



Regular article

Community effects of electrification: Evidence from Burkina Faso's grid extension[☆]Maika Schmidt^a , Alexander Moradi^b ,*^a Department of Economics, University of Sussex, UK^b Department of Economics, University of Bozen-Bolzano, Italy

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ABSTRACT

We study the effects of Burkina Faso's large scale electricity grid expansion 2008–2017, using both community and household-level data. We show that the timing of electrification was driven by engineering constraints and thus largely exogenous. We estimate the impact of electrification using a staggered difference-in-differences (DiD) approach, where not-yet treated communities serve as the comparison group. Despite low household connection rates, we find strong positive effects on luminosity, drinking water provision and school electrification, suggesting that grid connection enables community-level infrastructure. At the household level, we find increases in ownership of electric appliances and financial inclusion. Importantly, effects spill over to households that do not have an electricity connection.

1. Introduction

Universal access to affordable, reliable, and modern energy services is a key Sustainable Development Goal (SDG). While significant progress toward this goal has been made globally, sub-Saharan Africa lags behind, with nearly half its population lacking electricity as of 2018. The International Energy Agency (IEA) estimates that achieving universal electrification in the region by 2040 would require investments of around \$60 billion annually (IEA, 2019). Given these high costs, understanding the developmental impact of electrification is crucial.

In this paper we estimate the impact of community electrification in Burkina Faso on a wide range of development outcomes. Our analysis exploits the roll-out of electrification across 558 localities between 2008 and 2017.¹ We find that while more developed localities were prioritized for grid connection, the timing of electrification was driven primarily by engineering rather than economic constraints. This quasi-exogenous variation allows us to estimate electrification effects using a difference-in-differences (DiD) approach with staggered treatment,

comparing electrified to not-yet electrified communities. Additionally, we employ an instrumental variable strategy based on a theoretical “least-cost” grid network.

Our findings highlight substantial community-level benefits, despite relatively low household electricity uptake. Electrification led to a substantial rise in nighttime luminosity, more electrified schools and improved drinking water systems. We also find suggestive evidence of accelerated population growth in electrified localities. At the household level, electrification boosted ownership of electrical appliances and financial inclusion, even among households that lack private electricity access. These results hint at potential spillover effects, where benefits extend beyond direct users of electricity.

A vast body of literature documents positive effects of rural electrification on development, particularly outside of Africa. In the U.S., rural electrification boosted farm productivity and land values (Kitchens and Fishback, 2015), as well as agricultural employment and population growth (Lewis and Severnini, 2020). In Asia and Latin America, studies have found income gains, increased female labor force participation,

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¹ A locality refers to a settlement (town or village); the terms locality and community are used interchangeably.

and improvements in education and health (e.g., Barron and Torero, 2014; Grogan and Sadanand, 2013; Khandker et al., 2013, 2012; Lipscomb et al., 2013; Van de Walle et al., 2017). However, the evidence for sub-Saharan Africa is more mixed.² Several studies find limited income effects (Ankel-Peters et al., 2024; Bensch et al., 2011; Bernard and Torero, 2015; Dinkelman, 2011; Lenz et al., 2017) and uncertain firm productivity gains (Akpanjar and Kitchens, 2017; Peters et al., 2011). In fact, several studies questioned the cost-effectiveness of rural electrification programs in sub-Saharan Africa given the low uptake and high infrastructure costs (Ankel-Peters et al., 2024; Bos et al., 2018; Lee et al., 2020; Lenz et al., 2017).

Many benefits of electrification accrue at the community level, benefiting even households without direct electricity access, e.g. due to improved provision of public services, increased local economic activity, and spillovers from connected households. In fact, many alleged benefits work through this channel (World Bank, 2008; Chaplin et al., 2017). For the U.S. as well as Asian and Latin American countries, studies have documented substantial community-wide benefits of electrification (Kitchens and Fishback, 2015; Lewis, 2018; Lewis and Severini, 2020; Chakravorty et al., 2016; Burlig and Preonas, 2024; Lipscomb et al., 2013; Khandker et al., 2013; Van de Walle et al., 2017).

Studies on community-level electrification impacts in the African context remain limited. Moreover, most research focuses on the *intensive margin* — the impact of increasing household connection rates within already electrified communities — rather than the *extensive margin*, which examines the effects of introducing electricity to previously unelectrified localities (Akpanjar and Kitchens, 2017; Bernard and Torero, 2015; Chaplin et al., 2017). Two notable exceptions are Dinkelman (2011), who studied labor market effects in newly electrified townships in post-Apartheid South Africa, and Lenz et al. (2017) and Ankel-Peters et al. (2024) who studied electricity uptake in Rwandan communities.

Spillover effects are likely more substantial at the extensive margin of electrification. When a community first gains grid access, it enables the provision of electricity-powered public goods, such as street lighting, water pumps, and improved health facilities, which benefit a broader population. Furthermore, if positive community effects are ignored, comparisons between households with and without electricity within the same community can lead to an underestimation of the overall impact.

Studies outside Africa focusing on the *extensive margin* of community electrification indeed confirm large positive spillover effects to non-electrified households (e.g. Khandker et al. (2013) for Vietnam and Van de Walle et al. (2017) for India). Preliminary evidence from Africa also points to externalities. In rural Kenyan communities, connected households share the benefits of electricity with neighbors, by letting them watch television or charge their mobile phones (Lee et al., 2020), while in Rwanda and Tanzania, electrified households contribute to neighborhood safety by providing external lighting (Bensch et al., 2011; Chaplin et al., 2017).

Thus, to fully grasp the importance of electricity, it is crucial to examine the provision of public goods and spillovers at the extensive margin. Our study offers new insights from Burkina Faso, focusing on both community and household levels. We find that electrification enables community-level infrastructure, which has some positive impacts on closely related outcome variables. This understanding is particularly important given the ongoing debate about the cost-effectiveness of grid expansion in sub-Saharan Africa.

The remainder of the paper is organized as follows. Section 2 describes the data sources. Section 3 outlines Burkina Faso's electrification strategy, while Section 4 details the estimation strategy. Results are presented in Section 5 and discussed in Section 6.

² African countries covered include Benin (Peters et al., 2011), Ethiopia (Bernard and Torero, 2015), Ghana (Akpanjar and Kitchens, 2017), Kenya (Lee et al., 2020), Rwanda (Bensch et al., 2011; Lenz et al., 2017; Peters and Sievert, 2016), South Africa (Dinkelman, 2011) and Tanzania (Chaplin et al., 2017).

2. Data

We compiled a geocoded data set on electrification roll-out in Burkina Faso from 2008 to 2017. To this we added data at the locality and household level. Details are explained in Web Appendix A.

Electrification. We have rich spatial information on the electrification strategy implemented in 2008, which effect we intend to study (Ministère des Mines, 2008, thereafter MEPRED). We created a list of all localities connected to the national grid and their years of electrification.³ The *Annuaire Statistique 2017* was our main source (INSD, 2018). With the help of a map showing the electricity grid in 2017, we geocoded the electrified localities as well as the power lines used to connect them. Overall, there were 660 localities which were connected to the grid between 1954 and 2017. For 20 localities, we were unable to match the locality name to a location. Out of 640 georeferenced localities, 558 localities were electrified between 2008 and 2017.

Luminosity. We use cloud free satellite imagery of stable night-light from (i) the Defense Meteorological Satellite Program (DMSP) and (ii) the Visible Infrared Imaging Radiometer Suite (VIIRS). DMSP measures luminosity in digital numbers (DN), for the years 1992–2013, ranging between 0 and 63 (capped), available at 30 arc-second grid resolution (ca. 1×1 km).⁴ VIIRS measures luminosity in radiance in nano Watts per square centimeter per steradian, for the years 2012–2018, at 15 arc-second grid resolution. VIIRS offers the higher quality recordings detecting light at lower spectra with higher spatial accuracy (Gibson et al., 2021).

We construct two measures of luminosity based on the highest pixel value lit within each locality's polygon.⁵ First, we define a measure of extensive margin, a dummy variable indicating whether the locality was visible from outer space. Second, we construct a measure of intensive margin to quantify the degree of luminosity for ever-lit localities. We merged the DMSP and VIIRS series into a single metric, adjusting the values of the former based on the statistical correlation of their luminosity readings during 2012 and 2013.⁶

Other Data at the Locality Level. Furthermore, we collected detailed information on population, health facilities, schools, roads, drinking water provision, and irrigation. The purpose of collecting this data is two-fold. First, these community characteristics allow us to assess the development status of localities prior to electrification, and thus let us study selection into electrification. Second, some of the measures are available over time and thus allow us to estimate the impact of electrification on community characteristics.

We have population counts from the 1996, 2006, and 2019 censuses (Institut National de la Statistique et de la Démographie, 2000, 2011; Ministère de l'Eau et de l'Assainissement, 2020b).⁷ We added population data for 2015 using the Burkina Faso - High Resolution Settlement Layer from Facebook and CIESIN (2019).⁸ We retrieved data

³ We do not have information on isolated mini-grids or photovoltaic systems. They are not considered part of the national grid.

⁴ Top-coding is not a concern in our context. For localities electrified after 2008, the highest DN is 42.

⁵ Alternatively, luminosity can be calculated using mean pixel values within a locality's polygon, or by creating buffers of different sizes (e.g., 1 2 or 5 km) around the centroid or the brightest pixel of the locality, in line with Min et al. (2013), Dugoua et al. (2018) and Burlig and Preonas (2024). Our results are robust to these alternative specifications.

⁶ Li et al. (2020) proposed combining the two series into a single "harmonized" dataset. However, in our context, this approach leaves a large discontinuity in 2014, coinciding with the transition between series (Web.Appx. Figure B.5). Furthermore, the harmonized data exhibits implausibly large heaping at DN = 6, strongly reducing annual variation. Thus, we opted to predict radiance from DN directly.

⁷ Census data often contain inconsistencies due to omitted, merged, re-named, or misspelled localities. We were unable to track 52 electrified localities across all censuses.

on health facilities in 2007 from [Ministère de la Santé \(2008\)](#). We have information on five types of facilities: (i) CHR: Centre hospitalier régional (N = 12); (ii) CM: Centre médical (N = 34); (iii) CMA: Centre médical avec antenne chirurgicale (N = 41); and (vi) CSPS: Centre de santé et de promotion sociale (N = 1355) and (v) dispensaries (N = 86).⁹ We geocoded annual census data of primary schools for the years 2003–2013 ([Ministère de l'Éducation Nationale et de l'Alphabétisation, 2019](#)). The source informs about electricity at school, student enrollment, school sanitation facilities and teaching materials. We retrieved the location of asphalt roads from Michelin road maps ([Jewab and Storeygard, 2020](#)). We collected data on the timing of AEPS/PEA drinking water installations, their power sources ([Ministère de l'Eau et de l'Assainissement, 2020a](#)) as well as installation year and number of manual water pumps ([Ministère de l'Eau et de l'Assainissement, 2018](#)). Finally, we digitized the location of irrigation systems in 2008 (N = 97) (Figure 5, [Ministère des Mines, 2008, 2008](#)) and major, permanent cattle markets from the 2019 Survey of Livestock Markets (Table 1, [Minot and Elahi, 2020](#)).

Household survey data. We took household and individual level data from the 1993, 2003, 2010, and 2014 rounds of the Burkina Faso DHS ([INSD and ICF](#)). For confidentiality reasons, DHS displaced the true GPS coordinates of urban and rural clusters by up to 2 and 5 km respectively ([Burgert et al., 2013](#)). In our setting, this built-in measurement error could lead to a potentially large attenuation bias, if DHS clusters are matched to the wrong localities. We therefore apply a novel technique to assign DHS household clusters to electrified localities. For each DHS cluster, we calculated a *probability score* that describes the probability of the cluster lying in a given locality. We then only included DHS clusters in our analysis with a probability score of at least 25%, excluding multiple matches with lower scores for the same DHS cluster. We show that this strategy leads to a sample representative of treatment times and administrative regions. Details of the matching strategy are described in Web Appx. A.3.

The 1993, 2003, 2010 rounds are standard DHS surveys, whereas the 2014 round is a Malaria Indicator Survey (MIS) offering less information on health and well-being. This means that we could only observe some development outcomes until 2010. To overcome this limitation, we added a survey from the Living Standards Measurement Study (LSMS) carried out in 2014.¹⁰ We identified LSMS cluster locations by region, province, commune and place name. Thus, we can perfectly match LSMS clusters to the correct electrified community and assign all of them a probability score of 1.¹¹ Overall, we have 219 DHS and 67 LSMS clusters, with roughly 22 households per cluster.

Summary statistics are reported in Web. Appx. Table B.2.

⁸ The settlement layer provides population estimates at a 1 arc-second resolution (approximately 30 m), based on satellite images and building recognition, to which then population derived from sub-national census data was proportionally allocated. We assigned population raster grids to localities following a procedure similar to [Baruah et al. \(2020\)](#) and [AFRICAPOLIS \(2022\)](#). First, we removed single hamlets from the layer. Second, we defined the boundary of a locality as the continuous built-up area within 100 m. Thus, we constructed a buffer of 100 m and joined all overlapping polygons. Finally, we calculated the total population within each polygon of a georeferenced electrified locality.

⁹ The CSPS are the first level of care corresponding to primary healthcare centers. Staffed with about three health workers and two unqualified volunteers, they are responsible for 5 to 23 villages, providing basic outpatient services, including maternity care and administering vaccination programs in the villages. CM and CMA are the first referral level and are equipped with beds and surgical facilities ([Marschall and Flessa, 2011](#)). CHR are regional hospitals.

¹⁰ This is the only LSMS survey available for our time frame of interest.

¹¹ Web Appx. Table A.1 report the number of DHS and LSMS clusters assigned to localities electrified from 2008 onward. We excluded the 1998 DHS survey round from our analysis. Only a few, selected places were sampled during this survey. As a consequence, the survey does not represent the electrified localities well.

3. Background: Electrification in Burkina Faso

Burkina Faso is a small, poor, overwhelmingly rural country in West Africa.¹² It is also one of the least electrified countries in the world.¹³ In 2004, the government announced a national electrification strategy aimed at reducing poverty and ensuring equitable access to electricity across provinces at the lowest possible cost. By 2008, concrete planning activities had advanced. “Mainstreaming Energy for Poverty Reduction and Economic Development” (MEPRED) developed the methodology and reviewed several supply options including (i) the rehabilitation and construction of diesel thermal plants, (ii) the development of photovoltaic solar energy, and (iii) the utilization of biomass. These options were ultimately deemed too expensive, which led Burkina Faso to prioritize the construction of a unified national grid and its integration with the grids of neighboring countries ([African Development Fund, 2009](#), hereafter ADF).

Fig. 1 illustrates the progress in electrification in Burkina Faso between 1993 and 2017. Prior to 2008, household electricity access increased gradually, but the number of electrified localities remained limited. Between 2008 and 2017, however, the number of electrified localities increased eightfold, from 81 to 660. Fig. 2 maps the expansion of the national grid during this period. Most of the large cities were electrified before 2008. Indeed, the capital Ouagadougou and the second-largest city gained access to electricity as early as 1954, and by 2000, 30 of the 32 towns with populations exceeding 10,000 in 1990 were connected. From 2008 to 2017, the grid extended to smaller, less populated localities. Despite this, the urban-rural divide remained stark. In 2014, 58.4% of urban households had access to electricity, compared to just 4.7% in rural areas.

In 2014, electricity supply accounted for approximately 1.3% of Burkina Faso's GDP. The state subsidized electricity. The subsidies amounted to 0.33% of GDP in 2016. Burkina Faso's electricity provider demonstrates slightly better efficiency than the average for African countries. In 2014, electricity losses, calculated as the difference between electricity fed into the grid and actually billed amounted to 17% (mean for sub-Saharan African countries: 22%, sd: 10) ([Trimble et al., 2016](#)). These losses include technical losses during transmission and non-technical losses such as theft, meter tampering, under-reading and under-recording of consumption. Furthermore, Burkina Faso's electricity reliability is on par with the average for African countries. According to the 2014 Afrobarometer (q94) survey, 68% of connected households reported having electricity “most or all of the time”, 17% “about half of the time”, and 14% “occasionally”.

Synergies of Electrification. The government's electrification plan proposed using electricity to strengthen the delivery of public goods such as (i) public lighting, (ii) lighting systems in schools, (iii) lighting and refrigeration for health centers, (iv) water pump systems, and (v) irrigation pump systems for off-season crops ([Ministère de l'Économie et du Développement, 2004](#); [Ministère des Mines, des Carrières et de l'Énergie, 2008](#)). In Section 5.2, we will examine whether outcomes can be attributed to electrification or were resulted from a broad bundle of coordinated development programs implemented concurrently.

Electrification was expected to have wide-ranging effects. [African Development Fund \(2009\)](#) speculated that semi-urban electrification would help the cottage industry¹⁴ and trading activities, engaged in

¹² In 2000, Burkina Faso had a population of 11.6 million, with a GDP/c (PPP) of 1300 (constant 2017 international \$) and 80% of the population employed in agriculture; in 2004, an estimated 57% lived below the \$1.90 a day poverty line ([World Bank, 2020](#)). Between 2000 and 2018, per capita income grew steadily by an average of 3.5% per year (sd: 0.6).

¹³ Burkina household's access to electricity ranked among the lower 20th to 30th percentile of sub-Saharan African countries in the 2000s and roughly maintaining this rank later on (Web Appx. Figure B.1).

¹⁴ The cottage industry refers to small-scale manufacturing activities carried out in homes.

mainly by women. In the health sector, electric refrigerators could replace unreliable gas-powered ones, improving the storage of medicines and vaccines. Furthermore, small medical devices and sterilization equipment could be used, and new health equipment installed. In education, electrification could support evening classes, enable internet-based learning, and improve working conditions for teachers and health personnel, ultimately improving attendance and retention rates (African Development Fund, 2009; Ministère des Mines, des Carrières et de l'Énergie, 2008). However, whether these anticipated effects materialized remains unclear. Even where grid electricity was introduced, photovoltaic systems may have continued to be the primary energy source for schools and health facilities (Ministère des Mines, des Carrières et de l'Énergie, 2008). Moreover, electricity may not have been the key constraint to development as assumed, but rather one of several interconnected challenges

Selection into Electrification. Ministère des Mines (2008) proposed extending the 33 kV network to localities with significant electricity demand and strong development potential. To identify these localities, they followed a systematic approach. First, they calculated a composite Development Potential Index (DPI) for each locality, incorporating three equally weighted dimensions: health, education, and economic development. Sub-indices included indicators such as the presence of health facilities, access to water, educational institutions, population size, agricultural facilities, markets, road infrastructure, and financial institutions (see Web Appx. Table B.1 for a detailed list and weights). Next, to maximize impact for the benefit of the greatest number of people, the population within the catchment areas of these localities was used to designate *development poles* that would be prioritized for electrification. In our analysis, we control for whether a locality is classified as a development pole, its DPI,¹⁵ as well as individual sub-indices.

Ministère des Mines, des Carrières et de l'Énergie (2007) deemed the most cost-effective electrification strategy to expand the network of 33 kV lines within a 50–60 km radius around the existing distribution lines. The 33 kV lines would typically serve the regional centers. Smaller localities could then be connected using single-wire earth return (SWER) lines. SWER lines are cheaper, because they use earth as the return path for the current eliminating the need for a second wire. However, their lower power transfer capacity makes them suitable only for areas with low electricity demand. Then, for localities farther away from the national grid, for which extending the grid would be prohibitively expensive, isolated off-grid diesel power plants were proposed. Finally, it was decided to extend the distribution network to nearby localities where geographic proximity to the new lines ensured economic viability.

Actual electrification certainly deviated from planning. Ministère des Mines (2008), for example, identified 329 development poles and proposed connecting 486 localities via 33 kV lines and 1037 via SWER by 2014. However, by 2017, only 527 localities had gained access to electricity. So what type of localities got electrified? Table 1 examines the covariates of electrification 2008–2017.

In col. (1), we find a significantly positive correlation between being assigned development pole status and the probability of getting actually connected to the grid between 2008 and 2017. In addition, a locality's own population and DPI are positively associated with electrification. These three variables are jointly highly significant ($\chi^2 = 806.7$, $p\text{-val} = 0.00$). Overall, more developed localities were more likely to become electrified.

In col. (2), we add additional measures of development not captured by the DPI, such as dummies for administrative capitals and the square

¹⁵ Ministère des Mines (2008) published DPI scores for only a subset of localities. We reconstructed the DPIs for all localities using available sub-index data. Web Appx. Figure B.2 shows that our DPI closely matches reported DPIs, with regression coefficients matching sub-index weights.

root of the number of primary school students. While coefficients for the DPI and pole decrease due to a certain extent of multicollinearity, the Pseudo R^2 increases. This suggests that the DPI does not fully capture the development factors influencing electrification. In other words, the decision to electrify a community depended on its development status, including factors beyond those measured by the DPI.

In col. (3), we introduce engineering considerations that determined the expansion dynamics of the grid. Specifically, we include a dummy variable for whether a locality was within 60 km of a 33kV line in 2007, the government's reported maximum distance for rural electrification at the time (Ministère des Mines, des Carrières et de l'Énergie, 2007). We also include the distance to the hypothetical 33 kV network that minimizes the line length when connecting all development poles (MST). We find localities further away from the pre-existing and optimal grid were less likely to be connected.

Selection in the Timing of Electrification. In col. (4)–(6) of Table 1 we use the same set of explanatory variables to test the covariates of the timing of electrification. Higher DPI localities got connected earlier (col. (4)). However, the correlation with development pole and population is rather weak. As before, adding development indicators not captured by DPI increases the Pseudo R^2 (col. (5)).

Interestingly, col. (6) suggests that engineering constraints played a large role in the timing of electrification. It turns out that the distance variables to the existing grid and the optimal grid (MST) are highly significant, whereas the DPI, development pole and population are jointly not significant ($p\text{-val}: 0.13$). Thus, the timing of electrification did not depend so much on a locality's development status. It appears that it is governed by engineering concerns, i.e. that the network has to expand from the existing network.

Overall, localities were selected for electrification based on their size, development and growth potential. The correlations highlight the serious endogeneity concerns when comparing electrified to non-electrified localities. However, we follow a different strategy comparing localities that were actually connected, but in different years. Those localities should be more similar, as they all qualified for electrification. Indeed, we found that the timing is far less correlated with development. The state of the 33 kV distribution network determined the timing when localities were electrified. Localities closer to the existing network were connected earlier. Moreover, it appears that there is a lot of noise in the timing - in fact, we do not find any correlation between planned and actual date of electrification ($p\text{-val}: 0.55$).¹⁶ Nevertheless, endogeneity issues may remain a concern, which we try to mitigate in three ways. First, we use observable, pre-existing indicators as controls. In particular, we control for the DPI score of each location. Second, we empirically test the assumption of conditional parallel pre-trends. Third, as an extension we exploit an instrumental variable strategy based on the engineering aspect of the electricity grid.

4. Estimation strategy

4.1. Difference in difference estimation with staggered treatment timing

We study the impact of electrification in Burkina Faso at two levels. First, using panel data on localities, we analyze community-level infrastructures and outcomes such as night-lights and electricity powered public services. This approach also allows us to assess whether electrification came as a bundle, with improvements in other public goods such as water, sanitation and education. Second, we match household data from multiple rounds of DHS and LSMS surveys to the electrified localities, using repeated cross-sections to analyze the effects of electrification at both the household and individual levels. Here, we can disentangle private benefits from community-wide impacts, including potential spillovers to non-electrified households within electrified localities.

¹⁶ The proposed date of electrification comes from Ministère des Mines (2008).

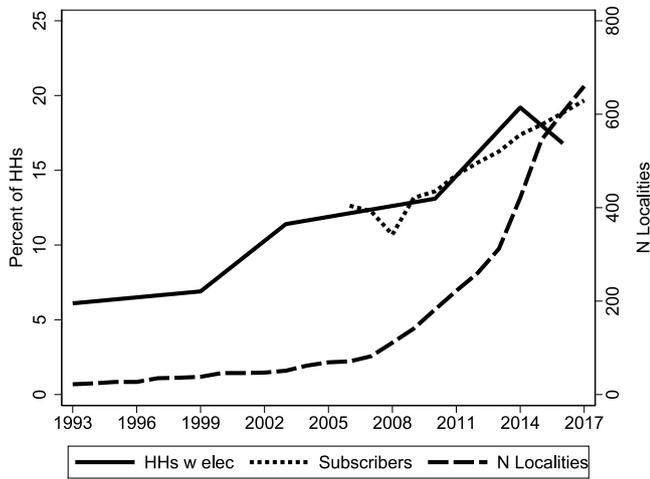


Fig. 1. Electrification in Burkina Faso 1993–2017.

Notes: This graph shows various measures of access to electricity. The percentage of households with electricity is from the 1993, 1999, 2003, 2010, 2014 and 2016 DHS (INSD and ICF, 2016). We also calculated the percentage of households that subscribed to the services of the electricity providers SONABEL and FDE. We divided the number of subscribers reported in Burkina Faso (2021) by the average household size derived from INSD and ICF (2016). Finally, the number of electrified localities (secondary axis) is sourced from INSD (2018).

Baseline estimation for the panel of localities. To capture the temporal dynamics of electrification, we employ a panel event-study approach. Using a panel of localities l observed over calendar years t , electrification acts as the *event*. Localities are treated in different years g_t , with $T_{lt} = t - g_t$ representing the years since electrification. Electrified localities are the *treatment group*, while those not yet electrified serve as the *comparison group*. Treatment is irreversible, meaning treated localities remain treated. In the absence of staggered treatment adoption or heterogeneous treatment effects, we could rely on the canonical two-way fixed effects (TWFE) model to estimate the dynamic impacts:

$$Y_{lt} = \alpha + \sum_{k=1}^K \gamma_k 1(T_{lt} = k) + \mu_l + \delta_t + X'_l \Gamma + \epsilon_{lt} \quad (1)$$

where Y_{lt} is the outcome variable, μ_l and δ_t are locality and year fixed effects, and X_l are a set of (time invariant) controls. The term $1(T_{lt} = k)$ is an indicator for the k th year relative to electrification.

However, staggered treatment adoption and heterogeneous effects may bias TWFE estimators (Borusyak et al., 2024; De Chaisemartin and D’Haultfoeuille, 2022; Sun and Abraham, 2021). To address these issues, we employ the imputation estimator proposed by Borusyak et al. (2024).¹⁷

The Borusyak et al. (2024) imputation estimator (called BJS from here on) proceeds in three steps. First, *within untreated observations only*, we estimate:

$$Y_{lt} = \mu_l + \delta_t + \epsilon_{lt} \quad (2)$$

Second, we use this model to predict the expected untreated potential outcomes for treated observations $\hat{Y}_{lt}(0)$. Treatment effects are then calculated for each treated observation as $\tau_{lt} = Y_{lt} - \hat{Y}_{lt}(0)$. Third, we aggregate these effects across observations. To estimate the treatment effects by length of exposure, we estimate τ_T , where $T_{lt} = t - g_t$ denotes the number of years since treatment. We also report the overall average treatment effect on the treated (ATT) τ , balanced with respect to event time.

Dynamic Estimation with Controls. To account for dynamic differences between localities with different development potential, we

extend equation (2) including a set of control variables that capture a locality’s potential for future development. We interact these control variables with year dummies to allow flexible time trends based on observable pre-treatment differences.

$$Y_{lt} = \mu_l + \delta_t + X'_l \lambda_t + \epsilon_{lt} \quad (3)$$

where X_l is a vector of pre-treatment, time-invariant controls measured before 2008 and that were significant predictors of the year of electrification. Specifically, X_l includes whether the locality was designated a “development pole” by the central government, the Development Potential Index (DPI), and a measure of student enrollment in 2007. These pre-treatment measures of the community’s development potential are then interacted with dummies for calendar years.

Baseline estimation for the repeated cross-sectional survey data. To estimate household and individual-level effects of community electrification using survey data, we use repeated cross-sections. Similar to the panel setup above, the classic TWFE estimator is unsuitable due to staggered treatment timing. We once again implement BJS. The regression model in the first step, estimated from only untreated observations, now becomes:

$$Y_{ilrt} = \alpha + \delta_t + \theta_r + X'_{lr} \lambda + A'_{it} \Gamma + \epsilon_{ilrt} \quad (4)$$

where i describes a household (or individual) observed at time t in locality l in region r . Y_{ilrt} is the outcome variable, and X_{lr} is a vector of control variables at the locality level (measured before 2008). A'_{it} is a vector of household level control variables that are time varying but exogenous to treatment (e.g., level of education among adult household members). Time fixed effects are denoted δ_t . In this set-up, individual (or locality) fixed effects cannot be included due to the lack of panel data. However, we include region fixed effects θ_r and we expand the set of control variables to counter the lack of locality fixed effects. Just like X_l in the locality-level estimation, X_{lr} includes whether the locality is a development pole, the DPI, and primary school enrollment in 2007. Additionally, X_{lr} includes whether the locality is a level 2 or 3 administrative capital, population before the treatment period (in natural logarithms), and survey fixed effects (whether the observation came from LSMS or DHS). By including X_{lr} we control for the covariates that were significant predictors of the year of electrification, as in col(5) in Table 1.

Dynamic estimation for the repeated cross-sectional survey data. To allow differences in trends based on a locality’s development potential, we extend Eq. (4) by including community-level control variables interacted with year dummies:

$$Y_{ilrt} = \alpha + \delta_t + \theta_r + X'_{lr} \lambda_t + A'_{it} \Gamma + \epsilon_{ilrt} \quad (5)$$

allowing Y_{ilrt} to vary flexibly over time based on measures of the localities’ development potential captured in the vector X_{lr} . These locality-level control variables in X_{lr} (the predictors of year of electrification from col (5) in Table 1) are measured before the first treatment period - i.e. pre 2008. We interact them with year dummies to allow flexible time trends based on pre-treatment locality characteristics. A'_{it} is again a vector of household level control variables that are measured at time t , but assumed to be independent of treatment.

Community versus private effects. The survey data includes information on whether households have private access to electricity. This allows us to disentangle the private effects of electrification from community or spillover effects. To estimate the latter, we re-estimate Eqs. (4) and (5) for households *without* private access to electricity. This yields a lower-bound estimate of the community- or spillover effects. This provides a lower-bound estimate, because it compares households in electrified communities that never connect to electricity (never-takers) with households in non-electrified communities, which include both never-takers and those that would connect if electrified (compliers). It is reasonable to assume that complier households differ substantially from never-taker households in their socio-economic status. To

¹⁷ We employ the Stata package `did_imputation` to implement the estimator.

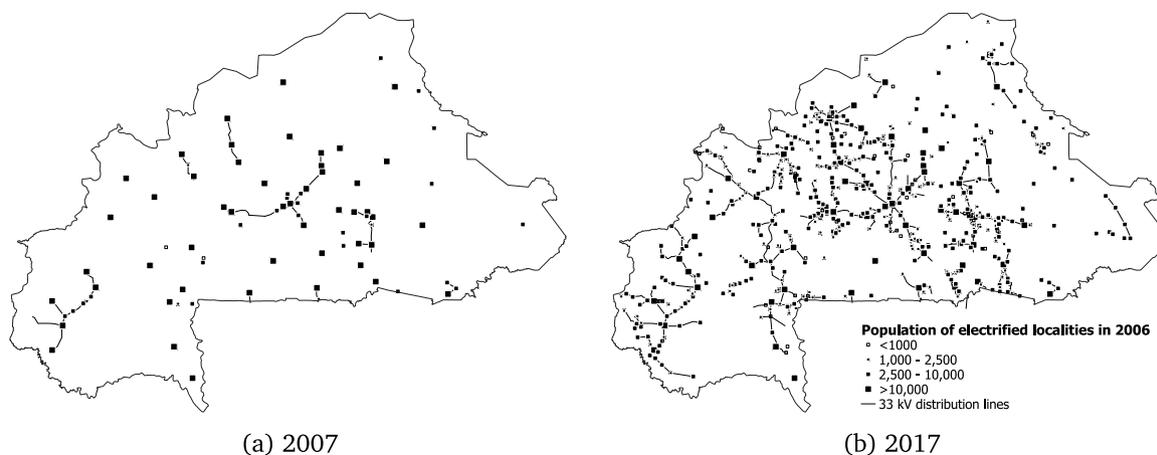


Fig. 2. Electrified localities 2007 and 2017.

Notes: The maps show the electrified localities and their population in 2006 (Institut National de la Statistique et de la Démographie, 2011). Fig. 2(a) shows the situation in 2007. N(localities <1000; 1000–2500; 2500–10,000; >10,000) = 2; 6; 26; 47. Fig. 2(b) shows the situation in 2017. N(localities) = 61; 184; 277; 56. The 33 kV power lines are the main distribution network. See Web Data Appendix for data sources.

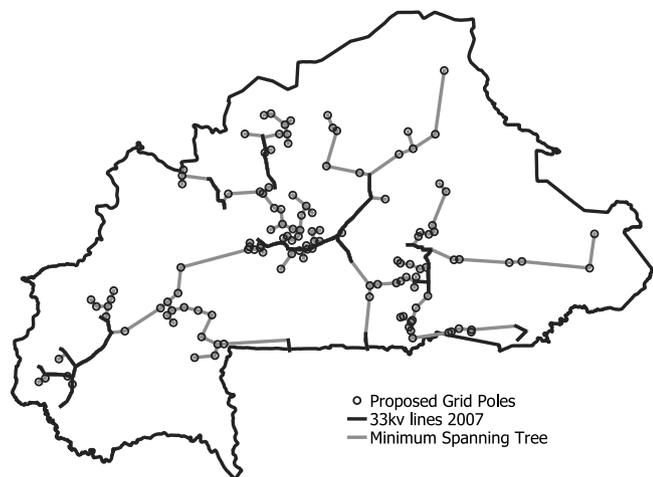


Fig. 3. Proposed grid poles and minimum spanning tree.

Notes: The map shows the proposed development poles by Ministère des Mines (2008). Taking into account the existing 33kV grid of 2007 the network that would connect the development poles minimizing network length is the Minimum Spanning Tree (MST).

address potential bias arising from these differences, we control for pre-determined socio-economic characteristics, including the gender and age of the household head and the primary and secondary education of adult women in the household.¹⁸

4.2. Identifying causal treatment effects

Identification. An obvious threat to identification is selection into electrification. Web Appendix Table B.2 indeed shows that at baseline electrified localities systematically differ from those not electrified. Further, Table 1 confirms that a locality’s development potential was an important determinant of electrification. We argue that eligibility criteria were common to all electrified localities and that engineering constraints, particularly the state of the 33 kV distribution network, largely determined the timing of electrification. We address this selection bias when exclude never electrified localities and limit the

¹⁸ Data on men’s education is unavailable in some surveys and is therefore excluded from the analysis.

comparison group to those localities that were not-yet electrified at time t . However, this may not remove the endogeneity issue entirely.

First, towns that are electrified earlier may nevertheless differ in their trajectories from those that were electrified later. Thus, the parallel-trend assumption is key for identification. We initially assume (in the notation of the locality regression) that untreated potential outcomes follow a fixed-effects model: $E[Y_{it}(0)] = \alpha_l + \beta_t$. This assumes no systematic differences in trends across localities l in the absence of treatment. We can further weaken this assumption by allowing trends to vary based on time-invariant observables. Thus, the parallel trend assumption becomes: $E[Y_{it}(0)|X_l] = \alpha_l + X'_l\gamma_t$.¹⁹ We test the parallel-trend assumption using a standard Wald test for pre-treatment trends. However, as Roth (2022) has noted, this type of test frequently fails to detect minor violations of the parallel pre-trend assumption that can still meaningfully influence the estimated post-treatment impacts. Following Roth (2022), we calculate the worst-case pre-trend scenario consistent with our data, assessing the maximum potential violation of parallel trends that conventional tests might miss. Additionally, we evaluate the robustness of our findings by calculating the “breakdown” point of significant results due to minor changes in the slope of pre-trends (Rambachan and Roth, 2023).

Second, there must be no anticipation effects. We argue that this is very likely the case, due to the unpredictable nature of the exact arrival of the grid.

Finally, the estimation requires a sufficient coverage of untreated observations for the control-treatment comparison. Therefore, we cannot estimate effects for 2017 localities electrified in 2017, as they purely serve as control units. Naturally, for data that run only up to 2014, localities electrified 2015–2017 will serve as controls.

Internal versus external validity. Our analysis is based on localities electrified between 2008 and 2017. This strategy strengthens internal validity by addressing endogeneity concerns stemming from positive selection, as outlined below. However, excluding never-treated localities and focusing on this specific period may compromise external validity. If, for example, electrification were eventually extended to the remaining 7000 localities in Burkina Faso, and those electrified during the study period had greater potential for positive outcomes than the average locality, our estimated effects may overstate the impact for the population as a whole. This reflects an inherent trade-off between internal and external validity.

¹⁹ The assumption for the household model with controls in levels is analogous: $E[Y_{it}(0)|X_l] = \alpha_{il} + X'_l\gamma_t$.

5. Results

5.1. Community-level effects (locality data)

We first examine whether electrification enabled community infrastructure development. Fig. 4 shows the evolution of luminosity, AEPS/PEA drinking water systems, and school electrification, while the upper panel of Table 2 reports the estimated Average Treatment Effect on the Treated (ATT) for $T = [0, 6]$ balanced with respect to event time, along with a conventional cluster-robust Wald test for pre-treatment trends $T = [-6, -1]$.

Electrification significantly increased luminosity. At $T = -1$, only 3.6% of localities had detectable night-lights. From $T = 1$ onward, this share rose sharply before stabilizing approximately four years post-electrification (Fig. 4(a)). The estimated ATT indicates a 39.5% percentage point (pp) increase in lit localities during the six-year post-electrification period. A similar upward trend is observed at the intensive margin (Fig. 4(b)).²⁰ This pattern suggests a temporary growth effect in luminosity leading to a sustained higher level of night-time illumination.

The literature typically does not distinguish between extensive and intensive margins. At $T = 4$, the combined effect on radiance is 0.3, equivalent to 0.8 DN.²¹ This increase is larger than the +0.5 DN observed in 141 Senegalese and Malian villages electrified between 1992 and 2009 (Min et al., 2013) and is twice as large as the +0.4 DN effect estimated for India (Burlig and Preonas, 2024). Increased luminosity may stem from various sources. Min et al. (2013), for example, attributed rising night-lights in Senegal and Mali primarily to street lighting, whereas much of the economics literature associates luminosity with broader economic activity (e.g., Henderson et al., 2012).

Furthermore, electrification benefited public infrastructures. AEPS (Adduction d'Eau Potable Simplifiée) are small-scale piped water systems with storage tanks and public standpipes sometimes serving several villages, whereas PEAs (Point d'Eau Autonome) are standalone water points, such as boreholes fitted with motorized pumps, providing localized access to safe drinking water. These systems often rely on electrical pumps, which, after electrification, can be powered by grid electricity instead of diesel or solar. The timing of new installations is striking. Grid-powered AEPS/PEA installations increase sharply starting one year after electrification (Fig. 4(c)). Most installations take place between three and five years post-electrification, with virtually no additions by year six. Since new installations add to the cumulative stock, by $T = 5$, 16% of localities had installed a grid-powered system.

Electrification led to a notable increase in the share of electrified schools. As Fig. 4(d) shows, the share of electrified schools rose immediately at $T = 0$, with an effect size of 12.7 pp for $T = [0, 3]$. This represents a substantial improvement from the pre-electrification baseline of 22% (Table 2).

Finally, we examine student enrollment and population growth. We find little evidence that electrification increased student enrollment (Table 2). In contrast, electrified localities appear to experience faster population growth than those not-yet electrified. The estimated 4 pp increase suggests a significant effect, likely driven by migration. However, this effect should be interpreted with caution, as it relies on population measurements from 2006 and 2015. In Section 5.4 we apply a TWFE specification that better suits the available demographic data.

Results are qualitatively similar across models with and without controls (Table 2). As for the parallel-trend assumption, we cannot reject the null that pre-treatment coefficients are jointly zero in either

of the two models. To further validate the parallel-trend assumption, we conducted a power analysis following Roth (2022). Results, shown in Web Appendix Figure B.7, indicate that point estimates are well above the continuation of the worst detectable pre-trend consistent with our data. However, results for water provision and school electrification display greater sensitivity; the ATT loses significance if pre-trend slopes are adjusted upward by about 25% (Web Appendix Figure B.8).

5.2. Electrification or a bundle of development programs?

Electrification can improve the delivery of other public services. Consequently, policymakers may seek to pair electrification with other development initiatives to amplify the overall developmental impact. This raises an important question: Are the observed effects driven by electrification, or are they the result of accompanying initiatives? To explore this, we focus on education and water.

Education. We examine additional indicators of school infrastructure that are typically targeted by development programs in education (UNESCO, 2022). If school investments were part of a coordinated effort alongside electrification, we would expect to observe improvements in school infrastructures unrelated to electrification. However, we find no significant effects, either pre- or post-treatment, across a wide range of schooling investment dimensions, including the number of public primary schools, school sanitation facilities, and teaching resources (Panel B, Table 2).²² Overall, the evidence suggests that while grid expansion was used to electrify schools, it did not coincide with broader investments in school infrastructure or education.

Access to Clean Water. Strategic planning documents emphasized synergies between electricity and water development (Ministère des Mines, des Carrières et de l'Énergie, 2008; CIFAME (Commission Intersectorielle de Facilitation de l'Approche Multisectorielle dans le domaine de l'Énergie), 2008). Was electrification bundled with water investments?

First, the decentralized nature of water provision in Burkina Faso makes systematic coordination unlikely. In peri-urban localities, water supply is managed at the municipal level, often through Water Users Associations (Humphreys and Schwartz, 2018; Ouedraogo, 2010). The state-run utility, ONEA, primarily serves large urban areas. Indeed, ONEA played a negligible role in our sample, managing water systems in only five localities (Ministère de l'Eau et de l'Assainissement, 2020a). Moreover, municipalities receive minimal central funding: between 2007 and 2015, between 2007 and 2015, direct transfers to municipalities accounted for merely 8% of public expenditure on water supply (Humphreys and Schwartz, 2018). Instead, municipalities heavily rely on donor funding and service fees. The multitude of donors and the fragmented governance structure complicate coordinated water provision. For instance, Humphreys and Schwartz (2018) report 16 different donors providing funding for water projects across just four municipalities.

Second, we employ a placebo strategy to test for bundling of electrification with water infrastructure investments. If such bundling existed, systematic patterns in water provision following grid expansion would be expected. However, our data does not support this. Human-powered pumps continued to be widely installed post-electrification; between 2008 and 2016, localities already connected to the grid installed 1274 human-powered water pumps. While these manual pumps may complement grid-powered systems in areas unsuitable for electrical infrastructure, a coordinated rollout would imply a distinct increase synchronized with grid expansion. However, row 1 of Panel C Table 2 indicates that the installation of human-powered pumps neither surged nor declined systematically with electrification.

²⁰ Web Appx. Figure B.4 reports separate estimates for DMSP and VIIRS. Conclusions remain unchanged.

²¹ This conversion is based on the 2012/13 correlation between DMSP and VIIRS, where one radiance unit in VIIRS corresponds to 2.7 DN in DMSP.

²² Wall maps were selected as a representative indicator due to their highest correlation with the first principal component in a principal component analysis. Results for the available set of teaching materials are provided in Web Appx. Table B.3.

A similar logic applies to substitutes for grid power. Simplified Drinking Water Supply Systems (AEPS) and autonomous water access points (PEA) can be powered by any energy source. In the presence of coordinated bundling, we would expect either a substitution of grid power for solar power or an increase in solar installations coinciding with grid expansion if they were complementary. Again, solar remained an important primary energy source for electric pumps used in AEPS/PEA water systems, but the data indicates no systematic pattern: Solar-powered water system installations show neither significant pre-trends nor notable changes post-electrification.²³

Taken together, grid electricity is unlikely to have been well-coordinated with other development programs. Instead, grid expansion appears to have enabled water provision by reducing costs, rather than being part of a systematically bundled development initiative.

5.3. Community versus private effects (survey data)

Fig. 5 shows the impact of community electrification on household electricity uptake and asset ownership over time. Visually, there are no apparent conditional pre-trends discernible (a full discussion of the diagnostics for parallel trends follows below). The analysis encompasses two specifications: the first controls only for level-differences in pre-treatment community characteristics. The second specification allows for time-trends to vary flexibly by pre-treatment community characteristics.²⁴ Results are very similar across the two specifications. Household electricity access is virtually zero before electrification but then increases to around 20% six years after grid connection. We also observe small increases in ownership of radios, televisions, and refrigerators. Estimates for individual event-times are noisy, as the composition of treatment and control locations varies across survey rounds. For example, confidence intervals tend to be very large from $T = 5$ onward, because our sample only extends to year $t = 2014$, meaning we can only observe $T = 5$ for localities electrified in 2008 or 2009.

Due to the noise in individual event-time estimates, we estimate the simple ATT for 0 to 6 years post-electrification (Table 3). Just as in Fig. 5, we present results for the two alternative specifications (columns entitled “Levels” and “Trends”). For household assets other than electricity, we estimate both the *total effects* of community electrification, which include all households, and the *spillover effects* on non-electrified households. Electricity uptake is significant but remains low, reaching about 10% of households from a near-zero baseline. We find large and significant increases in radio ownership of 6 to 10 percentage points, with spillover effects identical to the total effect. In contrast, while community electrification significantly increases television and refrigerator ownership, these effects are limited to electrified households, likely because these appliances require a continuous power supply.

We also estimate the effect of community electrification on household access to piped drinking water, whether through private connections or public standpipes. Before electrification, only 5% of households had access to piped drinking water, a share that nearly doubles post-electrification. This finding aligns with the observed expansion of water provision systems at the community level. While spillover effects are imprecisely estimated, point estimates suggest they are only slightly smaller than the total effect.

The household level effects of community electrification accrue slowly over a number of years, as can be seen in Fig. 5. To take this into account, we separately estimate short- and medium-term effects of community electrification on household electrification and asset ownership. The results are shown in Web Appendix Table B.4. Immediate effects, 0

to 1 years after electrification, are very limited. However, medium-term effects 2 to 6 years after electrification are larger and more statistically significant than the overall ATTs reported in Table 3. Electricity uptake over this medium-term is on average 15 percent, and access to piped water increases by 7 percentage points, an impressive 140% increase from pre-electrification levels.

We report the p -value from a Wald test of joint significance of pre-treatment coefficients (6 years before electrification), none of which indicate statistical significance for any dependent variable. To assess whether pre-trend tests may be underpowered, we apply the diagnostics from Roth (2022) and Rambachan and Roth (2023) (Web Appendix Figures B.9 and B.10). The results on household electricity access are not sensitive to potential violations of the parallel trends assumption, while for the other variables there is a risk that pre-trend tests are underpowered. This is likely due to small sample size when estimating coefficients for individual time periods.

Finally, we tested but did not find any significant effect of community electrification on child health outcomes like infant mortality rates and child malnutrition (stunting and wasting), or health inputs such as prenatal care and birth attendance (Web Appendix Table B.5).

5.4. Alternative estimation strategies

The dynamic staggered regression estimator is our preferred specification, because it nicely illustrates the dynamics and makes efficient use of sample size. In this section we apply alternative identification strategies. First, we apply a standard static two-way fixed effects estimator (TWFE) to a reduced sample of villages, such that there are only two periods and two groups. Reducing our sample to a two by two matrix means that a standard TWFE estimator will be unbiased, but it requires a large reduction in sample size and change in sample composition. Second, we employ an instrumental variable approach to a cross-section of villages. This serves as a robustness check and additionally allows us to analyze additional development outcomes for which we do not have long panel data.

Canonical DiD setup. We follow a standard DiD set-up and estimate a static two-way fixed effects estimator (TWFE), as in Eq. (1), but with only two periods and two groups. For outcomes at the level of locality (l) in region r :

$$Y_{lrig} = \alpha + \beta Treat_g * Post_t + \mu Treat_g + \delta Post_t + \theta_r + X'_{lr} \Gamma + \epsilon_{lrig} \quad (6)$$

where g is now divided into a treatment group with $Treat_g = 1$ if $g = [2008, 2009]$ (localities electrified in 2008–2009) and a comparison group with $Treat_g = 0$ if $g = [2015, 2017]$ (localities electrified in 2015–2017). We observe both groups in a pre-treatment period ($Post_t = 0$ if $t = [2003, 2007]$) and in a post-treatment period ($Post_t = 1$ if $t = [2010, 2014]$). δ captures differences between the pre- and post periods. μ captures time-invariant differences between treatment and comparison group. β is the DiD estimate. For household-level outcomes the model adds household controls, just as in the main specification. Standard errors are clustered at the locality level. For the locality level analysis, we have yearly data for the pre- and post-treatment periods, whereas for the household level analysis the pre-period consists of the 1993 and 2003 DHS surveys, and the post-period consists of the DHS and LSMS surveys from 2010 and 2014.

Instrumental Variable Approach. We previously showed that conditional pre-trends do not statistically differ between localities electrified in different years, indicating that the identifying assumption may plausibly hold. However, some variables exhibit low statistical power in the pre-trend tests. As a robustness check, we employ an alternative identification strategy using an instrumental variable approach.

The timing of grid expansion is largely dictated by engineering constraints. Grid extension follows a contiguous process — standalone grids are of little practical use. Thus, localities closer to the existing network tend to be connected earlier, while those positioned along expanding transmission lines were connected as the network extended.

²³ Web Appendix Figure B.6 plots the pre- and post-electrification coefficients.

²⁴ In both models we also control for household characteristics, which, while not affecting point estimates, improves precision.

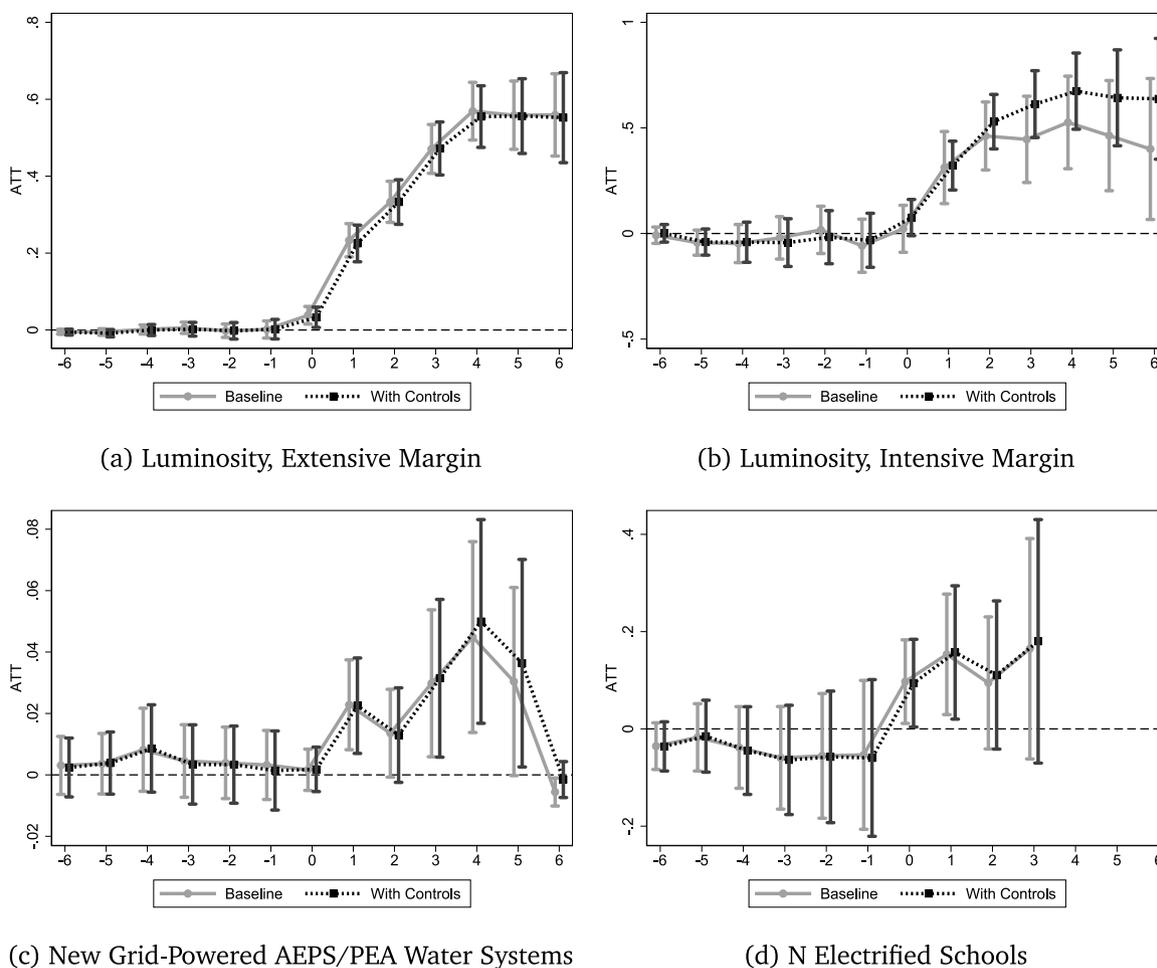


Fig. 4. Luminosity, water supply, and school infrastructure before/after electrification. *Notes:* This figure presents average treatment effects on the treated (ATT) by length of exposure to electrification, along with 95% confidence intervals. The horizontal axis represents the length of exposure to the treatment ($T = [-6, 6]$). The zero line refers to the average of all pre-treatment estimates. The estimator is the imputation estimator proposed by Borusyak et al. (2024), and standard errors are clustered at the locality level. Pre-treatment parameters are reported as short differences ($Y_t - Y_{t-1}$). Sub Fig. 4(a) depicts the proportion of localities with non-zero night-lights (extensive margin), while Sub Fig. 4(b) displays the maximum radiance within ever-lit localities (intensive margin). The DMSP and VIIRS night-light datasets were harmonized using the correlation between the two series during their overlapping years (2012/2013). Sub Fig. 4(c) shows newly installed water systems (AEPS/PEA) powered by grid electricity. See Web Data Appendix for data sources.

However, using distance to the existing network as an IV would be naïve, as the existing grid is likely correlated with economic activity, with more remote localities being systematically less developed. Column (4) of Table 1 confirms this pattern.

To construct a valid instrument, we model a hypothetical least-cost grid network. We assume that the government wants to electrify all localities designated as “development poles” in the cheapest way possible. Taking the 2007 existing grid network and development poles as vertices, we construct a minimum spanning tree (MST) that minimizes total network length. Since network length is the primary cost determinant, this MST approximates the least-cost network. The resulting network is shown in Fig. 3. The log distance of the (non-development pole) communities to this least-cost electricity network then serves as an instrument for the timing of electrification. The instrument effectively captures complier localities that were connected earlier because they happened to be “on the way” as the grid was expanded to the development poles.

The key identifying assumption behind this instrumental variable strategy is that a locality’s distance to the least-cost electricity network affects the timing of its electrification but does not directly influence economic outcomes beyond its effect on grid access. To address potential endogeneity from variables used in the modeling of the least-cost network, we condition for proximity to the 2007 grid (within 60 km) and log distances to the development poles.

Constructing hypothetical - and therefore exogenous - networks is a well-established strategy in the infrastructure literature. Faber (2014), for example, constructed a hypothetical least-cost spanning tree network to model the expansion of China’s National Trunk Highway System. Similarly, Lipscomb et al. (2013) simulated the expansion of Brazil’s electricity grid 1960–2000 under purely geography-based cost considerations. Other studies emphasize network connectivity as the primary determinant of expansion. Jedwab and Moradi (2016) modeled a hypothetical least-cost colonial African railway network, predicting how major cities would have been connected under a unified system. Frye (2024) constructed an IV from a proposed but never-built historic highway plan, applying an algorithm that prioritized segments based on their importance for network connectivity.

Our IV set-up is then as follows. At the locality level, outcomes are measured between 2010 and 2014 and treated as a pooled cross-section. Localities electrified in 2008–2009 are the treatment group. Localities electrified in 2015–2017 represent the comparison group (as they were not yet treated in 2010–2014). Our instrument is log distance to the MST. We simultaneously control for whether the locality is a pole and distance to the nearest pole to take into account that economic activities may be spaced concentrically around poles. Compliers are then localities that were connected earlier because they were located “along the optimal grid network”. We estimate the reduced form (RF)

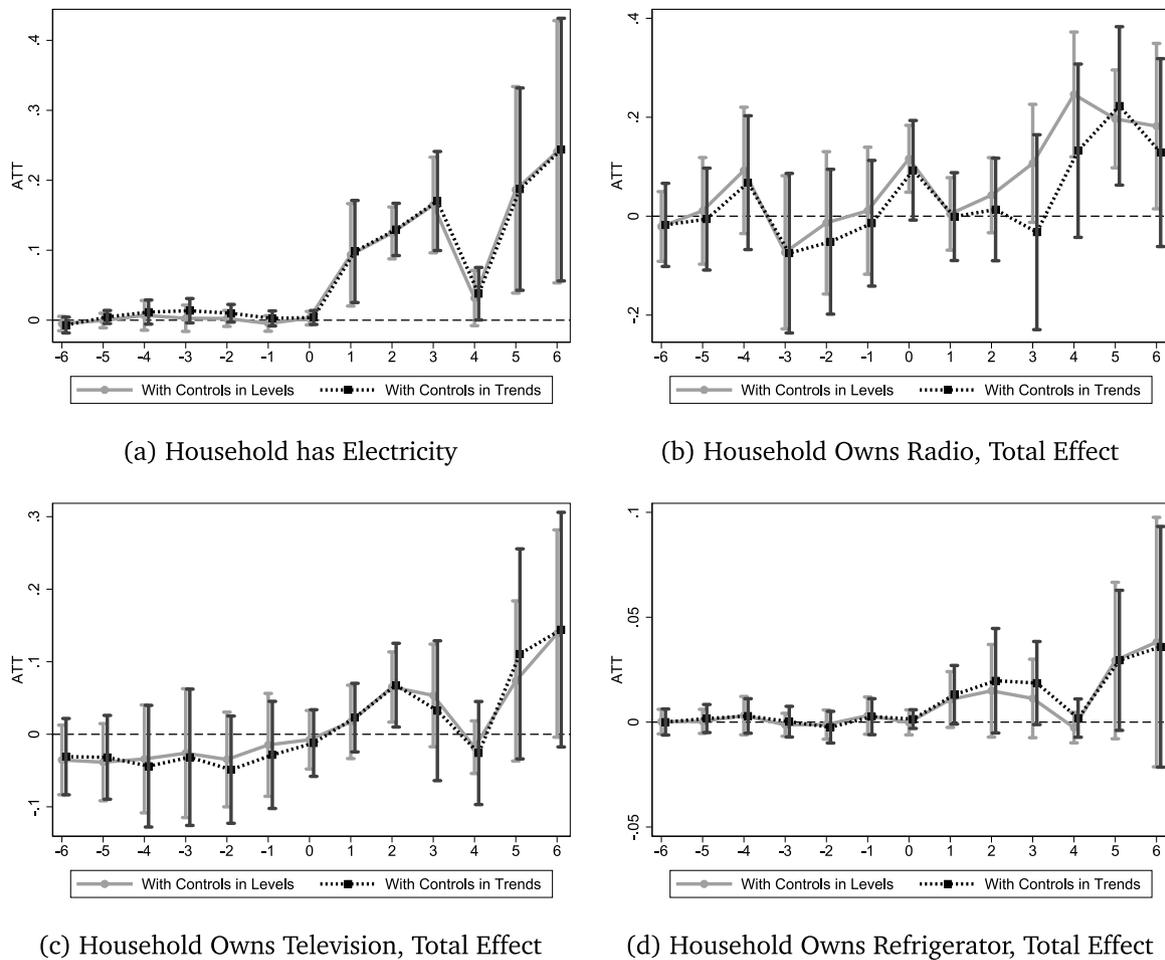


Fig. 5. Household access to electricity and electric appliances before/after electrification. Notes: This Figure shows average treatment effects by length of exposure to electrification with 95% confidence intervals. The horizontal axis indexes the length of exposure to the treatment $T = [-6,6]$. Estimator is the Borusyak et al. (2024) imputation estimator and standard errors are clustered at the locality level.

and the 2SLS. For outcomes at the locality level, the reduced form can be written as

$$Y_{lrt} = \alpha + \beta \ln(\text{Distance to MST})_{lr} + X'_{lrt} \gamma + \theta_r + \epsilon_{lrt} \quad (7)$$

for locality l in region r and years $t = [2010, 2014]$. Control variables X include indicator variables for the locality being a development pole, 60 km to the nearest 33 kV line in 2007, province and commune capital, and number of primary school students in 2007, logged distance to pole and the DPL. For outcomes at the household level, the model additionally includes household-level controls, and standard errors are clustered at the locality level.²⁵

Note that the IV generates a LATE estimate (not an ATT). In addition, the sample has changed, because we dropped localities electrified in the years $t = [2010, 2014]$. Hence, results may not be directly comparable to the BJS estimates from Tables 2 and 3.

Results for the community-level variables are presented in Table 4. Starting with the ATT using the BJS imputation method for localities electrified between 2008/2009 and 2015–2017, we find significant increases in luminosity at the extensive and intensive margin, AEPS/PEA drinking water systems, and electrified schools. The canonical 2×2

²⁵ Community control variables are the same as in the main specification, but we have added the logged distance to the nearest development pole and whether the locality is within 60 kms of the nearest 33 kv line in 2007. These distance measures allow us to isolate the exogenous part of the variation in our instrument: the logged distance to the MST.

DiD estimator, while producing slightly larger estimates for luminosity and smaller for school electrification and water systems, supports these findings.

In subsequent columns, we restrict our analysis to the post-treatment period 2010–2014. OLS essentially estimate the difference between electrified and non-electrified localities conditional on controls. The estimated effects lie in between the BJS and TWFE point estimates.

Finally, the instrumental variable regression produces point estimates for the local average treatment effect (LATE), which tend to be higher than the baseline OLS. Notably, the effect on nightlight intensity triples while the electrification of schools doubles, while the effect on student enrollment becomes positive. However, the estimate are not statistically different from the BJS, TWFE or OLS estimator. As indicated by the Kleibergen–Paap and Montiel–Pflueger F-statistic, the instrument strength is adequate, except for luminosity at the intensive margin, where the large drop of non-lit localities weakens the instrument. Overall, this battery of alternative identification strategies corroborate the robustness of our findings from the staggered DiD model, pointing to consistent improvements in community infrastructure post-electrification.

Results at the household level are reported in Table 5. The first column shows our preferred dynamic estimator, as used in Table 3, but applied to the reduced sample. The second column shows the standard TWFE estimator (also applied to the reduced sample). The two DiD specifications yield very similar estimates of the ATT. Both show a large and significant increase in household electricity uptake, as well as significant increases in television and refrigerator ownership. These

Table 1
Covariates of electrification 2008–2017.

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var./Estimator	Electrified 2008–2017/Probit			Year of Electr./Ordered probit		
Sample	Localities not yet electrified in 2007			Only localities electrified 2008–2017		
Development Pole	0.81*** (0.07)	0.48*** (0.09)	0.52*** (0.09)	0.08 (0.11)	0.24** (0.12)	0.28** (0.13)
LN(Population)	0.30*** (0.04)	0.28*** (0.05)	0.32*** (0.05)	0.07 (0.08)	0.13* (0.07)	0.08 (0.09)
DPI	4.44*** (0.39)	2.62*** (0.47)	2.03 (0.48)	-2.56*** (0.56)	-1.48 (0.76)	-0.73 (0.63)
Within 60 km to grid 2007			0.09 (0.10)			-0.49*** (0.16)
LN(Dist. to grid 2007 (km))			-0.12** (0.04)			0.07 (0.06)
LN(Dist. to dev. pole (km))			0.12** (0.06)			-0.06 (0.09)
LN(Dist. to MST (km))			-0.20*** (0.05)			0.26*** (0.09)
Observations	7672	7672	7332	502	502	502
Region FE	YES	YES	YES	YES	YES	YES
Dev. Indicators	NO	YES	YES	NO	YES	YES
Pseudo R2	0.290	0.322	0.338	0.044	0.052	0.080

Notes: This table tests covariates of electrification. Unit of observation are localities. Localities electrified before 2008 are excluded. Col. (1)–(3) estimate a probit model where the dependent variable is a binary indicator of whether the locality was electrified between 2008 and 2017. Col. (4)–(6) estimate an ordered probit model where the dependent variable is the year of electrification, treated as an ordinal outcome. Development pole is a dummy indicating whether the locality was identified as such by *Ministère des Mines (2008)* due to a high DPI value and population in the catchment area. The Development Potential Index (DPI) is a composite measure of development indicators (see Web Appx. Table B.1 and estimated for all localities using available data. The Minimum Spanning Tree (MST) is calculated using the proposed 33kV poles. “Dev. Indicators” refers to additional indicators including the square root of the number of primary school students and dummy variables for administrative capitals (levels 2 & 3). All regressions include region fixed effects. Standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1.

Table 2
Average treatment effects of electrification.

	Mean (T = -1)	Baseline		Controls		Pre-Trends (p-value)		Nt/N
		ATT	se	ATT	se	Baseline	Controls	
Panel A: Outcome variables								
1. Luminosity, Ext. Margin	0.04	0.395***	(0.026)	0.394***	(0.029)	0.320	0.145	1842/13 329
2. Luminosity, Int. Margin	0.02	0.376***	(0.089)	0.498***	(0.071)	0.551	0.527	1124/6766
3. New grid-Powered AEPS/PEA water systems	0.00	0.020***	(0.005)	0.023***	(0.005)	0.939	0.911	1842/17 135
4. N electrified schools †	0.22	0.127**	(0.064)	0.139**	(0.071)	0.252	0.203	440/5353
5. N students in public primary schools †	52.08	-3.482	(2.809)	-0.054	(3.273)	0.171	0.382	440/5353
6. Annualized population growth (%)	-1.88	4.269	(3.522)	4.261	(3.542)	0.657	0.387	366/1899
Panel B: Testing for simultaneous schooling investments								
1. N public primary schools †	0.75	-0.029	(0.036)	0.003	(0.042)	0.419	0.657	440/5353
2. N latrines †	1.32	0.149**	(0.071)	0.012	(0.076)	0.746	0.397	440/5353
3. N wall Maps †	3.37	0.140	(0.199)	0.218	(0.218)	0.168	0.265	440/5353
Panel C: Testing for coordinated water investments								
1. New manual water pumps	0.45	-0.016	(0.042)	0.003	(0.323)	0.323	0.316	1842/17 135
2. New solar-Powered AEPS/PEA water systems	0.02	-0.011	(0.009)	-0.006	(0.320)	0.320	0.142	1842/17 135

Notes: This table reports Average Treatment Effects on the Treated (ATT), balanced with respect to event-time for $T = [0, 6]$, and for $T = [0, 3]$ for variables marked with a dagger †), using the imputation estimator of *Borusyak et al. (2024)*. Panel A presents key outcome variables, while Panel B and C cover broader investments in school and water infrastructure. For each variable, we estimate two models: i) a “Baseline” model with locality and year fixed effects; and ii) a “Controls” model adding development-pole-by-year fixed effects and time-invariant controls (as measured in 2007) interacted with year dummies. These controls include the square root of primary school enrollment in 2007 and the predicted development potential index. In Panel B, the number of wall maps serves as a representative indicator for teaching materials. Results for additional teaching materials are provided in Web Appx. Table B.3. Panel C reports new installations of hand- and foot-operated pumps as indicated by the pump trade marks India, India Mark II, Vergnet, ABI, DIAFA, Kardia, and Volanta. The test for parallel pre-trends is conducted by regressing the outcome on dummies for periods $T = -6, \dots, -1$ using only non-treated observations. This corresponds to a cluster-robust Wald test, assessing whether pre-treatment trends are statistically different from zero. The final column reports the number of treated units (Nt) across all horizons and the total number of observations (N) used in the estimation.

effects are larger than in the full sample (Table 3); this could be either due to effect heterogeneity across localities, or due to the different time structure in this reduced sample, where we observe more localities longer after electrification. The coefficients for radio ownership and access to piped water are positive and of similar magnitudes as in the main estimation, but they are not statistically significant. The IV estimates show very large and significant effects for electricity access,

radio, and television ownership. The Montiel–Pflueger F-statistics are reasonably large, suggesting that the instrument is not weak.

An important advantage of the IV strategy is that it only requires cross-sectional data, allowing us to expand the analysis to data that were not collected in the earlier survey rounds. Table 6 shows results for additional household level variables available only for 2010 and/or 2014. Just as before, localities electrified in 2008–2009 are

Table 3
Household asset ownership - Average treatment effects (0–6 years after electrification).

Dependent variable	Total effect		Spillover effect		Pre-trend p-value		Mean Dep t=-1	N
	Levels	Trends	Levels	Trends	Levels	Trends		
Controls in:								
1. Electricity	0.10*** (0.02)	0.10*** (0.02)			0.39	0.29	0.00	6082
2. Radio	0.10*** (0.03)	0.06* (0.03)	0.10*** (0.03)	0.06* (0.03)	0.26	0.45	0.54	6085
3. Television	0.04** (0.02)	0.04** (0.02)	-0.02 (0.01)	-0.02 (0.01)	0.68	0.87	0.11	6085
4. Refrigerator	0.01** (0.01)	0.01*** (0.01)	0.00 (0.00)	0.00 (0.00)	0.75	0.65	0.00	6084
5. Piped Water	0.05** (0.02)	0.04* (0.03)	0.03 (0.02)	0.03 (0.03)	0.93	0.93	0.05	6086

Notes: This table shows the average treatment effects (ATT) based on [Borusyak et al. \(2024\)](#) for the time frame $T = [0, 6]$. Unit of observation is the household, unit of treatment is the locality; there are 201 localities electrified between 2008 and 2017. Each cell represents a separate estimation model and shows the average treatment effect of electrification on the dependent variable. The columns entitled “Total” calculate the average treatment effects for all households, while the columns entitled “Spillover” calculate the average treatment effects only for households who do not have access to electricity. The dependent variables are all binary, taking the value 1 if the household owns (at least one of) the item. “Piped water” equals one if the household has access to piped water either privately in the household or publicly in the vicinity (i.e. a public standpipe). The community controls are all significant variables from column 5 of [Table 1](#). They are included either in levels or in levels and trends (as specified in the column header). Household controls comprise the sex and age of the household head (plus age squared), as well as the share of adult women in the household who have any primary or secondary education, respectively. Pre-trend p-values refer to the F-test for joint significance of the 6 periods preceding treatment. Standard errors, clustered at the locality level, in parentheses. See [Web Data Appendix](#) for data sources.

*** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$.

the treatment group and localities electrified in 2015–2017 represent the comparison group. OLS results are provided as a benchmark. The instrument is again the log distance to the minimum distance spanning tree (MST). Panel A of [Table 5](#) shows the treatment effects for the full sample, while Panel B shows the spillover effects on non-electrified households. We find a large increase in the ownership of mobile phones, regardless of whether the household has an electric connection. This is what one expects if community electrification improves the opportunities for mobile phone charging for everyone. Importantly, we also find a large increase in the share of households that have a bank account. We cautiously interpret this as an improvement in financial inclusion. Mobile phones have the potential to give individuals in remote areas with limited infrastructure access to financial services ([Mothobi and Grzybowski, 2017](#)). The FinScope Burkina Faso Consumer Survey (2016) found that only 40% of adults have access to services from any kind of formal financial institution such as banks and other regulated organizations, 21% rely purely on informal financial services, and 39% are completely financially excluded ([FinMark Trust, 2016](#)). Access in our rural and peri-urban sample is significantly lower still. Part of the problem is the physical inaccessibility of financial service points. The vast majority of adults do not know where the nearest bank, ATM, or microfinance institute is and do not use them. These service points tend to be only accessible by car or motorbike, particularly in rural areas. In contrast, mobile money agents are more easily accessible and tend to be in walking distance. Indeed, 30% of adults used mobile money services in 2016 ([FinMark Trust, 2016](#)). Thus, increased access to mobile phones through community electrification has the potential to improve financial inclusion, particularly for individuals that do not have physical access to traditional financial service points.

6. Discussion and conclusion

In this paper we studied Burkina Faso’s grid extension. The rich and detailed information on planning helped us to shed light on likely sources of endogeneity. The government explicitly targeted more developed localities in terms of their own population, their catchment area and existing private and public infrastructures. Grid supply followed assumed demand. This is probably a typical setting in developing countries. It would let us expect positively biased estimates of the impact when comparing electrified against non-electrified localities.

We argued that the timing of electrification is more exogenous, because grid expansion is bound by engineering considerations. Expanding the grid means that localities closer to the existing network will be connected earlier. Costs are reduced by minimizing total network length. We showed that those engineering variables can predict the timing of electrification well, not only trumping but rendering economic variables insignificant. This motivated us to choose not-yet electrified localities as comparison group and a staggered DiD approach as preferred identification strategy. As a robustness check, we also employ an instrumental variable strategy based explicitly on these engineering considerations.

Our evidence draws a more positive picture of the effectiveness of electrification. In line with much of the literature ([Burlig and Preonas, 2024](#); [Min et al., 2013](#); [Min and Gaba, 2014](#); [Dugoua et al., 2018](#)), we found a significant increase in luminosity after a community got connected to the grid. If, as argued by [Min et al. \(2013\)](#), luminosity reflects mostly street lights, our results suggest significant public investment into street lighting. Of course, luminosity may also reflect private economic activity, household wealth and GDP more broadly ([Henderson et al., 2012](#); [Hodler and Raschky, 2014](#)). In fact, our finding of increases in the household ownership of certain assets underlines this possibility. We found positive effects on public goods that are complementary to electricity. The arrival of the grid enabled the electrification of schools and grid-powered drinking water systems.

However, we also found a number of non-results contradicting claims of the transformative power of electrification. We did not find significant effects on enrollment rates, children’s nutrition and health status, possibly because health inputs did not improve substantially either. Indeed, analyzing various dimensions of public investments, we find it unlikely that electrification was bundled with other development expenditures.

Our findings highlight the importance of considering the extensive margin (the “arrival of the grid”) rather than attempts to increase the electricity uptake of households in already electrified communities (the intensive margin). This is particularly important when positive spillovers exist at the community level. In our context, this was the case. With the arrival of the grid, more households own electric appliances that require electricity such as radios, TVs, refrigerators and mobile phones. Some of these effects also extend to households that do not have their own electricity connection. We find particularly

Table 4
Community effects, Alternative identification strategies.

Treatment group: Community Electrified 2008–2009								
Control group: Community Electrified 2010–2014								
Pre-Treatment period Post-Treatment period	2002–2007 2010–2014			None 2010–2014				
	DiD: BJS	DiD: TWFE	Nt/N	OLS	Reduced form	IV: 2SLS	Montiel–Pflueger F	N
1. Luminosity, Extensive margin	0.267*** (0.036)	0.396*** (0.046)	324/3267	0.348*** (0.053)	−0.084*** (0.030)	0.455*** (0.158)	18.96	1376
2. Luminosity, Intensive margin	0.214** (0.106)	0.511*** (0.086)	240/1577	0.299* (0.160)	−0.181** (0.092)	0.959* (0.575)	10.83	665
3. New grid-Powered AEPS/PEA water systems	0.030** (0.013)	0.022 (0.017)	324/3285	0.025* (0.014)	−0.008 (0.006)	0.042 (0.036)	19.07	1380
4. N electrified schools †	0.245*** (0.090)	0.164 (0.128)	216/2733	0.259*** (0.098)	−0.102** (0.045)	0.551** (0.266)	18.89	828
5. N students in public primary schools †	−4.105 (5.468)	−7.481 (5.304)	216/2733	−4.085 (7.072)	−3.368 (3.346)	18.172 (19.143)	18.89	828
6. Annualized population growth (%)	−	−	−	1.004 (1.233)	−0.739 (0.499)	4.010 (2.625)	17.71	272

Notes: Standard errors in parentheses, clustered at the locality level. Column (DiD: BJS) reports Average Treatment Effects on the Treated (ATT) for $T = [0, 6]$ (and $T = [0, 3]$ for those variables marked with a dagger †), using the imputation estimator of [Borusyak et al. \(2024\)](#). Region fixed effects are used instead of locality fixed effects due to sample size constraints. Column (2) reports the canonical TWFE estimator. Column (3) reports the number of treated units (Nt) across all horizons and the total number of observations (N) used in the estimation of DiD BJS. Column (4) reports baseline estimates. Column (5) reports the reduced form estimates using log distance to planned grid poles as instrumental variable when controlling for log distance to development poles and the 33 kV distribution network of 2007 as controls. Column (6) reports 2SLS estimates. Column (7) reports F-statistics for weak instruments according to Kleibergen–Paap and [Montiel Olea and Pflueger \(2013\)](#). The final column reports number of observations in estimations columns (4)–(6). See Web Data Appendix for data sources.

*** p<0.01, ** p<0.05, * p<0.1.

Table 5
Effects of community electrification on household asset ownership, Alternative estimation strategies.

Treatment group: Community Electrified 2008–2009							
Control group: Community Electrified 2015–2017							
Pre-treatment period Post-treatment period	1993 & 2003 2010 & 2014		None 2010 & 2014			Mean (pre)	
Dependent variable	DiD: BJS	DiD: TWFE	OLS	Reduced Form	IV: 2SLS	Montiel–Pflueger F	
1. Electricity	0.17*** (0.04)	0.20*** (0.04)	0.20*** (0.04)	−0.05* (0.03)	0.21** (0.11)	16.38	0.01
2. Radio	0.05 (0.05)	0.08 (0.05)	0.06 (0.06)	−0.07*** (0.02)	0.31** (0.12)	16.40	0.52
3. Television	0.09*** (0.03)	0.09** (0.04)	0.10*** (0.03)	−0.04** (0.02)	0.18* (0.09)	16.38	0.03
4. Refrigerator	0.03** (0.01)	0.03** (0.01)	0.01 (0.01)	−0.01 (0.01)	0.02 (0.03)	16.38	0.00
5. Piped Water	0.05 (0.04)	0.05 (0.06)	−0.03 (0.04)	0.03 (0.02)	−0.13 (0.09)	16.40	0.05
Number of households (N)	3232	3232	1866	1866	1866		
Number of localities (T/C)	101 (26/75)	101 (26/75)	69 (17/52)	69 (17/52)	69 (17/52)		

Notes: Unit of analysis is the household, unit of treatment is the locality. The first two columns show difference in difference estimates based on a reduced sample of observations, where we consider only localities electrified pre 2010 or post 2014. This sample creates a 2×2 matrix that allows for a classic two-way fixed effects (TWFE) DiD model. We show results using BJS estimation ([Borusyak et al., 2024](#)) on this reduced sample, alongside the TWFE results. The next three columns show estimates based on cross-sectional data from 2010 and 2014: ordinary least squares, and IV estimates including reduced form and two-stage least squares (2SLS). The last two columns report F-statistics testing for weak instruments, according to [Montiel Olea and Pflueger \(2013\)](#), and the mean of the dependent variable in the pre-treatment period. All estimations control for community-level characteristics (pre-treatment) and region fixed effects. Household-level controls are not included, as these would not be valid for the IV specification (and models should be directly comparable). Standard errors, clustered at the locality level, in parentheses. See Web Data Appendix for data sources.

*** p<0.01, ** p<0.05, * p<0.1.

strong increases in the ownership of mobile phones and radios, both of which are typically battery operated and do not require a constant electric connection. We did not find spillovers in the ownership of TVs and refrigerators, which require a constant source of power. This is suggestive of the possibility that electricity is sold on in the community, with connected households giving temporary access to their neighbors. Indeed, in the 2016 FinScope Survey over a third of adults who own a mobile phone reported charging it at a shop or kiosk close by and 8% reported charging it at a neighbor’s house. [Peters and Sievert \(2016\)](#) find that across rural settings of sub-Saharan Africa, the most common

income-generating use of household electricity consists of providing services to the local community, such as mobile phone charging or “cinemas”. “Similarly, [Lenz et al. \(2017\)](#) observe that in Rwanda, grid-connected households often rent out mobile phones to display films or football matches for a fee, while those without connections save on batteries, kerosene, and mobile phone charging costs by benefiting from informal sharing in electrified areas.

We also demonstrated that electrification increases the proportion of households with bank accounts, including for unconnected households. Financial exclusion remains a large problem in Burkina Faso.

Table 6
Household access to mobile phones and banking - Cross-Sectional estimation of the effect of community electrification.

Cross-Sectional analysis using data from 2010, 2014 Treatment group: Community Electrified 2008–2009 Control group: Community Electrified 2015–2017						
Panel A: Total effects						
Dependent variable	OLS	Reduced form	IV: 2SLS	Montiel–Pflueger F	Mean (Control)	N
1. Mobile phone	0.03 (0.04)	−0.04*** (0.01)	0.19** (0.09)	16.04	0.67	1862
2. Bank account	0.06* (0.03)	−0.04* (0.02)	0.16** (0.09)	15.93	0.08	1845
Panel B: Spillover effects						
Dependent variable	OLS	Reduced form	IV: 2SLS	Montiel–Pflueger F	Mean (Control)	N
1. Mobile phone	0.00 (0.04)	−0.04*** (0.01)	0.18* (0.10)	12.01	0.67	1744
2. Bank account	0.02 (0.04)	−0.04** (0.02)	0.18** (0.09)	11.85	0.08	1730

Notes: Unit of analysis is the household, unit of treatment is the locality. OLS, reduced form, and 2SLS estimations are based on 69 localities, 17 of which are in the treatment group. The dependent variables are all binary, taking the value 1 if the household owns at least one of the item. All estimations control for household and community-level controls and region fixed effects. The last three columns report F-statistics testing for weak instruments (Montiel Olea and Pflueger, 2013), the mean of the dependent variable in the control group, and the number of observations (households). Standard errors, clustered at the locality level, in parentheses. See Web Data Appendix for data sources.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

As mobile money services have been expanding rapidly (N'dri and Kakinaka, 2020), ICTs become more important (Karakara and Osabuohien, 2019). The fact that mobile phone ownership and bank accounts simultaneously expand in electrified localities can be seen in this light.

We found suggestive evidence that electrified localities may have outgrown not-yet electrified localities in terms of population. Most likely, this result stems from migration from never-electrified localities (outside our sample). People probably will not move to essentially similar localities in order to benefit from a slightly earlier date of electrification. We would rule out natural growth. First, we did not find large measurable effects on child mortality. Second, we would expect, if at all, a negative effect of electrification on fertility (Akpanjar and Kitchens, 2017; Grimm et al., 2015; Grogan, 2016).

Overall, this paper has demonstrated that there is an important community dimension to the benefits of electrification, comprising both the provision of public goods, as well as spillover effects from electrified households to the community. The community dimension is well recognized for other infrastructure investments, such as roads and water provision, but the literature studying the welfare effects of electrification has so far focused mainly on the private benefits of household electric connections at the intensive margin. An accurate assessment of the welfare effects of grid extension needs to take into account public goods at the community level and spillover effects.

CRediT authorship contribution statement

Maika Schmidt: Conceptualization, Data curation, Formal analysis, Investigation, Methodology, Software, Validation, Visualization, Writing – original draft, Writing – review & editing. **Alexander Moradi:** Conceptualization, Data curation, Formal analysis, Funding acquisition, Investigation, Methodology, Resources, Software, Supervision, Validation, Visualization, Writing – original draft, Writing – review & editing.

Appendix A. Supplementary materials

Supplementary material related to this article can be found online at <https://doi.org/10.1016/j.jdeveco.2025.103556>.

Data availability

Replication package: Harvard Dataverse (DOI: <https://doi.org/10.7910/DVN/WOORX>).

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