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(En-)'lightening' children: Assessing the impacts of access to electricity on learning achievement levels

Somdeep Chatterjee[1] | **Shiv Hastawala**[2] | **Jai Kamal**[3]

[1]Indian Institute of Management Calcutta, Kolkata, India

[2]Binghamton University – SUNY, Binghamton, New York, USA

[3]Indian Institute of Management Jammu, Old University Campus, Jammu, India

Correspondence
Somdeep Chatterjee, Indian Institute of Management Calcutta, Diamond Harbor Road, Joka, Kolkata 700104, India.
Email: somdeep@iimcal.ac.in

Abstract

The welfare impacts of electrification are well documented in the literature, including the effects of electricity on school enrolment. However, the spillover effects of electrification on children's achievement levels are scarce. We use three complementary but distinct econometric models to establish a causal relationship between electrification and test scores using nationally representative household panel data from India. We find positive results irrespective of the choice of econometric model, and these results seem to be mediated by changing time-use patterns of children with access to electricity. We first exploit the plausibly exogenous variation in access to electricity due to a universal electrification program in the state of West Bengal in India and we find positive effects of electrification on children's test scores. By age group, we find that younger cohorts benefit more in terms of their reading scores than older cohorts. Then, to ascertain external validity of these results, we replicate them over a nationally representative sample using fixed effects and instrumental variables estimation and find similar results. At the intensive margin, we find that access to more hours of electricity positively affects test scores. We identify an increase in time spent by children on study-related activities as the potential channel for these results.

KEYWORDS
education economics, rural electrification, test scores

JEL CLASSIFICATION
I25, I28

1 | INTRODUCTION

An important strategy for economic development is upgrading the infrastructure in the power and energy sector owing to its positive effects on industrialization and productivity (Rud, 2012). There is ample empirical evidence which suggests that access to electricity leads to higher household income (Bridge et al., 2016; Hasan & Mozumder, 2017), provides indirect health benefits (Barron & Torero, 2017) and has potential labour market consequences (Dasso & Fernandez, 2015; Dinkelman, 2011). Keeping in mind the sustainable development goals of the United Nations, one of which aims to provide access to modern energy sources to all by the year 2030, a lot of developing economies have ambitiously revised their universal electrification programs. Yet, the relevant literature suggests that there are no convincing causal estimates of the positive impacts of electricity infrastructure on learning achievement levels of children (Glewwe et al., 2011).

While some studies do find that access to electricity leads to higher educational attainment (Kumar & Rauniyar, 2018; Lipscomb et al., 2013) and investments in human capital (Barron & Torero, 2014), attempts to estimate impacts on learning have largely suggested null effects (Seo, 2017) or perverse effects (Dasso et al., 2015). This paper contributes to this literature by providing consistent and robust evidence of a positive impact of electricity access on standardized test scores of children, using a quasi-experimental study and an external validity analysis, utilizing two different datasets.

We provide novel and, to the best of our knowledge, the first evidence of positive impacts of electricity access on test scores, contrary to the existing evidence in the literature. These effects appear to be mediated through changes in time-use patterns of children with access to electricity. Given that electricity is a modern source of energy as compared to traditional sources such as fuelwood, time spent by children in collecting fuel seems to be lower with access to electricity. Also, time spent on studying and doing homework seems to be higher, suggesting potential substitution of effort from fuel collection to education reflecting in increased learning levels. We provide evidence on this using data on household time-use patterns.

We use test scores as an outcome variable because there is evidence that test scores are good predictors of long-term economic outcomes. In a sample of NLSY children, log male earnings increased by around 0.17 log points for every standard deviation difference in test scores (Neal & Johnson, 1996). The impact grows greater over time, as demonstrated by Carneiro et al. (2005) who add additional years of observation. Rivera-Batiz (1992) finds that arithmetic test scores raise the likelihood of full-time employment. High-school GPA is also a strong predictor of later education and earnings (French et al., 2015). And using longitudinal data, Rose (2006) finds that math scores raise both employment status and earnings. In the context of the United Kingdom, Watts (2020) finds that math and reading scores at age 16 raise individuals' earnings 17–34 years later in life. Swedish data also shows that cognitive ability scores are strong predictors of wages and earnings. But such associations are not limited to developed countries alone. A study from Mexico as well shows that primary-, middle-, and high-school test scores are strong predictors of future education and labour market outcomes (De Hoyos et al., 2018). All the evidence points towards a strong and positive correlation between test scores and later life outcomes. Hence, test scores are an appropriate economic outcome to look at from the human capital point of view.

There is both theory and evidence for why electricity access is expected to contribute to better academic performance. According to the Economic Survey 2019–20, Indian schools' access to contemporary teaching methods and strategies facilitated by electricity availability aids students' overall development and boosts their interest in studying. It states that reliable power connections are necessary for access to Information and Communication Technologies (ICT) and the teaching of computer skills in schools, and it also shows that states with lower school electricity usage correlate with lower rates of literacy (GOI, 2020). In Papua New Guinea, power supply is rarely provided for primary and elementary school instructors, which causes hardship for both the families of teachers sent to these institutions and the instructors themselves. However, Cabraal et al. (2005) suggest that rural communities with access to home electricity may be able to recruit more capable educators, and via this route, electricity might indirectly contribute to better local educational outcomes.

Additionally, one of the key advantages of electricity is that it provides far more high quality light than kerosene lamps. Adults and children alike may read in this favourable environment in the evening, making it easier for them to continue their academic pursuits (Barnes et al., 2002). To bolster this claim, children turned out to be the biggest beneficiaries of the dissemination of solar home systems to the Ugandan poor and they utilized the solar lighting to finish their schoolwork at night (Bamanyaki & Harsdorff, 2009). In rural Nigeria, having access to household electricity affects education by allowing for lighting that can be used for reading, leisure, and entertainment, and by changing how children spend their free time (Olanrele et al., 2020). In the context of other African countries[1] where constrained lighting was ranked as the main barrier to learning and doing homework, school children switched from studying 1.7 h per night on average to 3.1 h per night on average before and after buying solar lights, respectively (Harrison et al., 2016). Related to these findings, Olanrele et al. (2020) develop a conceptual framework under which lighting from electricity access leads to increased time devoted to studying and reduced time spent collecting fuel for lighting. Evidence for this theory can be seen in some causal studies across developing countries[2] which find that electricity access raises children's time spent studying at home (Aguirre, 2017; Barron & Torero, 2014; Khandkar et al., 2012) and reduces their time spent on fuel collection (Khandkar et al., 2012). Another possible mechanism comes from the context of Kenya, wherein Ye (2017) suggests that the spillover effect of each household cluster's electrification on nearby ones in terms of raised schooling years could be because of children's increased drive and learning from one another. Thus, one would expect all such electricity access-generated human capital investments made by children to lead to better test scores.

There are two ways in which our findings are unique. First, the majority of the existing studies focus on school level electrification. An absence of an impact on test scores for such interventions should therefore be viewed as potential *inelasticities* in learning outcomes with respect to school-level infrastructure. In our study, the focus is on household and village level electrification, which attempts to capture the effects of household/non-school level inputs in the education production function. Given that the literature already establishes increases in educational attainment with access to electricity (Khandker et al., 2014), this may imply that harnessing the complementarities in household inputs are critical for this increased attainment to translate into higher achievements, as opposed to an exclusive focus on school-level inputs.

Second, the few studies that estimate the impacts of household electrification on educational outcomes do not usually report results on learning outcomes (Agoramoorthy & Hsu, 2009; Khandker et al., 2014). Furukawa (2014) links test scores and household access to solar lamps to find negative impacts. Although it is potentially exploiting the channel of household-level complementarity of inputs, the experimental design targeted only about 200 students. We, on the other hand, study the access to electricity from a universal electrification drive, potentially affecting millions of households. Additionally, our external validity analysis focuses on a more massive scale using a large nationally representative household level panel dataset from India covering over 40,000 households. As a result, our findings have important policy implications in terms of replicability.

Our main hypothesis is that electricity access improves learning outcomes (achievement levels). In order to test this hypothesis, we first perform a quasi-experimental analysis using policy variation from an Indian state. Specifically, we study a universal electrification drive undertaken by the government of West Bengal, known as *Sabar Ghare Alo* (SGA), which was introduced in 2012. We exploit variation in access to this program generated by the timing and the institutional features of the policy, which targeted to achieve universal electrification in 11 socio-economically disadvantaged districts out of a total 19 at the time of implementation.[3] Using a complementary dataset with richer outcome variables over a longer span of time, we find that this program led to increases in reading test scores of children. However, we do not find any conclusive evidence of an impact on math test scores. We compare the chosen districts to the other districts of West Bengal, before and after policy implementation. Most of our estimates are intent-to-treat (ITT) effects of the intervention in a pure reduced-form framework. We use test score data from the Annual Status of Education Report (ASER) surveys and find significant correlations at the village and household level between access to electricity and test scores.

Second, to ascertain external validity of the results from the quasi-experimental study, we use data from the two rounds of the India Human Development Survey (IHDS) to estimate the relationship between electricity access and test scores. After factoring in both time and village fixed effects, we find positive impacts of access to electricity on reading and math test scores. We use several individual and household-level controls including demographic variables in these regressions, especially the ones that are likely to be correlated with test scores as well as access to electricity. Despite these efforts, one cannot guarantee the conditional exogeneity of the indicator of access to electricity, which is our main independent variable of interest. Therefore, following Bai et al. (2017), we employ an instrumental variables strategy using the aggregate village-level electrification rate excluding the household in question as an instrument for access to electricity for estimating the same effect. We find largely consistent and robust results suggesting that the basic inference, regarding positive impacts of electricity access on learning, is fairly general and not a spurious association. The findings of the external validity analysis are in line with the main results of the paper suggesting that access to electricity does lead to improved learning levels in children.

2 | BACKGROUND

Rural electrification has been one of the key mandates of the Government of India. Both the central government and state governments have directed efforts to increase access to electricity. Various central government programs such as Accelerated Rural Electrification Program, Rajiv

Gandhi Grameen Vidhutikaran Yojna, and Deen Dayal Upadhayaya Gram Jyoti Yojna have tried addressing the issues of electrification access and reliability of supply (Rathi & Vermaak, 2018).

Historically the condition of electricity supply has been poor. State Electrification boards (SEBs), owing to inadequate financial resources, could not provide reliable services that resulted in low electrification rates. As a result, in April 1998, the Central Electricity Commission (CERC) and the State Electricity Regulatory Commissions (SERCs) were set up to monitor the SEBs and help them by setting a reasonable tariff and by solving interstate exchange of electricity (Khandker et al., 2014). Thus, administrative reforms, coupled with a sequence of electrification programs, have apparently improved electricity access and electricity supply in India.

As of 29th April 2018, India has reportedly achieved 100% electrification when electricity reached each village.[4] According to the definition, a village is said to be electrified if 10% of the households and all the public buildings are electrified.[5] Though all the villages are officially electrified, the government's primary challenge is to increase take-up at the household level. One possible reason for the low take-up rate could be an unreliable electricity supply, frequent outages, and poor services. Also, low-income households can find it challenging to pay electricity bills, and hence they often opt-out of getting connected. Moreover, there is evidence that access is further constrained for households with *political elites* leveraging their personal networks to get preferential access (Chatterjee & Pal, 2021).

Given that villages are under-serviced even today, both the direct and spillover benefits of electricity are essential from a policy perspective. Electrification drives in rural areas are relevant from the poverty alleviation perspective as the emphasis on policies that enhance support for rural development has significant impact on poverty reduction (Imai et al., 2017). Electricity access has many potential benefits at both the macro and micro levels. At the macro level, electricity access is positively associated with GDP (Chen et al., 2007; Narayan & Singh, 2007; Ozturk, 2010) and productivity, firm performance, and employment (Dinkelman, 2011; Gibson & Olivia, 2010). At the micro-level, it affects healthcare (Lenz et al., 2017), income (Parikh et al., 2015), fuel choice (Heltberg, 2004), and education (Khandker et al., 2014). As documented by Khandker et al. (2014), electrification affects educational outcomes via increasing school enrolment by about 6% for boys and 7.4% for girls. Also, the average completed schooling years increase by about 0.3 and 0.5 for boys and girls, respectively. Electrification increases uptake of cleaner fuel which further leads to better health outcomes such as improved lung capacity and educational outcomes such as higher attendance and more years of schooling (Biswas & Das, 2022; Silwal & McKay, 2015).

While an increase in enrolment is a good indicator of education, it does not give a clear picture of children's learning. According to the Annual Status of Education Reports (ASER) reports, there is a significant mismatch in enrolment rates and test scores. Thus, our main research interest is the extent to which access to electricity affects test scores. Essentially, we are interested to test if access to electricity can lead to better learning levels in children, which would provide evidence of positive spillovers of programs promoting household level electrification and rationalize such policy efforts.

3 | QUASI-EXPERIMENTAL STUDY

3.1 | Sabar Ghare Alo: A state-level electrification program

In order to find out the effects of electricity access on children's test scores, we study a specific electrification program in one of the states in India. Sarba Griha Deep Prakalpa (Sabar Ghare

Alo), which literally translates to 'universal electrification of households', was a scheme launched by the Government of West Bengal that aims to achieve 100% household electrification in 11 out of the 19 districts in the state. Expected beneficiaries of the scheme were estimated to be 28,214 villages, 2.1 million BPL (Below Poverty Line) households, and 1.7 million APL (above poverty line) households.[6]

Execution of the project began in July 2012 and was likely to be finished within a couple of years.[7,8] Towards this end, the then Planning Commission (now NITI Aayog) of the Government of India had approved the proposal with an estimated cost of 25.11 billion INR as Central Assistance to the State Government under the Backward Regions Grant Fund (BRGF) in December 2012.[9,10] The implementation of the scheme was to be carried out by the West Bengal State Electricity Distribution Company Limited (WBSEDCL) through installation of 33/11 KV substations in the districts. The scheme was expected to provide relief to people who had been suffering from low voltage and deficient power supply, and to buttress the local economy. For instance, the government reports claimed that for the Paschim Medinipur district in particular, the scheme was expected to positively affect agriculture as well as small and medium industries in terms of output and expansion.[11]

Since the government identified the 11 potentially disadvantaged districts for project implementation, it gives us potential identifying variation to do an impact evaluation of this reform. Figure 1 shows the various West Bengal districts by SGA status. The 11 identified districts to administer SGA are marked in orange, the rest are marked in yellow. Given the time period of implementation from 2012, the program would have taken approximately 2 years to have completed, as per government estimates. Therefore, we compare outcomes in these socio-economically backward regions to outcomes in other areas, before and after 2013, allowing for reasonable penetration time for the program.

As per the 2011 census, the literacy rate in West Bengal was 76% which is almost similar to the national average of 74%. West Bengal is the 4th most populous state in India with Uttar Pradesh (67% literacy), Maharashtra (83% literacy) and Bihar (61% literacy) ahead of it. Given the closeness of the state's literacy rate to the national average and constituting almost 8% of the country's population, West Bengal, in general provides an interesting sample to study the effects of electrification on education in the Indian context.

3.2 | Data

We use the Annual Survey of Education Report (ASER) as our data source, which is a survey carried out by the ASER Centre every year to assess the status of education in rural India. These repeated cross-sectional surveys are conducted across the nation and are representative at the state level. The sample comprises 20 villages from each of the 580 rural districts of India, and anywhere between 20 and 30 randomly selected households from every village. The surveys cover rural children between the age of 3 and 16 who are either enrolled in school, have never been to school, or have dropped out of school. Out of these, the surveys test children between the age of 5 and 16 on math and reading skills. Because these tests are administered at the subject's home rather than at their school, it helps to assess achievement scores aside from contribution by school level inputs. These tests are conducted in the subject's local language and evaluate them in terms of levels of learning by asking them 4 questions on each test. The reading test judges whether they can recognize a letter, a word, can read a grade 1 text, and a grade 2 text. The math test judges whether they can recognize single digit numbers, double digit

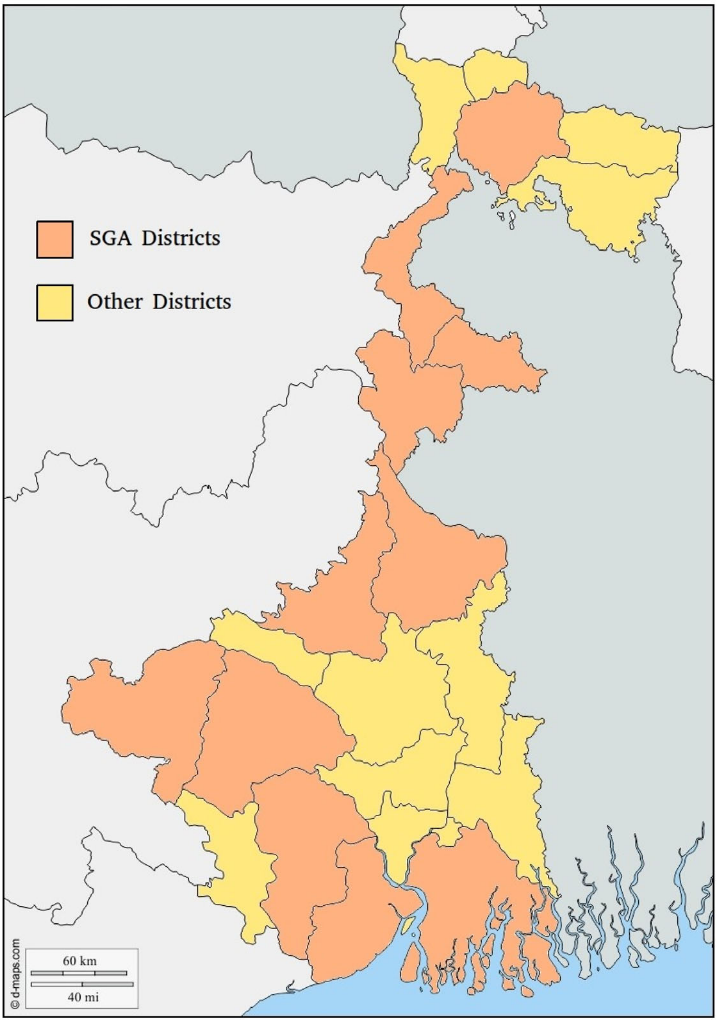


FIGURE 1 SGA and non-SGA districts in West Bengal. [Colour figure can be viewed at wileyonlinelibrary.com]

numbers, can carry out two-digit subtraction with carry-over, and three digit by one digit division. The child is graded at the highest level that they can solve. The score ranges from 0 to 4, where 0 means the child could not solve the most basic question while 4 means that they could solve the highest level question. For our analysis, we use test scores in the standardized form. ASER provided us with their survey data for the years 2007–2014, 2016, and 2018.

We use two variables as the outcomes of interest, namely the reading score and the math score. The main independent variables considered are electricity access at the village level and at the household level. Simple linear regressions of the outcomes on the independent variables show positive and significant results for both these outcomes. The controls used for this purpose include mother's education, that is, the grade up to which the subject's mother had attended school, the mother's age, family size, that is, number of members in the household, the child's gender, and the child's age. However, the independent variables related to electricity access might be endogenous to the model, meaning that they might not be affecting just the outcomes but also correlated with the error term due to issues of selection. To address this potential endogeneity, we use the rural electrification policy from West Bengal as a source of possible exogenous variation. Intuitively, this policy should have increased the subjects' access to electricity at the village as well as the household level.

3.3 | Estimation and main results

3.3.1 | Correlations of test scores with access to electricity

One way to check if electricity access leads to better test scores is to run regressions of test scores on a dummy variable for access to electricity. We run the following specification for each individual i in district d for year t the sample using the 2007–2016 ASER datasets as described above:

$$TestScore_{idt} = \alpha_d + \beta \cdot ElectricityAccess_{it} + \gamma_1 \cdot X_{it} + \epsilon_{it} \tag{1}$$

In Equation (1), α_d represents district fixed-effects and controls for time invariant district specific characteristics that may confound the estimates. *ElectricityAccess* is a dummy variable representing whether the child has access to electricity. We use two versions of this. First, we use a dummy for access to electricity in the household. Second, we look at the effects of access to electricity in the village where the child resides. *TestScore* represents the standardized reading or math test scores for child i in time t. Demographic controls represented by X include age of the child, the mother's education, family size, the child's gender, and the mother's age. The coefficient β gives us the difference in mean test scores between children who have access to electricity and those who do not. However, these point estimates should be viewed as just correlations since the assumptions required to establish causal links are very stringent. For β to be a causal effect of electricity access on test scores, the access to electricity must be exogenous, that is, uncorrelated with the error term ϵ. It is quite possible that test scores of children are higher in areas that are better developed which are also the areas more likely to have electricity access, making β a biased estimate of the true effect.

That said, correlations are still useful as descriptive figures and as a starting point because they provide a sense of which way the effect might go. Even if one does not believe the magnitude of the effects, it may still provide an idea about the direction of causality. An idea about whether test scores in general are higher with electricity access, even without causal connotations, can still be an interesting policy statistic. We find that these basic correlations are not only suggestive of positive effects of electricity access on test scores but are also precisely estimated as reported in the Online Appendices in Table A1.

We find that test scores for children are positively correlated with access to electricity. Columns 1 and 2 from Table A1 suggest that the average child with access to household electricity has a higher reading test score by 0.102 σ compared to the one without access. The average child who has electricity in his village scores 0.030 σ points higher in reading than one without access to electricity in the village. Similarly, the effects for math test scores are reported in Columns 3 and 4 which range from 0.088 σ for household access to 0.020 σ for village level access. All the estimates are statistically significant at the 99% level of confidence. However, one cannot claim that these higher scores are particularly because of access to electricity due to reasons described above. It is quite possible that children in the electrified villages are intrinsically different from those in other villages. In the following subsection, we present a framework that may help alleviate concerns about such endogeneity.

3.3.2 | The *Sabar Ghare Alo* natural experiment

As discussed above, the rural electrification endeavour in West Bengal, that is, the *Sabar Ghare Alo* (SGA) program provides variation in timing and access to electricity driven by policy implementation. The program was introduced in 2012 and 11 backward districts were identified for universal electrification. Therefore, this setting provides an ideal 'natural experiment' where the 11 districts can be considered as *treated* units and we can compare the means of outcomes in these districts to other ones which can be considered as *control* units. However, since the choice of these districts was not random, it is not exactly a pure experiment implying that the simple mean comparisons are unlikely to avoid issues of selection bias. Luckily, ASER provides data for pre-2012 years as well and this allows us to run a difference-in-difference framework comparing treated and control districts before and after the policy. The identification assumption is that in the absence of the electrification intervention, the differences in outcomes of the treated and control districts would not be any different post-2012 compared to the trend differences pre-2012. We provide some suggestive evidence in favour of this evidence later.

We propose to run the following regression specification:

$$TestScore_{idt} = \alpha_d + \alpha_t + \beta_1 \cdot SGA_{idt} + \beta_2 \cdot After_t + \theta \cdot X_{it} + \upsilon_{it} \tag{2}$$

In Equation (2), α_d represents district fixed effects as before and α_t represents year fixed effects. The dummy variable $After_t$ takes the value 1 if the year is greater than 2012 to capture the post-policy time period. The dummy variable SGA_{idt} takes the value 1 if individual i lives in a treated district and the time period is post-2012. For all other cases, it takes the value zero. Essentially, it is an interaction of the $After_t$ dummy with a dummy that would identify a treated district. However, since we use district fixed effects, the latter dummy gets subsumed in the main equation. The interpretation is that θ is similar to γ from Equation (1) and we continue using the same demographic control variables as above.

Under the identifying assumption discussed earlier, β_1 gives the causal estimate of the SGA program on test scores of children. Consider the counterfactual, that is, the absence of the SGA program. The above methodology implies that in the counterfactual this β_1 would be statistically indistinguishable from zero. Therefore, any statistically significant estimate of β_1 should in the presence of the policy imply an impact of SGA, under this assumption about the counterfactual. Table 1 presents the results from these regressions.

We find that the impact of access to electricity, as proxied by exposure to SGA, on test scores is positive. Columns 1 and 5 in Table 1 present results from the regression Equation (2) excluding the demographic controls. Therefore, the estimated coefficients are the simple difference-in-differences of the outcomes, after accounting for district fixed effects. All regressions include district fixed effects only. Column 1 suggests a 0.059 σ point pre- to post-policy increase in reading test scores between the average child in treated and control districts after controlling for time invariant district specific effects. However, we find that this difference-in-differences for math scores in Column 5 and beyond is close to 0 and statistically insignificant. We also

TABLE 1 The effect of the SGA program on standardized test scores.

	Reading score				Math score			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SGA effect	0.059*** (0.013)	0.052*** (0.011)	0.067*** (0.0138)	0.051*** (0.011)	0.000 (0.013)	−0.004 (0.011)	−0.0004 (0.014)	−0.0056 (0.011)
R^2	0.03	0.39	0.40	0.39	0.03	0.38	0.39	0.38
Demographic controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Village-level controls	No	No	Yes	Yes	No	No	Yes	Yes
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	No	No	No	Yes	No	No	No	Yes
Observations	106,910	99,137	53,162	99,137	106,395	98,664	52,846	98,664

Note: The sample consists of all surveyed households in West Bengal using publicly available ASER data from 2007 to 18. All columns report results from different regressions. All test scores are in standardized form. Demographic controls include child age and gender, mother's age, mother's education, and family size. Village-level controls include indicators for the presence of an all-weather road, a post office, a ration shop, a bank, a government primary school, a government middle school, a government secondary school, and a private school. Robust standard errors reported in parentheses.

*$p < .1$.

**$p < .05$.

***$p < .01$.

include combinations of demographic controls, village controls, and year fixed effects in columns 2–4 and 6–8 and find that the essence of the results does not change much.

The almost null effect on math scores that we observe could indicate towards the possibility that lighting from electricity alone may not be enough to raise math test scores since math is a cognitively challenging subject. The learning process in math depends on a host of inputs (Bottoms & Carpenter, 2003; Demir et al., 2009; Kiwanuka & Damme, 2015; Shin et al., 2009), especially teaching methods and teacher competency (Saritas & Akdemir, 2009). Indeed, there is causal evidence that specific classroom teaching methods raise math test scores while others do not (Cordero & Gil-Izquierdo, 2018; Lavy, 2011). However, data limitations do not allow us to test this hypothesis empirically.

3.3.3 | Which children gain the most?

Overall, we found that exposure to the SGA program potentially led to upwards of a 0.05 σ increase in reading test scores of children. A natural question to ask here is whether all children are affected in the same way. Are there age-specific gainers and losers from this policy? Our sample includes all children in West Bengal surveyed by ASER from 2007 to 2018.

The ASER sample asks the same questions to evaluate cognitive abilities to children in the age group of 5–16. Since this is a wide bracket of ages, we breakdown our sample into smaller groups and run the same regressions as above. Figure 2 plots the estimated coefficients for impacts of SGA on reading test scores for different cohorts in the sample. Since effects on math scores are imprecise, we focus our analysis only on reading scores for this section.

We find that the effects on reading test scores seem to be most prominent for the younger children. Those in the age group of 5–6 years, who are yet to enter primary school, seem to be gaining as much as children who have just entered primary school, that is, aged 7 to 8 years. However, the effects do not seem to be significant for older children. Apparently, the children not exposed to SGA are not much worse off compared to exposed children in the age group of 9–12 years while there seems to be no difference at all for 13–16 year old children.

One possible explanation for such effects would be the presence of diminishing returns to the inputs in the education production process. A younger child from a disadvantaged district is more likely to record higher marginal gains in basic reading ability when provided with some nudge through provision of better inputs. However, as children get older, their natural ability of reading is likely to develop. For such children the same nudge is unlikely to produce large marginal gains, which may be a reason why the magnitude of the effects converge to zero in Figure 2 with increasing age of the children.

3.3.4 | Does access to electricity actually improve with SGA exposure?

To be able to convince ourselves that the estimated effects above are indeed operating through the channel of electricity access and are not some pure reduced form general equilibrium effects of SGA working like a standard anti-poverty program, we must check if access to electricity actually improved due to SGA. Since the main hypothesis of this paper is to estimate the effects of electricity access on test scores of children, such an exercise can be thought of as the first stage of a 2SLS estimation paradigm, where SGA is an instrument for access to electricity.

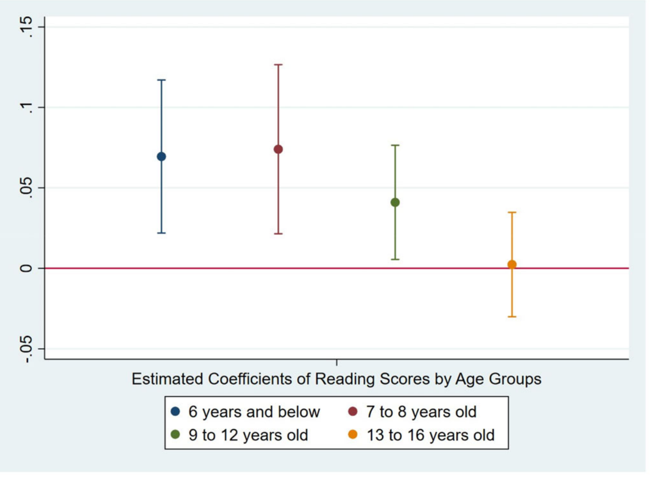


FIGURE 2 Impact of SGA on standardized reading scores by age group. All points represent coefficients from different regressions of reading test scores and include demographic controls and district fixed effects. Vertical lines represent 95% confidence intervals. [Colour figure can be viewed at wileyonlinelibrary.com]

However, we refrain from reporting 2SLS estimates as such because of two main concerns. First, the model is likely to be underidentified given that we believe that the SGA effects work through either village or household level electrification, or potentially both. Second, if it operates through both these channels, then clearly the exclusion restriction is not satisfied. Therefore, out of abundance of caution, we take the more conservative approach and base our analysis on the ITT estimates reported above. Consequently, this section is focussed on identifying potential mediating channels for the ITT effects, and not the first stage regressions, per se.

We run a simple difference-in-difference specification as follows:

$$ElectricityAccess_{idt} = \omega_0 + \omega_1 \cdot After_t + \omega_2 \cdot (Treat_d * After_t) + \alpha_t + \mu_{it} \tag{3}$$

where $Treat_d$ is a dummy indicating treated districts, $After_t$ is a dummy for post-policy time period as before, α_t are year fixed effects, and ω_2 is the difference-in-difference estimate of impact of SGA on electricity access at village or household level, as the case maybe. We are able to execute this regression because ASER contains information on access to electricity at the household and village level of the individual i in district d for year t. However, in the absence of general district level observables or other variables that may affect the presence of electricity infrastructure, we cannot include additional controls. Household or individual demographic controls are not meaningful for these regressions. To try and avoid confounders, we include district fixed effects in variations of the above regression, to at least account for time invariant characteristics of districts. Results are reported in the Online Appendices in Table A2.[12]

We find that households were more than 8% likelier to have access to electricity with exposure to SGA as reported in column 1 of Table A2. Controlling for unobserved district specific characteristics raises this number to upwards of 9% which is a substantial increase in electricity access potentially caused by the SGA intervention. At the village level, these numbers are a little smaller. Roughly about 6.5% more villages were electrified due to exposure to SGA as per recorded responses of the surveyed households. This is unsurprising because the SGA program aimed to achieve rural electrification in the target districts earmarked for the policy. The policy statements claimed to have affected a majority of the villages in these districts and as a result, the village electrification effects are positive.

3.3.5 | Parallel trends assumption

The main identification strategy used above would yield causal estimates of SGA on outcomes of interest under the assumption of common pre-trends as discussed. The underlying identification assumption deals with the unobserved counterfactual that estimated difference in-differences would have been zero had there been no intervention. Since this is not testable, we attempt to see if the trends in the relevant outcome for the pre-policy years are similar for the treated and control groups. As a result, we take district-year averages of test scores and represent them in Figure 3 by treatment status.

The figure suggests that trends in the main outcome of interest, namely, reading test scores averaged for districts, seem to be parallel over the years. It is also not surprising that in terms of levels, the treated districts are mostly below the control districts as treated districts were identified in a way such that the most backward ones are earmarked for the program. It is reassuring to see that immediately after the policy year, marked by the vertical line in the figure, the outcome for the treated district shows a spike up, which is perhaps the effect we pick up in our analysis. This initial spike leads to a permanent increase in the levels for the treated districts and although the slopes eventually become parallel, the relative difference between treated and control groups is narrower post-policy.[13] Also, it is worth noting that the gap between control and treated districts widened in 2008 and came down in 2009, widening again in 2010. Since, these are raw trends, one needs to interpret these with caution. As a second best, therefore, we propose to supplement our analysis using a falsification exercise as follows.

3.3.6 | Falsification exercise

Despite checking for the parallel trends assumption, there may still be concerns regarding the results above. Is the research design picking up effects of unobserved confounders that are not accounted for? Are the results sound enough to be claimed as causal? To test for this, we conduct a falsification exercise. A falsification test looks for a correlation that should be absent if the research design is sound. Thus, falsification testing may be viewed as a tool for ensuring the validity of a study conclusion and, more broadly, strengthening causal inference. Hence, we run a falsification exercise on the pre-treatment duration of the SGA program using a different dataset. Since the SGA program effects are expected to be seen only after 2012 when it was introduced, we do not expect to see any statistically significant effects of it in the treatment districts before the program was rolled out.

In this subsection, we use the India Human Development Survey (IHDS) data set. We provide finer details on the IHDS in the next section but just as a snapshot, the IHDS is a nationally-representative household survey with two rounds carried out in 2004–05 and 2011–12 (we make use of data from both the rounds). Since both the rounds of the IHDS were conducted before the SGA was rolled out, we do not expect to see any effects of the program on test scores. As before, the first source of variation comes from the treatment versus control districts

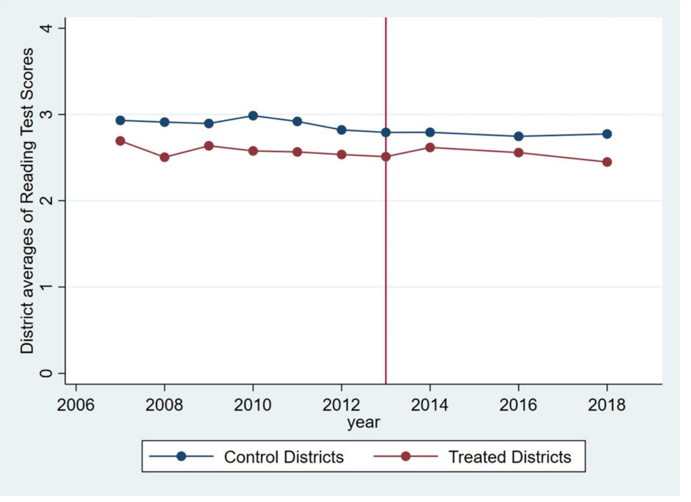


FIGURE 3 Parallel trends in reading test scores over the years by treatment status. [Colour figure can be viewed at wileyonlinelibrary.com]

in West Bengal but the second source of variation comes from considering the first round of IHDS as pre-treatment and the second round as post-treatment. The results are reported in the Online Appendices in Table A3. None of the results in this table are statistically significant, which gives us more confidence in our previous results pointing towards causal inference.[14]

4 | EXTERNAL VALIDITY

From the previous section, we know that the SGA program had a positive effect on children's test scores. But do these results hold up outside of the state of West Bengal? In other words, would we still see similar results in a context different from that of SGA so that we may claim that the results are externally valid? Furthermore, if that is indeed the case, what are the potential mechanisms via which electricity access raises children's test scores? In this section, we use a nationally representative dataset to ascertain external validity using information on nationwide household-level access to electricity. Here, we discuss the data sources and summary statistics, and describe our empirical models—pooled OLS with fixed effects and instrumental variables.

Based on households' electrification status, we divide them into two groups—those who have access to electricity and those who do not. Our primary interest lies in assessing the impact of the household's electrification status on children's test scores. First, we estimate this after controlling for fixed effects such as time fixed effects and village fixed effects. And then, we use the village electrification rate excluding the household in question as an instrument for the electrification status to estimate the effect of electrification on test scores. Irrespective of the method, we find a strong and positive effect of access to electricity on test scores. We then move on to testing household-level factors (household income and fuel collection time) and individual-level factors (time spent on study-related activities) that may be potential mechanisms driving our main result of a positive effect of electricity access on test scores. Lastly, we conduct a bunch of robustness checks to make sure that our results are not sensitive to choice of specification.

4.1 | Data

We use the India Human Development Survey (IHDS) for this part of our analysis (The survey was conducted in two rounds, where the first-round (IHDS-I) was conducted in 2004–05, and the second one (IHDS-II) was conducted in 2011–12. We have used both rounds of the survey. The survey contains data such as demographic, socio-economic, and educational outcome at both the individual and household levels. The survey comprises rich data on our key variable of interest, that is, children's test scores on reading, math and writing.

IHDS is a nationally representative survey of 41,554 households conducted in 2004–2005.

The initial IHDS sample consists of 26,734 rural and 14,820 urban households. Out of the 593 districts in India in 2001, 384 are included in IHDS. The sample is spread across 1503 villages and 971 urban blocks. IHDS-II re-interviewed 83% of the original households as well as split households residing within the village and an additional sample of 2134 households. The final sample size for IHDS-II is 42,152 households; 27,579 rural and 14,573 urban. These households are spread across 33 states and union territories, 384 districts, 1420 villages, and 1042 urban blocks.

The descriptive statistics of key variables from the data are shown in the Online Appendices in Table A4. The scoring system of IHDS-I had four-levels for reading and three-levels for math. A reading score of 0 means the child cannot read, one means the child can read letters, two means the child can read words, three means the child can read sentences, and a score four means the child can read a story. Similarly, a math score of 0 means the child cannot recognize numbers, one means the child can recognize numbers, two means the child can perform subtraction, and three means the child can perform division. The second survey round of IHDS has a similar scoring system. For our purposes, we use standardized test scores for the most part. The age group of the children tested in the IHDS data-set is 8–11 years old.

Table A4 in the Online Appendices suggests that all test scores of IHDS-I are higher than that of IHDS-II. Similarly, all the test scores of those children who have access to electricity are higher than those who do not have access to electricity. As expected, in line with the electrification efforts of the Government of India, the household electrification rate has risen from 78% in 2004–05 to 88% in 2011–12.[15]

4.2 | Estimation and results

4.2.1 | Pooled OLS with fixed effects

Identifying the causal effect of electrification on test scores of children is challenging because of potential endogeneity of access to electricity. As alluded to in the quasi-experimental study above, the standard OLS approach to estimate this effect would not be sufficient because there may be some time-invariant unobservable characteristics which confound the estimates. In addition, we may expect the presence of village-level unobservable characteristics that can bias our causal estimation further. We therefore propose to estimate a pooled OLS model with village fixed-effects and time fixed-effects to alleviate concerns about these specific unobservables as represented by Equation (4). We further use a host of controls and propose to identify the effects under the assumption of conditional exogeneity of our main variable of interest.

$$TestScore_{ijvt} = \beta_0 + \beta_1 \cdot HasElectricity_{ijvt} + \beta_2 \cdot X_{it} + \beta_3 \cdot X_{jt} + \alpha_t + \alpha_v + \epsilon_{ijvt}, \tag{4}$$

where $HasElectricity_{ijvt}$ is a dummy variable which takes the value '1' if an individual i belonging to household j from village v has access to electricity in survey year t, otherwise it takes the value '0'. $TestScore_{ijvt}$ is the standardized test score obtained by child i on an assessment. X_{it} and X_{jt} are individual and household controls, respectively from survey year t. Household controls include highest education level among all household members, number of children in the household, number of individuals in the household, and whether the household belongs to an urban area. Individual-level controls include age, gender, and marital status. α_t and α_v are time and village fixed effects, respectively. Our parameter of interest is β_1, which captures the effect of having access to electricity on test scores.

The results of pooled OLS model with fixed-effects are presented in Table 2. The first column of the table reports the estimates without any fixed effects, the second column reports results only with year fixed effects, and the third column reports results with both village and time fixed effects. The results from our most preferred specification are reported in the last column, and the main estimates are highlighted in bold. As shown in the first panel of the table, the effect of electrification on reading scores is positive, and the result can be interpreted as the average reading test scores of the children with electricity access being 0.251 σ higher than the children with no electricity access. The direction of this result is coherent with the quasi-experimental study in the previous section. Similarly, we find a statistically significant and positive effect on math scores too.

4.2.2 | Instrumental variables estimation

To re-assure ourselves of the fact that the results from the previous analysis are not weak in terms of their predictive power, we propose another strategy. Following the framework in Bai et al. (2017), we use the village electrification rate excluding the household the individual being surveyed belongs to as an instrument for electricity access. Such IVs have been widely used in the literature to capture the unobserved heterogeneity in infrastructure placements (Dang & La, 2019; Rao, 2013; Sedai, 2021, Sedai et al., 2020; Sedai et al., 2021; Vanaja, 2018). For constructing this IV, the number of households in a village that have access to electricity minus the household in question is divided by the total number of households in that village. Theoretically, there are two conditions that any instrument must satisfy. First is the relevance condition which requires the instrument to be strongly correlated with the main variable of interest, that is, regional aggregation of electricity access is a strong predictor of household access. Second is the exclusion restriction which requires the instrument to not be correlated with the error term. In other words, this means that the instrument can affect the outcome variable only via the original variable (electricity access) and not through any other channel, that is, village-level electrification affects children's test scores only via electricity access at the child's household. In the standard instrumental variable regression setup, this entire procedure is performed in two stages, more commonly known as two-stage least squares (2SLS), something that we perform shortly.

The idea behind the IV is that of social networks and peer effects in the context of demand for electricity: if neighbours in the village obtain electricity and reap the economic and social benefits of electricity access, then having no electricity for one's own household may indicate a lower socioeconomic position. Hence, electricity availability in neighbouring households is expected to raise one's own electricity access, which means the relevance condition is satisfied. Additionally, the instrument's exogeneity requirement also holds since there is no apparent

TABLE 2 Pooled OLS with fixed effects.

	No FE	Year FE	Both FE	Both FE
Panel A: Reading				
Has electricity	0.570***	0.579***	0.360***	**0.251*****
	(0.023)	(0.023)	(0.026)	(0.025)
R-squared	0.054	0.055	0.273	0.317
N	24,197	24,197	24,197	24,180
Panel B: Math				
Has electricity	0.557***	0.566***	0.313***	**0.200*****
	(0.023)	(0.023)	(0.024)	(0.023)
R-squared	0.051	0.052	0.297	0.344
N	24,098	24,098	24,098	24,081
Year FE	No	Yes	Yes	Yes
Village FE	No	No	Yes	Yes
Controls	No	No	No	Yes

Note: The sample consists of all households surveyed in either rounds of IHDS. Each column represents a separate regression. The last column, also the most preferred specification, uses control variables. Controls used in the above regressions are—age, gender, marital status, highest education of any adult member of household, number of children, household size and residential status (urban/rural). Panel A reports results for reading scores and Panel B reports results for math scores. Test scores are standardized. Standard errors are clustered at the village level and are reported in parentheses.
*$p < .1$.
**$p < .05$.
***$p < .01$.

reason to believe that electricity availability in neighbouring households could have a direct impact on children's test scores in one's own household. On the other hand, we expect that households' own electricity access and the availability of other infrastructures has an impact on individual test scores in that household. In other words, the education production function accepts infrastructure inputs from the child's own household and not from that of the neighbours. Thus, excluding the child's own household from the instrument is vital to the exclusion restriction.

The regression equations for both the stages are as follows:

$$HasElectricity_{ijvt} = \theta_0 + \theta_1 \cdot VillElecRate_{-vt} + \theta_2 \cdot X_{it} + \theta_3 \cdot X_{jt} + \alpha_t + \epsilon_{ijvt} \ (\textbf{1st Stage}) \tag{5}$$

$$TestScore_{ijvt} = \psi_0 + \psi_1 \cdot \widehat{HasElectricity}_{ijvt} + \psi_2 \cdot X_{it} + \psi_3 \cdot X_{jt} + \alpha_t + \epsilon_{ijvt} \ (\textbf{2nd Stage}) \tag{6}$$

Here, $HasElectricity_{ijvt}$ is a dummy variable which takes the value '1' if an individual i belonging to household j from village v has access to electricity in survey year t and it takes the value '0' otherwise. $\widehat{HasElectricity}_{ijvt}$ is the predicted value of electricity access. $VillElecRate_{-vt}$ is the electrification rate in the village v (excluding household j that individual i belongs to) in the survey year t. $TestScore_{ijvt}$ is the test standardized score obtained by child i on an assessment. X_{it} and X_{jt} are individual and household controls from survey year t, while α_t represents year fixed effects. Household controls include highest education level among all household members, number of children in the household, number of individuals in the household, and whether the household belongs to an urban area. Individual level controls include age, gender, and marital status. Our parameter of interest is ψ_1 from Equation (6), which captures the effect of access to electricity on test scores.

First, we run the regression from Equation (5) and predict the probability of a household having electricity access (the first stage *F*-statistic for reading test scores is 17,958.56 with a *p*-value of .0000 and that for math test scores is 17,905.89 with a *p*-value of .0000). Using these predicted values, we then run the second stage regression from Equation (6). Results of the second stage regression are reported in Table 3. These results are in line with the previous method used for the estimation of the effect of electricity access on test scores. The interpretation of the results is similar to the pooled OLS method. Children with access to electricity have 0.455 and 0.393 standard deviation higher test scores in reading and math, respectively. The Sargan-Hansen *J*-statistic for the second stage regressions is 0.000, which means that the regression equation is exactly identified. We also include district fixed effects and report the results in Table A11 of the Online Appendix and find that the results are very similar.

4.3 | Potential mechanisms

At this point, we have ample evidence to support the claim that electricity access raises children's test scores. But what exactly is it about electricity access that makes children score better? In other words, what is the story behind our findings? In this subsection, we explore the potential channels through which the effect of electricity access on test scores can be explained. We first look at household level factors that can explain the positive effect of electricity access on test scores and then at individual level factors that may provide an alternative explanation. It turns out that in the first case, the spillover effect of household income is the major contributing component. Apart from such household level variables, the IHDS dataset also contains information on a rather interesting individual level variable time spent by boys and girls in the household on collecting fuel. The reason why this variable is interesting is because we believe that, one, fuel-collection time can instead be spent on studying if one does not have to go out looking for fuel, and two, this time expenditure should be lower for households with electricity access. And indeed, our results suggest that the children with electricity access spent less time collecting fuel and more on activities related to academics. Lastly, we identify school enrolment as one of the potential channels.

4.3.1 | Household income and fuel collection time

The results reported in column 1 of Panel A of Table 4 suggest that the average annual income of households having access to electricity is 12,271 rupees more that of the households without electricity. The literature suggests that children from high income households perform better on test scores in general (Dahl & Lochner, 2012). Columns 2 and 3 of Table 4 suggest that weekly time spent on collecting fuel is lower for both boys and girls from households with electricity access. Thus, these results suggest that children save some time because access to electricity reduces the need for alternative fuels (Khandker et al., 2014).

TABLE 3 Instrumental variable.

	Reading	Math
Predicted electricity access	0.455***	0.393***
	(0.024)	(0.024)
R-squared	0.152	0.167
N	24,180	24,081

Note: Both the regressions include year fixed effects. The reported results are second stage results. The first stage *F*-statistic for reading test scores is 17,958.56 with a *p*-value of .0000 and that for math test scores is 17,905.89 with a *p*-value of .0000. The second stage Sargan-Hansen *J*-statistic is 0.0000 for both the regressions. The sample consists of all households surveyed in either round of IHDS. Each column represents a separate regression for each outcome variable. Controls used in the above regressions are—age, gender, marital status, highest education of any adult member of household, number of children, household size and residential status (urban/rural). Standard errors are clustered at the village level and are reported in parentheses.

*$p < .1$.

**$p < .05$.

***$p < .01$.

We expect some part of the extra time saved by not going for fuel collection to be spent on educational activities. One concern is that households without electricity access could be using clean fuels for cooking purposes and this could lead to biased estimates. However, the likelihood of access to clean fuels lowers with ownership of electricity-operated assets like refrigerator and TV (Ali & Khan, 2022). Moreover, based on our calculations from the ACCESS 2016 data set (Aklin et al., 2016), 94.39% of Indian households without access to electricity do not use clean fuels as their primary resource for the purpose of cooking. This gives us confidence in our results.[16]

4.3.2 | Time spent on educational activities

Results from Panel B of Table 4 suggest that time spent on educational activities is higher for children with electricity access than those without. As is evident, the results for both home-work and private tutoring is positive and significant. However, the results for time spent in school is not statistically significant. One possible reason could be that schooling time is exogenously decided by school authorities and the electricity access at home hardly matters in that case. The positive effect of electricity access on the time spent doing home-work explains the improvement in test scores. This is consistent with the idea that more time spent studying improves test scores (Gettinger, 1985; Maltese et al., 2012). Also, the positive effect on the duration of private tutoring adds more clarity to the potential increase in test scores. Literature suggests that the private tutoring is prevalent in India and increases the achievement scores (Azam, 2016; Chatterjee et al., 2020). Thus, increased time on studying can be one potential channel through which electricity access impacts the learning outcomes.

4.3.3 | School enrolment

We find improvement in school enrolment as one of the potential channels for better test scores. School Enrolment and test scores are associated with each other in many settings

TABLE 4 Potential mechanisms.

Panel A: Household-level factors			
		Fuel collection time	
	Household income	**Boys**	**Girls**
Has electricity	12.271***	−10.453	−5.444
	(1.826)	(7.090)	(4.706)
R-squared	0.372	0.257	0.244
N	19,806	7391	7804
Panel B: Individual-level factors			
	Time spent on		
	Schooling	**Home work**	**Private tutoring**
Has electricity	−0.072	0.616***	0.542***
	(0.226)	(0.137)	(0.109)
R-squared	0.374	0.316	0.375
N	23,166	23,128	22,197

Note: The sample consists of all households surveyed in either round of IHDS. Each column represents a separate regression for different outcome variables. Household income is measured in thousands and fuel collection time is measured in minutes. Time spent on educational activities—schooling, home work, and private tutoring—is measured in hours. Controls used in the above regressions are—age, gender, marital status, highest education of any adult member of household, number of children, household size and residential status (urban/rural). Both year fixed effects and village fixed effects are used in the regression specification. The reason for a drop in the observations for fuel collection is the presence of missing values in the IHDS dataset. Standard errors are clustered at the village level and are reported in parentheses.

*$p < .1$.

**$p < .05$.

***$p < .01$.

(Filmer & Schady, 2009; Kremer et al., 2009). An improvement in school enrolment rate may not necessarily result in better test scores but improvement in test scores could be attributed to better school enrolment rates. The extensive literature on this line of enquiry finds a positive relationship between school enrolment and test scores. For instance, Vermeersch and Kremer (2005) find positive effect of school meal program on both test scores and enrolment. The results for school enrolment as a potential mechanism in case of West Bengal are reported in Table 5.

4.4 | Robustness checks

In this subsection, we report results for additional robustness checks to further support the initial results of this section. First, we report results for both rounds of the IHDS survey separately to resolve concerns regarding different scoring rubrics used in both the rounds. Second, we use different measures of test scores. Third, we report results for intensive margins and finally, we report results for a randomization placebo test.

TABLE 5 School enrolment a potential channel.

	School enrolment	
SGA effect	0.025***	0.027***
	(0.005)	(0.004)
R-squared	0.012	0.203
Controls	No	Yes
N	163,388	131,439

Note: The sample consists of all surveyed households in West Bengal using publicly available ASER data from 2007 to 18. All columns report results from different regressions. All test scores are in standardized form. Demographic controls include child age and gender, mother's age, mother's education, and family size. Robust standard errors reported in parentheses.
*$p < .1$.
**$p < .05$.
***$p < .01$.

4.4.1 | Separate results for IHDS rounds

One concern may arise regarding the reliability of the estimates because the assessment tools such as the questions used and the scoring system of both the rounds of IHDS were slightly different. To address this, we conduct our analysis separately for each round. Results for our most preferred specification are shown in the Online Appendices in Table A5. It is clear from the table that the results for IHDS-I and IHDS-II are positive and significant and hence the main coefficient of interest, that is, impact of electricity access on test scores is consistent across survey years.

To mitigate any concerns that may persist because of measurement issues, we use alternative measurements for the main outcome of interest, that is, test scores. Because the test scores use grading rubrics such that they are ordinal variables rather than being continuous, we first estimate a separate regression for each test score level. Then, we use standardized test scores in the form of z-scores so that the different grading rubrics can be compared. Finally, we use the Angrist-Levy Index for test scores.

4.4.2 | Test score measurement: Ordinality of test scores

We estimate a separate regression for each test score level. There are five levels for the reading scores and four levels for the math scores. To address concerns related to ordinality of the data, the results are reported in the Online Appendices in Tables A6 and A7. All the coefficients are significant and positive, which is consistent with our main estimates.

4.4.3 | Test score measurement: Actual test scores

There are five levels for the reading scores and four levels for the math scores. The results are reported in the Online Appendices in Table A8. Similar to our main estimates, they are positive and significant. In terms of effect sizes, children with access to electricity have higher reading and math scores by 0.334 and 0.206, respectively. Please note that because of the scoring schemes of the test scores[17] being dissimilar, there is a difference in means of different test scores. As far as interpretation is concerned, for instance, the results related to the math score can be interpreted as the average test scores of the children with electricity access are 0.206 higher than the children with no electricity access. Given the relatively small mean of the math score, this effect is comparable with the reading score. In terms of effect sizes, children with electricity access have around 13% higher reading scores and 13% higher math scores when compared to those who do not have access to electricity.

4.4.4 | Test score measurement: Angrist-Levy Index

Angrist and Lavy (1997) suggested an alternative way of measuring test scores. In order to generate the Angrist-Levy Index, we first obtain standardized test scores and then assign the index a value of 0 if the standardized test score is 0, a value of 1 if it is less than or equal to one half, and 2 otherwise (Chakraborty & Jayaraman, 2019). In line with our main estimates, the results are positive and significant, as shown in the Online Appendices in Table A9.

4.4.5 | Intensive margin effects

So far, we have focused on the impact of electricity connection status on test scores, and we find that having access to electricity helps increase test scores. Now, we seek to answer a similar and more interesting question of the impact of electricity hours on test scores in order to provide more details on how this relationship works. Merely having an electricity connection does not necessarily mean that households would be able to leverage all the benefits of electricity. They require an adequate electricity supply to reap the actual benefits of electricity access. There are two major problems faced by households in this respect—lack of supply and unreliable supply.

Reliability of electricity is a concern where the supply of electricity is adequate. An irregular pattern of electricity can significantly hamper children's plans for studying at home. In our case, a sudden electricity cut might shift the focus of children from studies to something else like a household chore, and thus we would expect those children to not do well in terms of academics, resulting in lower test scores. However, due to unavailability of data, we are not able to test the impact of the unreliability of electricity on test scores. Thus, we test for the effect of total electricity hours.

A more severe problem as compared to reliability is the shortage of supply. Power shortage is a very serious problem in several areas of India (Allcott et al., 2016). We hypothesize that differences in electricity supply lead to differences in children's test scores. We test the impact of hours of electricity using the following regression:

$$TestScore_{ijvt} = \gamma_0 + \gamma_1 \cdot HoursofElectricity_{ijvt} + \gamma_2 \cdot X_{it} + \gamma_3 \cdot X_{jt} + \alpha_t + \epsilon_{ijvt}, \tag{7}$$

where $HoursofElectricity_{ijvt}$ is the actual number of hours electricity supply is available to the household. All the other coefficients are the same as in Equation (4). Please note that unlike our main estimates, we are not using village-level fixed effects because there is not enough variation in electricity supply hours for different households in the same village.

The results in Table 6 can be interpreted as test scores increasing by 0.01 σ with a 1 h increase in electricity supply. In line with our expectations, our results suggest that the children with more hours of electricity supply perform better on both the test scores.

TABLE 6 Intensive margin effects.

	Reading	Math
Hours of electricity	0.010***	0.004**
	(0.001)	(0.001)
Mean score	2.585	1.1545
R-squared	0.119	0.089
N	19,119	18,923

Note: The sample consists of all households surveyed in either rounds of IHDS-I and IHDS-II. Each column represents a separate regression for different test scores. First column reports results for reading scores, second column reports results for math scores and the last column reports results for writing scores. Controls used in the above regressions are—age, gender, marital status, highest education of any adult member of household, number of children, household size and residential status (urban/rural). Year fixed effects are used in the regression specification. Standard errors are clustered at village level and are reported in parentheses.

*$p < .1$.

$p < .05$.*$p < .01$.

From a policy perspective, it is essential to understand the effect of hourly access in detail.

A different and easy way to look at the relationship is to categorize households based on access to electricity. We divide households into four groups based on hours of electricity access (0–6 h, 6–12 h, 12–18 h, and 18–24 h). We estimate the following equation where electricity access is measured using a vector of four dummy variables, each representing households from a different group.

$$TestScore_{ijvt} = \delta_0 + \delta_1 \cdot HoursDummy_{ijvt} + \delta_2 \cdot X_{it} + \delta_3 \cdot X_{jt} + \alpha_t + \epsilon_{ijvt}, \tag{8}$$

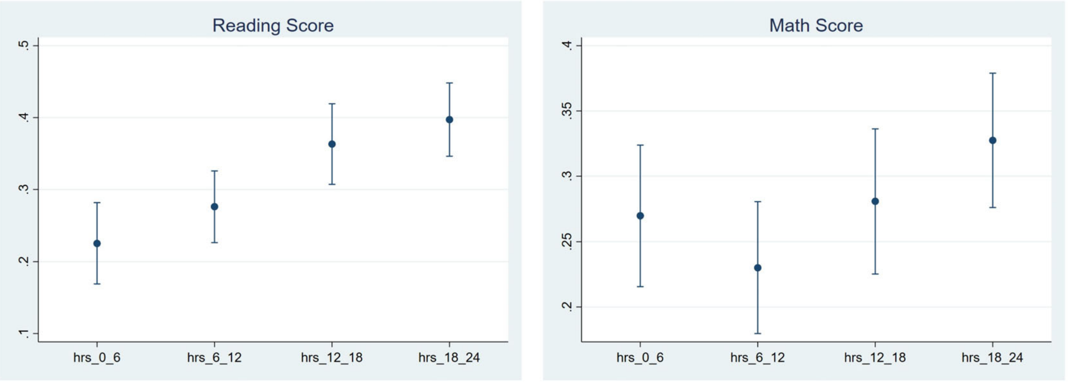
where $HoursDummy_{ijvt}$ is a vector of four dummy variables representing the actual hours of electricity supply for the household. All other coefficients are the same as in Equation (4). Please note that similar to specification 7, we are not using village-level fixed effects because there is not enough variation in the electricity supply hours for different households in the same village.

Figure 4 shows that the children belonging to higher access to electricity class have higher test scores. Also, the point estimates for each test score increase as we move from one class to a higher class, which shows that we do not observe diminishing results of higher electricity access.

4.4.6 | Test of randomization

Following Bharadwaj et al. (2014), we conduct a placebo test in which we randomly assign the electricity connection status to households instead of their actual status and estimate the regression Equation (4). We assign a 'fake' electricity connection status, and then perform 500 simulations. We conduct a simulation exercise for each outcome variable separately, and we plot all 500 coefficients for each outcome separately. If our identification strategy picks up the correct effects, we expect most of the coefficients in the simulated regressions to be insignificant, and we also expect the magnitudes to be very small.

In Figure 5, we plot the coefficients obtained from the random simulation for both the outcomes. As is evident from the figure, the coefficients are centred around zero, and the magnitude of the coefficients is much smaller than our actual estimates. We observe similar results



Note: No village fixed effects (There is not enough variation within village). Test Scores are standardized.

FIGURE 4 Effect of electricity hours on test scores. No village fixed effects (there is not enough variation within village). Test scores are standardized. [Colour figure can be viewed at wileyonlinelibrary.com]

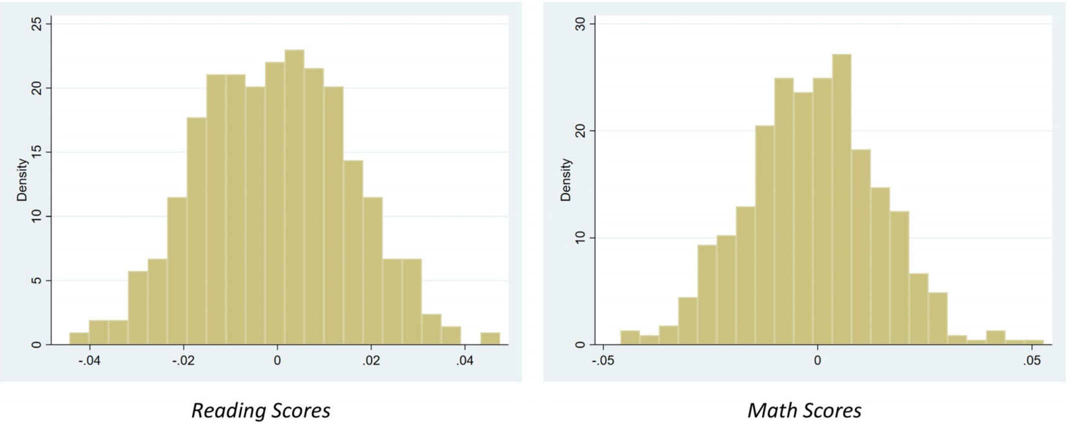


FIGURE 5 Test of randomization. [Colour figure can be viewed at wileyonlinelibrary.com]

for both the test scores, which gives us confidence that our identification strategy is picking up the true causal estimates.

5 | CONCLUDING REMARKS

This paper has explored the effect of access to electricity on children's test scores. To overcome the potential challenges of causal estimation such as selection bias and unobserved factors, we conduct two mini-studies. The first study consists of exploiting plausibly exogenous variation in electricity access generated by a rural electrification project from the Indian state of West Bengal using a difference-in-differences estimation design. Then, to ascertain external validity of the results obtained before, we attempt to replicate them over a nationally representative sample using fixed effects and instrumental variables estimation.

The results indicate that children with electricity access have higher reading and math test scores. In the case of the quasi-experimental study, we find that children with electricity access have 0.052 σ points higher reading scores when compared to those who do not have access to electricity but the effect on math scores is imprecise, and that these numbers for the external validity analysis are 0.251 σ and 0.200 σ, respectively. The results from both the mini-studies are similar. These results are statistically significant and are not sensitive to specifications as well. Various robustness exercises give us confidence in our results.

A major implication of these findings is in terms of human resource development policy formulation by governments in countries like India. A significant focus in similar developing countries is given to improving educational attainment through demand-side interventions such as conditional cash transfers or supply-side interventions such as improving schooling infrastructure. Despite such efforts, the fact remains that even though in most developing countries in general, and in India in particular, enrolment and participation rates in terms of attendance improve among children, learning levels either remain unaffected or are perversely affected. Our research suggests that one way to enhance learning is to facilitate ancillary infrastructure development, which may help leverage complementarities in the education production process. The inability to harness the complementarities through government policy has been primarily identified as a major factor for the missing link between participation and learning. An impact evaluation of electrification projects allows harnessing such complementarities. Therefore, simple impact evaluations of such projects would, in general, underestimate the potential spillovers to other sectors such as education.

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DATA AVAILABILITY STATEMENT

This study uses two rounds of India Human Development Survey (IHDS-I and IHDS-II) and ASER data. The IHDS-I data that support the findings of this study are openly available in ICPSR at https://www.icpsr.umich.edu/web/DSDR/studies/22626, reference number 22626. The IHDS-II data that support the findings of this study are openly available in ICPSR at https://www.icpsr.umich.edu/web/DSDR/studies/36151, reference number 36151. The ASER data that support the findings of this study are available on request from the ASER Centre (http://www.asercentre.org/).

ORCID

Somdeep Chatterjee https://orcid.org/0000-0002-9825-2227

ENDNOTES

[1] Kenya, Malawi, Tanzania, and Zambia.

[2] Aguirre (2017) is written in the context of Peru, Barron and Torero (2014) in the context of Northern El Salvador, and Khandkar et al. (2012) in the context of India.

[3] The state currently has 23 districts after several were split up into two for administrative purposes.

[4] https://www.livemint.com/industry/energy/all-villages-electrified-but-last-mile-supply-a-challenge-11577642738875.html.

[5] https://www.thehindu.com/news/national/power-ministry-feels-no-need-to-change-electrification-definition/article23732316.ece.

[6] It is often argued in the literature that the households that are politically connected are more likely to recieve the benefits of social welfare programs (Das, 2015; Das et al., 2021).

[7] See https://westbengal.gov.in/portal/web/guest/sabar-ghare-alo.

[8] See https://wb.gov.in/government-schemes-details-sabargharealo.aspx.

[9] See http://forestsclearance.nic.in/writereaddata/FormA/Wildlife/6111612591214RWB1TcOST BENE.pdf.

[10] See p. 137 of https://bit.ly/3rXYxxj.

[11] See http://forestsclearance.nic.in/DownloadPdfFile.aspx?FileName=6111612591214RWB1TcOST%20BENE.pdf&FilePath=../writereaddata/FormA/Wildlife/.

[12] We use heteroskedasticity consistent robust standard errors as our most preferred structure. We also try clustered standard errors at the district level and the results largely hold, although some precision is lost for the estimates. This is a concern because there are only a few districts in the sample and we run into a few clusters problem where inference on cluster robust standard errors is infeasible (Table A10).

[13] We supplement our parallel trends exercise with the falsification exercise which follows. However, we acknowledge that this is not a perfect test for the unobserved counterfactual assumption on which our identification relies. In the presence of ideal data, one would potentially attempt to show no systematic trends between SGA and non-SGA districts, for instance using child-level data to show aggregated trends. We do not show results for math test scores as the main effects are imprecise, but the same can be made available by the authors on request.

[14] One concern here could be that the Backward Region Grant Fund (BRGF) of the government was also implemented around similar times and may have differentially affected the SGA districts, posing a threat to identification. However, it must be noted here that the BRGF was in place for a few years even before SGA was announced and therefore the interaction effect of the time and the treatment dummy is unlikely to be confounded by the BRGF policy impact and therefore our identification goes through.

[15] While attrition in some form may be of concern for missing data in ASER—in our case we are not worried about it. This is because the estimation is impacted and suffers from selection issues if there is selective attrition. That would imply here that households selectively opt out of the ASER survey rounds due to the SGA program which is very unlikely.

[16] The idea is that household time-use patterns change in response to the policy. Therefore, households may have more time to devote to human capital investments because of reduction in time spent on household production.

[17] Reading scores range from 0 to 4 and Math test scores range from 0 to 3.

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SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

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