas emission, the second relates to a change in the allocation of intrafamily time, greater awareness, and access to information. However, the literature does not completely support these impacts as causes. Studies that find evidence supporting them exist [14] alongside others unable to state that such causal relationships actually exist. [15]

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1 See Kassem (2017) and Rud (2012)
2 See Kassem (2017)
3 See Peters, Vance e Harsdorff (2011) Akpan, Essien e Isihak (2013) and Asaduzzaman, Barnes e Khandker (2009)
4 See Rud (2012)
5 See Dinkelman (2011) Lipscomb, Mobarak e Barham (2013), Grogan e Sadanand (2013), Salmon e Tanguy (2016) and Chakravorty, Emerick e Ravago (2016)
6 See Arraiz e Calero (2015), Portela, Carraro e Ribeiro (2017) and Asaduzzaman, Barnes e Khandker (2009)
7 See Asaduzzaman, Barnes e Khandker (2009), Bensch, Kluve e Peters (2011), Walle et al. (2013),Grogan e Sadanand (2013), Lipscomb, Mobarak e Barham (2013) and Chakravorty, Pelli e Marchand (2014)
8 See Arraiz e Calero (2015), Asaduzzaman, Barnes e Khandker (2009), Bensch, Kluve e Peters (2011), Bensch, Kluve e Peters (2013)
9 See Asaduzzaman, Barnes e Khandker (2009), Barron e Torero (2014) , Khandker, Barnes e Samad (2012), Walle et al. (2013)), Aguirre (2017)
10 See Grogan (2016), Khandker, Barnes e Samad (2012)
11 See Asaduzzaman, Barnes e Khandker (2009), Dasso, Fernandez e Nopo (2014)
12 See Accinelli e López (2015), Arraiz e Calero (2015), Baron (2014), Barron e Torero (2014) and Barron e Torero (2017)
13 See Arraiz e Calero (2015), Grogan (2016), IEG (2008), Barron e Torero (2014), Peters e Vance (2011), Cornwell e Robinson (1988) , Greenwood, Seshadri e Yorukoglu (2005) and Gonzalez e Rossi (2006)
14 For an example see Baron (2014), Barron e Torero (2014) and Barron e Torero (2017).
15 See Arraiz e Calero (2015).

In the first group, Accinelli e López (2015) estimate the probability of getting a cough in rural Peru and conclude that connection to electricity was a protective factor against a cough. With no external validity and no causal impacts found, their study is evidence of the effects that electrification may have on respiratory diseases. Still, in a study of rural areas in Peru, Arraiz e Calero (2015) use a propensity score matching approach to evaluate the impacts of access to electricity via solar panels. Although finding impacts on intrahousehold time allocation and education, they do not find any evidence of impact on incidence of respiratory diseases.

Based on a randomized controlled trial in El Salvador, Baron (2014), Barron e Torero (2014) and Barron e Torero (2017) find empirical evidence that household electrification reduces indoor air pollution, causing a reduction in the incidence of respiratory diseases in children. Moreover, Baron (2014) presents evidence that two years after baseline, the fine particulate matter concentration was on average 63% lower in electrified households than in the control group. As a result, the incidence of respiratory infections among children fell by almost 40%. The effects found are strong and steady over time, different from the impacts found in the cookstove literature.[16] Significantly, the mechanism behind the reduction in air indoor pollutants in Baron (2014) is the substitution of kerosene as source of lightning to electric powered lighting, with no evidence of changes in cooking practices. Still on the impact of air quality, Jayachandran (2009) analyzes the effects of massive wildfires in Indonesia on child mortality and finds strong evidence supporting the hypothesis that prenatal exposure to smoke increases child mortality.

As already suggested regarding electrification impacts on fertility rates, Arraiz e Calero (2015) use a propensity score matching approach and find no evidence supporting the fertility-reduction hypothesis. Among the studies that find evidence of fertility reduction, a World Bank study of Peru, Ghana, Laos, and the Philippines identifies a negative correlation between fertility and access to electricity, indicating that access to new technologies could explain such a result (IEG (2008)). Grogan (2016) investigates how household electrification in Colombia affected fertility and finds a negative impact. Interestingly, Peters e Vance (2011) find a positive impact of electrification on fertility for urban households, while the effect on rural households has the opposite sign. According to the authors, the impacts of electricity on child-care costs and information provision cause this heterogeneity.

Although these are not studies directly related to electrification, Ferrara, Chong e Duryea (2012) and Jensen e Oster (2009) show how the presence of television reduces the fertility rate in Brazil and India respectively, through changes in intrafamily time allocation. On the other hand, Peters e Vance (2011) find impacts with distinct signs for rural and urban households. While in rural households the presence of televisions reduces the fertility rate (the result in line with those those presented by the international literature), in the case of urban households the impact is positive. Cornwell e Robinson (1988) find that in the United States electrification is associated with reduced fertility in poorer states, but the impact has the opposite sign in richer states. In

[16] See Hanna, Duflo e Greenstone (2016).

another study of the United States, Greenwood, Seshadri e Yorukoglu (2005) explain the "baby boom" as a response to the greater intensity of household electricity use. As discussed by Grogan (2016), few studies attempt to identify causal relationships between fertility and electrification in developing countries.

One of the few studies focused on health and based on a randomized controlled trial carried out in Argentina, Gonzalez e Rossi (2006) identifiy high-quality electrification processes as the cause of reduction in the birth of underweight children and in the mortality rate due to food poisoning of children up to five years old. The adoption of refrigerators in households could drive such impacts (i.e., better quality facilities would lead to fewer power outages, implying fewer occurrences of ingesting spoiled foods), but no empirical evidence supports the authors' arguments on this driver.

As indicated by Aevarsdottir, Barton e Bold (2017), although the variables of interest for economic development are being studied, the channels through which these impacts occur are multiple and affect a number of intermediate variables. In fact, some studies show the importance of complementary conditions to yielding the full benefits promoted by electrification. Some of the studies that identify these conditions are: Dinkelman (2011) and Grogan e Sadanand (2013) for women's employment, Khandker, Barnes e Samad (2012) and Khandker, Barnes e Samad (2013) for education improvements, and Barron e Torero (2014) for health.

In relation to the econometric approach to overcoming the endogeneity problem, the literature suggests different techniques. Some papers follow *randomized controlled trials*, while others use natural-experiment methods such as *propensity score matching*, *difference-in-difference* and *instrumental variables*. Some of the following studies may or may not be directly linked to the electrification impact literature, but provide important econometric approaches to a similar problem.

In a randomized controlled trial medical study on indoor air pollution and respiratory health in Guatemala, Bruce et al. (1998) find that the incidence of cough and other respiratory diseases was significantly higher among women using woodstoves. As already mentioned, Gonzalez e Rossi (2006) conduct a randomized controlled trial in Argentina and find a causal relation between high-quality electrification and reduction in births of underweight children and the mortality rate. Barron e Torero (2014), Baron (2014) and Barron e Torero (2017) explore household electrification random variation in El Salvador and conclude that a causal relation exists between rural electrification and reduction of respiratory diseases and increased educational investment. A study in Ethiopia by Bernard e Torero (2015) reports that GPS information combined with randomly distributed discount vouchers for electricity connection show a large neighbors effect on the probability of household connection. Aevarsdottir, Barton e Bold (2017) provide experimental evidence of the positive impact of electrification on labor supply and income. Moreover, evidence shows that electrification improves indoor air quality, thus reducing the incidence of respiratory diseases. Using randomized controlled trials in Kenya,

Lee, Miguel e Wolfram (2018) explore random variation in the number of connections to the grid and administrative energy-cost data to provide evidence that the consumer surplus of electrified households is less than usually expected. In addition, they do not find significant impacts on educational, economic, or health outcomes.

Khandker, Barnes e Samad (2012) and Khandker, Barnes e Samad (2013) analyze the impacts of rural electrification in Bangladesh and Vietnam, respectively, finding positive impacts on income and education. To circumvent endogeneity problems, the authors use different identification strategies, such as propensity score matching, difference-in-difference, and instrumental variables. In the case of Bangladesh, access to electricity has led to an income increase of more than 12% in households and a positive impact on schooling years and children's study time. In Vietnam, the increase in income is even higher at 28%, while the impacts on education are similar to those to those found by the previous study.

Following the recent trend of using econometric techniques to solve the endogeneity problem, Jalan e Ravallion (2003), using a propensity score matching approach, investigate the role of access to piped water in the frequency of children's health gains in India, and find a significant reduction in the incidence of diarrhea among children with access to piped water. Mu e Walle (2007) evaluate the impacts of rural road rehabilitation in Vietnam and provide evidence of meaningful average impacts on the development of local markets. Moreover, these are strongly heterogeneous effects, with poorer households reflecting higher impacts. Bensch, Kluve e Peters (2011) use household data to implement propensity score matching across connected and disconnected villages in Rwanda and find strong significant positive effects on income and children's education. In a more recent study, the same authors provide evidence on the relation between electricity usage on lighting and night activities in rural Senegal, suggesting that access to electricity increases the time spent studying. Moreover, an impact on perceived security also appears (Bensch, Kluve e Peters (2013)). Arraiz e Calero (2015) use a propensity score matching approach and find impacts of electrification on the time that families remain awake, with such impact greater in the case of women, a result probably related to the increase in productivity of domestic work. In addition, with results in the international literature, the authors find evidence that women spend a larger portion of their time on labor-market activities, while for children the impact goes toward increasing time dedicated to studies.

Finally, the studies using the instrumental variable approach and the instruments implemented are described. The first group of papers uses a measure of distance as the instrument. Jacoby (2000) investigates the impacts of roads on rural development, using the distance between farms and the nearest road in Nepal, and finds that through the connection of markets, the presence of roads has positive economic impacts that are larger for poorer households. Using the distance to the nearest transmission line as an instrumental variable, Aguirre (2017) finds evidence that rural electrification in Peru increases the average study time of children by more than one hour and a half per day. Using the distance between the municipality and the nearest dam, Grogan

(2016) investigates how household electrification in Colombia affects fertility, women's labor supply, and children's education.

Dinkelman (2011) studies the impact of a rural electrification program in South Africa, using the land gradient as the source of exogenous variation, and identifies important changes in the labor market and in family-time allocation. According to the author, the presence of electricity increases the productivity of domestic work, mostly performed by women, who started to have more time to dedicate themselves to the labor market. Still using the land gradient as instrument, Duflo e Pande (2007) investigate the productivity impacts of allocating large dams in India, exploring the fact that land gradient is an exogenous variable that affects dam allocation. They find negative impacts on districts that receive the dam, and positive impact in districts located downstream from the dam.

In a study of Brazil, Lipscomb, Mobarak e Barham (2013) estimate the development effects of electrification by using a simulated grid expansion to consider the hypothesis that all the investments would have been made based on cost, and find large positive effects on development. Applying the same strategy for the Philippines, Chakravorty, Emerick e Ravago (2016) provide evidence of electrification causing significant welfare improvements, while Kassem (2017) finds a causal relation between electrification and industrial development.

Using transmission-line density as instrument, Chakravorty, Pelli e Marchand (2014) investigate the impact of electrification on household income and find strong evidence of a positive effect on non-agricultural incomes.

Finally, Arvate et al. (2017) also follow an instrumental variable approach using the LpT eligibility rule to study the relation between lightning and violent crimes in Brazil and show a reduction in homicide rates in electrified municipalities. Still using the LpT eligibility rule, Portela, Carraro e Ribeiro (2017) study how access to electricity impacts children's time allocation in Brazil, finding a positive impact on the likelihood of being enrolled in school and not working.

3 The "Luz para Todos" Program

Next, the Brazilian electrification program which provided the exogenous shock to the probability of being connected to the grid, explored by the identification strategy of this study, is presented.

Like other developing countries, Brazil had successfully expanded access to electric power in urban areas, but somehow not in rural areas. Data from the 1980 demographic census show that more than 80% of rural households in Brazil did not have access to electricity, and the 2000 census shows that number remained practically unchanged after 20 years . To change that picture and attempt to promote economic growth, the Federal Government, at the end of 2003, settled the National Electricity Universalization Plans, known as *"Luz para Todos"* (Lights for All, LpT), the goal of which were to provide universal access in rural areas by 2008. Rural electrification was set as a strategic social-development vector. Almost 80% of all LpT household connections were made between 2004 and 2010, as Figure 2 shows.

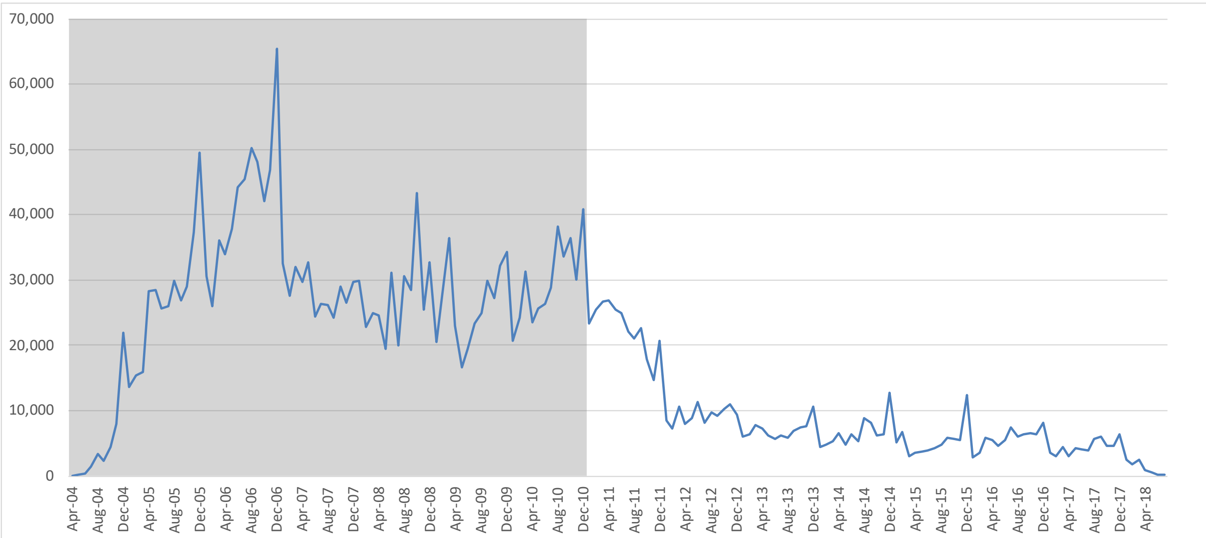

Figure 2 – Monthly LpT household connections
Source: EPE/MME

From 2004 to 2008, the program invested more than R$ 20 billion, reaching the goal of 10 million households in 2009. From 2004 to 2010, the program performed more than 2.6 million connections, of which almost 50% took place in the north and northeastern regions of the country, as Figures 3 and 4 show, illustrating the municipality electric-coverage variation between 2000 and 2010. Additionally, Figures 6 and 7 present the nightlight-emission variation for the same period, while Figure 5 shows the LpT municipality number of connections and the eligible municipalities.

The National Electricity Universalization Plans were instituted by the National Electric

Energy Agency's (ANEEL) Resolution 223[1] on April of 2003. According to ANEEL's Resolution, each one of the electricity-distribution companies in the country had to meet connections targets based on pre-program municipality electric-connection rates. Moreover, depending on household income and location, the distribution companies were to attend to any connection request at no cost. If the connection request did not require a transmission-line extension, the distribution company has to connect the household immediately. Connections requiring transmission-lines extension were to start the following year.

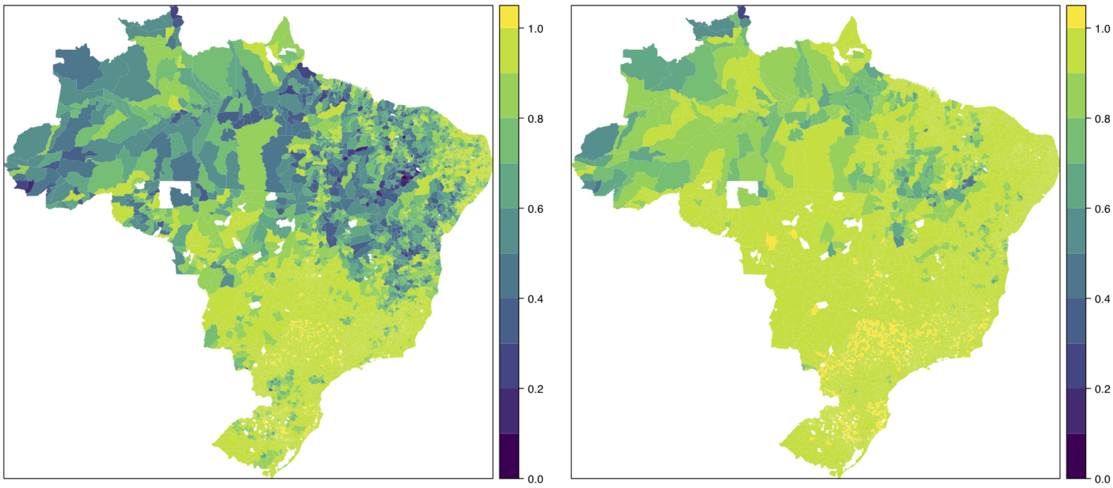

Figure 3 – Municipality access to electricity in 2000 (left) and 2010 (right)
Source: IBGE

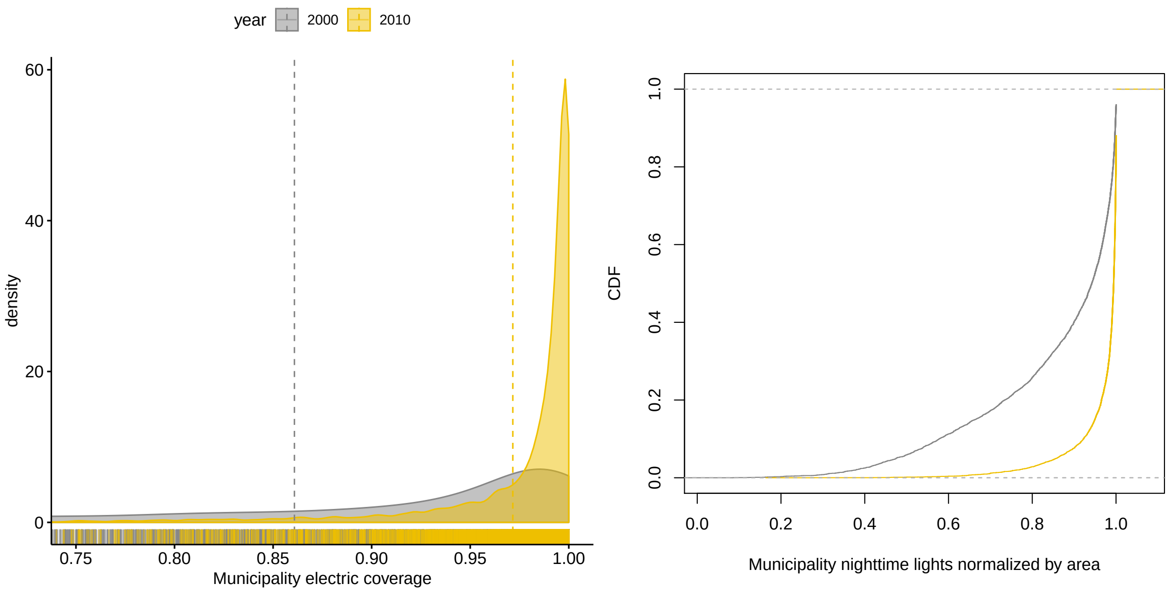

Figure 4 – Municipality electric coverage densities and CDFs by year
Source: IBGE

1 Available at http://www.aneel.gov.br/cedoc/res2003223.pdf.

The LpT was formally instituted by Decree No. 4.873 of November 11, 2003, and it was basically a fulfillment anticipation of Resolution 223 goals. It was funded with federal resources from the Energy Development Account and the Global Reversion Reserve, as well as electric power and subnational (states and municipalities) funds. However, the federal government, state governments, and electric companies shared the investments. The federal government's accountability for 82% of total investment, as well as the fact that the non-refundable loans granted by the federal government had to comply with the rules stated above, enable researchers to use the electric-coverage criterion as a source of exogenous variation in access to electricity. Figure 5 shows eligibility due to the 85% coverage criterion. The program targeted only rural-area consumer units that requested the connection to the local distribution company.

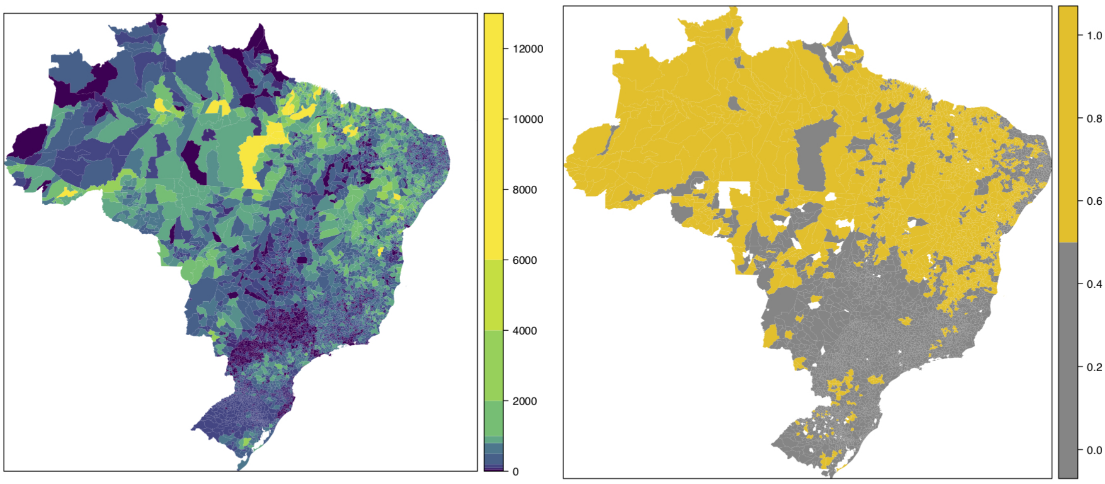

Figure 5 – Municipality LpT conections (left) and Eligibility (right)
Source: MME and IBGE

Following the LpT program manual, the access to federal financial resources should prioritize electrification projects in areas and municipalities that met as many of the following criteria:

- municipalities with electrification coverage of less that 85%, based on the 2000 census data;
- municipalities with human development index lower than the state's average;
- communities affected by hydroelectric power plants or by electrical system works whose liability is not defined for the project developer;
- rural electrification projects targeted at the productive use of electric power and at fostering integrated development;
- public schools, health centers and water wells;
- rural settlements;
- development of subsistence agriculture or family-run handicraft activities;

- smallholdings and medium-sized rural estates;
- rural electrification projects for populations living in the vicinity of nature conservation units;
- rural electrification projects stalled for lack of funds and aimed at rural communities; and
- rural electrification projects for populations in areas specifically used by special communities, such as racial minorities, remaining *quilombos* communities, extractive communities, etc.

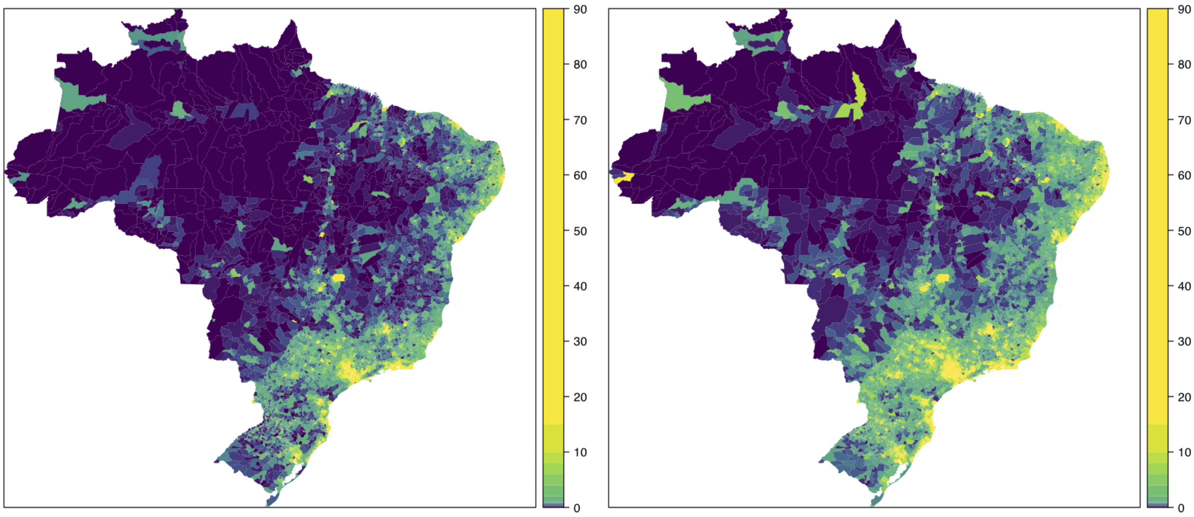

Figure 6 – Municipality nighttime lights normalized by area in 2000 (left) and 2010 (right)
Source: NOOA/NCEI

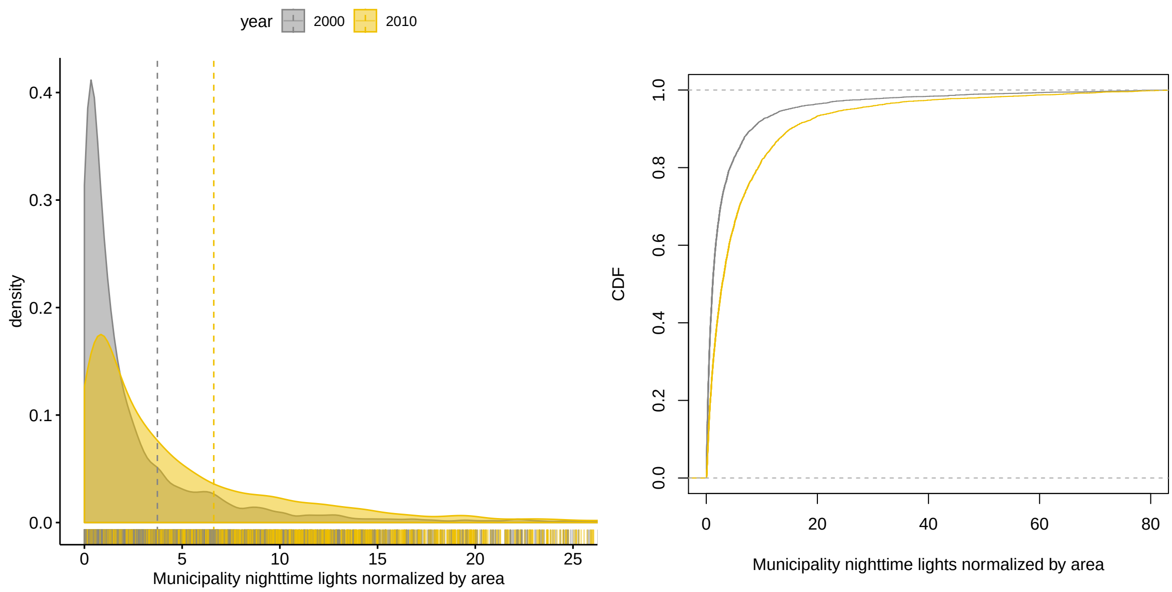

Figure 7 – Municipality nighttime lights normalized by area densities and CDFs by year
Source: NOOA/NCEI

The program must fulfill its goals by the end of 2008. ANEEL's Normative Resolution 365 of May 2009 extended that deadline to the end of 2011. However, because the 2010 Census showed households mainly in the north and northeast regions still without electric-energy supply, the federal government extended the LpT deadline to 2014.

To coordinate the extension plans, each distribution company should have submitted to the regulatory agency an annual plan. In case the company did not meet its goals, it had to present a formal document explaining why[2]. As shown by Rachter (2014), these documents are valuable program-execution reports and provide strong evidence supporting this study's instrumental-variable hypothesis. In fact, the companies usually reported that the municipality geography characteristics, such as land gradient, and resulting higher connection costs justified a large part of the target-year change requests. Therefore, distribution companies initially directed investments to municipalities with lower connection cost, i.e., municipalities with flatter ground, denser population, and location closer to the existing electric substations and transmission lines. According to West, Dwolatzky e Meyer (1997), these are the three main factors that reduce average distribution construction costs.

This study thus explores an exogenous variation using average municipality land gradient as the instrument, following Dinkelman (2011). The priority criteria set out in the Universalization Plans and the LpT program generated exogenous variations in access to electricity among municipalities. However, due to variation in extending the connection grid cost described above, these rules were not sharply followed. The expansion of the rural electrification grid induced by the LpT rules followed cost-related incentives. For that reason, an instrument-variable approach is implemented, using both sources of exogenous variation: the LpT eligibility rule and municipality land gradient.

[2] All these documents are available at http://www2.aneel.gov.br/area.cfm?idArea=750&idPerfil=2.

4 Data

This section describes the data sources used for the study's analysis. The main variables are from the 2000 and 2010 Brazilian Censuses (*Instituto Brasileiro de Geografia e Estatística* - IBGE) and the Brazilian Health Ministry (DATASUS). The instrumental variable was calculated using data from EMBRAPA, the Brazilian Agricultural Research Company, which provides information from the Shuttle Radar Topography Mission (SRTM) satellite. Additionally, the nightlight emissions are from the U.S. National Oceanic and Atmospheric Administration.

4.1 Instituto Brasileiro de Geografia e Estatística

The evaluation of the electrification impact on health outcomes uses socioeconomic data from the 2000 and 2010 Brazilian Censuses. The Brazilian Census contains data at both individual and household levels, so the two databases were combined, first at the household level and then at the municipality level. Therefore, variables at the individual level were averaged for individuals living in the same household; then, household data were averaged for municipalities. The result is panel a data for 2000 and 2010 for every municipality in Brazil. During this period, some municipalities were created, such that the analysis works with minimum comparable areas[1].

From this data source, information was collected on household location (urban or rural), number of household residents, whether the household has electric energy, toilet, piped water, sewage disposal, and the number of *Bolsa Família* recipients[2]. Every municipality with more that 50% of its households in rural areas is considered "rural".

4.2 Brazilian Health Ministry

Several health-related variables were constructed at the municipality level: mortality and hospitalization rates by disease, gender and age, fertility rate, child's birth-weight share, vaccine coverage and vaccine-specific doses, and number of some hospital and ambulatorial procedures executed. The disease classification used in this paper strictly follows the World Health Organization's *International Statistical Classification of Diseases and Related Health Problems* (ICD), as shown in Table 1. Moreover, some analysis was conducted on specific diseases, as reported in the following sections.

As a robustness check, every ICD chapter was analyzed. The health data were obtained mainly from the Brazilian Health Ministry *Departamento de Informática do Sistema Único de Saúde* (Department of Informatics of the Unified Health System—DATASUS). Besides

[1] Data on minimum comparable areas were provided by IBGE.
[2] *Bolsa Família* is a federal conditional cash-transfer program that started in 2003.

health-related variables, DATASUS also provided population-related variables. The respective population groups were used to construct gender-specific and age-specific rates.[3]

Table 1 – International Classification of Diseases and Related Health Problems - ICD

Chapter	Blocks	Title
01	A00–B99	Certain infectious and parasitic diseases
02	C00–D48	Neoplasms
03	D50–D89	Blood and blood-forming organs and certain disorders involving the immune mechanism diseases
04	E00–E90	Endocrine, nutritional and metabolic diseases
05	F00–F99	Mental and behavioural disorders
06	G00–G99	Nervous system diseases
07	H00–H59	Eye and annex diseases
08	H60–H95	Ear and mastoid process diseases
09	I00–I99	Circulatory system diseases
10	J00–J99	Respiratory system diseases
11	K00–K93	Digestive system diseases
12	L00–L99	Skin and subcutaneous tissue diseases
13	M00–M99	Musculoskeletal system and connective tissue diseases
14	N00–N99	Diseases of the genitourinary system diseases
15	O00–O99	Pregnancy, childbirth and the puerperium
16	P00–P96	Certain conditions originating in the perinatal period
17	P00–P96	Congenital malformations, deformations and chromosomal abnormalities
18	R00–R99	Symptoms, signs and abnormal clinical and laboratory findings, not elsewhere classified
19	S00–T98	Injury, poisoning and certain other consequences of external causes
20	V01–Y98	External causes of morbidity and mortality
21	Z00–Z99	Factors influencing health status and contact with health services

4.3 Shuttle Radar Topography Mission - EMBRAPA

To overcome the endogeneity problem and identify the causal impact of electrification on health outcomes, the land gradient is used as an exogenous source of variation in access to electricity increase over time. The land gradient was constructed for each municipality using digital relief data obtained through SRTM satellite images with a resolution of 90 meters. Figure 8 shows the data, which is available worldwide and the same as those used by Dinkelman (2011).

The Brazilian data were obtained by combining 810 SRTM raster files available at the EMBRAPA website. From the relief data, it was possible to construct a unique raster file with land gradient for the entire country,[4] and then take the average for each municipality. The land gradient is measured in degrees.

4.4 National Oceanic and Atmospheric Administration

Data were collected from the U.S. National Oceanic and Atmospheric Administration. The daily images[5] taken by U.S. Air Force satellites are collected and processed by the NOAA Defense Meteorological Satellite Program—Operational Line Scan (DMSP-OLS). The images are averaged across the year and report light intensity for each 30 arc-second pixel[6] on a scale from 0 to 63.

3 Fertility, mortality and hospitalization rates are reported per thousand people.
4 This data is available only for inland municipalities, so it excludes municipalities such as Fernando de Noronha and Ilhabela, for example
5 The satellites photograph the earth daily between 8:30pm and 10:00pm local time
6 Approximately 1 km^2 at the Equator.

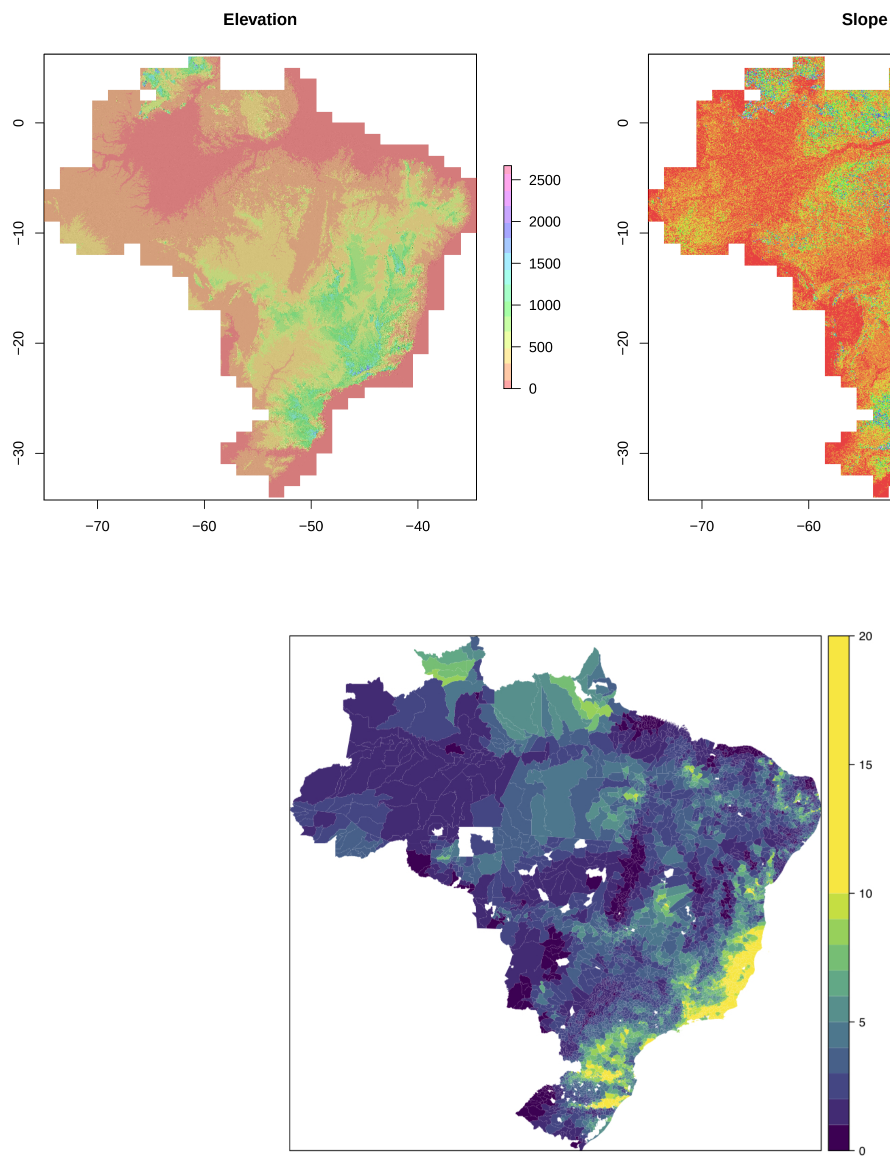

Figure 8 – Brazil's elevation, land gradient and municipality average land grandient
Source: SRTM

The use of nighttime light in the field of Economics was popularized by Chen e Nordhaus (2011) and Henderson, Storeygard e Weil (2012) using this data source as a proxy for economic activity. In addition, recent work has shown that nighttime light can be used to detect electrification.[7]

It is important to note that nighttime lights emission does not measure electric energy used for purposes other than illumination, thus representing a lower bound on household electric consumption. Moreover, a potential problem related to the use of nighttime lights is the lack of information on the household level. For example, an increase in nighttime lights emission can be

[7] See Min et al. (2013) for Senegal and Mali, Min e Gaba (2014) for Vietnam, and Chand et al. (2009) and Dugoua, Kennedy e Urpelainen (2018) for India.

caused by the increase in public illumination of streets. However, as the LpT’s main objective was not to increase streetlight infrastructure, this should not be a concern.

5 Empirical Strategy

5.1 Differences-in-Differences Approach

The data availability suggested the use of a difference-in-difference (DiD) approach. Data from two periods and a set of treated municipalities were at hand. Since its introduction by Ashenfelter (1978) and Ashenfelter e Card (1985), the linear DiD model is a benchmark tool for impact evaluation. This approach defines a treatment effect as the difference between two potential outcomes, following Rubin (1974) to define a potential outcome as a function of treatment status.

This approach allows the researcher to evaluate the effect of an exogenous change on the dependent variable. The impact identification compares the difference in the level of this variable between a treatment group and a control group, before and after the treatment implementation. Therefore, this procedure eliminates the time-invariant omitted-variable problem, as well as other problems related to the functional form.[1] The adoption of this identification strategy assumes that every transitory shock is uncorrelated with treatment.

One possible concern in the environment analyzed in this study is the possibility that just prior to the LpT program, the municipalities' electric coverage drops, making treatment more likely. This possibility is known in the literature as "Ashenfelter's dip,"[2] and is ruled out by controlling for the baseline electric coverage interacted with a time trend. Moreover, the program was created at the end of 2003, defining the 2000 municipality electric coverage as an eligibility criterion.

Additionally, this approach assumes that the randomization hypothesis that rules out selection of the untreated holds in the first difference (Blundell e Dias (2008)). However, it does not eliminate the possibility of selection of unobservables; it only restricts its source. In fact, the DiD approach excludes the possibility that the selection is due to transitory individual effects. Therefore, in general, it only identifies the average treatment effect on the treated (Blundell e Dias (2008)).

The usual identification assumptions apply, enabling assumption of correct linear specification of the conditional mean, parallel-trends assumption, homogeneous treatment effect, and unconfoundedness.[3] Perhaps the most important assumption is that the *stable unit treatment value assumption* (SUTVA) holds, as first assumed by Rubin (1978) and Rubin (1990). This is a crucial assumption, as Angrist, Imbens e Rubin (1996) point out: "*SUTVA implies that potential outcomes for person i are unrelated to the treatment status of other individuals*."

[1] See McMillen (2010).
[2] See Ashenfelter (1978) and Heckman, LaLonde e Smith (1999).
[3] This implies that treatment assignment is independent of the outcome, eventually conditional on controls.

5.2 Model

To illustrate the empirical strategy, first consider the estimation of equation 5.1 by ordinary least squares, where Y_m is an outcome of interest, E_m is the municipality electric coverage, θ_m are the municipality fixed effects, and ε_m is an idiosyncratic error term. If these were randomly distributed across municipalities, conditional on fixed effects, E_m would be orthogonal to non-observables. Thus, δ_1 would be an unbiased causal estimator of the access to electricity impact on the outcome of interest.

$$Y_{mt} = \delta_0 + \delta_1 E_{mt} + \theta_m + \varepsilon_{mt} \tag{5.1}$$

However, as presented in previous sections, the grid expansion in Brazil was not randomly determined, so the causal effects cannot be estimated as proposed in equation 5.1. The LpT program gave priority to rural municipalities and, among this priority group, the expansion was made considering household characteristics, such as average income. Therefore, as these characteristics are probably correlated with outcomes of interest, estimating equation 5.1 would result in a biased estimator, due to the electric coverage endogeneity.

One possibility for solving this endogeneity problem is to add covariates to the model specification above. This procedure would control for pre-treatment socioeconomic municipality attributes and characteristics that are time-variant and potentially correlated with both electric coverage and other covariates. However, this would only control for the selection of observables.

As posed by Barron e Torero (2014), the main issue in the rural electrification impact literature is the identification of causal effects. In fact, the potential endogeneity faced by the literature turns identifying causal relations of interest into a challenge. As the electrification process is not random, recent studies have used temporal variation of processes for electrification expansion and instrumental variables as sources of exogenous variation for their identification strategies. These studies have found that the endogeneity problem can generate two potential sources of bias. One is due to a reverse causality problem. From one side, there is a direct causality direction from access to electric power to better health indicators; from the other, it is also true that better health indicators may be related to a better household financial situation that may cause the access to electric power. The second possible source of bias is an omitted variable bias problem. Recent papers therefore propose an identification approach exploring time variation and instrumental variables. Among those studies, instrumental variables are: land gradient,[4] distance to hydroelectric dams,[5] and distance to the electricity line.[6]

Using the distance from the municipality centroid and the nearest transmission line and energy substation as a control could be endogenous (see Dinkelman (2011)). For illustration

[4] See Dinkelman (2011), Grogan e Sadanand (2013).
[5] See Grogan e Sadanand (2013).
[6] See Khandker, Barnes e Samad (2012) and Walle et al. (2013).

purposes only, raster files containing this information for a pre-LpT program period were collected from the Brazilian Energy Ministry. Figure 9 illustrates the existing transmission lines and substations in 2000. Moreover, as the grid-expansion decision was made at the municipality electricity-supplier level, the study should use clusters at the electricity-supplier level. As one municipality could have more than one electricity-supplier company, the one supplying most households was considered the only supplier to that municipality. The data on electricity-supplier companies were from the Brazilian Electric Energy National Agency (ANEEL - *Agência Nacional de Energia Elétrica*).

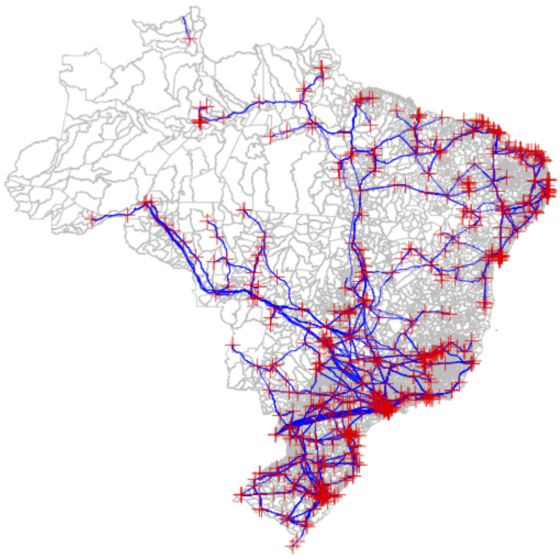

Figure 9 – Existing transmission lines and substations in 2000.
Source: EPE/MME

As stated previously, to overcome the endogeneity problem, the effect of rural households' access to electricity on health outcomes of interest is estimated using an instrumental variable approach. The proposed instrument explores the LpT priority criteria interacted with a cost-related variable as a source of exogenous variation in electric-power distribution.

Since the most general LpT criterion was the 85% coverage rule, it is expected that in 2010 households located in municipalities with less than 85% of electric coverage in 2000 would get greater access to electricity expansion than households that, in 2000, had more than 85% of electric coverage. Moreover, this difference is considered to be caused mainly by the LpT program. This is not a sharp rule, leading to proposal of an instrument that has the eligibility rule variable interacted with the municipality land gradient.

The use of land gradient as a source of exogenous variation nevertheless imposes some concerns. Among them is the fact that this variable may have a direct impact on agricultural production outcomes, thus affecting municipality income. Dinkelman (2011) argues that in its South African environment, the gradient impact on agricultural productivity is small, since most families are not farming. To check this, the researchers followed a DiD approach and regressed

the municipality per capita agricultural GDP on the municipality land gradient interacted with a period dummy variable, municipality fixed effects, a dummy time variable and the average municipality years of schooling. The results suggest that there is no statistically significant correlation between the land gradient and the agricultural per capita GDP variation between 2000 and 2010. The standard errors were clustered at the municipality level.[7] Another possible concern is that usually flatter areas are more populated than steeper ones, which may result in differential health outcomes independent of electric-energy connection. For that reason, the land gradient is used as a control, allowing for heterogeneous effects across the gradient dimension.

Therefore, based on the LpT eligibility rule, the municipality land gradient, and the data of the 2000 and 2010 demographic censuses, this study estimates a difference-in-differences with instrumental variable model that consists of a two-stage estimation. The first stage estimates the endogenous variable, household access to electric power, as a function of the instrument and other covariates, while the second stage relates the dependent variables as a function of the predicted value for the endogenous variable obtained in the previous stage and other covariates.

The first stage estimates the following equation:

$$\begin{aligned} E_{mt} = \delta_0 + \theta_m + \delta_1 Y_{2010} + \delta_2 (Y_{2010} * R_m * C_m) + \delta_3 (Y_{2010} * R_m) + \\ \delta_4 (Y_{2010} * C_m) + \sum_{j=1}^{J} \beta_j x_{jmt} + \sum_{k=1}^{K} \alpha_j \omega_{kmt} + \delta_5 E_{2000,m} * Y_{2010} + \nu_{mt} \end{aligned} \tag{5.2}$$

where, E_{mt} is the proportion of households in municipality m with electricity in period t; θ_m is the municipality fixed effect; Y_{2010} is the 2010 year dummy; R_m is equal to one if municipality m has less than 85% electric coverage in 2000; C_m is the proposed cost-related instrument, i.e., land gradient; x_{jmt} represents one of the J mean covariates at the household level (years of schooling, number of people living in the household, presence of toilet, water connection and sewage infrastructure indicators); ω_{kmt} contains K control variables that vary across municipalities (proportion of *Bolsa Família* beneficiaries and agricultural per capita GDP); and $E_{2000,m} * Y_{2010}$ is a linear time trend for electric coverage rate for municipality m.

The covariates vectors constructed from the Demographic Census controls by observable factors that can affect both the access to electric energy and socioeconomic status. The 2000 electrification rate, interacted with a linear trend of time, controls for the existence of convergence of access to electric energy between more and less developed municipalities. The variable that measures the proportion of beneficiaries of the *Bolsa Família* program controls for the aggregate effect in the municipality of this important social policy, which may have affected poverty, health status, and income. Following Dinkelman (2011), any political factors are treated as omitted variables.

Note that this model can be interpreted as a difference-in-difference-in-differences appro-

[7] See Bertrand, Duflo e Mullainathan (2004).

ach.[8] The first difference is over time, because electrification coverage increased between 2000 and 2010 for eligible and non-eligible municipalities. The second difference is between eligible and non-eligible municipalities, while the third difference is across the land-gradient dimension, since the electrification coverage increase should have been larger for flatter municipalities. To illustrate this argument, the land gradient is considered as a dummy variable, splitting the municipalities into high and low land gradient, i.e., higher or lower than the median. First considering only the eligible municipality with high land gradient, the difference between 2010 and 2000 as reported in Table 2 is of 29 percentage points. This procedure can be repeated for other subgroups. In both eligible and non-eligible municipalities, the electric coverage for municipalities with flatter terrain is higher than for municipalities with higher average land gradient. The municipality electric coverage increased over time for all subgroups, but increased more in municipalities that were eligible and had lower land gradient. The difference in these differences can be interpreted as the LpT's causal effect. It is important to note that this holds if one assumes that, in the absence of the program, the variation in municipality electric coverage would not have been systematically different across eligibles and non-eligibles.[9] Repeating this procedure for the non-eligible municipalities and taking the difference between the difference-in-differences results produces an imprecise estimator of the difference-in-difference-in-differences approach, i.e., –1.9 percentage points. The first-stage estimated parameter for the triple interaction ($\hat{\delta_2}$) should therefore be close to this preliminary result.[10]

δ_2 thus estimates the land gradient marginal effect among eligible municipalities, which is expected to be negative, as steeper municipalities face larger electrification costs. Furthermore, δ_3 captures the different time trend in electrification between eligibles and non-eligibles and should be positive, as eligible municipalities should have had a larger increase in electric coverage between 2000 and 2010.[11] δ_4 captures the different time trend effects by degree of land gradient and should be a negative parameter.

8 See Jayachandran e Lleras-Muney (2009) and Alsan e Wanamaker (2017).
9 See Duflo (2001).
10 As reported in Table 63, the correct estimated parameter considering the land gradient as a dummy variable is –2.3 percentage points.
11 Note that the electric coverage is a censored variable, i.e., it cannot be larger than 100%.

Table 2 – Average municipality electric coverage by eligibility criterion, year and land gradient

Eligibility Rule	Land Gradient	2000	2010	**Difference**
Yes	High	0.633	0.925	**0.292**
		(0.003)	(0.002)	(0.004)
	Low	0.594	0.911	**0.318**
		(0.004)	(0.002)	(0.004)
	Difference	**-0.040**	**-0.014**	**0.026**
		(0.005)	(0.003)	(0.006)
No	High	0.949	0.994	**0.045**
		(0.001)	(0.0002)	(0.001)
	Low	0.939	0.991	**0.052**
		(0.001)	(0.0002)	(0.001)
	Difference	**-0.010**	**-0.003**	**0.007**
		(0.001)	(0.0003)	(0.001)

Note: Standard errors are in parentheses.

Including municipality fixed effects translates to the inability to explicitly control for the distance between the municipality centroid and the nearest transmission line or substation in 2000 (pre-LpT period). Moreover, due to the inclusion of municipality fixed effects, the inclusion of electric companies' fixed effects does not aggregate to the model. However, to deal with the fact that the investment decision was made at the electricity-company level, standard errors are evaluated considering clusters at the electricity-company level.

The second stage estimates the following equations:

$$\begin{aligned} Y_{mt} = \delta_0 + \theta_m + \delta_1 Y_{2010} + \delta_2(Year_{2010} * R_m) + \delta_2(Y_{2010} * C_m) + \\ \sum_{j=1}^{J} \beta_j x_{jmt} + \sum_{k=1}^{K} \alpha_j \omega_{kmt} + \delta_4 E_{2000,m} * Y_{2010} + \delta_6 \hat{E}_{mt} + \varepsilon_{mt} \end{aligned} \tag{5.3}$$

where, $\hat{E}_{mt}$ is the predicted value for access to electricity in municipality m in period t; and Y_{mt} represents health outcomes in municipality m in period t.

The main hypothesis of identification is that the rule for electric-power coverage in 2000 interacted with the cost-related instrument, conditional on observable factors and on the municipality fixed effects, has an exogenous effect on the probability of access to electric power by the household. Therefore, the identification assumption is that conditional on pre LpT grid infrastructure, municipality fixed effects, and baseline municipality characteristics, land gradient does not affect health outcomes independently of the assignment of electrification grid expansion.

Furthermore, the identification hypothesis implies that the land gradient cannot be related to other household infrastructure access. Graphs constructed and reported in Figure 10

illustrate this point. As expected, the land gradient has a negative correlation with electrification rate variation between 2000 and 2010, and this tendency is not present for other household infrastructure, such as access to piped water and sewage disposal. Some regressions were performed as a robustness check to test the consistency of this graphic analysis.

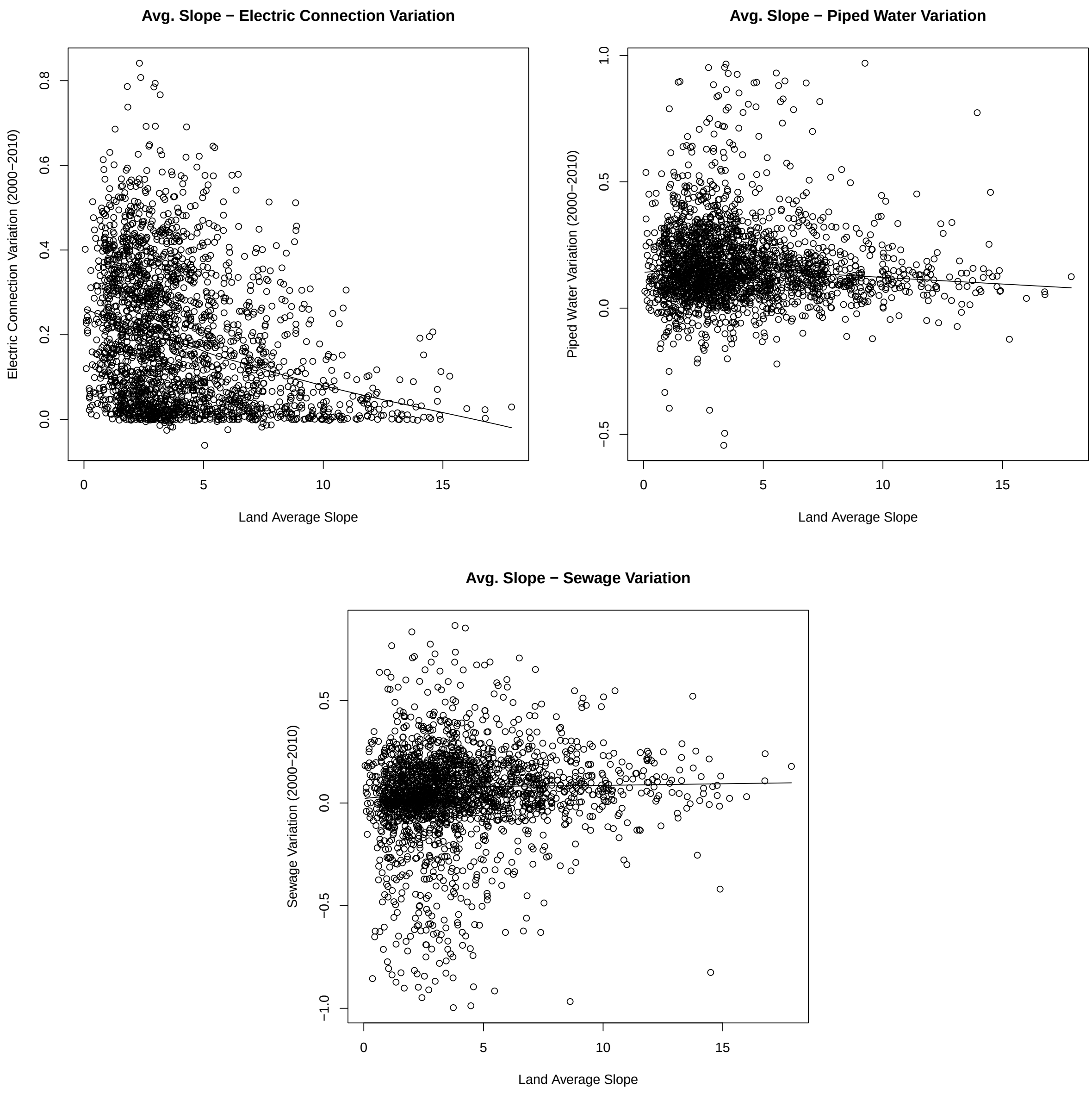

Figure 10 – Relation between municipality average land gradient and electric coverage, piped water and sewage variation

Source: SRTM and IBGE

As the identification strategy proposed is an instrumental variable DiD, the results must be interpreted as local average treatment effects (LATE, as initially posed by Imbens e Angrist (1994)). The effect of access to electric power is estimated for the group whose treatment condition is influenced by variation in the instrument, the compliers. As usual, it is assumed that there are no defiers (i.e., no one who could have access to electricity would deny it) and that

the instrument affects all individuals in the same way, by increasing the probability of access to electricity (usual monotonicity assumption). Note that the proposed instrument is a continuous variable that turns the identification of our compliers more difficult.[12] As a robustness check, the results are evaluated using a dummy instrument, i.e., the year dummy is interacted with the eligible dummy and a slope dummy that is equal to one if the municipality land gradient is lower than the land gradient median.

Additionally, a different specification is also estimated, which considers as instruments both the triple interaction between Y_{2010}, R_m and C_m and the interaction between Y_{2010} and R_m. If in the previous specification compliers are the households in municipalities induced into treatment by the LpT eligibility rule and the land gradient, in this alternative specification the set of compliers is extended, adding the households in municipalities induced into treatment by the eligibility criteria alone. Keeping the interaction between C_m and Y_m as a control in the second stage allows for health outcomes heterogeneity effects for different land gradients.

For the multiple-instruments specification, the 2SLS estimate can be interpreted as a weighted average of local average treatment effects, as long as a specific monotonicity condition is satisfied.[13] However, as observed by Heckman e Vytlacil (2005), the Imbens e Angrist (1994) monotonicity condition requires uniformity across individuals, restricting choice behavior to being homogeneous. This restriction would be a possible concern to this case, since it would imply that all municipalities responded to the eligibility rule and to the land gradient in the same way, a very strong assumption considering that there could be eligible municipalities with different land gradients and different treatment status (possibly due to the pre-LpT transmission line proximity).

As recently shown by Mogstad, Torgovitsky e Walters (2019), changing the Imbens e Angrist (1994) monotonicity condition to a partial monotonicity assumption (strictly weaker) solves this possible concern. The partial monotonicity condition holds if, holding all other instruments constant, the Imbens e Angrist (1994) monotonicity condition holds for each instrument separately. Thus, partial monotonicity is satisfied if each instrument makes every individual more likely to be treated. Moreover, partial monotonicity allows for heterogeneity in the instruments' relative impacts, so individuals may respond differently to each instrument, as long as they do so in the same direction.

Therefore, next is the presentation of this condition, highlighting its differences and directly following Mogstad, Torgovitsky e Walters (2019). Some notations are introduced to enable stating the assumptions formally. Consider a population $\mathcal{I}$ of individuals i. Individual i's potential treatment status depends on some instrument value Z_i so $D_i(z) \in \{0, 1\}$, where z takes values on a subset $\mathcal{Z} \subset \mathbb{R}^L$. Assume for simplicity that $L = 2$ and denote by $Z_{1,1}$ and $Z_{i,2}$ the

[12] Angrist e Pischke (2009) writes that *"2SLS with variable or continuous treatment intensity produces a weighted average derivative along the length of a possibly nonlinear causal response function"*.

[13] See Imbens e Angrist (1994).

two instruments. The monotonicity condition introduced by Imbens e Angrist (1994) follows.

Assumption 1. *$\forall z, z' \in \mathcal{Z}$ either $D_i(z) \geq D_I(z')\forall i \in \mathcal{I}$, or $D_i(z) \leq D_I(z')\forall i \in \mathcal{I}$*

As stated above, Heckman e Vytlacil (2005) note that Assumption 1 requires uniformity across individuals and not monotonicity in the instrument. This monotonicity condition is different from the usual condition in the literature referred to as Assumption 2, and neither implies Assumption 1 nor is implied by it, since it may be the case that the instrument does not have a monotonic effect on treatment choice.

Assumption 2. *If $z' \geq z$ (component-wise), then $D_i(z') \geq D_i(z)\forall i \in \mathcal{I}$*

To illustrate this point, consider a set of individuals j and k for which $D_i(z)$ is increasing in z. Imagine that individuals j are induced into treatment when Z_1 goes from z_1 to z_1', so $D_j(z_1) = 0$ and $D_j(z_1') = 1$, but this is not the case for individuals k, i.e. $D_k(z_1) = 0$ and $D_k(z_1) = 0$. The same holds in the opposite direction for instrument Z_2. In this case, Assumption 2 does not imply Assumption 1. Moreover, consider the case in which the instrument does not have a monotonic effect on treatment choice and $\mathcal{Z}_j \subseteq \mathcal{Z}_k$. In this case, Assumption 1 does not imply Assumption 2.

Assumption 2 has strong implications in terms of choice behavior, since it supposes that any two individuals who are indifferent to taking the treatment or not for some specific value of Z must have the same marginal rate of substitution between the two instruments.

To solve this, Mogstad, Torgovitsky e Walters (2019) introduce a new monotonicity condition, the partial monotonicity (Assumption 3)[14]. It is easy to see that Assumption 1 and Assumption 2 imply Assumption 3. This means that all municipalities are more likely to be treated if eligible, even if they differ in their responses to land gradient.

Assumption 3. *Take any $l = 1, 2, ..., L$ and let (z_l, z_{-l}) and (z_l', z_{-l}) be two points in $\mathcal{Z}$. Then either $D_i(z_l, z_{-l}) \geq D_i(z_l', z_{-l})\forall i \in \mathcal{I}$, or $D_i(z_l, z_{-l}) \leq D_i(z_l', z_{-l})\forall i \in \mathcal{I}$*

Mogstad, Torgovitsky e Walters (2019) show that assuming partial monotonicity enables relaxing the constraint on choice behavior. Thus, when considering the multiple instruments specification, partial monotonicity is assumed instead of assuming the usual monotonicity condition.

[14] Since our concern here is the two-instruments case, consider $L = 2$.

6 Results

The following section reports the main findings. For impartiality's sake, every CID chapter is analyzed for all age groups, for males and females. Only the main results are reported, with some being in line with the literature, and others not.

Presented first are first-stage results and some instrument robustness checks. Then second-stage results are analyzed for fertility, mortality and hospitalization rates, birth-weight shares, and vaccine coverage. Moreover, as there could be some heterogeneity on unobservables resulting in null average treatment effects, a marginal treatment effect investigation is also performed. As this is only an exploratory investigation, the methodology and respective results are reported in Appendix A.

6.1 First Stage

Table 3 shows the results for the first-stage estimation considering municipality fixed effects and clustered standard errors at the municipality level. The coefficients estimated for the instruments, i.e., the triple interaction between $Year$, $Eligible$ and $Land\,Gradient$, and this triple interaction plus the interaction between $Year$ and $Eligible$, confirm the identification hypothesis regarding the use of the LpT eligibility rule and a cost factor as instruments for household electrification. Being eligible due to the 85% LpT rule increases by almost 25 percentage points the probability of being connected in 2010.[1] Among the complier municipalities, for each average land gradient degree, the probability of being connected decreases by 0.9 percentage points. One land gradient standard deviation is equal to 2.8 degrees, so the effect magnitude is relevant as the increase in one land gradient standard deviation reduces the access to electricity probability by 2.5 percentage points. This result is in line with the findings of Dinkelman (2011) and is precisely estimated, as the coefficient of interest does not change as covariates are added as controls. This suggests that there is almost no correlation between these instruments and the covariates that also affect access to electricity.

Column 1 reports the simplest specification estimated, including only instruments, the interaction between $Year$ and $Land\,Gradient$, and municipality fixed effects. Column 2 includes a linear time trend interacted with the 2000 municipality electric coverage to control for the independent electric coverage convergence. Column 3 includes the agriculture per capita GDP and the share of *Bolsa Família* recipients to control, respectively, for specific shocks that can affect rural municipalities and for the conditional cash-transfer program. Column 4 includes household covariates (i.e., education, toilet, sewage disposal, piped water, proportion of fixed

[1] This result is in line with the findings of Portela, Carraro e Ribeiro (2017), which do not include the cost dimension in the instrumental variable strategy.

households, and garbage collection) to control for observables that can be related to the access to electricity, as well as to the health outcomes of interest in this paper.

Table 3 also reports the excluded instrument F Statistic, which suggests that the instruments used are strong. Moreover, the test on the joint weakness of instruments stresses their individual importance. In addition, the exclusion restriction hypothesis proposes that the land gradient can affect health outcomes through its impact on access to electric power, but this cannot be tested. Also estimated is how the instrumental variables affect the probability of having a sewage system (Table 4) and piped water (Table 5). The specifications estimated follow the same structure as in Table 3, and, as expected, the instruments do not affect those household infrastructure variables. These results therefore indicate the validity of the instruments.[2]

6.2 Second Stage

The following subsections report the results for the health outcomes analyzed. Recall that there are two possible second-stage specifications, depending on the instrument considered.

Considering only the triple interaction as instrument—that is, considering as compliers households in municipalities that were eligible but were induced into treatment only by its municipality land gradient—shows no evidence of electrification impacts on the health outcomes analyzed. The reasons for this result may be two-fold: first, there can actually be no effect for this set of compliers. The second and more probable reason is that controlling for the $Year * Eligible$ interaction allows for health-trend differences between eligible and non-eligible municipalities.

Therefore, maintaining this interaction as a control in the second stage eliminates the health data variability, resulting in non-significant estimates. Not controlling for this trend differential thus potentially creates a source of variation that is not as clean as it should be. Thus, we must interpret the results of this two-instruments approach as a methodological exercise only. That is, the true parameter value should be contained in the interval between the two estimated values. Hence, the following subsections report the results found when both the triple interaction, and the triple interaction plus the $Year * Eligible$ interaction are considered as instruments.

As posed previously, it is important to note the difference in the LATE when changing the set of instruments. Adding the $Year * Eligible$ interaction as an instrument extends the set of compliers, including to the previous compliers, all households in eligible municipalities. Moreover, it is important to note that the LpT program treated first municipalities that were eligible and where it was cheaper to extend the grid, but eventually, during the time range analyzed in this study, treated almost all eligible municipalities. In fact, between 2000 and 2010, 98% of all Brazilian municipalities (rural or not) had at least one household connection made by LpT.[3]

[2] For access to sewage system, the interaction between $Eligible$ and $Year$ is significant in all model specifications. However, the excluded instrument's F Statistic suggests that is it not a strong instrument when the agricultural per capita GDP and the share of *Bolsa Família* recipients are included as controls.

[3] By the end of 2004, almost 59% of all Brazilian municipalities had at least one LpT connection. This number for

The results are presented in the appendices C.1 (for fertility rate), C.2 (for mortality rate), C.3 (for hospitalization rate), C.4 (for birth-weight share) and C.5 (for vaccines), with a column structure similar to the one above.

6.2.1 Fertility Rate

The results for fertility rate are reported in Table 6 for the one-instrument case, and in Table 7 for the two-instruments case. The results for both approaches are inconclusive, i.e., there is no evidence of electrification impact on fertility rates among compliers. Extending the set of compliers in Table 7's first column shows a positive and significant impact of electrification on fertility rate; when including controls, the result becomes non-significant and negative. Interestingly, in the specifications that include the electric-coverage time trend, there is a positive and significant impact for the 2010 dummy, suggesting that, holding everything else constant, an increase of almost two standard deviations occurs in fertility rate between 2000 and 2010. Note that the average fertility rate for rural Brazilian municipalities in 2000 was 54/1000, and 46/1000 in 2010.

Still on this topic, it was investigated whether the electrification caused a reduction in the share of births by mothers less than nineteen years old. The causality mechanism in this case is two-fold: there could be an increase in education and/or a change in intrafamily time allocation with labor-market effects. However, no statistically significant effects are found in either of the model specifications.

6.2.2 Birth Weight Share

The increase in use of fridges and the better housing conditions created by electricity access may have consequences for pregnancy. Healthy mothers should be expected to give birth to healthy children. Therefore, how electricity access impacts the share of children born at less than 2.5 kg was analyzed. The results are reported in Tables 38 and 39, and no evidence was found of average impact, neither for the one-instrument case, nor for the two-instruments case.

6.2.3 Mortality Rate

Electrification impacts on mortality rates by ICD group were analyzed, and in a few cases disease-specific analysis was also performed. Almost all age-specific results were inconclusive, perhaps due to a time effect, as the electrification process is relatively recent. Besides, for the adult[4] age-group, reduction in mortality rates was found to have been caused by the electrification process for the following ICDs: neoplasms (Tables 8 and 9) and endocrine, nutritional and metabolic diseases (Tables 16 and 17). As expected, a reduction in mortality rates due to respiratory diseases (Tables 10 and 11) was also found.

the following years is: 86% (2005), 92% (2006), 94% (2007), 97%(2008) and 98% (2009).

[4] More than 20 years old.

For neoplasms, in the two-instruments case, a one-standard-deviation reduction in mortality rate was found when considering the total adult population. For the one-instrument case, there is no evidence of causal effects.[5] This effect is reduced when considering only males and is not significant for females. The result seems to be precisely estimated in the full-model specification, making this an interesting and unexpected result. Therefore, the researchers investigated whether it could be caused by the reduction in lung-cancer mortality rate, a disease that, *ex-ante*, was expected to be related to electrification, considering the reduction in emission of harmful gases inside the household. Consistent with the existing evidence, the only neoplasm that could be related *ex-ante* to an electrification process is lung cancer. For an increase of one percentage point in the municipality electric-power coverage, there is a small reduction in lung-cancer mortality rate only for females (in line with the international literature); by including the share of *Bolsa Família* recipients as a control, the result becomes non-significant, suggesting that the conditional cash-transfer program may drive the causal impact. For males, this evidence is not found in either of the instrumental variable approaches. Thus, it cannot be affirmed that the neoplasms mortality rate reduction is due to a reduction in lung-cancer mortality rate. The explanations for this result may therefore be two-fold: a reduction in the incidence of neoplasm caused by the greater access to electric power; or, more likely, an improvement in neoplasm treatment caused by the electrification process.

The first possibility can be tested using neoplasm hospitalization rates as a proxy for neoplasm incidence. In the next section, some evidence appears for the electrification process reducing the hospitalization of cancer patients, and this effect is gender-heterogeneous, with a larger effect for females, as expected.[6] To test the second possibility, the researchers tested whether the electrification caused an increase in chemotherapy procedures in Brazilian rural municipality public hospitals, choosing this procedure because it is a common cancer treatment which is also energy-dependent. Nonetheless, no evidence was found to support this hypothesis. In fact, when regressing the per capita chemotherapy treatments on the electric-coverage predicted value, a reduction was found of almost one standard deviation in the models that do not include the household covariates. This reduction may be caused by the reduction in hospitalization rates, as the following section shows.

Similarly, electric energy access can reduce the incidence of respiratory disease aggravation, as is the case for asthma, emphysema, chronic bronchitis, and other respiratory insufficiencies. In fact, important medical equipment related to the treatment of these diseases—oxygen concentrator, mechanical ventilator, CPAP (continuous positive airway pressure), BIPAP (continuous positive airway pressure at two levels), and oximeter—are electricity-dependent. For that reason, the researchers wished to test whether the electrification process caused any variation in the num-

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[5] For the adult male population, the estimated coefficients in the two approaches are very similar, even if the standard errors for the one-instrument case are larger.

[6] As posed by Barron e Torero (2014), women and children experience higher reductions in respiratory diseases incidence, as they are the group that spend more time inside the household.

ber of these respiratory-disease treatments, this was not possible since the treatment classification changed in 2008, making any comparison between data pre- and post-change imprecise[7].

For respiratory-system diseases, there is no evidence of impact when considering the one-instrument case (Table 10). However, when the total adult population was considered, half a standard deviation reduction in mortality rate was found (Table 11). Considering only males or only females, the impact is not significant. As posed by Diette et al. (2012), a larger impact for females and children was to be expected,[8] as this population subgroup usually spends more time inside the household. The result seems to be precisely estimated in the full-model specification, which was expected as presented in the literature review, as the electric-energy access makes it possible to abandon the use of fossil-fuel combustion for lighting purposes. Moreover, the oven transition driver, i.e., the shift from the combustion of animal feces and coal to an electric or gas oven, would create a similar expectation. This channel cannot be tested without oven-type information in the Brazilian census data. However, this is not a major concern since, as shown by Barron e Torero (2014), the main driver for the reduction in respiratory diseases is the shift to electric lighting.

Still on respiratory diseases, the shift from combustion lighting to electric lighting could be related to a reduction in the emphysema mortality rate. Tables 12, 13, 14 and 15 show that there is no evidence of such impact for males, but there is for adult females. For an increase of one percentage point in the municipality electric coverage, there is a reduction of almost one standard deviation in the adult female emphysema mortality rate. This result is precisely estimated and in line with the stylized fact that women spend more time inside the household, being more exposed to combustion-related pollutants.

For endocrine, nutritional, and metabolic diseases, an important reduction was found in mortality rate when considering the total adult population (Table 17). For a one percentage point increase in the municipality electric coverage, there is a 1.4-standard-deviation reduction in the endocrine, nutritional, and metabolic diseases mortality rate. The impact is reduced when only males are considered, and it is not significant for females. The result seems to be precisely estimated in the full-model specification. This is an interesting and unexpected result. Both in 2000 and 2010, more than 70% of all deaths caused by endocrine, nutritional, and metabolic diseases were caused by diabetes. For the one-instrument case, no evidence of impacts was found (Table 16).

According to the usual findings in the literature, some impact was expected on intestinal infections, especially for children. However, no evidence of such effect was found, neither when considering the entire ICD group, nor when selecting a subgroup of intestinal infections.[9]

[7] Note that in the chemotherapy case, this may also be an issue, but the classification comparison is easier.

[8] The impact was tested for all other age-specific groups, i.e., less than 1 year old, between 1 and 4 years old, between 5 and 9 years old, between 10 and 19 years old, but the results were inconclusive.

[9] Only the following diseases were considered: Other bacterial intestinal infections (A04), Other bacterial foodborne intoxications (A05), Other protozoal intestinal disease (A07), Viral and other specified intestinal infections (A08)

Moreover, for the one-instrument case, there is also no evidence of impact. The results by gender and age are reported in Table 18 for the one-instrument case and in Table 19 for the two-instruments case.

To investigate if there was an increase in the household use of fridges and if that increase was caused by the grid expansion, the researchers regressed the municipality proportion of households with a fridge on the electric-coverage predicted value and the usual controls. For the rural municipalities being studied, the average proportion of households with a fridge in 2000 was 53.8% and it increased to 83.7% in 2010. Among the eligible, the increase was even larger, from 61.8% in 2000 to 80.8% in 2010. Interestingly, for municipalities that were not eligible, the increase was much smaller, from 65.9% in 2000 to 67.1% in 2010, suggesting the importance of being eligible. Table 62 reports the results, which support the hypothesis.

The electric energy access could impact some diseases not yet investigated by the economic literature, such as mosquito-borne fevers[10] and rabies.[11] The former are transmitted by mosquitoes, the latter by bats. The mechanism driving the impacts is the same for all: the use of fans during hot season prevents sleeping with open doors and windows. No evidence of impact on mortality rates was found when the electric coverage time trend was included as a control. For the models not controlling for the electric coverage time trend, a reduction in the mortality rate of mosquito-borne fevers for some gender/age groups was found when considering the two-instruments approach.

If the use of fans and air conditioners can reduce the incidence of diseases such as those cited above, the use of poorly sanitized air conditioners may cause a disease known as Legionnaire's disease[12]. Conversely, no evidence of impact was found. Unfortunately, the data available do not allow testing these drivers without any information on household fan use. Besides, mosquito-borne fevers and rabies can be tested to see if the driver is an increase in vaccine coverage, as shown in a following section. There may nevertheless be impacts on vaccine-preventable diseases.[13] The main channel of impact transmission here is that without electricity, there are no fridges to store vaccines. No evidence was found of impact on vaccine-preventable-diseases mortality rates, but the following subsection reports impacts on hospitalization rates and on vaccine-coverage rates.

and Other Gastroenteritis and colitis of infectious and unspecified origin(A09).

[10] Dengue fever (classical dengue) (A90), Dengue hemorrhagic fever(A91), Other mosquito-borne viral fevers(A92), Yellow fever (A95), Plasmodium falciparum malaria (B50), Plasmodium vivax malaria (B51), Plasmodium malariae (B52), Other parasitological confirmed malaria (B53) and Unspecified malaria (B54).

[11] Rabies (A82).

[12] Other bacterial diseases, not elsewhere classified (A48).

[13] Meningitis, Diphtheria, Hepatitis A, Hepatitis B, Measles, Rubella infection by Haemophilus influenzae type B, Poliomyelitis, and Yellow Fever.

6.2.4 Hospitalization Rate

As for the mortality rates, the electrification impacts over hospitalization rates (in-patient hospital admission) were analyzed in public hospitals and a few private hospitals, by ICD group and, in a few cases, disease-specific analysis. As posed by Rasul e Lautharte (2019), this corresponds to more than 70% of hospitalizations in Brazil.

As the previous subsection shows, perhaps it is still early to find most of the literature impacts on mortality rates. But how did the recent Brazilian rural electrification process impact hospitalization rates? As was done for mortality rates, hospitalization rates were investigated according to ICD, gender, and age. Hospitalization rates can be an indicator for the incidence of diseases in the country, even if the relation is not strictly direct, since the hospitalization rate may increase due to an increase in the provision of public-health services that the electrification process may or may not cause.

To investigate this possibility, i.e., whether the electrification caused an increase in the provision of public-health services, the number of health facilities (Table 60) and the number of hospital beds (Table 61) were regressed on the electric-coverage predicted value. For the health facilities number, a positive electrification impact was found in the specification without controls. When controlling for the agriculture per capita GDP and share of *Bolsa Família* recipients, the effect becomes non-significant, indicating that the increase in health facilities number was actually caused by other public policies implemented in the period. Note, however, that steeper municipalities have fewer health facilities on average.

The outputs analyzed are the same as for the mortality rate. No evidence of impact for respiratory system diseases was found when considering the model specifications that include the share of *Bolsa Família* recipients. This holds for all age- and gender-specific analyses.

The focus was also restricted to a group of specific respiratory diseases.[14] For the specification that does not control for the electric-coverage time trend and the share of *Bolsa Família* recipients, positive impacts are found, indicating that the electrification may cause an increase in hospitalization rate, but the impact vanishes when the controls are added. This pattern holds for both genders and all age-specific groups. Therefore, electrification causes an increase in the hospitalization rate of respiratory infections, an effect that may be driven by the provision of better public health care. As for the specific case of these diseases, the electricity connection makes it possible to use the medical equipment cited in the previous section. As mentioned, that hypothesis cannot be tested specifically. For the two-instruments case, there is no evidence of impacts.

[14] We considered Acute bronchitis and acute bronchiolitis, Chronic sinusitis, Other diseases nose and paranasal sinuses, Other upper respiratory tract diseases, Bronchitis emphysema and other pulmonary diseases, Acute pharyngitis and acute tonsillitis, Acute laryngitis and tracheitis, Other acute upper respiratory infections, Obstructive diseases, Asthma and Other diseases of the respiratory system.

Restricting the attention to a group of intestinal infections, weak evidence[15] of an increase in hospitalization rates was found for female children of less than one year (Table 23). This does not hold for male children within the same age group (Table 21). For older age groups, there is no gender heterogeneity, so only age-specific results are reported. For older age-groups, there is still weak evidence of an increase in hospitalization rates, but as controls are added, the magnitude and the parameters significance decrease, indicating that it may not be a robust result. It was expected that the electrification would cause a reduction in hospitalizations due to intestinal infections in children, but actually an increase for almost all age-groups was found. Still, the increase in hospitalization rates may be the result of an increase in the provision of health services. This issue thus requires future mixed-methods research. For the two-instruments case, there is no evidence of impacts (Tables 20 and 22).

Similar to the finding on mortality rates, a reduction was also found in the admission of neoplasm adult patients (Table 25), with some gender-heterogeneity effect. As reported in Tables 27 and 29, the effects are significant at the level of 5% and larger for women. However, there is no evidence that these results are driven by lung-cancer hospitalizations, as shown in Tables 31, 33, 35 in the more conservative model specification—i.e., the more the control saturated specification, the more the results are not statistically significant. For the one-instrument case, no evidence of impacts was found (Tables 26, 28, 30, 32, 34). Once again, combined quantitative-qualitative research is required to address these questions.

For mosquito-borne fevers, there is evidence of an increase in hospitalizations for almost all age groups in the specifications, which does not control for the share of households in the municipality that receive the conditional cash-transfer program. Controlling for it, the results become statistically non-significant, suggesting that the increase in the previous specification is related with the conditional cash-transfer presence.

For vaccine-preventable diseases, there is no evidence that supports the hypothesis of electrification impact when considering non-adults, but there is weak evidence for adults (Tables 36 and 37). This is a counterintuitive result, as we should expect some impact on children as well. To further investigate the relation between electric energy access and the vaccines, the vaccine coverage variation between 2000 and 2010 was analyzed, along with the number of vaccine-specific doses applied in the country in the period. These results are reported in a following subsection.

6.2.5 Vaccine Coverage

Besides being a driver, vaccine coverage can be interpreted as a health outcome of interest per se. Therefore, the number of type-specific vaccine doses and the total vaccine coverage were analyzed.

[15] Statistic significance at 10% only.

The impact of electrification on the vaccine-specific number of doses was investigated. All available vaccine data was checked on DATASUS, i.e., BCG,[16] Rotavirus,[17] Meningococcus,[18] Hepatitis B,[19] Pneumonia,[20] Polio,[21] Yellow Fever,[22] MMR,[23] and DTP.[24]

No evidence of impact on the number of BCG vaccine doses (Table 41) was found. For the rotavirus vaccine, there is strong evidence suggesting that electrification increases the number of vaccine doses applied, as reported in Table 43. This is an important way to prevent severe diarrhea in children, a common cause of death in developing countries[25]. Moreover, the land-gradient impact is negative, indicating that steeper municipalities had a smaller increase in rotavirus vaccination. This is a pattern that does not hold for all vaccines. For example, the number of doses for the meningococcus vaccine was also positively impacted by the electrification, but with opposite land-gradient dynamics (Table 45). For hepatitis B, there is no evidence supporting the electric coverage impact, even if the municipality land-gradient has a negative impact in all model specifications (Table 47). Interestingly, the impact on the number of pneumonia vaccine doses is negative, as reported in Table 49. For the yellow-fever vaccine, there is weak evidence of positive electrification impact (Table 53) and for MMR, polio, and DTP, there is no evidence of impact (Tables 55, 51 and 57).

Moreover, whether there is any impact on the total vaccine doses (Table 58) and on total vaccine coverage (Table 59) was checked. In both cases, there is strong evidence of positive impacts caused by the increase in electric coverage. In addition, lower average effects are found on steeper municipalities.

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[16] Used against tuberculosis and recommended for newborns in areas with high tuberculosis incidence, such as Brazil, that has an average of 70 thousand cases every year according to the Pan-American Health Organization.

[17] the rotavirus is one of the main responsables for acute diarrheal diseases in childrens with less than 5 years.

[18] The menigogocal disease is an acute and fatal bacterian infection causing meningitis and the vaccine is recommended to all children with more than 2 months

[19] Hepatitis B is a viral infection on the liver and can be prevented by a vaccine, which application in newborns is recommended .

[20] Pneumonia is a respiratory infection caused by a bacteria and its vaccine is recommended to people with more than 80 years old.

[21] Polio is an acute viral infectious disease transmitted from person to person, mainly via fecal-oral, that can attack the nervous system and its vaccine is recommended for all children.

[22] Yellow fever is an infectious disease caused by a virus and transmitted by mosquitoes. Its vaccine is recommended for all individuals living in regions with yellow fever incidence, such as the Brazilian North Region.

[23] Is a vaccine against mumps, rubella and measles. The International Health Organization recommend a first dose to one year old children and a second dose between 15 months and 6 years.

[24] Is a vaccine against three infectious diseases: pertussis, tetanus and diphtheria. It is recommended for children, and for adults every 10 years.

[25] See Banerjee e Duflo (2012).

In the following section, several robustness checks are performed to ensure the validity of the results. Other situations have been evaluated to verify the robustness: the application of the identification strategy to diseases not related *ex-ante* to electrification, and the application to a pre-LpT period, using the 1991 and 2000 Brazilian censuses. The first situation is an attempt to evaluate the robustness of the second-stage estimations, while the second is a way to validate the first-stage identification strategy. Moreover, the results' robustness is analyzed changing the threshold rule determining rural municipalities. In the baseline specification, municipalities with more that 50% of households in rural areas are classified as rural municipalities. Here, other thresholds are evaluated as well. For the second-stage tests, only results for the two-instruments approach are reported; as for the one-instrument case, they are not significant, and the estimated parameters are similar to those resulting from the two-instrument approach.

Another robustness check proposed is to evaluate the results using a categorical instrument, as in this case the identification of compliers is direct. Thus, the continuous instrument is transformed into a dummy by creating a dummy equal to one if the municipality slope is lower than the municipality land-gradient median. As reported in Tables 63, 64 and 65, when considering only the electric coverage results, there is a larger effect when considering the categorical instrument, even if it is only significant at 10%. For the other household infrastructure variables, both the triple and the double interaction are not statistically significant. However, the excluded instrument F Statistic suggests that when considering the triple interaction as the only instrument, it is not a strong one. Second-stage estimation results will not be reported since the instruments are not valid, but they are quite similar to the results considering the continuous instrument. However, coarsening a continuous treatment into a binary variable can substantially upwardly bias the IV estimates by violating the exclusion-restriction assumption, as demonstrated by Marshall (2016).

One possible concern regarding electrification processes is the possibility of migration. It should be expected that municipalities connected to the grid would attract citizens from the nearby municipalities not yet connected. To investigate if this actually happened in this case study, the proportion of fixed households was regressed on the electric coverage predicted value, and no evidence of such stylized fact was found (Table 66).

To illustrate the electric-coverage variation between 1991 and 2000, two maps were plotted in Figure 11. Note that as the 2010 municipality shapefile was used, some municipalities did not appear in the maps, as they did not exist back in 1991. This is not an issue for the estimations since the minimum comparable areas were used between 1991 and 2000. The 85% coverage rule was considered applied for the municipalities' electric coverage in 1991. Moreover,

as the 1991 household controls are different from those used for 2000 and 2010, as in 1991 there was no conditional cash-transfer program, and there are no data for the agricultural per capita GDP for the year, Table 67 reports only the first two model specifications.

The triple interaction between the $Year$, $Eligible$ and $Land\ Gradient$ is not significative, and the excluded instrument F statistic suggests that this is not a good instrument, as was expected. However, the interaction between $Eligible$ and $Year$ is significant and the excluded instruments F statistic suggests that the instruments are strongly correlated with the municipality electric coverage. Perhaps the $Eligible$ and $Year$ interaction validity as instrument in this context is due to the increase in electricity coverage in the period analyzed. In 1991, the average electric coverage among rural[1] Brazilian municipalities was 53.8% and that number increased to 77.1% in 2000, a variation that is proportionally larger than the one observed between 2000 and 2010.

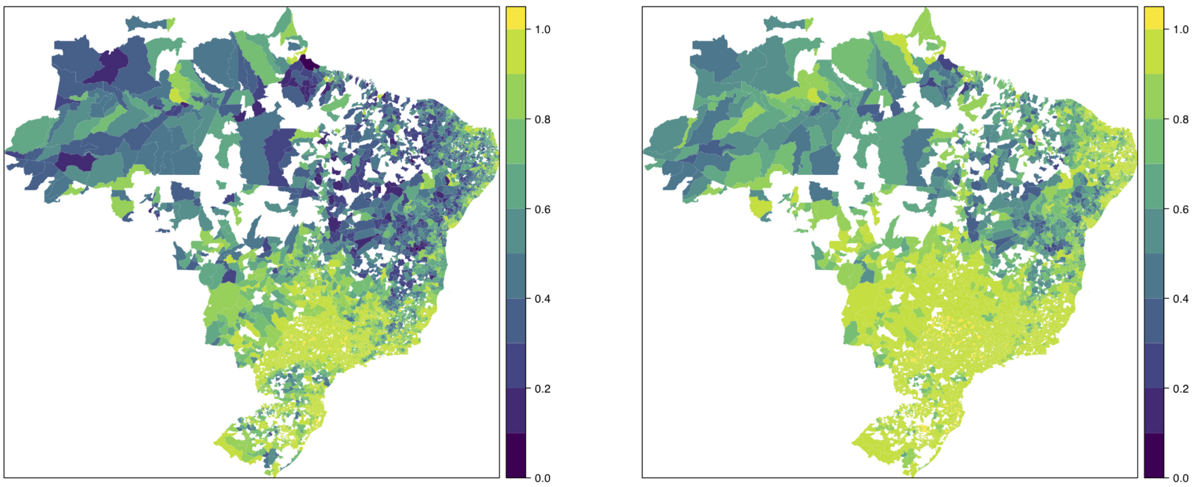

Figure 11 – Municipality access to electricity in 1991 (left) and 2000 (right)
Source: IBGE

Regarding the threshold considered to classify municipalities as rural, as a robustness check the same estimations presented above were performed considering two other thresholds: one more slack (25%) and another more strict (75%).

First, considering that every municipality with more than 25% of its households in rural areas is considered a rural municipality, a lower triple interaction magnitude effect and the same signs as in the main estimation first stage should be expected. Table 68 supports these expectations. In fact, when considering this extended set of rural municipalities, a slightly smaller effect for the triple interaction is found, but an unexpected positive sign for the interaction between $Year$ and $Land\ Gradient$.

[1] Recall that “rural” was considered to be every municipality with more than 50% of its households in rural areas.

For the stricter rural threshold, only those municipalities with more than 75% of households in rural areas were considered as rural. In this case, it should be expected that the instruments effects are larger, even if perhaps not as statistically significant as in the main first-stage estimates, as the number of municipalities drops significantly. Endorsing these expectations, Table 69 shows that for an increase of one land-gradient degree, there is a drop of 14 percentage points in electric coverage. The results for other household infrastructure appliances evaluated are not significant.

As diseases not related to electric-energy access *ex-ante*, cardiac insufficiency, heart attack, stroke, obesity, and diabetes were considered, both for mortality and hospitalization rates. Besides a reduction in diabetes mortality rate in adults (see Table 70) and a reduction in heart attacks mortality rate in adults (see Table 71), as expected, no evidence was found of electrification impacts. However, the diabetes mortality rate impact is only significant at the 10% level and is very small (less than 0.3 standard deviation). The impact on heart attacks mortality rate is significant at the 5% level and large, i.e., for an increase in one percentage point in municipality electric coverage, there is a reduction of almost one standard deviation in the heart-attack mortality rate. This was not an expected result, and future mixed-methods research is needed to understand its drivers.

8 Final Remarks

This paper was an attempt to bring new evidence to the literature by assessing the impacts of access to electric power in Brazilian households into health outcomes. As shown in the literature review, even if recent studies has already appointed to some positive impacts of electrification, the mechanisms that underlie these effects were unclear. Therefore, our study adds to the literature presenting evidence of rural electrification effects for Brazil. Moreover, non-usual health outcomes were also analyzed.

An instrumental variable difference-in-differences approach was adopted, exploring a federal electrification program as source of exogenous variation in the probability of being connected to the grid. Additionally, the municipality land gradient was also explored as source of exogenous variation, since the electrification program eligibility rule was not sharp.

The main findings were in line with international literature as we are not able to find evidence of electrification impacts on health outcomes.As a methodological exercise only, we flexibilize the instrument hypothesis and find that electrification reduces the incidence and mortality rate of some respiratory diseases with relevant gender-heterogeneity. In addition, it has negative impacts on cancer mortality rate. An unexpected result was that the electrification did not have any effect on intestinal infections.

A possible source of heterogeneity on unobservables was also investigated using a marginal treatment effect inspired approach. For that matter, considering the study case particularities, a specific method was explored, and both positive and negative aspects of it were addressed. The results validate the possibility of selection on unobservables and bring new evidence that explain null average treatment effects.

For future research on this topic, mixed-methods research should be considered. This approach would allow to address with more details the mechanisms driving the impacts found.

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Appendix

APPENDIX A – Heterogeneity on unobservables

Considering the triple interaction as the only instrumental variable, it has already been shown that there is no evidence of electrification impacts on the health outcomes analyzed. The reason for this may be two-fold: first, there may actually be no effect for the group of compliers analyzed; and second, there may be no average effect, but there could be some kind of heterogeneity among municipalities caused by unobservables.

This heterogeneity possibility is therefore investigated by adopting a marginal treatment effect inspired approach.[1]

A.1 Model

We build on the generalized Roy model and follow the general framework based on the potential outcomes model and a latent variable selection model, as initially proposed by Heckman e Vytlacil (1999). As described by Zhou e Xie (2018), in the marginal treatment effect framework, the latent variable selection model guarantees that the unobserved part of treatment status determination is expressed by a single latent variable. Moreover, it ensures that the treatment effect variation caused by the latent variable explains all the unobserved treatment effect heterogeneity possibly causing selection bias.

Therefore, the treatment variable is denoted as D_{hm}, where $D_{hm} = 1$ indicates that household h in municipality m was treated, and $D_{hm} = 0$ indicates the opposite. Moreover, let Y_{1hm} be the municipality potential outcome in case of treatment and Y_{0hm} the municipality potential outcome if not treated. To be more precise, these are modeled as:

$$Y_{0hm} = \mu_0(X_{hm}) + U_{0hm} \tag{A.1}$$

$$Y_{1hm} = \mu_1(X_{hm}) + U_{1hm} \tag{A.2}$$

where $\mu_{jm}(X_{hm})$ is the treatment state $j \in \{0; 1\}$ conditional mean of Y_{jhm} given a covariates vector, X_{hm}. Additionally, the identification of this model requires the conditional independence assumption, i.e., $(U_{0hm}, U_{1hm}, V_{hm}) \perp Z_{hm}|X_{hm}$. The assumptions made so far are the same assumed for the LATE identification [2]. Moreover, as full common support of the propensity scores in both treated and control groups for all values of X is rarely feasible, it is usual to assume linear separability, i.e., $\mathbb{E}[U_{jhm}|X_{hm}] = 0$. [3] Therefore, full independence is assumed,

[1] See Bjorklund e Moffitt (1987), Heckman e Vytlacil (1999), Heckman e Vytlacil (2005), Heckman e Vytlacil (2007a) and Heckman e Vytlacil (2007b).
[2] See Imbens e Angrist (1994).
[3] See Carneiro, Heckman e Vytlacil (2011).

i.e., $(X_{hm}, Z_{hm}) \perp (U_{0hm}, U_{1hm}, V_{hm})$. In this way, the shape of the marginal treatment effect curve will be independent of X, so the covariates determine only the intercept. This is a stronger assumption than the one needed for the LATE estimation that required only the orthogonality between the instrument and the errors conditional on the covariates, i.e., $Z_{hm} \perp (U_{0hm}, U_{1hm}, V_{hm})|X_{hm}$. The separability assumption imply that is possible to indentify the marginal treatment effect over the common support unconditionally and that the shape of the marginal treatment effect is independent of the covariates.

The latent variable selection model is defined as follows:

$$D^*_{hm} = \mu_D(X_{hm}, Z_{hm}) - V_{hm} \tag{A.3}$$

$$D_{hm} = \begin{cases} 1 & \textit{if} \quad D^*_{hm} \geq 0 \\ 0 & \textit{if} \quad D^*_{hm} < 0 \end{cases} \quad \Rightarrow D_{hm} = \begin{cases} 1 & \textit{if} \quad \mu_D(X_{hm}, Z_{hm}) \geq V_{hm} \\ 0 & \textit{if} \quad \mu_D(X_{hm}, Z_{hm}) < V_{hm} \end{cases} \tag{A.4}$$

where D^*_{hm} is the latent propensity to take the treatment, Z_{hm} is our instrumental variable, and V_{hm} is an i.i.d error term that indicates unobserved heterogeneity in the selection of treatment. Note that we can refer to V_{hm} as the unobserved distaste for treatment, as it enters the selection equation with a negative sign. Applying the *c.d.f* of V_{hm} i.e., F_V, to both sides of $\mu_D(X_{hm}, Z_{hm}) \geq V_{hm}$ enables bounding both sides within the unit interval. Thus, a municipality will be selected into treatment if $F_V(\mu_D(X_{hm}, Z_{hm})) \geq F_v(V_{hm})$. As proposed by Cornelissen et al. (2016), $F_V(\mu_D(X_{hm}, Z_{hm}))$ can be defined as the propensity score, denoted by $P(X_{hm}, Z_{hm})$, and $F_v(V_{hm})$ as the quantiles of unobserved distaste for treatment distribution, denoted by U_{Dhm}. Therefore,

$$D_{hm} = \begin{cases} 1 & \textit{if} \quad P(X_{hm}, Z_{hm}) \geq U_{Dhm} \\ 0 & \textit{if} \quad P(X_{hm}, Z_{hm}) < U_{Dhm} \end{cases} \tag{A.5}$$

From the separability assumption we have:

$$\begin{aligned} \mathbb{E}[Y|X_{hm} = x, P(Z_{hm} = p)] &= \mathbb{E}[Y_{0hm} + D(Y_{1hm} - Y_{0hm})|X_{hm} = x, P(Z_{hm} = p)] \\ &= x\xi_0 + x(\xi_1 - \xi_0)p + p\mathbb{E}[U_{1hm} - U_{0hm}|U_{Dhm} \leq u] \end{aligned}$$

where it is assumed that X_{hm} and Z_{hm} conditional means are linear, i.e, $\mu_D(X_{hm}) = X\xi_j$ and $\mu_D(Z_{hm}) = Z\psi_j$. The marginal treatment effect can thus be defined as the expected treatment effect conditional on the covariates and the normalized latent variable, as follows,

$$\begin{aligned} MTE(x, u) &= \mathbb{E}[Y_{1hm} - Y_{0hm}|X_{hm} = x, U_{Dhm} = u] \\ &= x(\xi_1 - \xi_0) + \mathbb{E}[U_{1hm} - U_{0hm}|U_{Dhm} = u] \end{aligned}$$

Note that $x(\xi_1 - \xi_0)$ represents the heterogeneity on observables while $\mathbb{E}[U_{1hm} - U_{0hm} | U_{Dhm} = u]$ represents the heterogeneity on unobservables.

Since U_{Dhm} represents the quantile of the distribution of V_{hm}, the marginal treatment effect evaluation over values of u reflects the treatment effect heterogeneity in the unobserved resistance to treatment.[4] Thus, for u close to zero, the marginal treatment effect is the treatment effect expected on individuals with a value of unobservables that makes them have low resistance to treatment.[5]

Given the usual independence and rank conditions, we can identify the marginal treatment effect using a local instrumental variables approach.[6] Thus, for any (x, u) within the support of the joint distribution of X and $P(Z)$:

$$MTE(x,u) = \frac{\partial \mathbb{E}[Y_{hm} | X_{hm} = x, P(Z) = p]}{\partial p} \text{, evaluated at } p = u \tag{A.6}$$

As there is no health data available at the household level, it is not possible to implement the model described above. Moreover, note that departing from a selection model from the individual perspective, where the treatment is binary, to a municipality aggregated perspective, the treatment becomes continuous and this change has to be taken into account. Therefore, as shown by Cornelissen et al. (2018), we could extend the original generalized Roy model to an ordered choice model, making the assumptions that it is possible to transform the continuous treatment into discrete, and that the errors in the selection and outcome equations are jointly normally distributed.[7]

As our interest is to explore the possibility of heterogeneity on unobservables, to keep it simple we will ignore the ordered choice model and assume a discrete treatment at the municipality level. This means that we will consider a municipality as treated if it was eligible due to the 85% LpT electric coverage rate rule. In this case we can only identify a weighted average of treatment unit changes on multiple complier groups that are defined by the relation between treatment and instrument across the treatment support. This approach is similar to a Regression Discontinuity Design method but it allows to estimate the complete treatment effects distribution on unobservables.

4 See Zhou e Xie (2018).

5 See Heckman e Vytlacil (2007b).

6 See Heckman e Vytlacil (1999).

7 In this case, instead of the traditional selection model in A.4 we will have:

$$D_{hm} = \begin{cases} 0 & \textit{if} \quad \mu_D(X_{hm}, Z_{hm}) - V_{hm} \leq \kappa_1 \\ 1 & \textit{if} \quad \kappa_1 < \mu_D(X_{hm}, Z_{hm}) - V_{hm} \leq \kappa_2 \\ 2 & \textit{if} \quad \kappa_2 < \mu_D(X_{hm}, Z_{hm}) - V_{hm} \end{cases}$$

where for simplicity we are distretizing the continuous treatment into three values and κ_1 and κ_2 are two thresholds that can depend on the regressors or be constants. In this ordered choice model there is a potential outcome for each one of the treatment values.

Note however that this procedure may upwardly bias the IV estimates by a violation of the exclusion restriction assumption as first demonstrated by Angrist e Imbens (1995) and more recently analyzed by Marshall (2016) and Andresen e Huber (2018). As shown by Andresen e Huber (2018), the binarization introduces exclusion restriction violations if the instrument affects the continuous treatment differently above and below the cutoff and these instrument-induced changes in the treatment affect the outcome of interest, i.e., if there are compliers that are not switched into treatment by the threshold definition and the treatment variation induced by the instrument affects the outcome of this group. In our case, we argue that this should not be a concern as we choose to discretize the treatment at the same threshold used as eligibility rule by the LpT program and there should not be any difference in the land gradient effect on electric coverage rate between eligibles an non-eligibles.

Therefore, by proceeding with the treatment binarization we identify a weighted average of marginal treatment effects, as shown by Hoderlein e Sasaky (2011). Nonetheless, this identified parameter may be affected by any continuous treatment shift induced by the instrument to cross the threshold, rather than the local impacts at the threshold only, as in a LATE approach. That is, we can only identify the sum of unit changes impacts that are weighted by the probability that these impacts occur among compliers when they are induced into the binary treatment by the instrument.[8]

In the rural electrification case analyzed in this paper, the selection of household h in municipality m and time t can be modelled into treatment by the latent propensity to treatment as follows:

$$\begin{aligned} D^*_{hmt} = {} & \alpha_0 + \alpha_1 R_m + \alpha_2 C_m + \alpha_3 Y_{2010} + \alpha_4 (R_m * C_m) + \alpha_5 (R_m * Y_{2010}) \\ & + \alpha_6 (C_m * Y_{2010}) + \alpha_7 (R_m * C_m * Y_{2010}) + \alpha_8 X_{hmt} + \theta_m + \varepsilon_{hmt} \end{aligned} \tag{A.7}$$

where recall that R_m is a dummy variable indicating the 85% electric-coverage rule, C_m is the cost-related instrument (land gradient), Y_{2010} is a dummy variable equal to one for 2010, X_{hmt} is a vector of covariates,[9] θ_m are the municipality fixed effects and ε_{hmt} is the idiosyncratic error term.

Aggregating for municipality m yields:

$$\begin{aligned} D^*_{mt} = {} & \beta_0 + \beta_1 R_m + \beta_2 C_m + \beta_3 Y_{2010} + \beta_4 (R_m * C_m) + \beta_5 (R_m * Y_{2010}) \\ & + \beta_6 (C_m * Y_{2010}) + \beta_7 (R_m * C_m * Y_{2010}) + \beta_8 \bar{X}_{mt} + \theta_m + \bar{\varepsilon}_{mt} \end{aligned} \tag{A.8}$$

8 See Andresen e Huber (2018).

9 The vector of covariates include the average years of schooling, an indicator for sewage, the municipality proportion of *Bolsa Família* recipients, an indicator for garbage collection and the municipality per capita agricultural GDP.

where $\bar{X}_{mt}$ represents the municipality covariates vector. Taking the first difference, the municipality electric coverage variation can be modeled between 2000 and 2010 as:

$$\Delta D^*_m = \beta_3 + \beta_5 R_m + \beta_6 C_m + \beta_7 (R_m * C_m) + \beta_8 \Delta \bar{X}_m + \Delta \bar{\varepsilon}_m \tag{A.9}$$

As the treatment is binarazed at the eligibility rule cutoff, we actually get that:

$$\Delta D^*_m = \beta_3 + \beta_6 C_m + \beta_8 \Delta \bar{X}_m + \Delta \bar{\varepsilon}_m \tag{A.10}$$

To estimate the marginal treatment effects a local IV is implemented. [10] The marginal treatment effects are thus identified as described by equation A.6. The estimation procedure starts by the selection into treatment identification (equation A.5) using a probability model (such as probit in the case analyzed in this paper) to estimate the propensity score. The next step is to assume a functional form for $p\mathbb{E}[U_{1hm} - U_{0hm} | U_{Dhm} \leq u]$ to estimate the outcome conditional expectation and calculate its derivative.[11] In the following analysis we assume that U_{1hm}, U_{0hm} and V_{hm} have a joint normal distribution, i.e., $(U_{1hm}, U_{0hm}, V_{hm}) \sim \mathcal{N}(0, \Sigma)$, where,

$$\Sigma = \begin{bmatrix} \sigma^2_{U_{1hm}} & \rho_{01hm} & \rho_{0hm} \\ \rho_{01hm} & \sigma^2_{U_{0hm}} & \rho_{1hm} \\ \rho_{0hm} & \rho_{1hm} & 1 \end{bmatrix}$$

A.2 Results

Considering the above model specification, the marginal treatment effects were estimated for fertility and mortality rates. Unlike the other regressions reported in the paper, here the total mortality rate is considered, with no distiction by gender, age or disease.

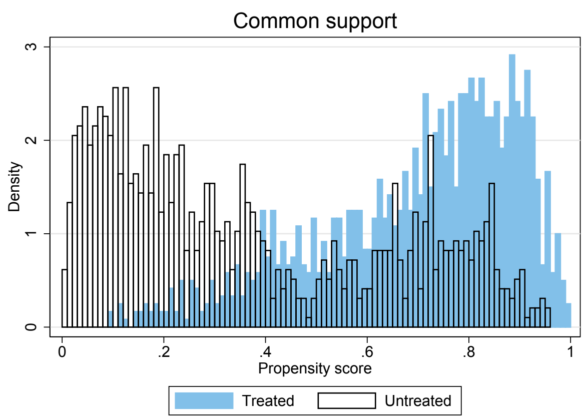

Figure 12 – Common support

[10] See Heckman e Vytlacil (1999).
[11] See Andresen (2018).

As reported by Figure 12 the estimated propensity score indicates, as expected, a larger probability to take the treatment for municipalities that were actually treated. Moreover, there are untreated municipalities with close to the unit propensity score. This is a result from the common support assumption.

For both outcomes analyzed an heterogeneity on unobservables pattern is found, as shown by Figure 13. Municipalities with lower unobserved resistance to treatment have positive electrification impacts, while for high-resistance municipalities the effect is negative. This indicates that municipalities less likely to enroll into treatment are the ones for which a reduction in fertility and mortality rates is observed. Additionally, since for the reported results X is fixed at a given value (at means), the resulting heterogeneity can be higher if the covariates variation is taken into account.[12] Note that the average treatment effect is almost null for both outcomes. The 95% confidence interval is based on bootstrapped standard errors.

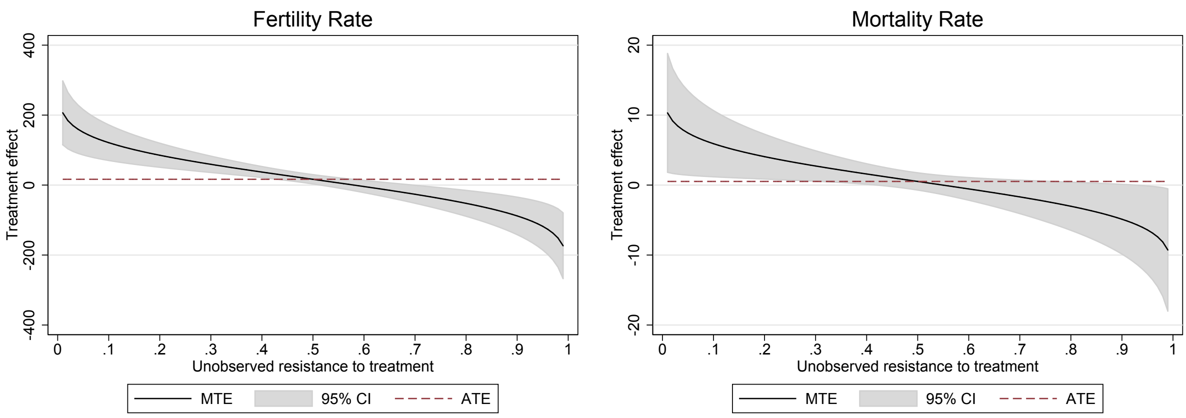

Figure 13 – Marginal Treatment Effects

[12] See Cornelissen et al. (2016).

APPENDIX B – First Stage Tables

Table 3 – First Stage - Electric Coverage

	Dependent variable:			
	Electric Coverage			
	(1)	(2)	(3)	(4)
Year 2010	0.056***	0.125**	0.051	0.028
	(0.003)	(0.058)	(0.058)	(0.056)
Eligible * Year * Gradient	−0.008***	−0.008***	−0.009***	−0.009***
	(0.002)	(0.002)	(0.002)	(0.002)
Eligible * Year	0.283***	0.277***	0.257***	0.240***
	(0.009)	(0.011)	(0.011)	(0.010)
Gradient * Year	−0.002***	−0.002***	0.001	0.002***
	(0.0005)	(0.0005)	(0.001)	(0.001)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Coverage * Time Trend	No	Yes	Yes	Yes
Agric. per capita GDP and Bolsa Família	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.933	0.933	0.937	0.940
Adjusted R^2	0.865	0.865	0.873	0.879
Residual Std. Error	0.068 (df = 2172)	0.068 (df = 2171)	0.066 (df = 2169)	0.064 (df = 2163)
Excluded Instrument (Elig.*Year*Grad.) F Stat.	14.211*** (df = 2173; 2172)	12.937*** (df = 2172; 2171)	20.714*** (df = 2170; 2169)	20.916*** (df = 2164; 2163)
Excluded Instruments (Elig.*Year*Grad. and Elig.*Year) F Stat.	1112*** (df = 2174; 2172)	650.27*** (df = 2173; 2171)	537.67*** (df = 2171; 2169)	511.56*** (df = 2165; 2163)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 4 – First Stage - Sewage disposal

	Dependent variable:			
	Sewage disposal			
	(1)	(2)	(3)	(4)
Year 2010	0.092***	−0.341***	−0.259**	−0.249**
	(0.016)	(0.121)	(0.119)	(0.121)
Eligible * Year * Gradient	0.005	0.004	0.005	0.005
	(0.005)	(0.005)	(0.005)	(0.005)
Eligible * Year	−0.125***	−0.087***	−0.064**	−0.061**
	(0.023)	(0.025)	(0.025)	(0.026)
Gradient * Year	0.001	0.001	−0.001	−0.001
	(0.003)	(0.003)	(0.003)	(0.003)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Coverage * Time Trend	No	Yes	Yes	Yes
Agric. per capita GDP and Bolsa Família	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.769	0.774	0.777	0.777
Adjusted R^2	0.537	0.546	0.552	0.552
Residual Std. Error	0.158 (df = 2172)	0.157 (df = 2171)	0.156 (df = 2169)	0.156 (df = 2164)
Excluded Instrument (Elig.*Year*Grad.) F Stat.	1.296 (df = 2173; 2172)	0.6156 (df = 2172; 2171)	1.3261 (df = 2170; 2169)	0.9929 (df = 2164; 2163)
Excluded Instruments (Elig.*Year*Grad. and Elig.*Year) F Stat.	29.642*** (df = 2173; 2172)	11.638*** (df = 2172; 2171)	3.889* (df = 2170; 2169)	3.457* (df = 2165; 2164)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 5 – First Stage - Piped Water

	Dependent variable:			
	Piped Water			
	(1)	(2)	(3)	(4)
Year 2010	0.195***	−0.006	−0.012	−0.080
	(0.015)	(0.057)	(0.060)	(0.058)
Eligible * Year * Gradient	0.003	0.002	0.002	0.002
	(0.003)	(0.003)	(0.003)	(0.003)
Eligible * Year	−0.018	0.0001	−0.002	−0.009
	(0.018)	(0.019)	(0.018)	(0.018)
Gradient * Year	−0.005**	−0.005**	−0.005**	−0.004*
	(0.002)	(0.002)	(0.002)	(0.002)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Coverage * Time Trend	No	Yes	Yes	Yes
Agric. per capita GDP and Bolsa Família	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.860	0.861	0.861	0.867
Adjusted R^2	0.720	0.722	0.722	0.732
Residual Std. Error	0.113 (df = 2172)	0.113 (df = 2171)	0.113 (df = 2169)	0.111 (df = 2164)
Excluded Instrument (Elig.*Year*Grad.) F Stat.	0.9673 (df = 2173; 2172)	0.5649 (df = 2172; 2171)	0.5216 (df = 2170; 2169)	0.3256 (df = 2164; 2163)
Excluded Instruments (Elig.*Year*Grad. and Elig.*Year) F Stat.	0.5457 (df = 2173; 2172)	0.7285 (df = 2172; 2171)	0.485 (df = 2170; 2169)	0.1752 (df = 2165; 2164)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

APPENDIX C – Second Stage Tables

C.1 Fertility Rate

Table 6 – Electrification effect on Fertility Rate - One instrument case

	Dependent variable: Fertility Rate			
	(1)	(2)	(3)	(4)
$Electr\hat{i}cCov.$	24.672	1.876	12.697	15.825
	(54.098)	(55.514)	(46.095)	(47.409)
Year 2010	-11.388^{***}	38.313^{**}	34.942^{**}	32.007^{**}
	(3.768)	(16.569)	(15.030)	(14.387)
Eligible * Year	-2.021	0.159	-3.054	-4.406
	(13.705)	(13.743)	(10.282)	(9.856)
Gradient * Year	-0.011	-0.025	0.080	0.154
	(0.260)	(0.260)	(0.194)	(0.186)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.679	0.691	0.692	0.696
Adjusted R^2	0.358	0.382	0.382	0.389
Residual Std. Error	13.673 (df = 2172)	13.415 (df = 2171)	13.405 (df = 2169)	13.334 (df = 2163)

Note: Standard Errors clustered at the municipality level. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Table 7 – Electrification effect on Fertility Rate - Two instruments case

	Dependent variable: Fertility Rate			
	(1)	(2)	(3)	(4)
$Electr\hat{i}cCov.$	-0.046	-0.022	0.045	0.136
	(0.191)	(0.190)	(0.189)	(0.192)
Year 2010	-10.843^{***}	38.223^{***}	35.928^{**}	32.942^{**}
	(1.427)	(14.453)	(14.703)	(14.399)
Eligible * Year	16.711^{***}	2.515	-0.865	-5.144
	(4.720)	(5.704)	(6.888)	(7.649)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.681	0.691	0.692	0.697
Adjusted R^2	0.361	0.382	0.383	0.390
Residual Std. Error	13.640 (df = 2173)	13.410 (df = 2172)	13.402 (df = 2170)	13.319 (df = 2164)

Note: Standard Errors clustered at the municipality level. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

C.2 Mortality Rate

Table 8 – Electrification effect on Mortality Rate - Neoplasms - More than 20 years old - Total - One instrument case

	Dependent variable:			
	Total			
	(1)	(2)	(3)	(4)
$\widehat{ElectricCov.}$	-40.751	-32.241	-46.110	-46.344
	(43.513)	(44.444)	(37.881)	(39.533)
Year 2010	18.978***	0.424	6.246	4.174
	(3.758)	(7.635)	(5.895)	(5.546)
Eligible * Year	1.788	0.974	5.787	5.875
	(10.860)	(10.865)	(8.343)	(8.199)
Gradient * Year	-0.120	-0.114	-0.278	-0.253
	(0.328)	(0.328)	(0.260)	(0.232)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.810	0.814	0.810	0.812
Adjusted R^2	0.619	0.628	0.619	0.622
Residual Std. Error	11.212 (df = 2172)	11.086 (df = 2171)	11.220 (df = 2169)	11.175 (df = 2163)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 9 – Electrification effect on Mortality Rate - Neoplasms - More than 20 years old - Total - Two instruments case

	Dependent variable:			
	Total			
	(1)	(2)	(3)	(4)
$\widehat{ElectricCov.}$	-33.707***	-28.318***	-20.412***	-18.382***
	(4.400)	(4.580)	(4.331)	(4.849)
Year 2010	18.495***	-0.128	4.378	2.928
	(1.677)	(4.058)	(4.536)	(4.696)
Gradient * Year	-0.089	-0.098	-0.212	-0.229
	(0.200)	(0.199)	(0.203)	(0.213)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.813	0.815	0.819	0.822
Adjusted R^2	0.625	0.630	0.638	0.642
Residual Std. Error	11.134 (df = 2173)	11.051 (df = 2172)	10.933 (df = 2170)	10.868 (df = 2164)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 10 – Electrification effect on Mortality Rate - Respiratory system diseases - More than 20 years old - Total - One instrument case

	Dependent variable:			
	Total			
	(1)	(2)	(3)	(4)
$\widehat{ElectricCov.}$	14.489	20.102	11.916	14.646
	(35.278)	(37.012)	(30.623)	(32.272)
Year 2010	6.671**	−5.567	−1.609	−0.819
	(2.894)	(6.817)	(4.432)	(3.954)
Eligible * Year	−7.397	−7.934	−4.852	−4.704
	(8.773)	(8.963)	(6.627)	(6.463)
Gradient * Year	0.158	0.162	0.055	−0.014
	(0.250)	(0.251)	(0.198)	(0.179)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.747	0.744	0.751	0.752
Adjusted R^2	0.493	0.488	0.501	0.502
Residual Std. Error	8.609 (df = 2172)	8.659 (df = 2171)	8.542 (df = 2169)	8.537 (df = 2163)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 11 – Electrification effect on Mortality Rate - Respiratory system diseases - More than 20 years old - Total - Two instruments case

	Dependent variable:			
	Total			
	(1)	(2)	(3)	(4)
$\widehat{ElectricCov.}$	−14.654***	−11.839***	−9.630**	−7.740*
	(3.240)	(3.519)	(3.765)	(4.440)
Year 2010	8.667***	−1.073	−0.042	0.178
	(1.222)	(3.160)	(3.252)	(3.268)
Gradient * Year	0.030	0.026	0.0003	−0.033
	(0.153)	(0.153)	(0.159)	(0.170)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.753	0.754	0.755	0.756
Adjusted R^2	0.504	0.507	0.508	0.510
Residual Std. Error	8.515 (df = 2173)	8.492 (df = 2172)	8.483 (df = 2170)	8.468 (df = 2164)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 12 – Electrification effect on Mortality Rate - Emphysema - More than 20 years old - Male - One instrument case

	Dependent variable:			
	Male			
	(1)	(2)	(3)	(4)
$\widehat{ElectricCov.}$	1.083	1.105	1.117*	1.124
	(0.777)	(0.807)	(0.674)	(0.689)
Year 2010	−0.087	−0.135	−0.138	−0.104
	(0.061)	(0.153)	(0.115)	(0.111)
Eligible * Year	−0.261	−0.263	−0.266*	−0.258*
	(0.195)	(0.199)	(0.149)	(0.143)
Gradient * Year	0.002	0.002	0.002	0.001
	(0.005)	(0.005)	(0.004)	(0.003)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.534	0.531	0.530	0.533
Adjusted R^2	0.066	0.060	0.057	0.060
Residual Std. Error	0.188 (df = 2172)	0.189 (df = 2171)	0.189 (df = 2169)	0.189 (df = 2163)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 13 – Electrification effect on Mortality Rate - Emphysema - More than 20 years old - Male - Two instruments case

	Dependent variable:			
	Male			
	(1)	(2)	(3)	(4)
$\widehat{ElectricCov.}$	0.056	0.048	−0.063	−0.102
	(0.069)	(0.073)	(0.069)	(0.077)
Year 2010	−0.016	0.013	−0.052	−0.050
	(0.026)	(0.078)	(0.081)	(0.086)
Gradient * Year	−0.002	−0.002	−0.001	−0.0003
	(0.003)	(0.003)	(0.003)	(0.003)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.595	0.595	0.598	0.601
Adjusted R^2	0.189	0.188	0.194	0.197
Residual Std. Error	0.175 (df = 2173)	0.176 (df = 2172)	0.175 (df = 2170)	0.175 (df = 2164)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 14 – Electrification effect on Mortality Rate - Emphysema - More than 20 years old - Female - One instrument case

	Dependent variable:			
	Female			
	(1)	(2)	(3)	(4)
$Electr\hat{i}cCov.$	−0.767	−0.728	−0.760	−0.511
	(1.360)	(1.402)	(1.233)	(1.254)
Year 2010	0.234*	0.149	0.150	0.198
	(0.135)	(0.250)	(0.189)	(0.174)
Eligible * Year	0.083	0.079	0.084	0.018
	(0.326)	(0.330)	(0.269)	(0.259)
Gradient * Year	−0.012	−0.012	−0.012	−0.011
	(0.012)	(0.012)	(0.010)	(0.008)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.517	0.518	0.517	0.524
Adjusted R^2	0.033	0.034	0.032	0.043
Residual Std. Error	0.390 (df = 2172)	0.390 (df = 2171)	0.390 (df = 2169)	0.388 (df = 2163)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 15 – Electrification effect on Mortality Rate - Emphysema - More than 20 years old - Female - Two instruments case

	Dependent variable:			
	Female			
	(1)	(2)	(3)	(4)
$Electr\hat{i}cCov.$	−0.442***	−0.411**	−0.386**	−0.425**
	(0.164)	(0.171)	(0.176)	(0.192)
Year 2010	0.212***	0.104	0.123	0.194
	(0.065)	(0.123)	(0.132)	(0.154)
Gradient * Year	−0.011	−0.011	−0.011	−0.011
	(0.007)	(0.007)	(0.008)	(0.008)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.522	0.522	0.523	0.525
Adjusted R^2	0.043	0.043	0.043	0.045
Residual Std. Error	0.388 (df = 2173)	0.388 (df = 2172)	0.388 (df = 2170)	0.387 (df = 2164)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 16 – Electrification effect on Mortality Rate - Endocrine, nutritional and metabolic diseases - More than 20 years old - Total - One instrument case

	Dependent variable:			
	Total			
	(1)	(2)	(3)	(4)
$Electr\hat{i}cCov.$	−37.494	−34.306	−24.774	−24.958
	(24.450)	(24.945)	(19.827)	(20.480)
Year 2010	10.017***	3.066	−1.398	−1.895
	(2.008)	(4.755)	(3.100)	(3.011)
Eligible * Year	6.984	6.680	3.158	3.210
	(6.130)	(6.115)	(4.382)	(4.224)
Gradient * Year	−0.250	−0.248	−0.127	−0.125
	(0.170)	(0.170)	(0.131)	(0.114)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.625	0.636	0.663	0.663
Adjusted R^2	0.249	0.270	0.325	0.323
Residual Std. Error	6.221 (df = 2172)	6.135 (df = 2171)	5.899 (df = 2169)	5.906 (df = 2163)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 17 – Electrification effect on Mortality Rate - Endocrine, nutritional and metabolic diseases - More than 20 years old - Total - Two instruments case

	Dependent variable:			
	Total			
	(1)	(2)	(3)	(4)
$Electr\hat{i}cCov.$	−9.977***	−7.414***	−10.751***	−9.681***
	(2.255)	(2.420)	(2.747)	(3.164)
Year 2010	8.132***	−0.717	−2.418	−2.576
	(0.847)	(2.448)	(2.404)	(2.538)
Gradient * Year	−0.130	−0.134	−0.091	−0.112
	(0.104)	(0.103)	(0.103)	(0.106)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.675	0.678	0.679	0.680
Adjusted R^2	0.349	0.355	0.356	0.357
Residual Std. Error	5.791 (df = 2173)	5.764 (df = 2172)	5.759 (df = 2170)	5.755 (df = 2164)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 18 – Electrification effect on Mortality Rate - Intestinal infections - Less that 1 year old - Total - One instrument case

	Dependent variable:			
	Total			
	(1)	(2)	(3)	(4)
$\hat{ElectricCov.}$	12.016	10.720	8.047	8.510
	(9.381)	(9.589)	(7.680)	(8.023)
Year 2010	-1.531^{**}	1.295	2.852	3.019^{*}
	(0.673)	(2.264)	(1.855)	(1.827)
Eligible * Year	-2.960	-2.836	-1.707	-1.704
	(2.384)	(2.382)	(1.728)	(1.678)
Gradient * Year	0.073	0.072	0.032	0.019
	(0.048)	(0.048)	(0.034)	(0.033)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.474	0.490	0.523	0.521
Adjusted R^2	-0.054	-0.023	0.044	0.037
Residual Std. Error	2.333 (df = 2172)	2.298 (df = 2171)	2.222 (df = 2169)	2.229 (df = 2163)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 19 – Electrification effect on Mortality Rate - Intestinal infections - Less that 1 year old - Total - Two instruments case

	Dependent variable:			
	Total			
	(1)	(2)	(3)	(4)
$\hat{ElectricCov.}$	0.353	-0.699	0.465	0.401
	(0.794)	(0.916)	(1.194)	(1.322)
Year 2010	-0.732^{***}	2.901	3.403^{*}	3.381^{*}
	(0.264)	(1.803)	(1.781)	(1.794)
Gradient * Year	0.021	0.023	0.013	0.012
	(0.033)	(0.033)	(0.032)	(0.033)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.545	0.549	0.552	0.552
Adjusted R^2	0.089	0.096	0.102	0.100
Residual Std. Error	2.169 (df = 2173)	2.160 (df = 2172)	2.154 (df = 2170)	2.156 (df = 2164)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

C.3 Hospitalization Rate

Table 20 – Electrification effect on Hospitalization Rate - Intestinal infections - Less that 1 year old - Male - One instrument case

	Dependent variable:			
	Male			
	(1)	(2)	(3)	(4)
$\widehat{ElectricCov.}$	216.259	188.591	132.335	153.542
	(179.095)	(183.543)	(147.349)	(155.307)
Year 2010	−40.978***	19.345	48.895*	57.118**
	(12.330)	(38.017)	(28.337)	(27.287)
Eligible * Year	−51.381	−48.736	−26.468	−29.451
	(45.489)	(45.542)	(33.082)	(32.654)
Gradient * Year	1.657**	1.640**	0.864	0.647
	(0.832)	(0.832)	(0.645)	(0.647)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.629	0.640	0.660	0.658
Adjusted R^2	0.256	0.278	0.319	0.312
Residual Std. Error	42.344 (df = 2172)	41.714 (df = 2171)	40.534 (df = 2169)	40.724 (df = 2163)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 21 – Electrification effect on Hospitalization Rate - Intestinal infections - Less that 1 year old - Male - Two instruments case

	Dependent variable:			
	Male			
	(1)	(2)	(3)	(4)
$\widehat{ElectricCov.}$	13.829	−7.615	14.797	13.375
	(14.288)	(16.540)	(20.806)	(24.036)
Year 2010	−27.114***	46.947*	57.439**	63.363**
	(4.573)	(25.583)	(25.198)	(25.406)
Gradient * Year	0.771	0.806	0.565	0.528
	(0.606)	(0.605)	(0.628)	(0.658)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.662	0.664	0.667	0.668
Adjusted R^2	0.323	0.327	0.333	0.333
Residual Std. Error	40.404 (df = 2173)	40.298 (df = 2172)	40.100 (df = 2170)	40.118 (df = 2164)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 22 – Electrification effect on Hospitalization Rate - Intestinal infections -Less that 1 year old - Female - One instrument case

	Dependent variable:			
	Female			
	(1)	(2)	(3)	(4)
$El\hat{ectric}Cov.$	184.209	163.883	106.407	109.612
	(144.476)	(148.134)	(118.443)	(124.243)
Year 2010	-34.260^{***}	10.056	38.457	41.748^{*}
	(10.450)	(33.560)	(25.931)	(25.025)
Eligible * Year	-40.619	-38.676	-16.753	-15.574
	(36.725)	(36.799)	(26.672)	(26.088)
Gradient * Year	1.080	1.067	0.309	0.061
	(0.781)	(0.781)	(0.601)	(0.562)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.612	0.622	0.645	0.646
Adjusted R^2	0.223	0.242	0.288	0.288
Residual Std. Error	37.567 (df = 2172)	37.124 (df = 2171)	35.973 (df = 2169)	35.973 (df = 2163)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 23 – Electrification effect on Hospitalization Rate - Intestinal infections - Less that 1 year old - Female - Two instruments case

	Dependent variable:			
	Female			
	(1)	(2)	(3)	(4)
$El\hat{ectric}Cov.$	24.181^{*}	8.180	32.011^{*}	35.492^{*}
	(12.793)	(14.521)	(17.906)	(20.795)
Year 2010	-23.300^{***}	31.960	43.866^{*}	45.051^{*}
	(4.122)	(24.166)	(23.700)	(23.845)
Gradient * Year	0.379	0.405	0.119	-0.002
	(0.523)	(0.523)	(0.534)	(0.554)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.646	0.648	0.652	0.652
Adjusted R^2	0.291	0.294	0.301	0.301
Residual Std. Error	35.903 (df = 2173)	35.810 (df = 2172)	35.633 (df = 2170)	35.647 (df = 2164)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 24 – Electrification effect on Hospitalization Rate - Neoplasms - More than 20 years old - Total - One instrument case

	Dependent variable:			
	Total			
	(1)	(2)	(3)	(4)
$Electr\hat{i}cCov.$	-2.452	-1.460	-4.794	-3.624
	(9.497)	(9.852)	(8.088)	(8.396)
Year 2010	2.145^{***}	-0.018	1.781	1.924
	(0.689)	(2.027)	(1.594)	(1.547)
Eligible * Year	-0.924	-1.019	0.324	0.136
	(2.406)	(2.442)	(1.802)	(1.753)
Gradient * Year	0.039	0.040	-0.007	-0.009
	(0.052)	(0.052)	(0.040)	(0.037)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.670	0.670	0.676	0.682
Adjusted R^2	0.339	0.339	0.350	0.360
Residual Std. Error	2.267 (df = 2172)	2.266 (df = 2171)	2.247 (df = 2169)	2.230 (df = 2163)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 25 – Electrification effect on Hospitalization Rate - Neoplasms - More than 20 years old - Total - Two instruments case

	Dependent variable:			
	Total			
	(1)	(2)	(3)	(4)
$Electr\hat{i}cCov.$	-6.092^{***}	-5.561^{***}	-3.358^{***}	-2.977^{**}
	(0.819)	(0.921)	(1.023)	(1.187)
Year 2010	2.394^{***}	0.559	1.677	1.895
	(0.275)	(1.510)	(1.500)	(1.516)
Gradient * Year	0.023	0.022	-0.003	-0.009
	(0.036)	(0.036)	(0.036)	(0.037)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.666	0.668	0.679	0.683
Adjusted R^2	0.332	0.334	0.356	0.362
Residual Std. Error	2.280 (df = 2173)	2.275 (df = 2172)	2.238 (df = 2170)	2.226 (df = 2164)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 26 – Electrification effect on Hospitalization Rate - Neoplasms - More than 20 years old - Male - One instrument case

	Dependent variable:			
	Male			
	(1)	(2)	(3)	(4)
$\widehat{ElectricCov.}$	-8.731	-8.648	-10.144	-10.155
	(9.018)	(9.315)	(7.867)	(8.200)
Year 2010	2.707^{***}	2.524	3.563^{***}	3.434^{***}
	(0.698)	(1.651)	(1.271)	(1.223)
Eligible * Year	0.896	0.888	1.597	1.697
	(2.290)	(2.318)	(1.778)	(1.738)
Gradient * Year	0.001	0.001	-0.024	-0.030
	(0.057)	(0.057)	(0.045)	(0.040)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.680	0.680	0.677	0.680
Adjusted R^2	0.359	0.360	0.353	0.356
Residual Std. Error	2.231 (df = 2172)	2.231 (df = 2171)	2.242 (df = 2169)	2.237 (df = 2163)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 27 – Electrification effect on Hospitalization Rate - Neoplasms - More than 20 years old - Male - Two instruments case

	Dependent variable:			
	Male			
	(1)	(2)	(3)	(4)
$\widehat{ElectricCov.}$	-5.203^{***}	-5.074^{***}	-3.053^{***}	-2.078^{*}
	(0.831)	(0.885)	(0.940)	(1.066)
Year 2010	2.465^{***}	2.022^{**}	3.047^{***}	3.074^{***}
	(0.299)	(0.971)	(0.998)	(1.021)
Gradient * Year	0.017	0.016	-0.006	-0.023
	(0.037)	(0.037)	(0.038)	(0.037)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.691	0.691	0.701	0.706
Adjusted R^2	0.381	0.381	0.401	0.410
Residual Std. Error	2.193 (df = 2173)	2.193 (df = 2172)	2.157 (df = 2170)	2.142 (df = 2164)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 28 – Electrification effect on Hospitalization Rate - Neoplasms - More than 20 years old - Female - One instrument case

	Dependent variable:			
	Female			
	(1)	(2)	(3)	(4)
$\widehat{ElectricCov.}$	3.546	5.432	0.245	2.677
	(13.851)	(14.485)	(11.696)	(12.130)
Year 2010	1.602	−2.509	0.034	0.469
	(0.984)	(3.492)	(2.846)	(2.828)
Eligible * Year	−2.706	−2.887	−0.918	−1.415
	(3.482)	(3.558)	(2.562)	(2.494)
Gradient * Year	0.075	0.076	0.008	0.010
	(0.072)	(0.072)	(0.054)	(0.052)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.595	0.591	0.607	0.608
Adjusted R^2	0.188	0.181	0.212	0.212
Residual Std. Error	3.492 (df = 2172)	3.506 (df = 2171)	3.440 (df = 2169)	3.439 (df = 2163)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 29 – Electrification effect on Hospitalization Rate - Neoplasms - More than 20 years old - Female - Two instruments case

	Dependent variable:			
	Female			
	(1)	(2)	(3)	(4)
$\widehat{ElectricCov.}$	−7.116***	−6.190***	−3.830**	−4.058**
	(1.197)	(1.391)	(1.616)	(1.910)
Year 2010	2.332***	−0.874	0.330	0.769
	(0.374)	(2.743)	(2.721)	(2.750)
Gradient * Year	0.028	0.027	−0.002	0.004
	(0.050)	(0.050)	(0.051)	(0.053)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.599	0.601	0.607	0.610
Adjusted R^2	0.197	0.200	0.212	0.216
Residual Std. Error	3.472 (df = 2173)	3.465 (df = 2172)	3.440 (df = 2170)	3.431 (df = 2164)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 30 – Electrification effect on Hospitalization Rate - Lung cancer - More than 20 years old - Total - One instrument case

	Dependent variable:			
	Total			
	(1)	(2)	(3)	(4)
$Electr\hat{i}cCov.$	0.964	1.049	0.600	0.695
	(1.218)	(1.263)	(1.036)	(1.066)
Year 2010	0.068	−0.118	0.109	0.080
	(0.100)	(0.223)	(0.156)	(0.151)
Eligible * Year	−0.359	−0.367	−0.194	−0.195
	(0.307)	(0.313)	(0.236)	(0.227)
Gradient * Year	0.015*	0.015*	0.009	0.007
	(0.008)	(0.008)	(0.007)	(0.006)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.547	0.543	0.569	0.573
Adjusted R^2	0.092	0.084	0.136	0.140
Residual Std. Error	0.308 (df = 2172)	0.309 (df = 2171)	0.301 (df = 2169)	0.300 (df = 2163)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 31 – Electrification effect on Hospitalization Rate - Lung cancer - More than 20 years old - Total - Two instruments case

	Dependent variable:			
	Total			
	(1)	(2)	(3)	(4)
$Electr\hat{i}cCov.$	−0.451***	−0.430***	−0.260*	−0.232
	(0.116)	(0.129)	(0.139)	(0.161)
Year 2010	0.165***	0.090	0.172	0.121
	(0.043)	(0.133)	(0.132)	(0.135)
Gradient * Year	0.008	0.008	0.006	0.006
	(0.005)	(0.005)	(0.005)	(0.005)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.569	0.570	0.575	0.580
Adjusted R^2	0.138	0.138	0.148	0.156
Residual Std. Error	0.300 (df = 2173)	0.300 (df = 2172)	0.299 (df = 2170)	0.297 (df = 2164)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 32 – Electrification effect on Hospitalization Rate - Lung cancer - More than 20 years old - Male - One instrument case

	Dependent variable:			
	Male			
	(1)	(2)	(3)	(4)
$\widehat{ElectricCov.}$	1.668	1.747	1.144	1.285
	(2.064)	(2.134)	(1.780)	(1.837)
Year 2010	0.057	−0.115	0.194	0.156
	(0.172)	(0.369)	(0.265)	(0.256)
Eligible * Year	−0.568	−0.576	−0.341	−0.335
	(0.520)	(0.528)	(0.405)	(0.391)
Gradient * Year	0.023	0.023	0.015	0.012
	(0.014)	(0.014)	(0.012)	(0.010)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.510	0.507	0.528	0.531
Adjusted R^2	0.018	0.013	0.053	0.056
Residual Std. Error	0.534 (df = 2172)	0.535 (df = 2171)	0.524 (df = 2169)	0.523 (df = 2163)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 33 – Electrification effect on Hospitalization Rate - Lung cancer - More than 20 years old - Male - Two instruments case

	Dependent variable:			
	Male			
	(1)	(2)	(3)	(4)
$\widehat{ElectricCov.}$	−0.571***	−0.572***	−0.369	−0.309
	(0.205)	(0.221)	(0.226)	(0.258)
Year 2010	0.210***	0.211	0.304	0.227
	(0.076)	(0.211)	(0.215)	(0.223)
Gradient * Year	0.013	0.013	0.011	0.011
	(0.009)	(0.009)	(0.009)	(0.009)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.534	0.534	0.537	0.541
Adjusted R^2	0.066	0.066	0.071	0.077
Residual Std. Error	0.521 (df = 2173)	0.521 (df = 2172)	0.519 (df = 2170)	0.518 (df = 2164)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 34 – Electrification effect on Hospitalization Rate - Lung cancer - More than 20 years old - Female - One instrument case

	Dependent variable:			
	Total			
	(1)	(2)	(3)	(4)
$El\hat{ectric}Cov.$	1.214	1.638	−0.068	0.057
	(3.618)	(3.754)	(3.007)	(3.071)
Year 2010	0.469	−0.456	0.395	0.295
	(0.319)	(0.639)	(0.416)	(0.385)
Eligible * Year	−0.789	−0.830	−0.176	−0.168
	(0.907)	(0.922)	(0.669)	(0.645)
Gradient * Year	0.026	0.026	0.003	0.0002
	(0.030)	(0.030)	(0.022)	(0.020)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.531	0.528	0.549	0.552
Adjusted R^2	0.061	0.055	0.095	0.099
Residual Std. Error	1.006 (df = 2172)	1.009 (df = 2171)	0.987 (df = 2169)	0.985 (df = 2163)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 35 – Electrification effect on Hospitalization Rate - Lung cancer - More than 20 years old - Female - Two instruments case

	Dependent variable:			
	Female			
	(1)	(2)	(3)	(4)
$El\hat{ectric}Cov.$	−1.896***	−1.703***	−0.847**	−0.745
	(0.382)	(0.408)	(0.408)	(0.479)
Year 2010	0.682***	0.014	0.452	0.331
	(0.142)	(0.312)	(0.332)	(0.338)
Gradient * Year	0.012	0.012	0.001	−0.0005
	(0.017)	(0.017)	(0.017)	(0.019)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.536	0.537	0.548	0.551
Adjusted R^2	0.070	0.073	0.094	0.097
Residual Std. Error	1.001 (df = 2173)	0.999 (df = 2172)	0.988 (df = 2170)	0.986 (df = 2164)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 36 – Electrification effect on Hospitalization Rate - Vaccine preventable diseases - More than 20 years old - Total - One instrument case

	Dependent variable:			
	Total			
	(1)	(2)	(3)	(4)
$Electr\hat{i}cCov.$	-0.840	-0.931	-0.721	-0.754
	(0.831)	(0.860)	(0.688)	(0.705)
Year 2010	0.039	0.237	0.114	0.091
	(0.061)	(0.180)	(0.146)	(0.143)
Eligible * Year	0.201	0.209	0.120	0.122
	(0.210)	(0.213)	(0.153)	(0.146)
Gradient * Year	-0.005	-0.005	-0.002	-0.001
	(0.005)	(0.005)	(0.004)	(0.003)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.547	0.540	0.566	0.569
Adjusted R^2	0.093	0.078	0.130	0.132
Residual Std. Error	0.188 (df = 2172)	0.189 (df = 2171)	0.184 (df = 2169)	0.183 (df = 2163)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 37 – Electrification effect on Hospitalization Rate - Vaccine preventable diseases - More than 20 years old - Total - Two instruments case

	Dependent variable:			
	Total			
	(1)	(2)	(3)	(4)
$Electr\hat{i}cCov.$	-0.050	-0.088	-0.187^{**}	-0.173^{*}
	(0.065)	(0.072)	(0.095)	(0.095)
Year 2010	-0.016	0.118	0.075	0.065
	(0.023)	(0.139)	(0.140)	(0.141)
Gradient * Year	-0.002	-0.001	-0.001	-0.001
	(0.003)	(0.003)	(0.003)	(0.003)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.583	0.583	0.586	0.590
Adjusted R^2	0.165	0.165	0.171	0.175
Residual Std. Error	0.180 (df = 2173)	0.180 (df = 2172)	0.179 (df = 2170)	0.179 (df = 2164)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

C.4 Birth Weight Share

Table 38 – Electrification effect on Birth Weight Share - Less than 2,5kg - One instrument case

	Dependent variable:			
	Birth Weight Share - Less than 2,5kg			
	(1)	(2)	(3)	(4)
$\hat{ElectricCov.}$	−0.079	−0.083	−0.080	−0.065
	(0.144)	(0.148)	(0.121)	(0.126)
Year 2010	0.019*	0.027	0.021	0.025
	(0.010)	(0.029)	(0.023)	(0.023)
Eligible * Year	0.018	0.018	0.015	0.012
	(0.037)	(0.037)	(0.028)	(0.027)
Gradient * Year	−0.001	−0.001	−0.001*	−0.001*
	(0.001)	(0.001)	(0.001)	(0.001)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.569	0.568	0.570	0.575
Adjusted R^2	0.136	0.134	0.137	0.145
Residual Std. Error	0.034 (df = 2172)	0.034 (df = 2171)	0.034 (df = 2169)	0.034 (df = 2163)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 39 – Electrification effect on Birth Weight Share - Less than 2,5kg - Two instruments case

	Dependent variable:			
	Birth Weight Share - Less than 2,5kg			
	(1)	(2)	(3)	(4)
$\hat{ElectricCov.}$	−0.010	−0.010	−0.013	−0.010
	(0.012)	(0.014)	(0.015)	(0.018)
Year 2010	0.014***	0.016	0.016	0.023
	(0.004)	(0.022)	(0.022)	(0.023)
Gradient * Year	−0.001*	−0.001*	−0.001*	−0.001*
	(0.001)	(0.001)	(0.001)	(0.001)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.579	0.579	0.580	0.582
Adjusted R^2	0.157	0.157	0.157	0.159
Residual Std. Error	0.033 (df = 2173)	0.033 (df = 2172)	0.033 (df = 2170)	0.033 (df = 2164)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 40 – Electrification effect on BCG Vaccine doses - One instrument case

	Dependent variable:			
	BCG Vaccine doses			
	(1)	(2)	(3)	(4)
$Electr\hat{ic}Cov.$	640.323	534.434	319.967	470.258
	(434.526)	(438.736)	(338.737)	(356.651)
Year 2010	−102.538***	128.329	245.967***	259.300***
	(30.352)	(99.634)	(73.727)	(74.260)
Eligible * Year	−154.209	−144.086	−56.886	−90.709
	(108.463)	(106.429)	(73.358)	(71.571)
Gradient * Year	1.662	1.595	−1.457	−1.559
	(1.927)	(1.911)	(1.345)	(1.296)
Observations	4,352	4,352	4,352	4,352
R^2	0.915	0.921	0.930	0.928
Adjusted R^2	0.831	0.841	0.860	0.855
Residual Std. Error	110.908 (df = 2172)	107.535 (df = 2171)	100.754 (df = 2169)	102.608 (df = 2163)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

C.5 Vaccines

Table 41 – Electrification effect on BCG Vaccine doses - Two instruments case

	Dependent variable:			
	BCG Vaccine doses			
	(1)	(2)	(3)	(4)
$Electr\hat{ic}Cov.$	32.777	−45.639	67.350	38.544
	(35.454)	(38.608)	(48.832)	(54.174)
Year 2010	−60.929***	209.934***	264.331***	278.534***
	(11.025)	(64.138)	(64.652)	(66.245)
Gradient * Year	−0.999	−0.872	−2.101	−1.927
	(1.378)	(1.366)	(1.341)	(1.319)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.930	0.931	0.933	0.934
Adjusted R^2	0.859	0.862	0.866	0.867
Residual Std. Error	101.204 (df = 2173)	100.180 (df = 2172)	98.797 (df = 2170)	98.253 (df = 2164)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 42 – Electrification effect on Rotavirus Vaccine doses - One instrument case

	Dependent variable:			
	Rotavirus Vaccine			
	(1)	(2)	(3)	(4)
$\widehat{ElectricCov.}$	$1,216.745^{**}$	$1,299.361^{**}$	$1,133.397^{**}$	977.908^{**}
	(545.162)	(585.025)	(453.219)	(440.160)
Year 2010	69.177^{*}	-110.949	-51.083	-50.299
	(38.835)	(139.099)	(104.307)	(94.648)
Eligible * Year	-254.278^{*}	-262.176^{*}	-209.125^{**}	-165.624^{*}
	(135.008)	(140.984)	(96.754)	(86.701)
Gradient * Year	-1.293	-1.241	-3.008	-4.121^{**}
	(2.521)	(2.549)	(1.884)	(1.712)
Observations	4,352	4,352	4,352	4,352
R^2	0.493	0.471	0.519	0.566
Adjusted R^2	-0.016	-0.060	0.036	0.127
Residual Std. Error	144.023 (df = 2172)	147.133 (df = 2171)	140.308 (df = 2169)	133.492 (df = 2163)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 43 – Electrification effect on Rotavirus Vaccine doses - Two instruments case

	Dependent variable:			
	Rotavirus Vaccine			
	(1)	(2)	(3)	(4)
$\widehat{ElectricCov.}$	214.953^{***}	243.871^{***}	204.726^{***}	189.649^{***}
	(43.026)	(48.817)	(62.670)	(70.169)
Year 2010	137.786^{***}	37.537	16.427	-15.179
	(14.562)	(66.834)	(67.795)	(68.919)
Gradient * Year	-5.680^{***}	-5.729^{***}	-5.377^{***}	-4.792^{***}
	(1.693)	(1.699)	(1.683)	(1.597)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.659	0.658	0.662	0.673
Adjusted R^2	0.317	0.315	0.323	0.343
Residual Std. Error	118.076 (df = 2173)	118.279 (df = 2172)	117.608 (df = 2170)	115.803 (df = 2164)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 44 – Electrification effect on Meningococcus Vaccine doses - One instrument case

	Dependent variable:			
	Meningococcus Vaccine			
	(1)	(2)	(3)	(4)
$\widehat{ElectricCov.}$	−33.175	26.950	38.577	−2.849
	(233.757)	(241.593)	(199.569)	(203.464)
Year 2010	12.562	−118.527***	−130.742***	−121.627***
	(15.380)	(42.354)	(28.391)	(26.408)
Eligible * Year	39.261	33.513	26.083	40.616
	(60.088)	(60.630)	(45.573)	(43.196)
Gradient * Year	2.991***	3.029***	3.305***	2.691***
	(1.117)	(1.118)	(0.867)	(0.804)
Observations	4,352	4,352	4,352	4,352
R^2	0.562	0.571	0.573	0.581
Adjusted R^2	0.123	0.140	0.143	0.158
Residual Std. Error	59.409 (df = 2172)	58.831 (df = 2171)	58.735 (df = 2169)	58.218 (df = 2163)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 45 – Electrification effect on Meningococcus Vaccine doses - Two instruments case

	Dependent variable:			
	Meningococcus Vaccine			
	(1)	(2)	(3)	(4)
$\widehat{ElectricCov.}$	121.502***	161.868***	154.406***	190.457***
	(19.221)	(23.924)	(28.546)	(31.695)
Year 2010	1.968	−137.507***	−139.162***	−130.240***
	(4.893)	(28.761)	(27.006)	(27.778)
Gradient * Year	3.668***	3.602***	3.601***	2.856***
	(0.806)	(0.809)	(0.832)	(0.823)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.562	0.564	0.565	0.573
Adjusted R^2	0.123	0.126	0.129	0.142
Residual Std. Error	59.407 (df = 2173)	59.299 (df = 2172)	59.218 (df = 2170)	58.762 (df = 2164)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 46 – Electrification effect on Hepatitis B Vaccine doses - One instrument case

	Dependent variable:			
	Hepatitis B Vaccine			
	(1)	(2)	(3)	(4)
$\widehat{ElectricCov.}$	647.409	472.418	347.035	360.789
	(421.381)	(413.273)	(329.226)	(338.020)
Year 2010	-49.811^{*}	331.716^{***}	401.307^{***}	388.138^{***}
	(26.954)	(96.152)	(79.366)	(78.560)
Eligible * Year	-126.398	-109.669	-58.312	-64.728
	(105.386)	(100.430)	(71.605)	(68.022)
Gradient * Year	-1.958	-2.069	-3.868^{***}	-3.678^{***}
	(1.510)	(1.472)	(1.147)	(1.162)
Observations	4,352	4,352	4,352	4,352
R^2	0.879	0.892	0.898	0.899
Adjusted R^2	0.757	0.783	0.796	0.796
Residual Std. Error	109.171 (df = 2172)	103.230 (df = 2171)	100.020 (df = 2169)	100.022 (df = 2163)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 47 – Electrification effect on Hepatitis B Vaccine doses - Two instruments case

	Dependent variable:			
	Hepatitis B Vaccine			
	(1)	(2)	(3)	(4)
$\widehat{ElectricCov.}$	149.430^{***}	30.903	88.088^{*}	52.729
	(32.537)	(36.378)	(48.758)	(49.854)
Year 2010	-15.706^{*}	393.828^{***}	420.131^{***}	401.863^{***}
	(8.428)	(72.958)	(73.913)	(76.543)
Gradient * Year	-4.139^{***}	-3.946^{***}	-4.529^{***}	-3.941^{***}
	(1.211)	(1.181)	(1.258)	(1.241)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.897	0.902	0.903	0.904
Adjusted R^2	0.794	0.805	0.806	0.807
Residual Std. Error	100.595 (df = 2173)	97.945 (df = 2172)	97.724 (df = 2170)	97.261 (df = 2164)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 48 – Electrification effect on Pneumonia Vaccine doses - One instrument case

	Dependent variable:			
	Pneumonia Vaccine			
	(1)	(2)	(3)	(4)
$\widehat{ElectricCov.}$	93.608	129.695	129.654	137.006
	(132.333)	(140.594)	(117.817)	(122.190)
Year 2010	25.153**	−53.526*	−49.630**	−34.373*
	(10.818)	(29.436)	(21.567)	(19.282)
Eligible * Year	−39.316	−42.766	−40.956	−39.810
	(32.778)	(33.901)	(25.444)	(24.298)
Gradient * Year	1.875**	1.898**	1.824***	1.483***
	(0.881)	(0.886)	(0.661)	(0.549)
Observations	4,352	4,352	4,352	4,352
R^2	0.569	0.557	0.558	0.571
Adjusted R^2	0.137	0.113	0.114	0.136
Residual Std. Error	34.208 (df = 2172)	34.690 (df = 2171)	34.665 (df = 2169)	34.229 (df = 2163)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 49 – Electrification effect on Pneumonia Vaccine doses - Two instruments case

	Dependent variable:			
	Pneumonia Vaccine			
	(1)	(2)	(3)	(4)
$\widehat{ElectricCov.}$	−61.288***	−42.476***	−52.222***	−52.465**
	(12.602)	(14.475)	(18.930)	(21.477)
Year 2010	35.761***	−29.305**	−36.408***	−25.932*
	(4.575)	(14.343)	(13.670)	(13.356)
Gradient * Year	1.197**	1.166**	1.360**	1.321***
	(0.554)	(0.553)	(0.533)	(0.511)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.595	0.600	0.601	0.614
Adjusted R^2	0.190	0.200	0.201	0.224
Residual Std. Error	33.158 (df = 2173)	32.951 (df = 2172)	32.925 (df = 2170)	32.447 (df = 2164)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 50 – Electrification effect on Polio Vaccine doses - One instrument case

	Dependent variable:			
	Polio Vaccine			
	(1)	(2)	(3)	(4)
$\widehat{ElectricCov.}$	−193.946	−217.621	−150.746	−177.388
	(542.451)	(537.847)	(426.964)	(444.506)
Year 2010	−17.928	33.690	18.892	1.848
	(33.054)	(120.575)	(134.073)	(142.795)
Eligible * Year	47.105	49.368	32.308	34.288
	(133.421)	(133.123)	(92.629)	(89.883)
Gradient * Year	−2.494	−2.509	−1.975	−1.706
	(1.630)	(1.631)	(1.246)	(1.404)
Observations	4,352	4,352	4,352	4,352
R^2	0.895	0.895	0.896	0.896
Adjusted R^2	0.790	0.790	0.791	0.791
Residual Std. Error	123.612 (df = 2172)	123.774 (df = 2171)	123.325 (df = 2169)	123.385 (df = 2163)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 51 – Electrification effect on Polio Vaccine doses - Two instruments case

	Dependent variable:			
	Polio Vaccine			
	(1)	(2)	(3)	(4)
$\widehat{ElectricCov.}$	−8.363	−18.869	−7.274	−14.199
	(36.979)	(36.967)	(47.435)	(49.598)
Year 2010	−30.638***	5.730	8.462	−5.423
	(9.036)	(131.430)	(141.904)	(150.139)
Gradient * Year	−1.681	−1.664	−1.609	−1.567
	(1.537)	(1.518)	(1.701)	(1.639)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.896	0.896	0.896	0.897
Adjusted R^2	0.792	0.792	0.792	0.792
Residual Std. Error	123.054 (df = 2173)	123.064 (df = 2172)	123.000 (df = 2170)	122.966 (df = 2164)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 52 – Electrification effect on Yellow Fever Vaccine doses - One instrument case

	Dependent variable:			
	Yellow Fever Vaccine			
	(1)	(2)	(3)	(4)
$\widehat{ElectricCov.}$	−361.674	−412.690	−460.480	−411.349
	(470.789)	(478.727)	(396.135)	(407.756)
Year 2010	27.456	138.684	163.641	183.648*
	(34.704)	(102.851)	(99.768)	(100.872)
Eligible * Year	102.592	107.469	126.318	110.572
	(115.917)	(116.936)	(85.808)	(80.847)
Gradient * Year	0.640	0.607	−0.049	0.324
	(2.319)	(2.327)	(1.835)	(1.641)
Observations	4,352	4,352	4,352	4,352
R^2	0.867	0.866	0.864	0.867
Adjusted R^2	0.734	0.731	0.728	0.733
Residual Std. Error	117.928 (df = 2172)	118.595 (df = 2171)	119.369 (df = 2169)	118.337 (df = 2163)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 53 – Electrification effect on Yellow Fever Vaccine doses - Two instruments case

	Dependent variable:			
	Yellow Fever Vaccine			
	(1)	(2)	(3)	(4)
$\widehat{ElectricCov.}$	42.513	19.968	100.469*	114.899*
	(41.541)	(44.010)	(54.572)	(63.868)
Year 2010	−0.225	77.818	122.862	160.202
	(13.942)	(90.960)	(95.621)	(99.739)
Gradient * Year	2.409	2.447	1.382	0.772
	(1.697)	(1.690)	(1.752)	(1.663)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.873	0.873	0.874	0.875
Adjusted R^2	0.746	0.746	0.748	0.750
Residual Std. Error	115.368 (df = 2173)	115.299 (df = 2172)	114.788 (df = 2170)	114.518 (df = 2164)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 54 – Electrification effect on MMR Vaccine doses - One instrument case

	Dependent variable:			
	MMR Vaccine			
	(1)	(2)	(3)	(4)
$Electr\hat{i}cCov.$	$1,203.727^{*}$	999.365	803.859	957.585
	(720.637)	(711.697)	(570.545)	(589.147)
Year 2010	-0.088	445.475^{**}	540.797^{***}	560.065^{***}
	(46.054)	(175.839)	(148.440)	(145.327)
Eligible * Year	-253.314	-233.777	-159.802	-197.331^{*}
	(177.858)	(171.597)	(122.041)	(116.298)
Gradient * Year	-6.187^{**}	-6.316^{***}	-8.871^{***}	-8.947^{***}
	(2.429)	(2.393)	(1.891)	(2.042)
Observations	4,352	4,352	4,352	4,352
R^2	0.670	0.694	0.712	0.707
Adjusted R^2	0.338	0.387	0.423	0.410
Residual Std. Error	188.826 (df = 2172)	181.777 (df = 2171)	176.375 (df = 2169)	178.280 (df = 2163)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 55 – Electrification effect on MMR Vaccine doses - Two instruments case

	Dependent variable:			
	MMR Vaccine			
	(1)	(2)	(3)	(4)
$Electr\hat{i}cCov.$	205.729^{***}	58.205	94.216	18.423
	(54.801)	(55.184)	(78.050)	(83.970)
Year 2010	68.261^{***}	577.877^{***}	592.385^{***}	601.908^{***}
	(14.586)	(133.930)	(136.317)	(133.660)
Gradient * Year	-10.557^{***}	-10.318^{***}	-10.681^{***}	-9.747^{***}
	(2.001)	(1.961)	(2.078)	(2.118)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.731	0.740	0.739	0.746
Adjusted R^2	0.462	0.478	0.477	0.490
Residual Std. Error	170.258 (df = 2173)	167.666 (df = 2172)	167.780 (df = 2170)	165.823 (df = 2164)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 56 – Electrification effect on DTP Vaccine doses - One instrument case

	Dependent variable:			
	DTP Vaccine			
	(1)	(2)	(3)	(4)
$Electr\hat{i}cCov.$	454.269	288.435	191.137	224.464
	(380.161)	(373.316)	(300.548)	(305.962)
Year 2010	-44.417^{*}	317.147^{***}	376.468^{***}	366.465^{***}
	(24.319)	(89.270)	(77.692)	(77.028)
Eligible * Year	−86.993	−71.139	−28.823	−40.748
	(94.656)	(90.702)	(65.266)	(61.296)
Gradient * Year	−1.613	−1.718	-3.216^{***}	-2.963^{***}
	(1.372)	(1.342)	(1.035)	(1.072)
Observations	4,352	4,352	4,352	4,352
R^2	0.903	0.912	0.915	0.916
Adjusted R^2	0.806	0.823	0.830	0.830
Residual Std. Error	100.383 (df = 2172)	95.816 (df = 2171)	93.864 (df = 2169)	93.860 (df = 2163)

Note: Standard Errors clustered at the municipality level. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Table 57 – Electrification effect on DTP Vaccine doses - Two instruments case

	Dependent variable:			
	DTP Vaccine			
	(1)	(2)	(3)	(4)
$Electr\hat{i}cCov.$	111.540^{***}	2.039	63.140	30.529
	(30.500)	(33.708)	(44.493)	(45.902)
Year 2010	-20.945^{***}	357.437^{***}	385.773^{***}	375.105^{***}
	(7.557)	(73.066)	(74.165)	(76.196)
Gradient * Year	-3.114^{***}	-2.936^{***}	-3.542^{***}	-3.128^{***}
	(1.115)	(1.089)	(1.153)	(1.161)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.912	0.916	0.917	0.918
Adjusted R^2	0.823	0.831	0.833	0.834
Residual Std. Error	95.844 (df = 2173)	93.594 (df = 2172)	93.193 (df = 2170)	92.754 (df = 2164)

Note: Standard Errors clustered at the municipality level. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Table 58 – Electrification effect on Vaccine doses

	Dependent variable:			
	Vaccine doses			
	(1)	(2)	(3)	(4)
$\widehat{ElectricCov.}$	$2,660.858^{***}$	$1,753.252^{***}$	$2,468.647^{***}$	$2,278.775^{***}$
	(487.773)	(541.994)	(712.939)	(736.501)
Year 2010	195.998	$3,330.799^{***}$	$3,639.460^{***}$	$3,365.258^{**}$
	(120.651)	$(1,205.293)$	$(1,259.536)$	$(1,314.145)$
Gradient * Year	-50.587^{***}	-49.119^{***}	-55.684^{***}	-53.149^{***}
	(16.774)	(16.535)	(18.102)	(17.669)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.959	0.960	0.960	0.961
Adjusted R^2	0.918	0.920	0.920	0.921
Residual Std. Error	1,527.728 (df = 2173)	1,512.650 (df = 2172)	1,514.046 (df = 2170)	1,504.505 (df = 2164)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 59 – Electrification effect on Vaccine Coverage

	Dependent variable:			
	Vaccine Coverage			
	(1)	(2)	(3)	(4)
$\widehat{ElectricCov.}$	0.317^{***}	0.309^{***}	0.270^{***}	0.248^{***}
	(0.045)	(0.051)	(0.063)	(0.068)
Year 2010	-0.016	0.011	-0.007	-0.005
	(0.014)	(0.105)	(0.107)	(0.104)
Gradient * Year	-0.002	-0.002	-0.001	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.545	0.546	0.548	0.553
Adjusted R^2	0.089	0.090	0.094	0.102
Residual Std. Error	0.129 (df = 2173)	0.129 (df = 2172)	0.129 (df = 2170)	0.128 (df = 2164)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

APPENDIX D – Mechanisms

Table 60 – Electrification effect on Health Facilities

	Dependent variable:			
	Health Facilities			
	(1)	(2)	(3)	(4)
$\hat{ElectricCov.}$	1.376***	1.012	0.423	−0.048
	(0.520)	(0.633)	(0.830)	(0.885)
Year 2010	0.503***	1.760	1.432	0.889
	(0.151)	(1.083)	(1.097)	(1.100)
Gradient * Year	−0.086***	−0.086***	−0.078***	−0.069***
	(0.024)	(0.024)	(0.024)	(0.024)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.941	0.941	0.942	0.942
Adjusted R^2	0.883	0.883	0.883	0.884
Residual Std. Error	1.579 (df = 2173)	1.577 (df = 2172)	1.575 (df = 2170)	1.570 (df = 2164)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 61 – Electrification effect on Hospital Beds

	Dependent variable:			
	Hospital Beds			
	(1)	(2)	(3)	(4)
$\hat{ElectricCov.}$	0.512	−1.282	−0.990	−0.820
	(2.062)	(2.263)	(2.577)	(3.092)
Year 2010	−1.565**	4.637	4.910	4.612
	(0.653)	(2.871)	(3.014)	(3.224)
Gradient * Year	−0.044	−0.041	−0.050	−0.064
	(0.091)	(0.091)	(0.094)	(0.095)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.977	0.977	0.977	0.977
Adjusted R^2	0.954	0.954	0.954	0.954
Residual Std. Error	5.972 (df = 2173)	5.965 (df = 2172)	5.967 (df = 2170)	5.966 (df = 2164)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 62 – Electrification effect on Fridge

	Dependent variable:			
	Fridge			
	(1)	(2)	(3)	(4)
$Electr\hat{ic}Cov.$	0.768***	0.899***	0.590***	0.516***
	(0.028)	(0.031)	(0.026)	(0.027)
Year 2010	0.168***	−0.283***	−0.441***	−0.413***
	(0.010)	(0.041)	(0.033)	(0.034)
Gradient * Year	−0.004***	−0.004***	−0.0004	0.0003
	(0.001)	(0.001)	(0.001)	(0.001)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.956	0.959	0.979	0.982
Adjusted R^2	0.912	0.917	0.959	0.964
Residual Std. Error	0.074 (df = 2173)	0.072 (df = 2172)	0.051 (df = 2170)	0.048 (df = 2164)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

APPENDIX E – Robustness Check Tables

Table 63 – Electrification effect on First Stage - Electric Coverage - Gradient dummy

	Dependent variable:			
	Electric Coverage			
	(1)	(2)	(3)	(4)
Year 2010	−0.332***	−0.785***	−0.832***	−0.542***
	(0.013)	(0.013)	(0.021)	(0.043)
Eligible * Year * Grad. Dummy	−0.032*	−0.028**	−0.030**	−0.023*
	(0.017)	(0.013)	(0.013)	(0.013)
Eligible * Year	0.141***	0.292***	0.285***	0.254***
	(0.014)	(0.009)	(0.010)	(0.010)
Gradient * Year	0.005**	−0.002***	−0.002*	−0.002*
	(0.002)	(0.001)	(0.001)	(0.001)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.865	0.939	0.939	0.946
Adjusted R^2	0.730	0.878	0.878	0.891
Residual Std. Error	0.124 (df = 2172)	0.083 (df = 2171)	0.083 (df = 2169)	0.078 (df = 2163)
Excluded Instrument (Elig.*Year*Grad.) F Stat.	3.7047 (df = 2173; 2172)	4.4798* (df = 2172; 2171)	5.0695* (df = 2170; 2169)	3.5073* (df = 2164; 2163)
Excluded Instruments (Elig.*Year*Grad. and Elig.*Year) F Stat.	66.109** (df = 2174; 2172)	783.15*** (df = 2173; 2171)	680.82*** (df = 2171; 2169)	472.4*** (df = 2165; 2163)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 64 – Electrification effect on First Stage - Sewage disposal - Gradient dummy

	Dependent variable:			
	Sewage disposal			
	(1)	(2)	(3)	(4)
Year 2010	0.756***	0.767***	0.961***	0.928***
	(0.018)	(0.034)	(0.046)	(0.089)
Eligible * Year * Grad. Dummy	0.035	0.035	0.041	0.027
	(0.028)	(0.028)	(0.027)	(0.026)
Eligible * Year	−0.125***	−0.128***	−0.094***	−0.098***
	(0.022)	(0.023)	(0.023)	(0.022)
Gradient * Year	−0.014***	−0.014***	−0.016***	−0.008***
	(0.003)	(0.003)	(0.003)	(0.003)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.865	0.865	0.871	0.891
Adjusted R^2	0.730	0.730	0.740	0.780
Residual Std. Error	0.194 (df = 2172)	0.194 (df = 2171)	0.190 (df = 2169)	0.175 (df = 2164)
Excluded Instrument (Elig.*Year*Grad.) F Stat.	1.6144 (df = 2173; 2172)	1.6024 (df = 2172; 2171)	2.3041 (df = 2170; 2169)	1.1244 (df = 2164; 2163)
Excluded Instruments (Elig.*Year*Grad. and Elig.*Year) F Stat.	21.566*** (df = 2174; 2172)	19.725*** (df = 2173; 2171)	9.1746*** (df = 2171; 2169)	11.039*** (df = 2165; 2163)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 65 – Electrification effect on First Stage - Piped Water - Gradient dummy

	Dependent variable:			
	Piped Water			
	(1)	(2)	(3)	(4)
Year 2010	$1,549.512^{***}$	810.396^{***}	$2,396.313^{***}$	2.521^{***}
	(53.826)	(76.033)	(87.563)	(0.194)
Eligible * Year * Grad. Dummy	-58.756	-52.461	5.065	0.058
	(37.460)	(34.818)	(26.873)	(0.043)
Eligible * Year	-607.764^{***}	-360.935^{***}	-99.160^{***}	-0.289^{***}
	(45.616)	(45.658)	(26.901)	(0.046)
Gradient * Year	35.780^{***}	23.907^{***}	-1.697	0.011^{*}
	(8.077)	(7.152)	(4.759)	(0.006)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.885	0.901	0.954	1.000
Adjusted R^2	0.769	0.802	0.907	1.000
Residual Std. Error	393.831 (df = 2172)	364.555 (df = 2171)	249.098 (df = 2169)	0.326 (df = 2164)
Excluded Instrument (Elig.*Year*Grad.) F Stat.	2.4602 (df = 2173; 2172)	2.2703 (df = 2172; 2171)	0.0355 (df = 2170; 2169)	1.8188 (df = 2164; 2163)
Excluded Instruments (Elig.*Year*Grad. and Elig.*Year) F Stat.	145.92^{***} (df = 2174; 2172)	51.738^{***} (df = 2173; 2171)	8.9829^{***} (df = 2171; 2169)	20.748^{***} (df = 2165; 2163)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 66 – Electrification effect on Share of fixed households

	Dependent variable:			
	Share of fixed households			
	(1)	(2)	(3)	(4)
$\widehat{ElectricCov.}$	0.009	−0.003	−0.025	−0.037
	(0.016)	(0.018)	(0.023)	(0.024)
Year 2010	−0.039***	0.00001	−0.010	−0.021
	(0.005)	(0.031)	(0.031)	(0.032)
Gradient * Year	−0.00001	0.00001	0.0002	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.955	0.955	0.955	0.957
Adjusted R^2	0.909	0.909	0.910	0.914
Residual Std. Error	0.046 (df = 2173)	0.046 (df = 2172)	0.046 (df = 2170)	0.045 (df = 2165)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 67 – Electrification effect on First Stage - Electric Coverage - 1991-2000

	Dependent variable:	
	Electric Coverage	
	(1)	(2)
Year 2010	0.064***	−0.150***
	(0.007)	(0.023)
Eligible * Year * Gradient	0.002	−0.001
	(0.002)	(0.002)
Eligible * Year	0.193***	0.261***
	(0.010)	(0.013)
Gradient * Year	−0.001	−0.002
	(0.001)	(0.001)
Time Trend		0.219***
		(0.022)
Municipality Fixed Effects	Yes	Yes
Electric Coverage * Time Trend	No	Yes
Observations	3,026	3,026
R^2	0.949	0.955
Adjusted R^2	0.897	0.909
Residual Std. Error	0.078 (df = 1509)	0.073 (df = 1508)
Excluded Instrument (Elig.*Year*Grad.) F Stat.	1.752 (df = 1510; 1509)	0.569 (df = 1509; 1508)
Excluded Instruments (Elig.*Year*Grad. and Elig.*Year) F Stat.	573.21*** (df = 1511; 1509)	435.76*** (df = 1510; 1508)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 68 – Electrification effect on First Stage - 25% rural threshold - Electric coverage

	Dependent variable:			
	Electric Coverage			
	(1)	(2)	(3)	(4)
Year 2010	0.047***	0.162***	0.086	0.081
	(0.002)	(0.053)	(0.052)	(0.049)
Eligible * Year * Gradient	−0.008***	−0.008***	−0.009***	−0.008***
	(0.002)	(0.002)	(0.002)	(0.002)
Eligible * Year	0.260***	0.251***	0.227***	0.210***
	(0.008)	(0.009)	(0.008)	(0.008)
Gradient * Year	−0.001***	−0.001***	0.001	0.002***
	(0.0003)	(0.0003)	(0.0003)	(0.0004)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	7,868	7,868	7,868	7,868
R^2	0.928	0.928	0.934	0.937
Adjusted R^2	0.855	0.856	0.867	0.874
Residual Std. Error	0.060 (df = 3930)	0.060 (df = 3929)	0.057 (df = 3927)	0.056 (df = 3921)
Excluded Instrument (Elig.*Year*Grad.) F Stat.	21.645*** (df = 3931; 3930)	19.184** (df = 3930; 3929)	26.311*** (df = 3928; 3927)	26.908*** (df = 3922; 3921)
Excluded Instruments (Elig.*Year*Grad. and Elig.*Year) F Stat.	1454*** (df = 3932; 3230)	876*** (df = 3931; 3929)	739.12*** (df = 3929; 3927)	676.27*** (df = 3923; 3921)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 69 – Electrification effect on First Stage - 75% rural threshold - Electric coverage

	Dependent variable:			
	Electric Coverage			
	(1)	(2)	(3)	(4)
Year 2010	0.056***	0.018	−0.115	−0.168
	(0.008)	(0.104)	(0.110)	(0.103)
Eligible * Year * Gradient	−0.015***	−0.015***	−0.015***	−0.014***
	(0.004)	(0.004)	(0.004)	(0.004)
Eligible * Year	0.364***	0.368***	0.331***	0.308***
	(0.021)	(0.027)	(0.025)	(0.025)
Gradient * Year	−0.001	−0.002	0.002	0.002
	(0.001)	(0.001)	(0.001)	(0.002)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	960	960	960	960
R^2	0.932	0.932	0.937	0.942
Adjusted R^2	0.863	0.863	0.873	0.880
Residual Std. Error	0.086 (df = 476)	0.086 (df = 475)	0.083 (df = 473)	0.081 (df = 467)
Excluded Instrument (Elig.*Year*Grad.) F Stat.	14.408*** (df = 477; 476)	10.945** (df = 476; 475)	12.828*** (df = 474; 473)	10.226*** (df = 468; 467)
Excluded Instruments (Elig.*Year*Grad. and Elig.*Year) F Stat.	290.38*** (df = 478; 476)	165.59*** (df = 477; 475)	135.77*** (df = 475; 473)	115.41*** (df = 449; 467)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 70 – Electrification effect on Mortality Rate - Diabetes - More than 20 years old

	Dependent variable:			
	Total			
	(1)	(2)	(3)	(4)
$\widehat{ElectricCov.}$	−0.029	0.056	−0.307*	−0.306*
	(0.116)	(0.128)	(0.158)	(0.181)
Year 2010	0.273***	−0.023	−0.214	−0.246
	(0.042)	(0.179)	(0.174)	(0.182)
Gradient * Year	−0.009*	−0.009*	−0.005	−0.004
	(0.005)	(0.005)	(0.005)	(0.005)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.621	0.621	0.633	0.634
Adjusted R^2	0.240	0.242	0.264	0.264
Residual Std. Error	0.308 (df = 2173)	0.308 (df = 2172)	0.303 (df = 2170)	0.303 (df = 2164)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01

Table 71 – Electrification effect on Mortality Rate - Heart attack - More than 20 years old

	Dependent variable:			
	Total			
	(1)	(2)	(3)	(4)
$\widehat{ElectricCov.}$	0.477***	0.507***	−0.359*	−0.494**
	(0.166)	(0.182)	(0.209)	(0.233)
Year 2010	0.197***	0.096	−0.339	−0.373
	(0.060)	(0.231)	(0.227)	(0.233)
Gradient * Year	−0.008	−0.008	0.002	0.005
	(0.008)	(0.008)	(0.008)	(0.008)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Electric Cov. * Time Trend	No	Yes	Yes	Yes
Agric. pc GDP and BF	No	No	Yes	Yes
Household Covariates	No	No	No	Yes
Observations	4,352	4,352	4,352	4,352
R^2	0.636	0.636	0.665	0.666
Adjusted R^2	0.271	0.271	0.328	0.329
Residual Std. Error	0.452 (df = 2173)	0.452 (df = 2172)	0.434 (df = 2170)	0.434 (df = 2164)

Note: Standard Errors clustered at the municipality level. *p<0.1; **p<0.05; ***p<0.01