Title
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Authors
Lee, Kenneth
Miguel, Edward
Wolfram, Catherine

Publication Date
2019-12-12

Series Name: WPS
Paper No.: 102
Issue Date: 12 Dec 2019

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Recommended Citation:

Lee, Kenneth; Miguel, Edward; Wolfram, Catherine. (2019). Does Household Electrification Supercharge Economic Development? Working Paper Series No. WPS-102. Center for Effective Global Action. University of California, Berkeley.

DOES HOUSEHOLD ELECTRIFICATION SUPERCHARGE ECONOMIC DEVELOPMENT?

Kenneth Lee
Edward Miguel
Catherine Wolfram

We are grateful to Felipe Vial, Zachary Obstfeld, Nishmeet Singh, Aishwarya Kumar, and Rongmon Deka for excellent research assistance. An earlier version of this paper was funded with support from the UK government, as part of the Department for International Development (DFID) supported Energy and Economic Growth (EEG) research program based at the Center for Effective Global Action (CEGA) and the Energy Institute at Haas (EI) at the University of California, Berkeley. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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December 2019
JEL No. O13,Q40

ABSTRACT

In recent years, electrification has re-emerged as a key priority in low-income countries, with a particular focus on electrifying households. Yet the microeconomic literature examining the impacts of electrifying households on economic development has produced a set of conflicting results. Does household electrification lead to measurable gains in living standards or not? Focusing on grid electrification, we discuss how the divergent conclusions across the literature can be explained by differences in methods, interventions, potential for spillovers, and populations. We then use experimental data from Lee, Miguel, and Wolfram (2019) — a field experiment that connected randomly-selected households to the grid in rural Kenya— to show that impacts can vary even across individuals in neighboring villages. Specifically, we show that households that were willing to pay more for a grid electrification may gain more from electrification compared to households that would only connect for free. We conclude that access to household electrification alone is not enough to drive meaningful gains in development outcomes. Instead, future initiatives may work better if paired with complementary inputs that allow people to do more with power.

Kenneth Lee
The University of Chicago Center in Delhi
DLF Capital Point Building
Baba Kharak Singh Marg Connaught Place
New Delhi, Delhi, 110001
India
kennethlee@uchicago.edu

Catherine Wolfram
Haas School of Business
University of California, Berkeley
Berkeley, CA 94720-1900
and NBER
wolfram@haas.berkeley.edu

Edward Miguel
Department of Economics
University of California, Berkeley
530 Evans Hall #3880
Berkeley, CA 94720
and NBER
emiguel@econ.berkeley.edu

A randomized controlled trials registry entry is available at https://doi.org/10.1257/rct.350-8.0

While launching the *Sustainable Energy for All* program to promote rural electrification in 2011, then-United Nations Secretary General Ban Ki-moon described energy as "the golden thread that connects economic growth, increased social equity, and an environment that allows the world to thrive" (SEFA 2012). Reinforcing this perspective is the strong, positive cross-country correlation between electricity consumption and GDP per capita documented in the macroeconomic literature (for example, Burke, Stern, and Bruns 2018), which we present in Figure 1. Today, nearly a billion people still live without access to electricity (IEA 2018). And, access to energy has re-emerged as a key priority for policymakers and donors in low-income countries. Electrification could allow poor households to have easy access to lighting for evening chores or studying, and power for phone charging and possibly for a range of new small business activities, both on and off the farm.

The idea of a government-subsidized mass electrification program can be traced back to the historical "big push" development efforts of the previous century. In the United States, initiatives like the Tennessee Valley Authority and the Rural Electrification Administration, both of which were launched in the 1930s, dramatically expanded electricity generation capacity and rural electrification rates across the American South and other regions. Recent research finds that these programs generated meaningful long-run economic benefits (Kline and Moretti 2014; Kitchens and Fishback 2015; Lewis and Severnini 2017).

Nearly a century later, substantial investments are still being made to expand energy access around the world. However, the focus of some of the influential development policies that are in place today – like the United Nations' *Sustainable Development Goal 7*, which targets universal access to energy by 2030, and the U.S. *Power Africa* initiative, which aims to add 60 million new electricity connections across Africa – is largely placed on increasing household electrification

rates. But the evidence on how much, and in what ways, modern-day residential electrification alone contributes to economic development is not always clear, and is sometimes in conflict.

In this paper, we discuss what we can learn from the past decade of microeconomic research on the impacts of household electrification, with the goal of highlighting how future initiatives can be better designed. We begin with an overview of how household electrification has traditionally been captured in official statistics, and then turn to some of the historical electrification programs from around the world, paying special attention to those that are most closely related to the settings that have been studied over the past decade or so.

Broadly, the earlier research from this period suggest that access to electricity is a driver of economic development. At the regional-level, electrification appears to increase manufacturing output (Rud 2012); agricultural and manufacturing employment (Kline and Moretti 2014); and the UN Human Development Index (HDI) and average housing values (Lipscomb, Mobarak, and Barham 2013). At the household-level, which is the focus of this paper, electrification leads to improvements in summary measures of well-being, such as income, expenditure, and consumption (World Bank 2008; Khandker, Barnes, and Samad 2012; van de Walle et al. 2015; Chakravorty, Emerick, and Ravago 2016). The primary mechanisms through which electrification impacts development outcomes include increases in labor supply, particularly for women (Dinkelman 2011; Grogan and Sadanand 2013); higher schooling attainment for children (Khandker et al. 2014; Akpandjar and Kitchens 2017); and better respiratory health (Barron and Torero 2017), among others.[1]

[1] A related literature addresses how low and middle income country firms respond to electricity shortages (the intensive margin) instead of the presence or absence of electricity (the extensive margin). Generally, firms invest in back-up generators as a substitute for grid electricity (Steinbuks and Foster 2010), which can limit their overall productivity losses (Allcott, Collard-Wexler, and O'Connell 2016); outsource, essentially substituting electricity inputs with other types of intermediate inputs (Fisher-Vanden, Mansur, and Wang 2015); or switch to more electricity-efficient technologies (Alam 2013).

However, a number of these studies rely on relatively strong and untested econometric assumptions, making it a challenge to disentangle the causal effects of electrification on development outcomes from other factors that may also be changing with electrification rates. There may also be lingering reverse causality issues, since economic growth – current or anticipated – may in turn drive greater electricity consumption. More recent studies exploiting experimental or quasi-experimental designs find far less pronounced impacts of electrification on both economic and non-economic outcomes, most of which are statistically indistinguishable from zero, at least in the medium-run (Burlig and Preonas 2016; Lee, Miguel, and Wolfram 2019).

Note that we do not conduct a comprehensive literature review, given that there is already excellent work along these lines. For example, see Bayer et al. (2019) for a systematic review; van de Walle et al. (2015) for a general literature review; Morrissey (2018) for a discussion on productive uses of electric power; Peters and Sievert (2016) for a discussion of the studies using African data; and Bernard (2012) for historical context on electrification initiatives in Sub-Saharan Africa. Instead, we attempt to fill a gap in previous reviews by discussing why the existing set of studies might reach such different conclusions, focusing on differences in econometric methods, the types of electrification interventions studied, the potential for spillovers, and differences in regions and populations. To demonstrate how impacts can vary across subgroups of the same population, we build upon the randomized controlled trial design in Lee, Miguel, and Wolfram (2019) to estimate the heterogeneous treatment effects of household grid connections in rural Kenya. We find suggestive evidence that greater gains from electrification are likely to be concentrated in certain subgroups of households. In our example, it is in households that are willing to pay more for an electricity connection at baseline.

Our main point is that providing poor households with access to electricity alone is not enough to improve economic and non-economic outcomes in a meaningful way. The literature documents large gains from electrification in a number of settings, but in many cases, we cannot rule out the possibility that other factors – either correlated with or visibly part of the electrification efforts – are driving economic outcomes. Universal energy access is arguably an important goal for global equity considerations. But large-scale contemporary initiatives to expand residential access to electricity may not produce meaningful economic impacts unless they are combined with complementary programs that will make electrical appliances more accessible, or are targeted towards regions that already benefit from complementary factors.

Measuring Access to Electricity

How electrification is defined and measured is important because it shapes our views on the nature of energy poverty and the solutions that are required. Access to electricity has historically been characterized as a binary state: that is, households have either been considered "on-grid" or "off-grid." In the World Bank's World Development Indicators (WDI) database, for example, the only regularly-tracked electrification data point is "access to electricity," which is presented as a simple percentage of the population and, crucially, is only recorded for the residential sector.

But electrification is clearly more than what is captured in a binary variable. The term "off-grid," for example, evokes images of remote, rural households that are too far away to connect to power. In Lee et al. (2016), we demonstrate how, just prior to the recent rapid expansion of the rural electricity grid in Kenya, the majority of households were "under-grid," or close enough to be connected to a low-voltage line at a reasonable cost. This is an important distinction because

the appropriate policy responses for under-grid communities (which could potentially be connected to the grid) may be different from those for truly off-grid communities, which may require the large-scale expansion of national grid infrastructure or stand-alone mini-grid or microgrid systems. Another dimension of access to electricity is the reliability of service, an issue that plagues grid-connected households in many low- and middle-income countries. In Nigeria, the electricity connection rate was nearly 60 percent in 2016, but the reliability of electricity was so poor that most people needed to obtain their power from small, diesel generators (as reported in Onishi 2015).

Efforts are underway to expand the way household electrification is measured. The World Bank's Energy Sector Management Assistance Program (ESMAP), for instance, has introduced a new approach called the Multi-Tier Framework (MTF), in which the measured level of electrification gradually increases with the capacity, duration, reliability, quality, affordability, legality, and safety of electricity access (available at: https://www.esmap.org/node/55526). But for now, we still lack basic data describing how energy poverty varies across space, both in access and in reliability. And, even with an expanded delineation of household access, variation in electricity services for non-residential customers, including factories, small businesses, schools, health centers, etc. will remain unmeasured. This has been a common limitation across most of the existing literature, which collapses all variation in electricity access into a single indicator. We return to this issue later in this paper, when discussing differences in the types of interventions studied.[2]

[2] In the online appendix available with this paper at the *Journal of Economic Perspectives* website, we present an example of a new approach to capturing energy poverty across Africa in terms of "missing" night lights, based on the difference between local population density and nighttime brightness, presented in online Appendix Figure A1.

Electrification Initiatives and Estimates

In Table 1, we summarize some of the historical rural electrification efforts that are closely related to the settings studied in the recent microeconomics literature.[3] For each initiative, we note the national and rural electrification rates and GDP per capita at the start and end of the electrification period.

What immediately stands out is how different many of these initiatives are from one another. For example, consider the wide range of starting income and electrification levels across the various initiatives. In the United States, the Rural Electrification Administration was formed in 1935, when GDP per capita was about $9,644 (in 2017 dollars), roughly eight times higher than the GDP per capita in Kenya and India at the beginning of their own respective initiatives. Based on the difference in average income levels alone, it is plausible that newly electrified households and farms in the 1930s United States would have been much better positioned to acquire complementary inputs compared to their more recent counterparts in Kenya and India.

The U.S. Rural Electrification Administration was distinctive for several other reasons as well. First, unlike the more recent initiatives in Kenya and India (in which government programs directly connected households and villages to the grid), it was designed to provide low-interest loans to newly-formed agricultural cooperatives that were themselves responsible for connecting farms to the grid and paying back the loans. Second, it was introduced at roughly the same time as a number of other New Deal-era programs — including public works programs, and fiscal and monetary reforms — and involved efforts to promote and raise awareness about the productive agricultural applications of electricity – such as cooled milk storage and spray irrigation – as well

[3] This list does include many large economies, although China is absent. Speculatively, we believe the list of countries largely reflects settings in which there is appropriate data for research.

as domestic applications like electric lighting, heated water, electric stoves, and washing machines (Kitchens and Fishback 2015). There was also an associated financing program to facilitate household purchases of appliances. We raise this example to highlight the contextual factors that may have also contributed to the success of the U.S. electrification experience.

How have researchers estimated the impact of electrification on household economic development outcomes across these various episodes? Nearly all existing studies use economic survey data to estimate versions of a regression equation in which the dependent variable is a key outcome of interest (such as labor supply or schooling years) for an observed unit (typically a household or a region) at a certain point in time, and the key explanatory variable is a measure of electrification, which is typically a binary variable indicating whether a household has an electricity connection.

Obvious issues arise if the coefficient on the electrification variable is interpreted as capturing the causal effect of switching from no connection to an electricity connection. The primary challenge is that electrification is likely to be correlated with other factors that jointly determine current and expected levels of the outcomes of interest. For example, consider a setting in which there were no subsidies for electricity connections. The households that are connected to power are probably those with higher incomes, wealth, access to credit, and education, or those who believe they would benefit most from an electricity connection. It would be misguided to conclude that any differences between connected and unconnected households can be attributed to differences in electricity access alone.

Similarly, consider how a government (or electric utility) might plan its roll-out of electricity infrastructure. If political concerns are prioritized, electric grid investments may be targeted towards districts that are favored by a ruling government party, and these same districts

could also be in line to benefit from myriad other government assistance programs.[4] Here, the electrification variable would capture a broader pattern of government favoritism. Alternatively, they may be targeted towards areas that are predicted to have greater potential for economic growth, perhaps due to the presence of a valuable local commodity or the establishment of a new industry that will attract additional labor, further boosting local economic activity. Clearly, it would be misguided to conclude that extending electrification to areas lacking this potential would generate the same effects.

In these examples, omitted variable bias would lead the analyst to overestimate the causal effect of electricity. Of course, these can be addressed using various well-known econometric strategies, including difference-in-differences, instrumental variables, regression discontinuity designs, randomized controlled trials, and other methods. But even amongst studies that use these methods, the past decade of work on this topic has resulted in a wide range of estimated effects.

To illustrate this point, we focus on two important outcomes that are prominently studied in the recent microeconomics literature: labor supply and education. Following the seminal work of Dinkelman (2011) on South Africa's experience with rural electrification in the 1990s, numerous studies have examined whether electrification impacts the allocation of household labor resources. The leading hypothesis is that the availability of electricity inside a home reduces the amount of time required for certain household tasks, and that this primarily frees women to pursue and benefit from external employment opportunities.

In Figure 2, Panel A, we present key estimates of the impact of electrification on labor supply, separating by male and female wherever possible. In order to compare different studies on

[4] For example, Min and Golden (2014) find evidence that politicians in India may manipulate the supply of electricity (for example, by allowing more theft to occur) to influence the outcomes of upcoming elections.

the same scale, each coefficient estimate is expressed as a percentage of the mean of the dependent variable. Along the bottom of the figure, we note the econometric strategy used to address the core identification problem for each estimate. In South Africa, rural electrification led to a large 9 to 9.5 percentage point increase in local female employment on a mean of 7 percent baseline female employment (Dinkelman 2011). Similarly large positive results are documented in Brazil (Lipscomb, Mobarak, and Barham 2013) and Nicaragua (Grogan and Sadanand 2013), two other studies that use instrumental variable approaches.

In more recent work, however, the pattern of a large and positive impact on female labor supply seems to disappear. For instance, van der Walle et al. (2015) find only a small effect in rural India using an instrumental variable approach; Burlig and Preonas (2016) find no economically or statistically significant effect in rural India using a regression discontinuity design; and in Lee, Miguel and Wolfram (2019), we find only a modest effect for women (and almost no effect for men) in rural Kenya using a randomized controlled trial.

Similarly, in Figure 2, Panel B, we present key estimates of the impact on education-related outcomes, again separating for boys and girls wherever possible. In theory, electrification introduces the possibility of electric lighting, which allows children to study for longer hours in the evening, and this may result in improved test scores and higher schooling attainment. Similar to labor supply, the earlier set of studies suggest that electrification has large, positive impacts on education-related outcomes. In Vietnam, Khandker, Barnes, and Samad (2013) use an instrumental variable approach to estimate a 0.9 year increase (21.9 percent) in schooling for girls. But more recent studies in India and Kenya find no statistically significant changes in school enrollment or

test scores, using instrumental variable, regression discontinuity, and randomized controlled trial approaches.[5]

How can we make sense of these conflicting results? In the next section, we discuss the role of differences in econometric methods, interventions, levels of measurement, regions, and populations in explaining these patterns.

Making Sense of Divergent Estimates

Different Methods

Electricity grid infrastructure is costly and long-lived, and its planning and construction requires the inputs of multiple stakeholders. Thus, it is rarely randomized, and instead is likely to be endogenous to a variety of economic and political factors. Although all of the studies presented in Figure 2 attempt to address selection bias in their own way, each approach relies on a set of assumptions.

Dinkelman (2011), for example, employs an instrumental variables method, utilizing land gradient as an instrument for the wave of rural electrification that followed the end of apartheid in South Africa. Higher land gradient raises the average construction cost of a household connection, and so it is likely to factor into the probability of electrification. In addition, it is not immediately clear why land gradient would be correlated with local employment other than through its effect on construction costs. Thus, it is plausible that using land gradient in an instrumental variable approach can produce unbiased estimates of impacts.[6]

[5] This pattern is also observed in a comparison of key estimates of the impacts of electrification on income, presented in online Appendix Figure A2.

[6] In technical terms, this is the same as saying that the "exclusion restriction" should hold. Note that the instrumental variable method requires that an instrument is *informative* (that is, $E(z_i E_i) \neq 0$, where z_i is the instrument and E_i is the electrification status for household i) and *valid* (that is, $E(z_i \varepsilon_i) = 0$, where ε_i is the error term in the regression described in the previous section). The latter condition is referred to as the "exclusion restriction."

Many of the studies on electrification use an instrumental variable approach in a similar way and attempt to isolate the variation in the electrification variable that can be attributed to a set of exogenous cost considerations. Lipscomb, Mobarak, and Barham (2013), for example, use a time series of hypothetical electricity grids — that simulate how the grid would have evolved had investments been based solely on geographic cost considerations — as an instrument for the actual evolution of the electricity grid in Brazil. Other studies construct instrumental variables based on distances between households (or communities) and the nearest grid infrastructure, assuming that proximity to existing infrastructure is correlated with the cost of grid extension but uncorrelated with current and future economic outcomes (for example, Khandker, Barnes, and Samad 2012; Van de Walle et al. 2015; Chakravorty, Emerick, and Ravago 2016). This approach is feasible and especially appealing considering the growing richness and availability of spatial economic data.

However, it is hard to rule out the possibility that the correlation between the instrument and the dependent variable runs through additional channels beyond electrification. Returning to the case of South Africa, land gradient may have been equally likely to have influenced the cost and placement of post-apartheid roads (or other infrastructure). Roads can reduce transportation time, making it cheaper to visit market centers, improving the conditions for local employment and other economic outcomes. This possibility raises questions about the validity of any geographic cost-based instrument, including in South Africa. During the same post-apartheid period, a large number of public investments were made across multiple sectors, and as with rural electrification, these investments were also largely targeted towards relatively poor and disadvantaged communities by the newly elected government of President Nelson Mandela. Of

course, researchers are well aware of these issues and have made efforts to address them.[7] But in our view, it is difficult to confidently eliminate all of the possible violations of the exclusion restriction. This is especially the case if electrification can interact positively with some unobserved and time varying factors, as this would result in overestimating the treatment effect.

More recent work has addressed these concerns using alternative econometric strategies. Burlig and Preonas (2016), for example, utilize a regression discontinuity design method, exploiting a population-based eligibility cutoff in India's Rajiv Gandhi Grameen Vidyutikaran Yojana (RGGVY) scheme, a massive national rural electrification program launched in 2005. When certain types of assignment rules (in this case, a cutoff based on village population) are followed, the regression discontinuity design method removes selection bias (here, by comparing villages immediately above and below the cutoff). However, these rules are not always cleanly implemented in low-income countries, forcing researchers to utilize "fuzzy" regression discontinuity design approaches. Burlig and Preonas, however, use satellite images of night lights to show that the RGGVY program did increase electricity availability and consumption, providing supportive evidence that the village population-based cutoff was implemented to a meaningful degree. As noted earlier, they find no evidence of economically or statistically significant impacts on village labor market or educational outcomes.

The obvious hurdle to implementing a randomized controlled trial of electricity grid infrastructure is that researchers find it hard to persuade policymakers to randomize the placement of infrastructure. The Lee, Miguel, and Wolfram (2019) study in rural Kenya, which we revisit

[7] Dinkelman (2011) addresses this concern by running a placebo test and other robustness checks. Bensch, Gotz and Peters (2019) perform alternative placebo tests and show that land gradient is correlated with employment outcomes in non-electrified areas, suggesting a violation of the exclusion restriction. They provide evidence that land gradient also influenced road placement.

later in this paper, is an exception.[8] Like Burlig and Preonas (2016), we find no evidence of meaningful economic, educational or other impacts among rural households.

Beyond the econometric approach, a common difference between studies that use randomized controlled trials and those that use other methods is the nature of data collection. In an experiment, researchers can design the questions administered through household surveys. As a result, it is possible to collect data on a wider range of outcomes and potential mechanisms than are typically available in the national administrative data that is often used in non-experimental studies. In our experiment in Kenya, for example, we collected a variety of information on energy-related outcomes, such as how much each household recently spent on electricity versus kerosene, the variety of electrical appliances owned and desired, the frequency of blackouts recently experienced, and so on; the majority of the studies summarized in Figure 2 are unable to utilize these types of data. The flip-side is that administrative data is often more representative and has many more observations, which offers benefits in terms of external validity and statistical power.

Different Interventions and Potential for Spillovers

Another factor contributing to the lack of consensus across studies is that the underlying intervention captured by the electrification variable is not always the same. For instance, the quality of an electricity connection probably varies across programs in terms of the reliability and capacity of power supplied, both of which influence the potential things one can do with electricity.

[8] To our knowledge, the only other randomized controlled trials of household electricity connections are: Barron and Torero (2017), which evaluates the impacts of grid connections in El Salvador on indoor air pollution and respiratory outcomes, and Bernard and Torero (2015), the first study that varies grid connections experimentally, which tests for the presence of social interaction effects in driving take-up decisions in Ethiopia but does not evaluate economic outcomes.

The design or scale of an electrification program can also result in local spillovers that are not easily measurable using household data. Many historical initiatives to expand electricity access were not only large in scale but also included investments in generation capacity, transmission lines, and other forms of public infrastructure. In Brazil, for example, Lipscomb, Mobarak, and Barham (2013) study the impacts of an electrification effort that entailed a massive upgrade to the nation's energy system. Over the second half of the twentieth century, Brazil witnessed a dramatic expansion in electricity access – the transmission network expanded from 2,359 kilometers in 1950 to 167,443 kilometers in 2000 – and substantial investments were also made to increase generation capacity. Much of this progress is owed to Eletrobras, the national electricity utility first established in 1961, which spearheaded the financing and coordination of electricity projects across the country.

If an electrification program is likely to have generated local spillovers, the unit of measurement is important. Studies that measure impacts at the household-level will not capture these spillovers to the same extent as studies that observe outcomes at the regional-level. In the example of Brazil, Lipscomb, Mobarak, and Barham (2013) measure impacts over a long timeframe and at the county-level, so any potential within-county economic spillovers are usefully captured in the estimates. Of course, the gains in the Brazil program and related cases flow from not just electrifying households but also schools, health clinics, and local enterprises, making these estimates less comparable to some recent electrification efforts that have targeted households. These features make the Brazil results more comparable to the historical U.S. studies.

Different Regions and Populations

A simple point, but one that is worth emphasizing, is that the impacts of household grid connections depend on what individuals are able to do with electricity. As a result, impact estimates may differ across local regions, or even across individuals within the same society. Across regions, differences may arise due to the presence or absence of local infrastructure and amenities. For instance, electrification may yield greater impacts in regions with better access to roads and linkages to neighboring commercial centers, as noted earlier. Impacts may also be greater in areas with existing industries that can benefit from cheaper sources of power, or in regions that are experiencing rising income levels due to external factors, like commodity price shocks. Fetter and Usmani (2019), for example, revisit the regression discontinuity design setting studied in Burlig and Preonas (2016) and demonstrate that the impact of India's RGGVY program on non-agricultural employment was higher in villages that simultaneously benefited from a boom in the price of a local commodity (guar). At the same time, Kline and Moretti (2014) find that the magnitude of benefits from the Tennessee Valley Authority program was the same across counties, regardless of whether they were more agricultural or featured any manufacturing at baseline, suggesting that further research into the nature of heterogeneous electrification treatment effects would be useful.

Across individuals within the same society, impacts may differ due to variation in individual income levels or access to credit. Wealthier households, by virtue of their ability to purchase more electrical appliances, are likely to be better positioned to benefit from access to electricity. Khandker et al. (2014) is one of the early studies to use an econometric approach to address this question. Using a cross-section of household survey data in India, they estimate a quantile regression of overall household income and expenditure on household electrification,

addressing the endogeneity of their electrification variable with an instrumental variable strategy. Their analysis – which relies on the arguably strong assumption that the community electrification rate is a valid instrument for household electrification – suggests that households in the highest quintile of income experience nearly double the expenditure impacts as households in the middle quintile. In the following section, we explore this possibility further, exploiting experimental variation from our randomized controlled trial research design in rural Kenya.

The studies discussed in this section offer important contributions to the literature on the impacts of electricity infrastructure, and each utilizes a creative and novel way to address the endogeneity of the electrification variable. But in our view, some skepticism of instrumental variables strategies based on geographic variation is warranted. In addition, it is important to consider the type of electrification intervention, as well as the other amenities that are being made available either through the electrification program or exogenously, as these factors could influence the magnitude of estimated impacts.

New Experimental Evidence on Heterogeneous Treatment Effects

Many existing analyses of heterogeneity rely on the inclusion of interaction terms in the regression specifications between a household's electrification status and observable covariates at baseline, like income, assets, and so on. Here, we build on the randomized controlled trial design in Lee, Miguel, and Wolfram (2019) to show what we can learn from an alternative approach to analyzing heterogeneity that compares households based on how much they are willing to pay for an electricity connection, a household characteristic that is rarely if ever captured in observational datasets.

In our experiment, we provided randomly-selected clusters of households in rural Kenya with an opportunity to connect to the grid at a subsidized price. In order to estimate a demand curve for grid connections, we randomly assigned the connection price across treatment communities. Specifically, one-third of the 75 treatment communities were offered a 29 percent subsidy to connect to the grid (that is, the effective price of a grid connection was reduced from the prevailing official price of $398 to $284); one-third were offered a 57 percent subsidy (the effective price was $171); and one-third were offered a full subsidy (the effective price was $0). Take-up varied dramatically across treatment arms: 95 percent of households accepted a fully subsidized connection; 28 percent took up at a 57 percent discount; and just 14 percent of households paid for a connection at a 29 percent discount, while even fewer control (unsubsidized) households connected to the grid over the study period.

Exogenous variation in electrification status, created by the randomized price offers, generated unbiased estimates of the impacts of electrification. Roughly 16 to 32 months after installation of a home grid connection, the average household showed little evidence of any meaningful economic or non-economic gains across a wide range of outcomes. Results are similar for the simpler comparison between the control group (in which almost no households were connected) and the full subsidy treatment group (in which nearly all households were connected).

How do these impacts vary across different population groups in this setting? Drawing on standard properties of "local average treatment effects" (related to the discussion in Kowalski 2016), we can separately estimate impacts for different types of households. Specifically, households in our experiment can be allocated into the following complier subgroups: (1) "never takers," meaning households that would not even accept a free connection; (2) "adopters of electricity only when the price is low," meaning households that are willing to accept a connection

when the price is $0 (one of the randomly assigned prices) and potentially up to $171; (3) "adopters of electricity when the price is high," meaning households that are willing to accept an electricity connection when the price is between $171 and $284; and (4) "always takers," meaning households that would pay more than $284. In the remainder of this section, we assess whether the subgroup of households that are willing to pay more for electricity – which may be correlated with wealth, access to credit, or to other unobserved dimensions of ability, ambition, or opportunity – end up benefiting more from an electricity connection than others.

A first step towards deriving treatment effects for different complier groups is to estimate their sample shares. It has long been understood that average treatment effects can be represented as the weighted-average of multiple marginal treatment effects that may differ across subgroups (Heckman and Vytlacil 1999, 2001). In our sample, 67 percent of households are "adopters *only* when the price is low" and 22 percent are "adopters when the price is high." The small shares of remaining households are either "never takers" or "always takers."[9]

The next step is to estimate separate local average treatment effects for each complier subgroup on a range of household outcomes, including among others: monthly electricity spending; the number of appliance types owned (including mobile phones, radios, televisions, etc.); monthly spending on kerosene; the share of household members that are employed or own their own businesses; household asset value; and a measure of recent health symptoms experienced by the household respondent. For the "adopters when the price is high" group, we can obtain these estimates from a two-stage least squares regression in which we drop the high- and low-subsidy treatment arms and regress the various outcomes on an indicator for whether a household has an

[9] Because we randomized price offers across communities, we need only the standard assumption of monotonicity to uncover unbiased estimates of these sample shares (Angrist and Imbens 1994). In online Appendix Note 2, we offer a formal description of our econometric approach to estimating heterogeneous treatment effects across complier subgroups in this setting.

electricity connection, instrumented with an indicator for whether the household was offered a medium- subsidy.[10] For the "adopters when the price is low," we can use the subgroup sample shares and back out the local average treatment effect by invoking the formula for weighted averages. For example, the local average treatment effect for compliers in the $0 treatment group is simply the weighted average of the local average treatment effects for the two complier groups of interest.[11]

We illustrate the results of this approach in Figure 3, where we compare local average treatment effects for "adopters only when the price is low" against those for "adopters when the price is high" across a key set of outcomes. Overall, "adopters when the price is high" appear to do far more with an electricity connection compared to their counterparts; the figure also contrasts these treatment effects with the mean characteristic in the control (unsubsidized) group. "Adopters when the price is high" spend more on electricity; experience greater savings on kerosene; and acquire a greater variety of appliances, such as mobile phones and televisions. The large difference in the number of appliance types owned across the two complier subgroups – a significant 83 percent for the increase for the "adopters when the price is high" versus a (non-significant) 11 percent decrease for those who connect only when it is free – is statistically significant at the 1 percent level. Similarly, "adopters when the price is high" also appear to enjoy more pronounced economic and non-economic impacts: they are more likely to become employed or own a business; more likely to experience an increase in total asset value; and more likely to report better health

[10] We can do this because compliers in the medium-subsidy group (in which the electricity connection price is $171) include both compliers at $171 as well as compliers at $284, by the monotonicity assumption.

[11] Detailed regression results are available in the online appendix. In online Appendix Table 1, we report mean values in the control group (column 1), the local average treatment effects for each of the two complier subgroups (columns 2 and 3), and the *p*-value of the difference between the local average treatment effects for each outcome (column 4). Note that we include the same set of variables presented in Table 3 in Lee, Miguel, and Wolfram (2019) to facilitate comparison to the full sample results.

outcomes (note that higher values on the recent health status index correspond to a lower number of recent symptoms reported). In additional results (not shown in Figure 3), we do not find that any subgroup experiences gains in student test scores.

Due to limitations in our sample size – a result of the limited number of households who choose to connect when it is not free – these results should be treated only as suggestive. Many of the estimated local average treatment effects are only marginally significant at traditional confidence levels, and we cannot reject equality of effects across the two complier subgroups in most cases. Yet the pattern of impacts in Figure 3 tells a remarkably consistent story indicating that those who are willing to pay more for an electricity connection are poised to benefit far more than those who only connect when it is free.

Naturally, our approach to estimating heterogeneous treatment effects leads to the question of how households in these complier subgroups are different from one another. Is it possible to identify households that will benefit the most from electrification using a standard set of observable characteristics? We use baseline household survey data collected in 2014 (note that all of these households were unconnected at baseline) to summarize the key differences between the groups.[12] Broadly speaking, "adopters when the price is high" appear to be wealthier and better-off in multiple ways: household heads in this group are more likely to have attended secondary school (21.0 percent versus 9.5 percent), report far higher monthly earnings ($24.39 versus $11.55), and hold a bank account (32.4 percent versus 14.7 percent), with this last difference statistically significant at the 1 percent level. "Adopters when the price is high" also have significantly higher asset ownership. In contrast, several other household characteristics that would seem to be less obviously correlated with wealth, including respondent age and gender as well as

[12] For a table of results, see online Appendix Table 2.

the household's distance to the nearest electricity distribution transformer, appear roughly similar across the two groups.

In our example, households that are willing to pay more for an electricity connection also appear to be observably richer and more educated at baseline. However, we cannot rule out the possibility that unobservables – like individual initiative, ambition or "spunk," or other oft-cited unobservables in wage equations – may be correlated with both household wealth, for example, and the ability to make the most of an electricity connection. This possibility suggests that this complier approach to studying heterogeneity, which is possible due to the experimental nature of this study, can be valuable in shedding additional light on how treatment effects vary across individuals. In Appendix Table A4, we report the results of a regression in which the treatment (household electrification) is interacted with an index of social and economic status at baseline, based on commonly-observed measures (e.g., education, income, etc.). This approach does not seem to predict larger effects for households in the top quartile of social and economic status at baseline, in contrast to the approach that compares the two complier groups.

Our main point is that the impacts of electrification can vary substantially across different types of individuals, even within a relatively homogenous sample of poor rural households in neighboring villages, in ways that are difficult to capture with commonly measured household observable characteristics.

Focusing on the Grid

We have largely focused on lessons from the past decade of research on the impacts of residential grid electrification, a growing area of investigation. In addition, the question of how governments can expand electricity access to maximize impact holds a great deal of policy-

relevance today. Across Sub-Saharan Africa, where roughly 600 million people are still without power, billions of dollars are being allocated towards expanding residential access to the grid. In Kenya alone, roughly $364 million was committed to the Last Mile Connectivity Project (LMCP) in 2015, in a project that promised to connect four million under-grid households to power (as reported in *Business Daily Africa*, at the official launch of the LMCP in May 2015).

But the grid is just one way to expand electricity access. Since the turn of the current century, countless entrepreneurs, donors, and policymakers have argued that decentralized, renewable energy technologies could allow off-grid households across the developing world to "leapfrog" the conventional grid, similar to how the introduction of mobile phones allowed populations to leapfrog the landline. Indeed, the home solar sector — a term we use to collectively refer to solar lanterns and solar home systems — has seen its estimated penetration rise rapidly across Sub-Saharan Africa. Increasing appliance efficiencies and reductions in the cost of photovoltaics (in addition to improvements in batteries) are some of the factors that may have contributed to this growth (Alstone, Gershenson, and Kammen 2015).

Solar lanterns offer just enough power to meet the basic standard of electrification in the World Bank's MTF, mentioned above. Grid connections can meet far higher standards, depending on their reliability. Increasingly, home solar companies are integrating pay-as-you-go technologies directly into their products, directly addressing the credit constraints that may limit take-up of new technologies in poor settings; in practice, these solar home systems are offered on credit and are remotely disabled if payments are not made on time. Pay-as-you-go has transformed the way these products are marketed, financed, and distributed. In some countries, like Uganda, pay-as-you-go is even allowing consumers to offer up their home solar products as collateral for new types of loans (Gertler, Green and Wolfram, 2019).

Separate randomized controlled trials have measured the impacts of home solar access on child study times, finding mixed results: home solar appears to increase study times, but decrease test scores in Uganda (Furukawa 2014); not increase study times in Kenya (Rom, Gunther, and Harrison 2017); and increase study times but only for boys in Rwanda (Grimm et al. 2017). These results highlight the lack of consensus about the educational benefits of home solar. That said, in countless rural households across the world, the increasing adoption of these products should, at the very least, reduce the usage of kerosene and dry cell batteries for lighting, resulting in some benefits to health and the environment.

Microgrids have also generated substantial interest, especially for geographically remote communities that are prohibitively expensive to connect to a national grid. Microgrids are typically defined as small networks of users connected to a centralized and standalone source of electricity generation and storage. They are capable of providing longer hours and higher capacities than home solar, and can also be powered with clean energy sources like solar, wind, and hydro. Technically, it is possible to integrate them into expanding national grids over the long-run, but it is too early to tell how widely this will happen in practice.

Recent research on the demand for microgrid connections has not been wholly positive, at times due to external factors. In Rajasthan, India, for example, Fowlie et al. (2018) document how demand for connections to privately-operated solar microgrids is very low, largely due to a perception that the government would soon be subsidizing connections to the central grid. Relatedly, in Bihar, India, Burgess et al. (2019) find that demand for connections to privately-operated solar microgrids is strongly influenced by the availability and quality of the central grid. At the same time, a number of private operators have built microgrids in Kenya that are operational and generating revenue, suggesting that demand is positive in some settings.

In addition to expectations about the arrival of the grid, fundamental consumer preferences can also limit the take-up of alternative energy. In Kenya, we document descriptive evidence at baseline suggesting that home solar does not satisfy a wide range of household energy needs, based on a survey of appliance ownership and aspirations (Lee, Miguel, and Wolfram 2016). Relative to households that primarily use kerosene, home solar users benefit from basic energy applications including lighting, mobile phone charging and, for some systems, television. However, once they have access to these basic end uses, the appliances they aspire to own next (for example, irons) require higher wattages that cannot be supported by most home solar systems, at least based on current technologies.

Discussion

Over the past decade, studies on the impacts of residential electrification on the well-being of households in low- and middle-income countries have generated conflicting results. While some studies estimate very large effects on household labor supply, for instance, others rule out point estimates that are even a quarter as large. We explore how differences in methods, interventions, and/or populations may help reconcile these disparate results.

Our main conclusion, based in part on our own recent research, is that the provision of home electrification alone is not enough to improve economic outcomes substantially for the world's poorest citizens. This perspective stands in contrast to the findings in earlier analyses in the literature which explore electrification impacts in middle-income countries, like South Africa. Although retrospective analyses of electrification in the United States in the 1930s point to very large impacts, these initiatives were introduced at a time when GDP per capita (in current dollars) was roughly eight times as large as comparable measures in contemporary Kenya and India. Also,

in some cases, the early US initiatives brought electricity to many sectors of the economy, including manufacturing facilities. Reconciling these cross-study differences presents its own identification challenge, as it is hard to know whether these differences are due to the choice of econometric method, the extent of the electrification initiative or to relative differences in starting incomes. With that said, our overall position is that the impacts of residential electrification may crucially depend on the extent to which households are positioned to take actions and/or make the complementary investments that will ultimately allow them to make the most out of an electricity connection.

Consistent with this view that context matters, our own recent work finds that heterogenous effects also exist within local areas. We exploit a feature of a recent experiment in Western Kenya that allows us to estimate heterogeneous treatment effects across different complier groups using the same identification strategy. We show that households that were only willing to connect to the grid when it was effectively free experience fewer economic gains than households that were willing to connect when the price was high. This result offers suggestive evidence of substantial heterogeneity in treatment effects, even within a sample of poor rural households that were all equally without electricity at baseline.

The question of how the impacts of electrification may vary across countries, or regions within a country, is likely to be of keen policy interest. We see expanding evidence in this area as an important task for future research. The degree of heterogeneity in treatment effects could naturally be much larger across rural and urban areas in the same country, or across countries with different income levels. On the one hand, understanding which households and areas are most likely to benefit from grid connections can help policymakers better target grid investments. On the other, if wealthier households are more likely to utilize and benefit from access to electricity –

due to their ability to make complementary investments or exploit new business opportunities opened up by access to power – expansion of the rural grid infrastructure could exacerbate economic inequality in rural areas of low-income countries, an outcome that is seldom discussed in the current policy debate. This would imply a fundamental tension in rural electrification programs between promoting economic growth and exacerbating inequality.

To date, both policymakers and researchers have often focused on the impacts of household electrification. For policymakers, this may reflect either a political calculus that those not presently connected to electricity are a potent group of potential supporters, the belief that electricity should be viewed as a basic right even for the very poorest citizens, or some combination of the two. In our view, the available evidence suggests that the provision of electrification to poor households is unlikely, on its own, to be economically transformative, at least in the short to medium run. As such, a singular policy focus on electrifying poor and mostly rural households may be misguided. Going forward, we believe that studying the long-run impacts of residential electrification, the interactions between electrification and contextual factors, as well as impacts of electricity access for non-residential consumers – including schools, health centers, and firms – are all likely to be fruitful research directions.

Acknowledgements

We thank Robert Fetter, Gordon Hanson, Enrico Moretti, Timothy Taylor, and Heidi Williams for helpful comments. We are grateful to Felipe Vial, Zachary Obstfeld, Nishmeet Singh, Aishwarya Kumar, and Rongmon Deka for excellent research assistance. An earlier version of this paper was funded with support from the UK government, as part of the Department for International Development (DFID) supported Energy and Economic Growth (EEG) research program based at the Center for Effective Global Action (CEGA) and the Energy Institute at Haas (EI) at the University of California, Berkeley.

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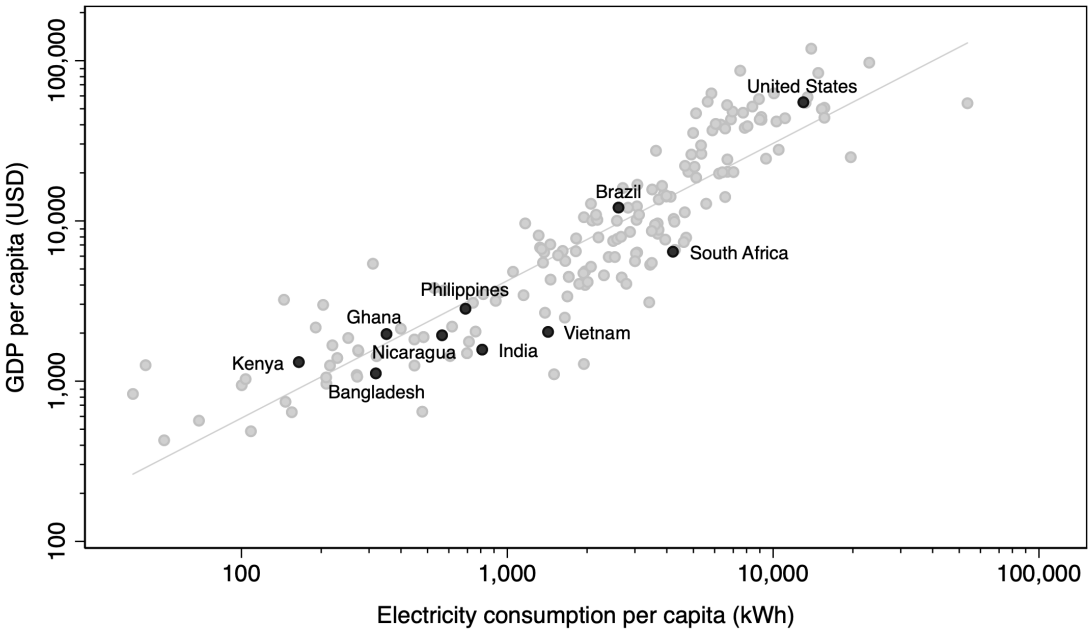
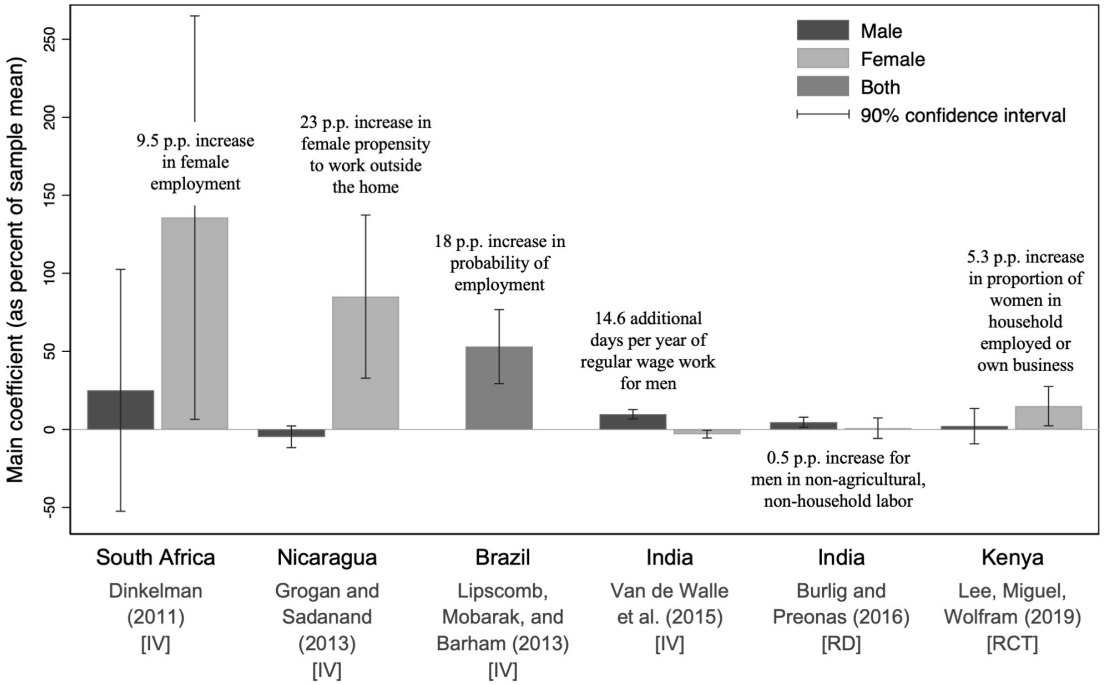
Figure 1: The positive correlation between electricity consumption and GDP per capita



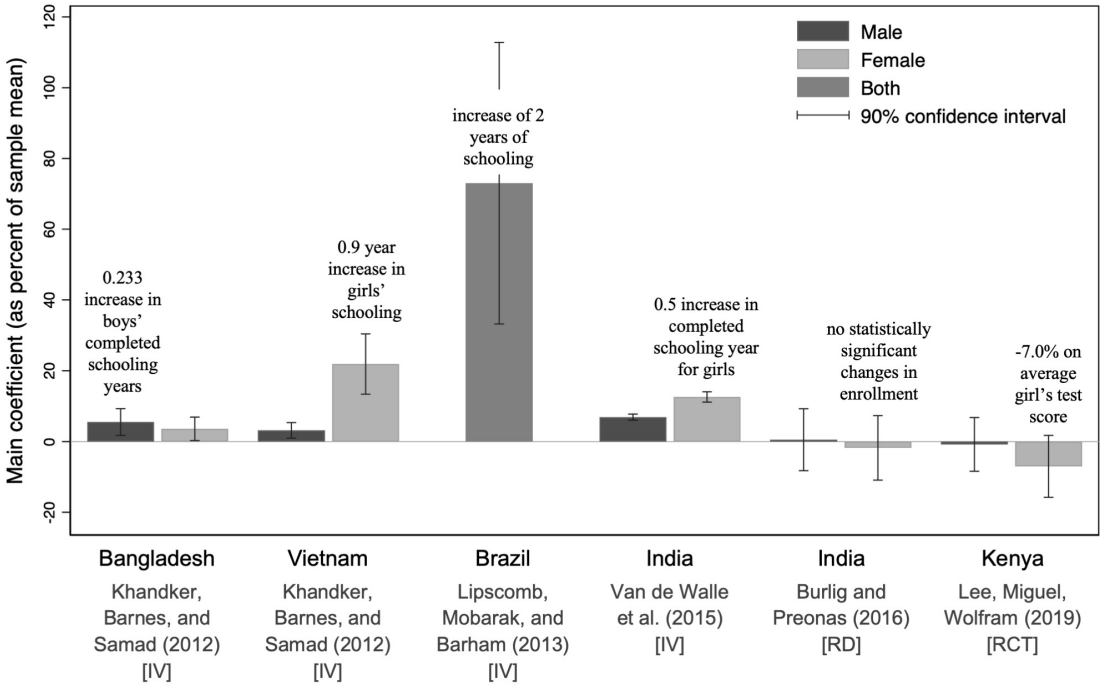
Notes: Both variables are presented on a logarithmic scale. GDP per capita data are in current U.S. dollars. 2014 data obtained from the World Bank DataBank.

Figure 2: Key estimates of the impacts of rural electrification

Panel A: Labor supply impacts

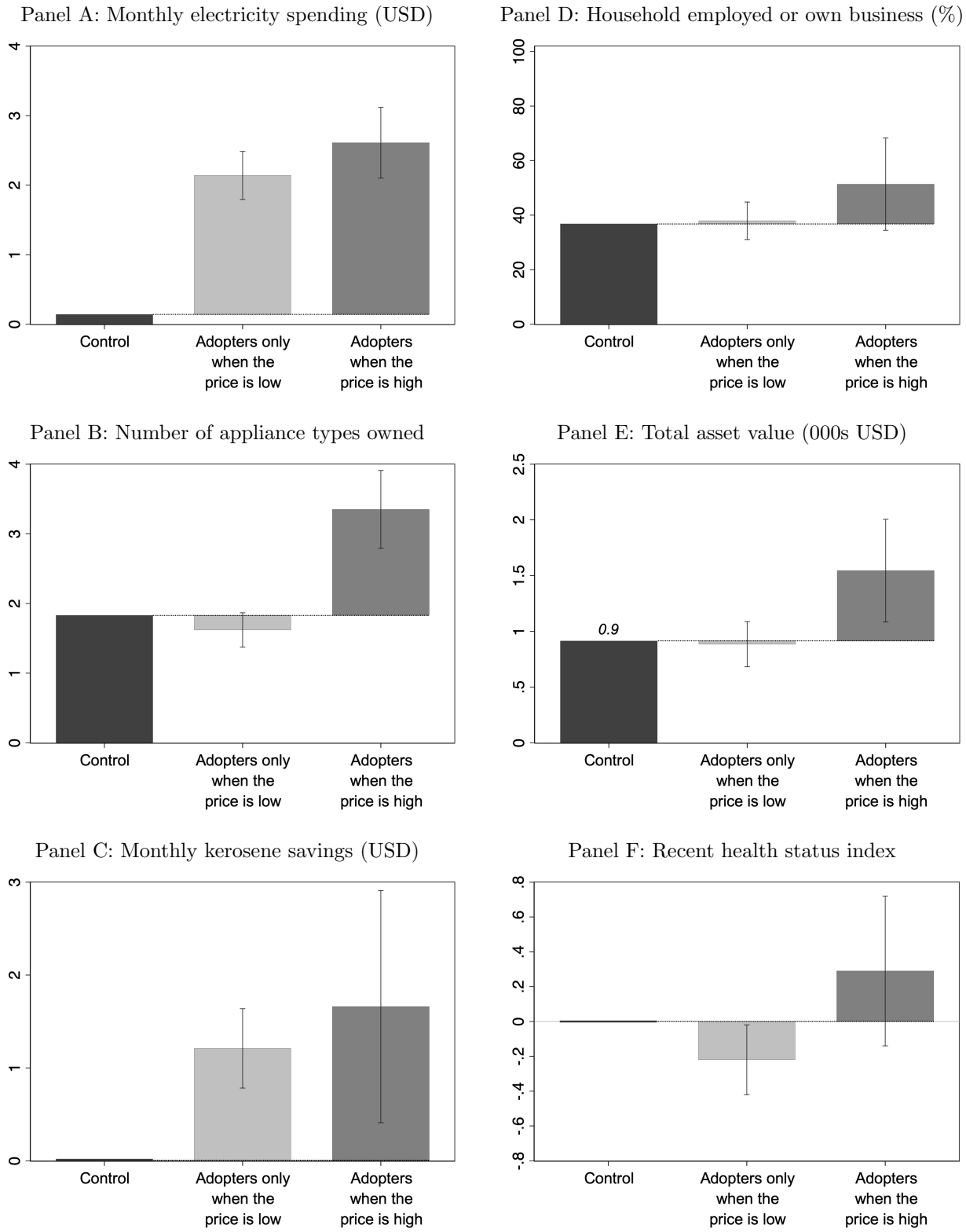


Panel B: Education impacts



Notes: For each study, coefficient estimates have been expressed as a percentage of the mean of the dependent variable.

Figure 3: Comparison of local average treatment effects between different complier groups



Notes: In Panel C, monthly kerosene savings are presented as relative to the control group mean of \$2.81. In Panel F, a positive value reflects a desirable outcome. See online Appendix Table 1 and associated discussion for additional outcomes and details.

Table 1: Historical rural electrification initiatives

Country	Major initiative	Change over period: Electrification: Nat'l (%)	Change over period: Electrification: Rural (%)	Change over period: GDP ($/cap.)	Est. cost ($ Bn)
USA 1935-1960	*Rural Electrification Administration (REA)* REA provided low-interest loans to newly-formed cooperatives to fund rural electrification, as part of the *New Deal*, which included fiscal and monetary reforms, public works projects, and new regulations.	67 to 98	< 10 to 96	9,644 to 19,678	4.0 (between 1935 and 1939)
Brazil 1960-2000	*Eletrobras Power Distribution Projects I, II* Between 1982 to 1991, Eletrobras I and II strengthened distribution networks, expanded supply, and increased rural access rates from 19 to 49 percent. The period also witnessed pubic investments across various sectors as well as policies to counter hyperinflation.	n/a to 94	< 10 to 75	2,929 to 6,813	24.4 (between 1982 and 1991)
Bangladesh 1977-present	*Rural Electrification Board (BREB)* Since the 1970s, BREB targeted universal access and other institutional improvements in rural areas that have also benefited from social mobilisation campaigns related to health, education, financial inclusion, and others.	n/a to 75	< 10 to 69	470 to 1,524	4.4 (as of 2016)
India (I) 1982-1999	*Integrated Rural Energy Program* Aimed to increase institutional capabilities to meet domestic energy needs (catered towards agricultural and rural development), as part of the *Minimum Needs Program,* which covered rural water supply, health, housing, roads, and others.	n/a to 60	24 to 71	456 to 834	n/a
Ghana 1989-present	*National Electrification Program (NEP)* Launched in 1989, NEP targeted universal access by 2020, focusing first on major population centers, while the *Self Help Electrification Program (SHEP)* aimed to connect rural areas within 20 kilometers of an existing transmission line.	23 to 78	n/a to 625	66 to 1,338	625

(Table continued on next page)

(Table continued from previous page)

Country	Major initiative	Change over period: Electrification: Nat'l (%)	Change over period: Electrification: Rural (%)	Change over period: GDP ($/cap.)	Est. cost ($ Bn)
South Africa 1994-1999	*National Electrification Programme (NEP)* Targeted 2.5 million new household connections, mainly in disadvantaged and rural areas, and all schools and clinics. Part of newly-elected government's *Reconstruction and Development Programme*, which initiated large investments across multiple sectors.	36 to 66	12 to 46	4,390 to 4,559	1.6
Vietnam 2000-2006	*Vietnam Rural Energy Project I* After the end of the U.S. trade embargo, Vietnam established its state utility and enacted power sector reforms. In 2000, the focus shifted towards remote, unelectrified communes and villages.	86 to 96	70 to 92	926 to 1,306	0.3 (between 2000 and 2007)
Philippines 2004-present	*Expanded Rural Electrification Program* Targeted electrification of all villages by 2008 and 90 percent of households by 2017, mainly by providing low-cost financing to cooperatives and promoting private sector investments.	73 to < 85	n/a to 74	1,899 to 3,105	n/a
India (II) 2005-present	*Rajiv Gandhi Grameen Vidyutikaran Yojana* The RGGVY program aimed to enhance electricity access in over 400,000 villages and connect more than 23 million households. National road connectivity and social security programs for rural areas were also implemented during this period.	67 to 84	57 to 78	1,084 to 2,193	12.9 (between 2012 and 2022)
Kenya 2007-present	*REA and Last Mile Connectivity Project* Rural Electrification Authority (REA) focused on connecting rural public facilities (e.g., schools, clinics, and markets). The Last Mile Connectivity Project (LMCP), which was first announced in 2015, is targeting universal access for households by 2030.	24 to 56	14 to 39	1,232 to 1,541	> 1.0 (including LMCP)

Notes: All GDP figures are in 2017 USD. For ongoing initiatives, end years report statistics for 2017, the latest available year. See online appendix for further details and references.

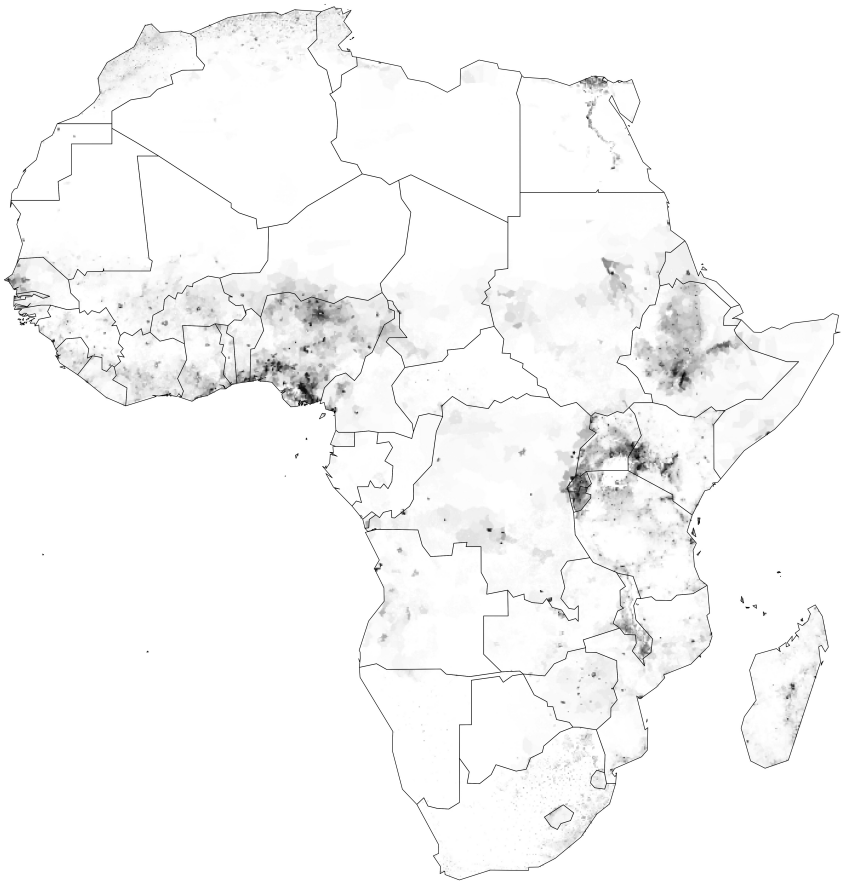
Supplementary Appendix for Online Publication

Does Household Electrification Supercharge Economic Development?

Kenneth Lee, Edward Miguel, and Catherine Wolfram

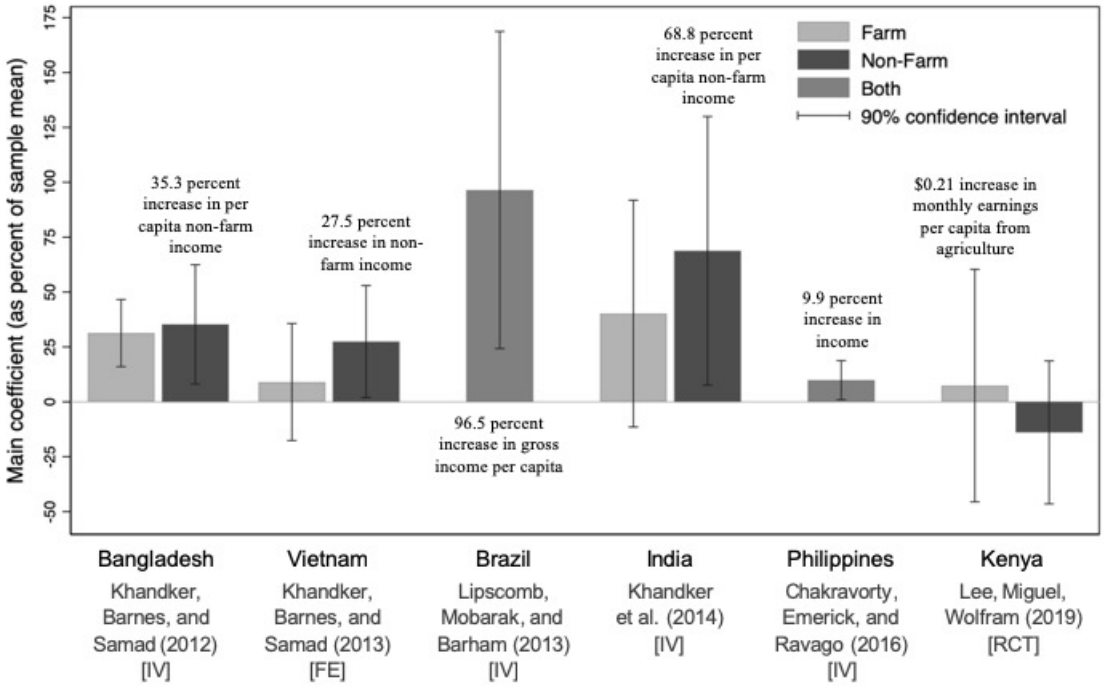
November 2019

Appendix Figure 1: Distribution of "missing" night lights in Africa



Notes: We combine data from satellite images of night lights in 2013, obtained from the National Oceanic and Atmospheric Administration's Defense Meteorological Satellite Program–Operational Line Scan (NOAA DMSP-OLS), with data from the Gridded Population of the World, Version 4 (GPWv4), provided by the Center for International Earth Science Information Network (CIESIN) at Columbia University, to predict where the largest gains in nighttime brightness would occur if everyone were able to enjoy the same levels of brightness as OECD countries. The simple procedure is as follows: (1) estimate $Log(N_{5km}) = \alpha + \beta Log(P_{5km})$ for grid cells in OECD countries; (2) using the estimated parameters, predict $\hat{N_{5km}}$ for grid cells in Africa; and (3) subtract N_{5km} from $\hat{N_{5km}}$ for grid cells in Africa to estimate "missing" night lights.

Appendix Figure 2: Key estimates of the impacts of rural electrification on income



Notes: For each study, coefficient estimates have been expressed as a percentage of the mean of the dependent variable.

Appendix Table 1: Local average treatment effects for different complier subgroups

	Control (1)	Adopter only when price is low (2)	Adopter when price is high (3)	p-value of diff. (4)
Share of sample (%)	100	67	22	
Panel A: Primary energy outcomes				
A1. Grid connected (%)	5.6	–	–	–
	(23.0)			
A2. Monthly electricity spending (USD)	0.14	2.00***	2.47***	0.28
	[0.91]	(0.21)	(0.31)	
Panel B: Additional energy outcomes				
B1. Electricity as main lighting (%)	5.2	86.8***	96.8***	0.01
	[22.2]	(2.4)	(2.6)	
B2. Number of appliance types owned	1.8	-0.2	1.5***	< 0.01
	[1.3]	(0.2)	(0.3)	
B3. Owns mobile phone (%)	84.3	-12.7***	18.5**	< 0.01
	[36.4]	(4.0)	(8.9)	
B4. Owns radio (%)	54.2	-5.1	23.5*	0.09
	[49.8]	(5.3)	(13.0)	
B5. Owns television (%)	17.9	-2.3	47.1***	< 0.01
	[38.4]	(4.6)	(10.7)	
B6. Owns iron (%)	4.1	-0.1	6.9	0.37
	[19.9]	(2.5)	(5.9)	
B7. Monthly kerosene spending (USD)	2.81	-1.21***	-1.66**	0.64
	[2.86]	(0.26)	(0.76)	
B8. Monthly total energy spending (USD)	11.66	4.62*	-16.64***	< 0.01
	[28.47]	(2.57)	(4.89)	
B9. Solar home system as main lighting (%)	11.8	-13.7***	-10.4	0.74
	[32.3]	(2.3)	(8.0)	

(Table continued on next page)

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	Control (1)	Adopter only when price is low (2)	Adopter when price is high (3)	p-value of diff. (4)
Panel C: Primary economic outcomes				
C1. Household employed or own business (%)	36.8	1.1	14.6	0.31
	[38.8]	(4.2)	(10.3)	
—— Household women-only (%)	34.5	6.0	18.2	0.44
	[44.5]	(5.1)	(12.1)	
—— Household men-only (%)	40.2	-2.5	11.3	0.43
	[45.6]	(5.5)	(13.3)	
C3. Total hours worked last week	47.0	-2.2	-4.2	0.81
	[24.7]	(2.7)	(6.6)	
C4. Total asset value (USD)	914	3	630**	0.09
	[961]	(123)	(280)	
C5. Per capita cons. of major items (USD)	133	-3	-23	0.67
	[142]	(14)	(37)	
Panel D: Primary non-economic outcomes				
D1. Recent health symptoms index	0	-0.19*	0.29	0.14
	[1]	(0.10)	(0.26)	
D2. Normalized life satisfaction	0	0.12	0.23	0.76
	[1]	(0.11)	(0.28)	
D3. Avg. student test Z-score	0	-0.08	-0.11	0.92
	[1]	(0.12)	(0.24)	
D5. Political and social awareness index	0	-0.09	-0.03	0.86
	[1]	(0.10)	(0.26)	
Panel E: Mean treatment effects on grouped outcomes				
E1. Economic Index (C outcomes)	0	-0.03	0.30	0.31
	[1]	(0.11)	(0.26)	
E2. Non-Economic Index (D outcomes)	0	-0.10	0.06	0.64
	[1]	(0.10)	(0.26)	

Notes: Heterogeneous treatment effects estimated using only round 1 data (roughly 16 months post-connection). The following variables (reported in table 3, Lee, Miguel, and Wolfram 2019) were not collected in round 1: "C2. Monthly household earnings (USD)," "D4. Avg. student KCPE test Z-score," and "D6. Perceptions of security index." Column 1 reports mean values in the control group, with standard deviations in brackets. Using the established sample shares, columns 2 and 3 display weighted-average local average treatment effects for each adopter group. Robust standard errors, displayed in parentheses, are estimated using a stacked regression approach.

Appendix Table 2: Differences between adopter groups at baseline, when all households were unconnected

	Adopter only when price is low ($0 \leq p < \$171$) (1)	Adopter when price is high ($171 \leq p \leq \$284$) (2)	p-value of difference (3)
Panel A: Household head (respondent) characteristics			
Female (%)	60.5	62.6	0.77
Age (years)	53.4	53.4	> 0.99
Senior citizen (%)	2.08	26.2	0.78
Attended secondary schooling (%)	9.5	21.0	0.05
Married (%)	64.6	69.5	0.47
Not a farmer (%)	18.5	26.6	0.20
Employed (%)	34.1	41.9	0.28
Basic political awareness (%)	9.9	15.7	0.25
Has bank account (%)	14.7	32.4	< 0.01
Monthly earnings (USD)	11.55	24.39	0.10
Panel B: Household characteristics			
Number of members	5.1	6.0	0.02
Youth members (age ≤ 18)	2.9	3.4	0.17
High-quality walls (%)	12.6	21.3	0.16
Land (acres)	1.9	2.1	0.71
Distance to transformer (m)	363.2	356.9	0.75
Monthly (non-charcoal) energy (USD)	5.16	6.73	0.05
Panel C: Household assets			
Bednets	2.2	2.7	0.03
Sofa pieces	5.5	8.1	< 0.01
Chickens	5.9	9.3	0.01
Radios	0.4	0.4	0.42
Televisions	0.1	0.3	< 0.01
Share of sample (%)	67	22	

Notes: Columns 1 and 2 report sample means for "adopters when the price is low" and "adopters when the price is high," respectively, at the time of the baseline survey. Column 3 reports p-values of the difference between the means. The basic political awareness indicator captures whether the household head was able to correctly identify the presidents of Tanzania, Uganda, and the United States. Monthly earnings (USD) includes the respondent's profits from businesses and self-employment, salary and benefits from employment, and agricultural sales for the entire household.

Appendix Table 3: Correlations between various characteristics observed at baseline

	Attended schooling	Not a farmer	Employed	Has bank account	Monthly earnings	Total asset value
Attended schooling	1					
Not a farmer	0.111	1				
Employed	0.057	0.249	1			
Has bank account	0.278	0.068	0.177	1		
Monthly earnings	0.245	0.179	0.332	0.288	1	
Asset value	0.148	-0.002	0.180	0.207	0.220	1

Notes: This table presents correlations between six respondent and household characteristics that were observed at baseline, and appear to be important differences between "adopters only when the price is low" and "adopters when the price is high," as shown in Appendix Table 2. These include whether the household respondent (1) attended secondary schooling; (2) is not a farmer; (3) is employed; and (4) has a bank account; (5) estimated monthly earnings (USD), which includes the respondent's profits from businesses and self-employment, salary and benefits from employment, and agricultural sales for the entire household; and (6) estimated value of assets at baseline. These six variables are combined to construct a baseline measure of "Social and Economic Status." We then construct a binary variable, SES, indicating whether a household falls into the upper quartile of this measure. SES is then used as an interaction variable in instrumental variables regressions estimating the impacts of household electrification, in order to explore heterogeneity. We can compare the results of this approach to the alternative approach presented in the paper, and shown in Appendix Table 1.

Appendix Table 4: Local average treatment effects with high "Social and Economic Status" interaction

	Control (1)	E (2)	SES (3)	$E \times SES$ (4)
Panel A: Primary energy outcomes				
A1. Grid connected (%)	5.6	–	–	–
	[23.0]			
A2. Monthly electricity spending (USD)	0.14	1.86***	-0.05	1.19***
	[0.91]	(0.13)	(0.09)	(0.38)
Panel B: Additional energy outcomes				
B1. Electricity as main lighting (%)	5.2	88.4***	-0.5	2.8
	[22.2]	(2.4)	(0.8)	(3.9)
B2. Number of appliance types owned	1.8	0.4***	0.6***	-0.2
	[1.3]	(0.1)	(0.1)	(0.2)
B3. Owns mobile phone (%)	84.3	-5.0*	4.9***	7.4*
	[36.4]	(2.7)	(1.9)	(4.4)
B4. Owns radio (%)	54.2	7.7**	12.5***	-13.1*
	[49.8]	(3.8)	(3.5)	(7.5)
B5. Owns television (%)	17.9	13.3***	17.0***	-5.6
	[38.4]	(3.8)	(3.2)	(7.9)
B6. Owns iron (%)	4.1	3.3**	5.7***	-3.5
	[19.9]	(1.4)	(1.9)	(3.7)
B7. Monthly kerosene spending (USD)	2.81	-1.29***	-0.43**	0.43
	[2.86]	(0.17)	(0.19)	(0.35)
B8. Monthly total energy spending (USD)	11.66	0.62	2.28	-5.06
	[28.47]	(2.34)	(1.85)	(3.20)
B9. Solar home system as main lighting (%)	11.8	-9.3***	12.5***	-13.2***
	[32.3]	(1.3)	(2.5)	(3.3)

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	Control (1)	E (2)	SES (3)	$E \times SES$ (4)
Panel C: Primary economic outcomes				
C1. Household employed or own business (%)	36.8	6.2*	12.5***	-4.3
	[38.8]	(3.7)	(2.6)	(5.9)
—— Household women-only (%)	34.5	11.3***	15.8***	-6.9
	[44.5]	(4.1)	(3.1)	(7.6)
—— Household men-only (%)	40.2	0.8	6.5*	2.0
	[45.6]	(4.2)	(3.6)	(7.6)
C3. Total hours worked last week	47.0	-2.1	2.6	1.5
	[24.7]	(1.5)	(1.7)	(2.9)
C4. Total asset value (USD)	914	150	352***	232
	[961]	(136)	(85)	(203)
C5. Per capita cons. of major items (USD)	133	0	45***	-30
	[142]	(12)	(10)	(20)
Panel D: Primary non-economic outcomes				
D1. Recent health symptoms index	0	-0.06	0.23***	0.01
	[1]	(0.08)	(0.08)	(0.18)
D2. Normalized life satisfaction	0	0.21***	0.08	-0.27**
	[1]	(0.08)	(0.06)	(0.14)
D3. Avg. student test Z-score	0	0	0.39***	0
	[1]	(0)	(0.09)	(0)
D5. Political and social awareness index	0	0.05	0.32***	-0.28*
	[1]	(0.06)	(0.07)	(0.16)
Panel E: Mean treatment effects on grouped outcomes				
E1. Economic Index (C outcomes)	0	0.10	0.48***	-0.01
	[1]	(0.09)	(0.07)	(0.17)
E2. Non-Economic Index (D outcomes)	0	0.03	0.41***	-0.29
	[1]	(0.07)	(0.07)	(0.18)

Notes: We report coefficients from separate instrumental variables regressions in which household electrification status (E) and the interaction between E and SES are instrumented with the three subsidy treatment indicators (as well as their interactions with SES). SES is a binary variable indicating whether a household is in the upper quartile of the Social and Economic Status index constructed using observable characteristics at baseline (see Appendix Table 3 for a list of components). Regressions are estimated using only round 1 data (roughly 16 months post-connection). All specifications include pre-specified household, student, and community covariates, excluding those captured in SES. See Appendix Table B6A in Lee, Miguel, and Wolfram (2019) for additional results. Robust standard errors clustered at the community level in parentheses. Asterisks indicate coefficient statistical significance level (2-tailed): $^{*}P < 0.10$; $^{**}P < 0.05$; $^{***}P < 0.01$.

Appendix Note 1. Sources for historical rural electrification initiatives

In Table 1, we summarize some of the historical rural electrification initiatives evaluated in the recent microeconomics literature. In general, electrification rates are obtained from various studies, including those cited in the paper, as well as World Bank Open Data. GDP figures are calculated using GDP and CPI statistics from World Bank Open Data. Annual average exchange rates are obtained from the International Monetary Fund. The costs of electrification initiatives are obtained from various sources. Unless otherwise indicated, the reported cost is assumed to be in current prices of the year in which it is reported. All costs are then converted to 2017 USD. The following is a list of the references that were consulted.

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Appendix Note 2. Estimating heterogeneous treatment effects

We are interested in estimating Local Average Treatment Effects (LATE) separately for a set of complier subgroups. Since any LATE (and any treatment effect) can be represented as a weighted average of Marginal Treatment Effects (Heckman and Vytlacil (1999, 2001a, 2001b, 2001c, 2005, 2007a, 2007b)), it can also be represented as weighted average of sub-LATE's. This means that if we obtain a LATE using an instrumental variable, it can be represented as a weighted average of sub-LATE's for more narrowly defined complier subgroups. Therefore, the first step to calculate LATE's for different complier groups is to estimate the share that each complier group represents in the sample. Under randomization, we only need to make a standard assumption of monotonicity (Angrist, Imbens 1994) to get unbiased estimates of these shares.

In our experimental situation in which there are different randomized subsidy levels across treatment villages, we can define complier group $c1$ as those individuals who take up a grid connection when offered the low subsidy but not when offered no subsidy; those who take up power even without a subsidy are the "always takers". Those who only take up a connection when offered at least the medium subsidy are called $c2$, and those who only connect under the full subsidy (free treatment) are $c3$. The monotonicity assumption is equivalent to assuming that if individual i takes up treatment under a low subsidy, they would also take it up if offered the medium or full subsidy. This leads to the following logic: if $x\%$ of the control group take up a connection (without a subsidy), then $x\%$ of the entire sample are always takers. Then if $y\%$ of those offered the low subsidy take up treatment, we assume there is the same share of always takers in this treatment group, and therefore $y\% - x\%$ corresponds to the share of $c1$ compliers. Next, if $z\%$ of those offered the medium subsidy connect, then $z\% - y\% - x\%$ corresponds to the share of $c2$ compliers, since always takers and $c1$ compliers would also take up treatment under the medium subsidy. Following the same logic we can estimate the share of $c3$ compliers and never takers. We will denote these shares as $\pi_{at}, \pi_{nt}, \pi_{c1}, \pi_{c2}$, and π_{c3}.

Along with these shares, estimating the LATE for $c1$ compliers (LATEc1) is straightforward, since it simply corresponds to the 2SLS regression of an outcome on the low subsidy (using only control and low subsidy observations). If we estimate a 2SLS regression of an outcome on the medium subsidy, we are estimating a weighted average of LATEc1 and LATEc2, since the compliers at the medium subsidy level include those who would not have complied under the low subsidy, but also those who would have connected under a low subsidy if it had been offered. We call this weighted average LATEc1,c2. This same logic applies to the full subsidy.

Exploiting the monotonicity condition yields the following key expressions, which are similar in notation to those presented in Kowalski (2016):

For $c2$ compliers:

$$\frac{\pi_{c1}}{\pi_{c1}+\pi_{c2}}\text{LATE}_{c1} + \frac{\pi_{c2}}{\pi_{c1}+\pi_{c2}}\text{LATE}_{c2} = \text{LATE}_{c1,c2}$$

$$\implies \boxed{\text{LATE}_{c2} = \frac{\pi_{c1}+\pi_{c2}}{\pi_{c2}}\text{LATE}_{c1,c2} - \frac{\pi_{c1}}{\pi_{c2}}\text{LATE}_{c1}}$$

$$\boxed{V(\text{LATE}_{c2}) = \left(\frac{\pi_{c1}+\pi_{c2}}{\pi_{c2}}\right)^2 V(\text{LATE}_{c1,c2}) + \left(\frac{\pi_{c1}}{\pi_{c2}}\right)^2 V(\text{LATE}_{c1}) - 2\frac{\pi_{c1}+\pi_{c2}}{\pi_{c2}} COV(\text{LATE}_{c1,c2}, \text{LATE}_{c1})}$$

For $c3$ compliers:

$$\frac{\pi_{c1}}{\pi_{c1}+\pi_{c2}+\pi_{c3}}\text{LATE}_{c1}+\frac{\pi_{c2}}{\pi_{c1}+\pi_{c2}+\pi_{c3}}\text{LATE}_{c2}+\frac{\pi_{c3}}{\pi_{c1}+\pi_{c2}+\pi_{c3}}\text{LATE}_{c3}=\text{LATE}_{c1,c2,c3}$$

$$\implies \text{LATE}_{c3}=\frac{\pi_{c1}+\pi_{c2}+\pi_{c3}}{\pi_{c3}}\text{LATE}_{c1,c2,c3}-\frac{\pi_{c1}}{\pi_{c3}}\text{LATE}_{c1}-\frac{\pi_{c2}}{\pi_{c3}}\text{LATE}_{c2}$$

$$\implies \boxed{\text{LATE}_{c3}=\frac{\pi_{c1}+\pi_{c2}+\pi_{c3}}{\pi_{c3}}\text{LATE}_{c1,c2,c3}-\frac{\pi_{c1}+\pi_{c2}}{\pi_{c3}}\text{LATE}_{c1,c2}}$$

$$\boxed{\begin{aligned}V(\text{LATE}_{c3})=&\left(\frac{\pi_{c1}+\pi_{c2}+\pi_{c3}}{\pi_{c3}}\right)^2V(\text{LATE}_{c1,c2,c3})+\left(\frac{\pi_{c1}+\pi_{c2}}{\pi_{c3}}\right)^2V(\text{LATE}_{c1,c2})\\&-2\left(\frac{\pi_{c1}+\pi_{c2}}{\pi_{c3}}\right)\left(\frac{\pi_{c1}+\pi_{c2}+\pi_{c3}}{\pi_{c3}}\right)COV(\text{LATE}_{c1,c2,c3},\text{LATE}_{c1,c2})\end{aligned}}$$

Baseline characteristics

The LATE's estimated implicitly come from the identification of the different complier groups using the monotonicity assumption. For example, LATEc2 is just the difference between the average outcome for the $c2$ compliers who were treated and the $c2$ compliers who were not. When it comes to baseline characteristics, it means we can identify the average for each complier group and treated/untreated combination. Following Abadie (2002), a number of studies have aimed to empirically identify the characteristics of different complier subgroups (for examples, see Card 2019; Mountjoy 2019; and Kline and Walters 2016). Here, we are able to calculate average baseline values corresponding to the combination of treated and untreated compliers within each subgroup using the assumption of monotonicity and the basic formula for weighted averages. As an example, consider the average characteristics X for those connected under the low subsidy. This average is a weighted average of the characteristics of the always takers and the treated $c1$ compliers. Since we can identify the always takers in the control group, we can back out the average characteristics for the treated $c1$ compliers. Similarly, the average characteristics X for those who remain unconnected under the medium subsidy is a weighted average of the never takers and those who only comply under the full subsidy, $c3$ compliers. Since the never takers are identified through those unconnected under the full subsidy, we can back out the average characteristics of the untreated $c3$ compliers. The following equations use the shares we previously described, $T=1$ to represent treated (connected) households, and $Z_1, Z_2,$ and Z_3 to represent the low, medium, and full subsidy respectively:

For treated compliers:

$$\boxed{\begin{aligned}E[X|c1]_t&=\frac{\pi_{at}+\pi_{c1}}{\pi_{c1}}E[X|T=1,Z_1=1]-\frac{\pi_{at}}{\pi_{c1}}E[X|T=1,Z=0]\\E[X|c2]_t&=\frac{\pi_{at}+\pi_{c1}+\pi_{c2}}{\pi_{c2}}E[X|T=1,Z_2=1]-\frac{\pi_{c1}}{\pi_{c2}}E[X|c1]_t-\frac{\pi_{at}}{\pi_{c2}}E[X|T=1,Z=0]\\E[X|c3]_t&=\frac{\pi_{at}+\pi_{c1}+\pi_{c2}+\pi_{c3}}{\pi_{c3}}E[X|T=1,Z_3=1]-\frac{\pi_{c1}}{\pi_{c3}}E[X|c1]_t-\frac{\pi_{c2}}{\pi_{c3}}E[X|c2]_t-\frac{\pi_{at}}{\pi_{c3}}E[X|T=1,Z=0]\end{aligned}}$$

For untreated compliers:

$$
\begin{aligned}
E[X|c3]_u &= \frac{\pi_{nt}+\pi_{c3}}{\pi_{c3}}E[X|T=0,Z_2=1] - \frac{\pi_{nt}}{\pi_{c3}}E[X|T=0,Z_3=1] \\
E[X|c2]_u &= \frac{\pi_{nt}+\pi_{c3}+\pi_{c2}}{\pi_{c2}}E[X|T=0,Z_1=1] - \frac{\pi_{c3}}{\pi_{c2}}E[X|c3]_u - \frac{\pi_{nt}}{\pi_{c2}}E[X|T=0,Z_3=1] \\
E[X|c1]_u &= \frac{\pi_{nt}+\pi_{c3}+\pi_{c2}+\pi_{c1}}{\pi_{c1}}E[X|T=0,Z=0] - \frac{\pi_{c3}}{\pi_{c1}}E[X|c3]_u - \frac{\pi_{c2}}{\pi_{c1}}E[X|c2]_u - \frac{\pi_{nt}}{\pi_{c1}}E[X|T=0,Z_3=1]
\end{aligned}
$$

In Appendix Table 2, we compare baseline characteristics for each complier subgroup. Specifically, we present the minimum variance weighted average for each subgroup, instead of presenting them separately for treated and untreated households. Furthermore, we present a weighted average (using their shares) of the baseline characteristics and LATE's of complier subgroups c1 and c2, primarily because π_1 and π_2 are quite small and thus pooling the data leads to more statistical power. There is also a meaningful conceptual distinction between those willing (and able) to pay "something" for a connection, versus those who only connect when it is completely free. As noted above, the weighted average for c1 and c2 is just the LATE and the average characteristics for medium subsidy treatment group compliers, and in this case the relevant sub-LATE can be obtained directly from that IV estimate.

To be able to estimate the standard errors for the LATE's and the baseline characteristics, we need to estimate a full covariance matrix, given the covariance terms that appear in the expressions above. To do this, we employ a stacked regression approach, which is numerically equivalent to a seemingly unrelated regression (SUR) approach. This allows for a straightforward estimation of the analytical standard errors, rather than having to rely on bootstrapped estimates. All of the data and code that generated the results are in this article's replication files.

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