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The effects of rural electrification in India: An instrumental variable approach at the household level

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ABSTRACT

Governments in developing countries are investing billions of dollars to increase electricity access in rural areas, but the literature on the impact of these investments has produced mixed results. We leverage a unique characteristic of household electrification policy in Uttar Pradesh, India, whereby only households within 40 m of an electricity pole are eligible for a legal electrical connection, to estimate the causal impact of electrification using a pre-registered instrumental variable design. With an original survey of 686 households across 120 habitations in the Bahraich district of Uttar Pradesh, we find that legal electrification has a positive impact on household expenditures, adult household activities, and ownership and usage of appliances. The results suggest a more optimistic picture about the impact of rural electrification than some previous studies.

1. Introduction

Electrification is typically seen to have a positive impact on households, business and a country's economy at large (Barnes et al., 2003; IEG-World Bank, 2008; Cabraal et al., 2005; Martins, 2005). These positive associations have spurred countries to expand access to their main electricity grid. Such associations, however, are generally based on correlations, and do not identify the causal effect of electrification. This is problematic because the expansion of the grid is typically driven by both economic and political considerations (Min, 2015). Moreover, some studies that have attempted to identify the causal impact of electrification have painted a much more mixed picture (e.g. Burlig and Preonas, 2016; Dinkelman, 2011; Aklin et al., 2017; Grimm et al., 2015).

To overcome some of these limitations, we deployed a pre-registered instrumental variable approach at the household level, exploiting a geographic discontinuity in legal access to electricity to causally identify the effect of legal electrification on a range of outcomes. We leveraged a law in Uttar Pradesh, India that stipulates the area around an electric pole that is eligible for electrification. Specifically, only households within a 40 m distance of a pole are officially eligible for a legal electricity connection from that pole, and households outside that range are not officially eligible for grid electricity. Since 40 m is a fixed distance that is independent of habitation electrification rates and other economic characteristics, by comparing households just within this distance of an electric pole to those just outside, we can identify the causal impact of legal grid electricity.

We conducted an original survey in early 2018 of 686 households across 120 habitations[1] in the Bahraich/Lakhimbur district border area of Uttar Pradesh, India. The survey asked questions across a wide range of potential outcomes of electrification, focused on economic behavior. We find that electrification has a positive impact on many of these outcomes: specifically, we show that it increases household expenditures, adult household activity, and the ownership and usage of appliances.

Besides offering a credible identification strategy, our analysis makes three key contributions to the literature. First, it has distinct advantages over previous similar designs to detect the impact of electrification using policy-generated geographic discontinuities. Burlig and Preonas (2016), for example, examine the effects of the 2004 Rajiv Gandhi rural electrification scheme (RGGVY) using a population threshold for eligibility. In their case, the electrification scheme specifically targets the smallest and least developed villages, where the preconditions for economic development are weak (Aklin et al., 2017).

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[1] A habitation is "a locality within a village where a cluster of families reside," similar to a neighborhood. (PHED, N.d.)

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Because our sample is not limited by these policy considerations, our findings may have greater generalizability. Using census data, we show that our sample is generally similar to the broader national population of India. Second, we avoid the problem of impact evaluation focused on the short term, common in randomized controlled trials. In our data, the average electrified household obtained a legal connection over four years ago. Finally, our original data collection allows us to measure a wide range of specific outcomes. We reach well beyond broad measures such as energy expenditure or savings, and instead consider several measures within the outcome families considered here. Economic indicators such as expenditures and household activity are complex, and our data allows us to test specific components of each indicator. In the manuscript, we display the hypotheses and results for three outcome families: expenditures, household activity and appliance ownership and use. Hypotheses, results and interpretations of results for outcomes related to kerosene purchases, lighting, satisfaction and attitudes, and general knowledge are available in the Appendix.

In the next section we discuss the existing literature on the effects of rural electrification. We then introduce a conceptual framework, outlining our hypotheses and the mechanisms we believe are operating. Then we present our research design, describing our approach and how it allows us to make causal claims about electrification. The results section presents the key findings of our paper: we identify the most important outcomes and show that electrification has a largely positive effect on household welfare. The final section concludes with policy implications of this study.

2. Impact evaluation of rural electrification

There is a large body of literature that has documented the positive effects of rural electrification. For example, the IEG-World Bank (2008) describes benefits of electrification in the form of better lighting, use of appliances, better time use as well improved health and education results. Similarly, Cabraal et al. (2005) document the effect that energy services have had on education, health, and gender equality. Martins (2005) investigates how electrification can improve both monetary and non-monetary indicators of human welfare. For India, Barnes, Peskin, and Fitzgerald (2003) examine how rural electrification can lead to benefits in education, lighting and irrigation.

While the findings by these early studies are useful to identify the correlation between electrification and positive outcomes on the household or communities level, most of these findings are based on comparing two groups of households or villages: those that have had access to grid electricity and those that have not had access. This approach, however, does not account for the fact that households that acquired an electricity connection may be qualitatively different from households that do not get electricity even though they may have been eligible for a grid connection. In other words, there is a selection bias that is part of some of these studies making causal claims problematic.

Some studies have used propensity score matching, instrumental variables, or a difference-in-differences approach in an attempt to address this selection problem and produce causal estimates. For example, Dinkelman (2011) investigates the effect of rural electrification in South Africa. Using community-level variation in the timing of electrification, she shows that electrification has a positive impact on employment. She finds that electrification increases hours of work for both men and women, but disproportionately increases male earnings. Using a series of hypothetical electricity grids, Lipscomb et al. (2013) examine the development effects of electrification in Brazil, and they find large positive effects of electrification on labor productivity across sectors. Khandker, Barnes, and Samad (2009) investigate the effect of rural electrification on economic and educational outcomes in Bangladesh. They find increases in both annual per capita expenditure and income and, in addition, they report an increase in the number of completed years of schooling and study time. In Vietnam, Khandker, Barnes, and Samad (2013) report similar findings, including an increase in school attendance. Bensch, Kluve, and Peters (2011) match similar households in Rwanda, and show that electrification has positive effects on lighting usage. Other studies have examined the historical impact of electrification in the United states, exploring the effects of electrification and infrastructure development on agricultural output and employment, and manufacturing (Kitchens and Fishback, 2015; Kline and Moretti, 2014).

But not all the evidence is conclusive about the positive effects of electrification. Through a field experiment in India, Aklin et al. (2017) find that basic lighting from off-grid sources does not always generate socioeconomic benefits. In another off-grid experiment in Rwanda, Grimm et al. (2015) find evidence of reduced alternative expenditure but only limited evidence for broader socioeconomic benefits. Similarly, Lee et al. (2018) randomly subsidize the cost of connecting to the grid in Kenya. In their evaluation around 18 months after implementation, they found that a mass electrification program would result in welfare losses for a community, and do not find positive economic impacts. Lenz et al. (2017) find only weak effects of grid electrification on income and other indicators of poverty. In what is perhaps the closest analogue to our study, Burlig and Preonas (2016) use a geographic regression discontinuity and high-resolution geospatial data to evaluate the impact of India's national electrification program. They find evidence of increased electricity usage, but little affirmation of other broad socioeconomic benefits. Similarly, Khandker et al. (2014) show that while rural electrification can reduce the time a household spends to collect firewood in addition to educational and economic improvements, these benefits are typically experienced by rich households. Indeed, Chakravorty and Pelli (2014) show that it is not just access to a grid connection but also the quality of power supply that impacts household incomes in rural India.

In general, studies that have used rigorous methods to detect the causal impact of electrification on socioeconomic well-being have produced a much more mixed picture than the early observational literature (Bayer et al., 2020). Moreover, none of the studies that have attempted to estimate the causal impact of electrification have focused on *legal* grid electricity connections. For developing countries this is a key distinction. In India, as much as one third of the electricity generated may be lost due to theft (Smith, 2004). The prevalence of theft contributes a number of maladies, including poor quality of electricity provided to consumers and issues with the sustainability of distribution companies. From the perspective of impact evaluation, it is legal connections that are of primary interest for policy-makers, since the prominence of illegal connections jeopardizes the quality and sustainability of electrification programs, potentially blunting the impact of electrification policies. In our design, compliers are those that have access to legal electrification and have taken up the connection.

3. Conceptual framework

To build upon these studies and offer concrete hypotheses for our analysis, in this section we introduce a conceptual framework. We are interested in the effect of legal electrification on three outcome groups: expenditures, household activity, and appliance ownership.[2]

The conceptual framework emphasizes the allocation and productivity of time use in the household (Dinkelman, 2011; Barron and Torero, 2014: e.g.). Household members select how to use their (activity) and where (inside or outside home). A household electricity con-

[2] In the pre-analysis plan we noted that we examine the effect of legal electrification on seven outcome families. The hypotheses and associated tests for all outcome families are included in the Appendix. In the main text, we focus on the most important outcomes. Note that we find that legal electrification has a positive impact on household expenditures, lighting used by children, adult activities, number of appliances, satisfaction with lighting, and knowledge about politics and popular culture.

nection changes the productivity of time inside the house through better lighting and electric appliances that improve efficiency, such as bright electric lighting for reading or a smart phone for telecommunications. When households members allocate their time, they consider the improved productivity that access to better lighting and electric appliances enable, shifting time toward activities that are now relatively more productive than without electricity. The household members also consider the income effect of household electrification, as they now need to pay the electricity bill and decide on appliance purchases but need less kerosene and other traditional fuels.

In general, we argue that legal electrification has positive effects on the above outcomes through three channels. First, electrification increases the hours and quality of lighting for households. This allows for greater time to be allocated to income-generating activities and also increases the hours available for leisure at home. The net effect is an increase in household expenditures and household activity. Second, electrification gives households the ability to use more appliances. This, along with the increase in houesholds' budgets, should increase the number of appliances that households own and employ, as housholds will be able to afford new technologies and make use of them. An increase in appliances can further increase household productivity and time available for leisure. Third, electrification can improve the health of households, as electric lights and appliances have less pollutants than other sources. This can increase productivity. We therefore expect positive effects on expenditures, household activity and appliance usage and ownership.

3.1. Expenditures

A number of studies have found a positive association between electrification and total household expenditure, which serves as a proxy for disposable income (e.g. Khandker et al., 2013; Khandker et al., 2014; Bhattacharyya et al., 2017). One possible mechanism previously identified in the literature is through an increase in employment and earnings (Dasso and Fernandez, 2015). We believe electrification will have positive effects on disposable income through three primary channels. The first is through its effect on lighting. Increasing reliable lighting for households can allow domestic tasks and chores to be accomplished at night-time, leaving day-time for income-generating activities. As noted in Van de Walle et al. (2013: p. 5), given proper lighting, "household members can then continue their enterprise work, domestic duties, homework and reading into the evening with potential positive effects on earnings and living standards." Moreover, household members that engage in income-generating activities at home can continue this work into the evening (Van de Walle et al., 2013).

Second, electrification should have positive effects on productivity. Electric appliances and lighting can have less pollutants than other fuel sources, increasing household health and thus productivity. Electricity may also allow households to invest in appliances that reduce the time necessary to complete domestic work. This may increase the time that households have for work. This effect may be especially salient for women, leading to increased female employment (Dinkelman, 2011; Burlig and Preonas, 2016). As time spent on income-generating activities increases, we expect households to be able to spend more on goods such as food and education. Meanwhile, we expect kerosene expenditures to decrease as households use electricity as a substitute.

Third, electricity can be cheaper than other fuel sources, decreasing the costs of heating, cooking and other energy intensive activities. Households can respond to this decrease in cost by either (i) increasing their fuel usage or (ii) reallocating the left over funds to other budgets. In the second case, we would observe an increase in expenditures.

These three mechanisms lead us to predict a positive effect of legal electrification on expenditures. Furthermore, this effect can occur without disruptions to the labor market that could have spillover effects on untreated households. Although we argue that electrification can have increase income-generating activities at home and productivity at home, this is unlikely to be at such a scale as to crowd out the labor of untreated households. Moreover, in the context we study, citizens are guaranteed work under the National Rural Employment Guarantee Scheme (NREGS) and therefore cannot be replaced in the labor market (Bose, 2017). Additionally, the wage level is set by this guarantee and shouldn't respond to minor shocks to the labor market.

To test this relationship, we employ four measures. First, we test the effect of legal electrification on total expenditure. In addition to total this, we also test three specific types of expenditures: expenditure on food (*food*), expenditure on education (*education*) and expenditure on kerosene (*kerosene*).[3] These measures differ from past studies, such as Bensch et al. (2011), which used energy expenditures as an indicator for disposable income. All variables are constructed through direct survey questions about expenditures. Our hypotheses related to these variables are that electrification should increase total, education and food expenditure, while lowering that on kerosene.[4]

3.2. Household activity

We next turn to activity within the household. In general, we expect that access to legal electrification will increase activity at home in two ways: by increasing the time available to households for activities that require light, and by increasing the returns to leisure activities. First, as above, increased lighting allows greater activity within the household after sunset. Household members can read, study and engage in other leisure activities at night-time. Second, access to electricity can increase the returns from leisure, incentivizing greater activity in the home (Barron and Torero, 2014). For example, access to more appliances like radios and televisions can make staying at home more attractive, compared than leaving the household for a public place. Third, electrification may make work at home more attractive, increasing the time spent on such activities (Dinkelman, 2011). However, as noted by Barron and Torero (2017), the net effect on economic activities of this increase in household activity is unclear, precisely becase of the multiple effects we laid out above. Electrification may make working from home more profitable, but also increases the incentives to engage in leisure activities.

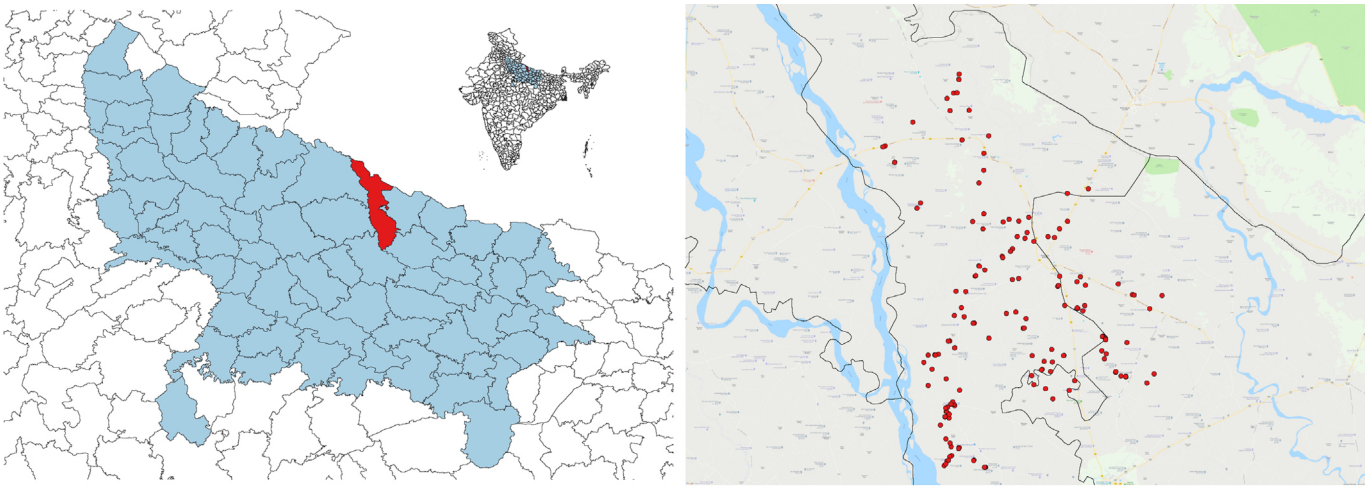
To test the effects of electrification on houeshold activity, we ask separate survey questions about household activity by adults and children, and create two outcome variables. For children's activity (*child activity*) we produce a count variable of the number of hours children spend at home in a given day, and adult's activity (*adult activity*) is the same for adults. We hypothesize that electrification should have a positive effect on both.

3.2.1. Appliances

Our last family of outcomes explores appliance usage (Boait et al., 2015). We therefore hypothesize that electrification should have a positive effect on both variables. The mechanism behind the hypothesized effect is straight-forward: with adequate electricity to use appliances, households will be able to make use of more appliances, and thus purchase and use them. Moreover, increased disposable income can be used to pay for non-essential appliances such as those used for entertainment. Additionally, as the availability of leisure time inside the household grows due to lighting, households will receive greater returns from investing in appliances. For example, some households in our sample reported owning radios, televisions, and mobile phones, which can be

[3] In the pre-analysis plan, we include a hypothesis for a fifth variable, total electricity expenditure, but survey implementation issues prevented us from obtaining results.

[4] When examining our raw data, we discovered several outlier observations for total and food expenditure. Thus, we estimate models without these observations, and also estimate models with the logarithms of these outcome variables. These are included in Section A8.5.



(a) Study Area (b) Pole Locations

Fig. 1. Location of study areas and grid electricity poles. Subfigure 1a shows the location of the Bahraich district of Uttar Pradesh, with the insert highlighting the location of Uttar Pradesh in India. Subfigure 1 b shows the physical location of the poles used for the study against the background of Google streetmaps with city locations, main roads and physical characteristics included.

used during households' increased leisure time. Thus, legal electrification should have a positive effect on the number of appliances owned, and the hours using them.

Appliances are a major mechanism transforming access to electricity into outcomes related to economic well-being and quality of life. Along with lighting, appliances work to free up hours of productivity during the day, increasing labor market participation Richmond and Urpelainen (2019). This effect is especially salient for women Dinkelman (2011). The extra hours can also be shifted to leisure, increasing quality of life. Access to appliances such as mobile phones, radios and television may also have benefits on outcomes as diverse as gender attitudes and fertility (Jensen and Oster, 2009).

To test this, we employ two oucomes. Specifically, we construct a count variable of the number of appliances used by households (*number of appliances*), and the number of hours using appliances (*appliance use*).

4. Research design

To examine the causal relationship between rural electrification and the outcomes introduced above, we employ a instrumental variable design, exploiting a law in the state of Uttar Pradesh, India which gives rural households differential access to legal electricity connections based solely on the distance of a household from a power pole (UPERC, 2010). Specifically, households are required to be within 40 m of a power pole in order to get electricity from that outlet. We sampled households that are within 20–35 m, and within 45–60 m of a power pole (see Section A1).[5] The former group of households would be eligible for an electricity connection from that particular pole whereas the latter group of households would be ineligible for that option.

We do not survey households that do not fall within 35–45 m of a power pole because it increases the likelihood of error on the part of our enumerators and state regulators. We decided to employ this measurement strategy before implementation because of qualitative information on likely measurement error: the threshold created by policy is not measured perfectly, leading to a higher likelihood of non-compliers around the threshold. By creating a buffer zone, we increase the proportion of compliers in our sample.

To gather the data, we conducted an original survey in early 2018 of 686 households across 120 habitations with a total of 154 electricity poles in the Bahraich district along the border with the Lakhimpur district border area of Uttar Pradesh (see Fig. 1). The survey was conducted after the announcement of the Saubhagya scheme to expand rural electrification connections, but prior to its implementation in this area.

We selected the area based on feasibility considerations. The study required an area that had the right mix of electrified and non-electrified households around the most remote power poles. Some districts, such as Sitapur and Barabanki, let alone Lucknow, already had such high pole densities that finding suitable households would be impossible. Conversely, some parts of Lakhimpur would have such low pole densities that finding enough electrified households would be a challenge. Based on these considerations, and after extensive field research both by the investigators and the survey team, we identified the Lakhimpur-Bahraich border as the most suitable area in central Uttar Pradesh.

Despite the constraints of finding a suitable location, our sample is fairly similar to the larger population. To demonstrate generalizability, we use data from the Indian census, as this is the most comprehensive source of population demographics. In Appendix Table A46, we present the means of demographic variables in our sample with the larger population of Uttar Pradesh and India. We also subset these samples by unelectrified households since our sampled location could be similar to this population along some dimensions. In general, our sample is similar to the broader state and national population along multiple measures. We have similar television ownership (11%) with the unelectrified population of Uttar Pradesh (13%), and our sample's mobile phone ownership (48%) is more in line with the national population (45%). We have slightly higher measures on the proportion of scheduled castes but this is expected given the location where we can find places with low pole densities. The proportion of banking services is higher than Uttar Pradesh or India more broadly, but this too is an expected increase given the recent push for financial inclusion by the national government (West, 2015). Radio ownership in our sample (4%) is lower than the state and national means, but this drop could be a reflection of the fact that our study took place seven years after the census.

[5] Households were also excluded if they are between the power pole and the village center, or the power pole and a different power pole. This is to prevent overlap with other poles: in pre-study fieldwork, it was discovered that if households were between the pole and the village, there is the potential for poles hidden behind buildings to not be identified by enumerators and to influence the treatment assignment.

Table 1
Summary of outcome variables, and the hypothesized directions of their relationships with legal electrification.

Family	Outcome	Description	Hypothesis
Expenditure	Total expenditure	Total household expenditure in a typical month measured in rupees.	⇑
	Expenditure on food	Expenditure on food by a household in a typical month measured in rupees.	⇑
	Expenditure on education	Expenditure on education by a household in a typical month measured in rupees.	⇑
	Expenditure on kerosene	Expenditure on kerosene by a household in a typical month measured in rupees.	⇓
Household Activity	Child activity	Number of hours children spend at home in a given day.	⇑
	Adult activity	Number of hours adults spend at home in a given day.	⇑
Appliances	Number of appliances	The number of appliances owned by a household.	⇑
	Appliance use	The number of daily hours using appliances by a household.	⇑

The research design was pre-registered through the Evidence in Governance & Politics (EGAP) website, where we outlined our sampling strategy, data collection, and analysis procedures.[6] Where there is deviation from the pre-analysis plan, due to implementation issues, it is noted in footnotes throughout the manuscript. While pre-registration is not typically standard in quasi-experimental studies, it is important for ensuring that we are not selectively presenting results, nor are we tailoring our methods to accommodate a desired outcome.

To determine our sample size, we conducted a power analysis, the code for which is included in the replication package. In Section A3 of the Appendix, we describe the methods used and assumptions made to conduct the power analysis, including a figure depicting the treatment effect necessary to achieve sufficient power.

4.1. Outcome variables

In the manuscript, we present results for three outcome families: household expenditures, household activity and appliance usage. The results for other outcomes listed in the pre-analysis plan are available in the Appendix. We show the outcomes and their predicted directions in tabular form in Table 1. The survey questions used to measure the outcomes and the constructions of the measures are available displayed in the Appendix. Because we test multiple outcomes within each family, we use Benjamani-Hochberg corrections (Benjamini and Hochberg, 1995) for the p-values we present in the results section. Families are comprised of outcomes related to a single theme.

4.2. Control variables

To sharpen our estimates and to demonstrate balance, we measure five pre-treatment covariates. These variables are the age of the head of househould (*age*), whether or not the head of household is Hindu (*Hindu*), where Muslim is coded as 0 and Hindu is coded as 1, whether or not the head of the household is in a scheduled caste or tribe (*Scheduled caste or tribe*), whether the household head is female (*female household head*), and whether or not the household head was born in the village (*birthplace*). We also include two covariates in the balance table and as controls in a robustness check. They are not included in the main estimation because they could be post-treatment. These are the education level of the household head, (*education level*), measured as the level of schooling completed,[7] and the age of the home (*age of home*). We use a covariate-adjusted approach because it is more efficient than the unadjusted estimator (Calonico et al., 2019), while the estimator remains consistent if the covariate adjustment is equivalent below and above the threshold.

We selected these control variables because, if there was selection into either side of the 40 m boundary, we would expect them to be unbalanced. This is because such variables are generally associated with greater economic status, and thus would be associated with electricity uptake. To show that these variables are associated with electricity uptake in general and that we would not expect balance if there was selection into electrification other than through the discontinuity, we use data from an external survey on electricity access in India from 2018 and estimate linear probability models to assess whether the variables shown here predict grid electricity uptake Jain et al. (2018). In Table A47 in the Appendix, we show that age, education and caste all predict uptake.[8]

4.3. Estimation strategy

To generate causal estimates of the effects of electrification, we employ an instrumental variable design based on local randomization (Cattaneo et al., 2015). As mentioned earlier, we exploit a law in Uttar Pradesh where households located within 40 m of a power pole are allowed legal access to grid electricity, while those located beyond 40 m cannot connect to the grid from the same electrical pole (UPERC, 2010). However, those beyond 40 m could still get access to the grid only through illegal, *katiya*, connections, through an error on the part of government installers, or by paying for another pole.[9] We are, therefore, able to use distance from power poles as an instrument which induces a discontinuous change in the probability of having a legal connection to grid electricity. Our approach is similar to a regression discontinuity, but we do not measure households directly at the border and thus use IV to estimate the effect within our chosen sample. However, the logic is the same: the discontinuity forces similar households to have different probabilities of being treated.

Within the two sides of the discontinuity, the probability of having a grid connection should not change, and thus we code the instrument

[6] The registered design can be found at http://egap.org/registration/3062.

[7] The variable is binary, coded as 1 if the respondent completed the 1st standard or higher. In our sample, 471 respondents had no formal education, and 215 had completed the 1st standard or above.

[8] The external data source did not have data on the birthplace of the head of household and the age of home.

[9] Although paying for a pole is technically feasible, in practice we don't expect to have cases where consumers purchased poles in our sample. This is for two reasons: first, if consumers were to purchase poles, we would not expect their household to be located far enough away from the pole to be captured in our sample. Purchased pole would likely be placed near households in order to avoid ambiguity in receiving access to electricity and to facilitate a stable connection. Second, purchasing a pole is costly, making it unlikely for the households in our sample. According to the cost data book from the Uttar Pradesh Electricity Regulatory Commission, for households beyond 40 m, the customer must pay variable line charges by meter of at least 200 rupees UPERC (2010).). To understand this cost, suppose, for example, that a household lives very close to the 40 m border, 5 m away. The cost of application would now increase from 800 rupees to 1800 rupees (800 rupees for the original charge and 200 for each meter). On top of that, the household might have to wait until enough other households also apply for a connection, as installing an additional pole to serve just one household would be very expensive for the electricity distribution company. Indeed, the household might have to pay a bribe because state regulations force the electricity distribution company to cover the extra cost beyond 200 rupees per meter. Anecdotally, the authors did not hear of poles that had been purchased when in the field prior to data collection.

Table 2
This table displays descriptive statistics regarding our sample. We show that there is variation in the level of connected households on either side of the discontinuity, but connection is heavily favored within the legal range.

Variable	N
Households	686
Poles	154
Habitations	120
Households in Legal Distance	344
Households Outside Legal Distance	342
Connections in Legal Distance	214
Connections Outside Legal Distance	11
No Connection	461
Percent Connected in Legal Range	62.2%
Percent Unconnected in Legal Range	38.8%
Percent Connected in Illegal Range	3.2%
Percent Unconnected in Illegal Range	96.8%

Table 3
This table depicts the balance between our pre-treatment covariates on either side of the discontinuity. Overall, balance is achieved: through t-tests, we are unable to reject the null hypothesis that the true difference in means is equal to 0 for any of the covariates except Age of Home. This lends support to the validity of our identification strategy. Covariates below the horizontal line were not included in the pre-analysis plan.

Covariate	Control Mean	Treatment Mean	p.value
Female Household Head	0.231	0.250	0.561
Birthplace	0.769	0.759	0.752
Age	42.020	41.950	0.940
Hindu	0.713	0.706	0.839
Scheduled Caste/Tribe	0.406	0.419	0.747
Education Level	0.316	0.311	0.894
Age of Home	13.890	16.840	0.005

(*distance*) as a binary variable: it take a value of 1 when households are within 20–35 m of a power pole, and 0 when they are within 45–60 m of a power pole. In this design, we thus only investigate households that are very close to the cutoff. In a classical regression discontinuity, households are examined at various distances and a continuous forcing variable is included as a covariate (Imbens and Lemieux, 2008). In a local randomization design, however, only households very close to the cutoff are investigated and the assumption is that the instrument produces a quasi-random assignment (Cattaneo et al., 2015). We opt for the local randomization design because a classical discontinuity design would require a large number of surveys at different distances, resulting in an expensive survey that is mostly irrelevant to the task at hand.

To account for error in measuring distances, we do not survey households that do not fall within these two ranges.[10] Our treatment variable (*legal electrification*) is binary as well, taking a value of 1 if the household is connected to the grid legally, and 0 if it has an illegal connection, or no connection. The number of households falling into the relevant groups is shown in Table 2.

Our estimation strategy takes the form of a two-stage least squares instrumental variable strategy. In the first stage, we estimate the effect of the instrument on the endogenous treatment variable. Thus, we estimate

$$\widehat{Legal}_{hpv} = \beta Distance_h + X_h + \mu_p + \epsilon_{hpv},$$

where $Legal_{hpv}$ is a binary indicator of legal electrification for household h around pole p in habitation v. X is a vector of pre-treatment covariates. μ_p are pole fixed effects. Standard errors are clustered at the pole level, as it is the randomization block.

The general form of the second stage estimation is:

$$Outcome_{hpv} = \beta \widehat{Legal}_{hpv} + X_h + \mu_p + \epsilon_{hpv}.$$

The validity of our estimation strategy hinges on several assumptions in order to identify the local average treatment effect (LATE). First, it requires deterministic monotonicity, where the level of treatment taken is a monotonic increasing function of the level of the instrumental variable. We argue that households are never less likely to become electrified when they have legal access to electrification. Second, identification requires no interference, meaning that the treatment assignment of one household does not affect the behavior of another household. For outcomes such as electricity usage, appliance ownership and other behaviors, we do not anticipate spillover effects. However, this assumption may be tenuous in the case of subjective satisfaction, aspirations and knowledge.[11] Third, identification requires that the instrumental variable has no direct effect on the outcome. Given that our instrument is based on a discontinuity, we believe the exclusion restriction holds. However, one possible threat to the exclusion restriction would be if households in the legal range are more likely to have illegal connections because they are located closer to the pole. However, our design limits the possibility of this threat because we only compare households within a close bandwidth to the boundary where legal connections are possible. Second, enumerators observed only 12 illegal connections in our entire sample, and none of these were located in the legal range. Third, given that a household is located in the legal area, the cost of acquiring a connection is minimal (800 Rupees or approximately 10.5 U.S. dollars), limiting the incentive to gain access to electricity through illegal means (UPERC, 2010).

Identification requires two further requirements which the following sections focus on. First, households located on either side of the discontinuity must in expectation be similar in pre-treatment covariates. To demonstrate this, we will show balance on all five of our control variables. Second, our instrument must not be a weak instrument for the treatment. We conduct a Wald test on the first stage of our estimation to show that this is not the case.

4.4. Balance

Our estimation strategy relies on the assumption that households located on either side of the discontinuity differ only in their access to electrification. To demonstrate that this is the case, we show balance on our five pre-treatment covariates. Specifically, we conduct t-tests for the means of our covariates to check for significant differences between variables that fall on either side of the discontinuity. We are unable to reject the null hypothesis that the true difference in means is equal to 0 for any of the covariates. The results are shown in Table 3.

We also note in Table A1 that there is no evidence of homes locating strategically close to electricity poles. For households connected to the grid, the average time that the household has been connected is about 4.3 years, while the average age of homes is more than 15 years. As homes are much older than connections, it seems highly unlikely that the homes would have been located to take advantage of the pole. Instead, poles were built later and connections made available to those homes that were within 40 m. This, in turn, explains why our pre-treatment balance tests indicate no difference in caste status, household

[10] This forms a natural "donut" design that avoids selection at the discontinuity point that might undermine causal inference. Nonetheless, we performed a standard McCrary test for selection at the cutpoint. The results suggested there were no problems with selection ($p = 0.666$).

[11] These outcomes are available in Appendix. A spillover for the knowledge outcome would be the control group gaining access to information they otherwise wouldn't have by either using other households' TVs or radios, or by conversing with treated households. If this was the case, we would expect our results to be biased downwards.

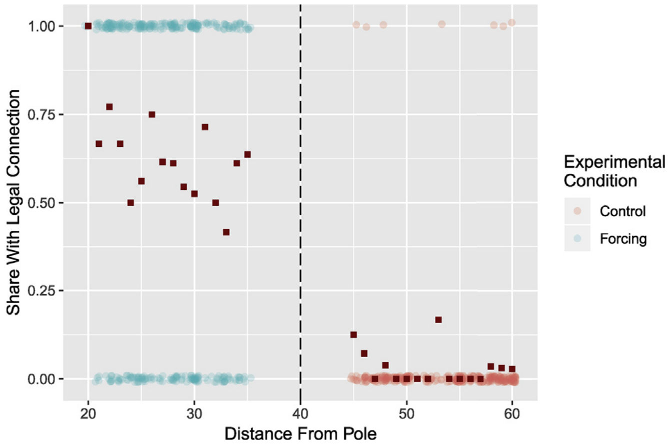


Fig. 2. This figure demonstrates the validity of using distance from power poles as an instrument for legal electrification. It shows that the probability of having a legal connection is much higher on one side of the discontinuity. The raw data is jittered to show clustering. The squares represent the share of observations with legal connections at each meter within our sample.

head age, or other covariates that should predict home location if there was self selection into households closer to poles.

4.5. First stage

For our instrumental variable estimation strategy to estimate a causal effect, our instrument must not be weak, or else the estimate will be biased towards OLS estimates. To test the strength of our instrument, we conduct an Wald test. The resulting F-Statistic is 435.32 with a p-value less than 0.01. We are able to reject the null hypothesis that the instrument is weak. We also show the relationship graphically, demonstrating a clear difference in the relationship between the instrument and the likelihood of being in the treatment group. This is shown in Fig. 2.

5. Results and discussion

Our results generally support the hypotheses laid out above. In every analysis, p-values are corrected through the Benjamini-Hochberg method within outcome families (Benjamini and Hochberg, 1995). We first present coefficient plots for all other outcomes, and then present unscaled results. Coefficient plots display standard scores, where for each outcome vector, we subtracted the mean and then divided by the standard deviation. Thus, the coefficients should be interpreted as the number of standard deviations above the mean value of the variable.

5.1. Full results

Fig. 3 shows the results for our main outcomes using scaled coefficient plots.[12] In all plots, standard errors are clustered at the pole level. Dependent variables are listed on the left side, and the coefficients represent the effect of the treatment on these outcomes.[13]

In the first plot, we show the results for expenditures. All coefficients are in a positive direction suggesting that electrification has a positive effect on total expenditure, expenditure on food and expenditure on education. Surprisingly, we also find that electrification increases

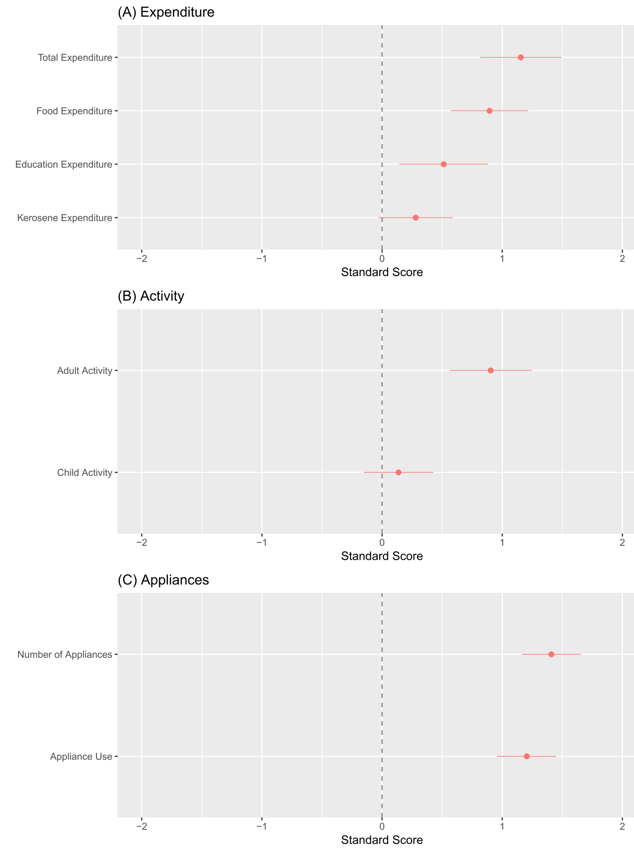


Fig. 3. Effect of Electrification on Indicators of Main Outcomes. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$. All p-values reported are corrected within outcome families using the Benjamini-Hochberg method.

expenditure on kerosene, though this result is not statistically significant at conventional levels. This could be explained partially by the fact that households still purchase kerosene because it is heavily subsidized. It also is similar to findings by Bensch et al. (2011), who found that electrification increased expenditure on energy in general.

Unscaled results for expenditures are shown in Table 4. Total expenditure and food expenditure see a massive increase for legally electrified households. The increase in expenditure on education is not as high, but is still significant.

Next, Subfigure B presents the results for our estimation of the relationship between electrification and activity within the household. The results conform to our expectations: electrification has a significantly positive effect on activity in the household for adults, and a positive but not significant effect on activity in the household for children. These positive effects are similar to those found by Bensch et al. (2011), who found that electrification increased the number of hours children spent studying at home. We show the unscaled results in Table 5.

The positive effects of electrification on household activities may serve as an explanation for the positive relationship between electrification and education in past studies (Hassan and Lucchino, 2016). The finding is similar to that of Barron and Torero (2014), who show that electrification leads to increased time investment in education.

We further examine the effects of legal electrification on adult activity by examining specific activities separately.[14] The results for the analysis are in Table 6. We find that legal electrification increases time spent at home for all activities except for leisure. This indicates that electricity is being used primarily to increase the earning potential of

[12] Tabular versions of all results are available in the Appendix.

[13] Since there may exist outliers in the expenditure variable due to misreporting, we estimated the models without them and also used the logarithm of the outcome variable. All results are available in the Appendix. Our results are robust to these changes.

[14] We did not pre-register this analysis.

Table 4
This table shows the unscaled relationship between legal electrification and expenditures. We test the relationship further accounting for outliers in section A3.3. p-values reported are corrected within outcome families using the Benjamini-Hochberg method.

	Dependent variable:			
	Total Expenditure (1)	Food Expenditure (2)	Education Expenditure (3)	Kerosene Expenditure (4)
Legal Electrification	4509.000***	3023.000***	655.000***	17.560*
	(672.500)	(555.800)	(239.900)	(9.849)
Constant	6865.000***	6031.000***	242.200	65.890***
	(1217.000)	(1028.000)	(413.700)	(15.420)
Control Variables	Yes	Yes	Yes	Yes
N Poles	152	152	152	152
Observations	685	686	686	686
Adjusted R^2	0.135	0.075	0.084	0.092

Note: *p < 0.1; **p < 0.05; ***p < 0.01.
Standard errors clustered at pole level. Pole fixed effects included.

Table 5
This table shows the relationship between legal electrification and household activity. We find that legal electrification increases adult activities in the household but has no effect on the activities of children. p-values reported are corrected within outcome families using the Benjamini-Hochberg method.

	Dependent variable:	
	Adult Activity (1)	Child Activity (2)
Legal Electrification	0.640***	0.122
	(0.123)	(0.131)
Constant	3.789***	2.402***
	(0.217)	(0.246)
Control Variables	Yes	Yes
N Poles	152	152
Observations	686	686
Adjusted R^2	0.365	0.172

Note: *p < 0.1; **p < 0.05; ***p < 0.01.
Standard errors clustered at pole level. Pole fixed effects included.

Table 7
This depicts the relationship between legal electrification and appliances. Legal electrification has a clear, positive effect on appliance ownership and usage. p-values reported are corrected within outcome families using the Benjamini-Hochberg method.

	Dependent variable:	
	Number of Appliances (1)	Appliance Use (2)
Legal Electrification	3.760***	11.120***
	(0.333)	(1.159)
Constant	2.038***	3.550
	(0.686)	(2.258)
Control Variables	Yes	Yes
N Poles	152	152
Observations	686	686
Adjusted R^2	0.461	0.354

Note: *p < 0.1; **p < 0.05; ***p < 0.01.
Standard errors clustered at pole level. Pole fixed effects included.

the household, which can explain why we observe an increase in expenditures, and an increase in appliances owned.

Finally, Subfigure C shows that the results for appliances also follow our predictions. Having a legal electrical connection increases both the number of appliances used by households are the amount of hours per day appliances are used. This result is intuitive, and follows past results from (Lee et al., 2016). Unscaled results are displayed Table 7. In the Appendix section A8.8, we show which appliances in particular electrified households use. Electrified households are more likely to use all types of bulbs and lights, refrigerators and radios.

As a whole, we find quite positive medium-term effects of legal electrification in a rural setting. As noted in the literature review, these positive results run contrary to past studies, such as the experimental approach of (Lee et al., 2018). These opposing findings could be due to the timeframe in which the evaluations took place– it is possible that effects only become clear after a longer period of time has elapsed. Moreover, it is important to note that in our setting, positive effects were found despite relatively low numbers of daily hours of electricity, as shown in Table A1 in Section A5 of the Appendix. This indicates that the quality of electricity supply cannot explain our positive results.

Table 6
This table shows the relationship between legal electrification and adult activity by activity. Legal electrification increases all activities except for leisure.

	Dependent variable:					
	Work (1)	Cook (2)	Charge (3)	Clean (4)	Leisure (5)	Time Spent (6)
Legal Electrification	1.877***	0.696***	0.156***	0.288***	−0.093	0.915***
	(0.377)	(0.150)	(0.045)	(0.092)	(0.236)	(0.283)
Constant	4.160***	1.710***	0.141	1.475***	2.848***	12.400***
	(0.592)	(0.233)	(0.110)	(0.186)	(0.527)	(0.538)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
N Poles	152	152	152	152	152	152
Observations	686	686	686	686	686	686
Adjusted R^2	0.159	0.274	0.080	0.198	0.306	0.372

Note: *p < 0.1; **p < 0.05; ***p < 0.01.
Standard errors clustered at pole level. Pole fixed effects included.

5.2. Robustness

In the Appendix, we estimate all of the above models also including village fixed effects to account for unobserved heterogeneity at the village level. All of our results are robust to the inclusion of these fixed effects. These results can be seen in section A8.3 of the Appendix. We further test our results by including covariates for the level of education attained by the head of household and the age of the homes in section A8.4 of the Appendix. In general our results hold, and no results change signs.

We also show reduced form results for our main analysis in A8.2. The estimation equation is

$$Outcome_{hpv} = \beta Distance_{hpv} + X_h + \mu_p + \epsilon_{hpv}$$

The intent-to-treat estimates are predictably lower than the local average treatment effect presented in the main analysis.

We also account for potential outliers for the Expenditures outcomes in the Appendix, in section A8.5. We re-estimate models without outliers, and also consider the logarithm of *Total Expenditure* and *Food Expenditure*. Our results are consistent across models.

6. Conclusion

Estimating the causal impacts of grid electrification has often proven difficult, given that such expansion is not randomly assigned. Moreover, estimating causal effects by looking at discontinuities at the village level can underestimate the impact of electrification when policies are targeted towards underprivileged areas (Burlig and Preonas, 2016). This is concerning for policy-makers, since they are spending billions of dollars and dedicating significant staff towards achieving the goal of rural electrification. Without solid evidence on benefits of household electrification in different circumstances, rural electrification schemes might produce unsatisfactory results.

We attempt to overcome these issues by exploiting a unique policy for grid access in Uttar Pradesh, India, which requires households to be within 40 m of an electricity pole to receive a legal connection. The major advantage of this quasi-experimental approach is that it allows us to investigate the medium-term outcomes of household electrification. Because the policy has been in force for years, the average household in our quasi-treatment group has had access to electricity for over four years. Other notable features of our design include an original survey, which allows us to collect data on a wide range of potentially important outcomes, and pre-registration to prevent data mining.

We find that this policy results in a substantial discontinuity in legal electrification rates between households within the 40 m area and those just outside the area that allows us to estimate the causal impact of electrification. The results, in turn, suggest that electrification has a significant and positive impact on most of our outcomes of interest, increasing household expenditures, adult household activity, and appliance ownership and usage.

These results suggest that access to grid electrification significantly improves the lives of households with access to it, and supports the efforts of governments to continue expanding this access. Possible explanations for our positive results include the fact that we did not target underprivileged villages and that the households in our sample were typically electrified over four years ago. We did not limit our intervention to basic energy access, as in Aklin et al. (2017), or focus on short-term outcomes, as in Lee et al. (2018). Our results highlight the possibility that household electrification does produce significant benefits, but these materialize only slowly over time.

At the same time, of course, it bears remembering that our design is based on the assumption of quasi-randomness. A long-term randomized controlled trial could shed light on possible bias from any weaknesses in the quasi-randomization process. Such a study would be a natural next step in the research program on impact evaluation of rural electrification. The challenge of household electrification remains a major issue in Sub-Saharan Africa, and a long-term study based on a randomly assigned intervention could offer new insights into whether, how, and to what extent governments should prioritize household electrification over other goals.

Furthermore, although we considered several families of outcomes in this study, electrification can affect all facets of life, and thus other outcomes should be considered for future studies. For example, electrification may increase land or property values, further increasing the long-term wealth of electrified households. A well-designed, long-term study of a randomly assigned electrification scheme could effectively determine whether this effect exists.

CRediT authorship contribution statement

Daniel Robert Thomas: Conceptualization, Methodology, Writing - original draft. **S.P. Harish:** Conceptualization, Methodology, Writing - original draft. **Ryan Kennedy:** Conceptualization, Methodology, Writing - original draft. **Johannes Urpelainen:** Conceptualization, Methodology, Writing - original draft.

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jdeveco.2020.102520.

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