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Are the Benefits of Electrification Realized Only in the Long Run? Evidence from Rural India

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Abstract

Experimental studies find smaller benefits of electrification than observational studies. Is this because the latter typically observe benefits after a longer period of time? Using three waves of data from the Human Development Profile of India and the Indian Household Development Survey of Indian rural households, we quantify the impacts of short-term (0-7 years) and long-term (7-17 years) electricity access on household well-being. We use a propensity-score-weighted-difference-in-differences design that controls for spillover effects and find that electricity access increases consumption and education in the long term, and reduces the time spent by women on fuel collection, although we do not find significant effects on agricultural income, agricultural land holding, and kerosene consumption. Per capita consumption grows by 18 percentage points more over seven years in the long-term connected group than in the control group. Short-term effects are smaller and not statistically significant for any outcome variable.

JEL Classification: O13, Q40

Keywords: Electricity access, impact assessment, South Asia

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1 Introduction

Does development simply provide the means to enable electricity access or can interventions to increase access raise incomes? There is little consensus in the academic literature on whether increased electricity access has a positive causal effect on incomes and other correlates of development. Randomized controlled trials (RCTs) have mixed results (Lee et al., 2020a; Bayer et al., 2020). On the other hand, observational or quasi-experimental studies often show positive impacts of electricity access on development indicators (Lee et al., 2020a; Burke et al., 2018). This disparity has been attributed to liberal assumptions in observational studies' empirical strategies (Lee et al., 2020a). However, observational studies typically measure impacts a longer period after connection than experimental studies do. Is the difference between their findings because of the identification method or because of the duration of connection to electricity?

To address this question, we compare the results of 16 impact evaluation studies included in the systematic review conducted by Bayer et al. (2020).[1] Figure 1 plots an indicator of the positiveness of impacts (calculated by first assigning 1 to positive impacts, 0 to neutral impacts, and -1 to negative impacts, and averaging for each study) on five development variables against the duration of connection to electricity. There is a positive association between the positiveness of impact and the duration of connection ($p = 0.027$) and also between positiveness of impact and the type of study, but the latter is not statistically significant ($p = 0.3031$). [2] Given the small sample, these results cannot be definitive, but they do suggest that the effect of duration of connection is worth exploring further. The paucity of studies investigating the time-associated benefits of electrification on social and economic development presents a major gap in the literature, which remains to be studied.

We estimate the effects of short-term (0-7 years) and long-term (7-17 years) electricity access on rural household well-being using two waves of the India Household Development Survey (IHDS). The main period of analysis covers the years from 2004-5 to 2011-12, a period that overlaps with the Rajiv Gandhi Grameen Vidyutikaran Yojana (RGGVY), an expansive rural electrification program launched in 2004-5. We analyze households that were first electrified after 1994, and before 2011-12. According to the World Bank Global Electrification Database, less than half the population of India (49.8%) had electricity in 1994, while by 2011-12 the number had risen to 79.9%, with more than half a billion people having been brought onto the

[1] For full details see Appendix A

[2] In a regression of the positiveness of impact on the logarithm of the duration of the connection , the methodology of the study (observational or experimental), and the technology (grid or off-grid), only duration of connection has a somewhat statistically significant effect ($p = 0.069$). As there are only 16 data points in our sample, we expect the level of statistical significance to be low.

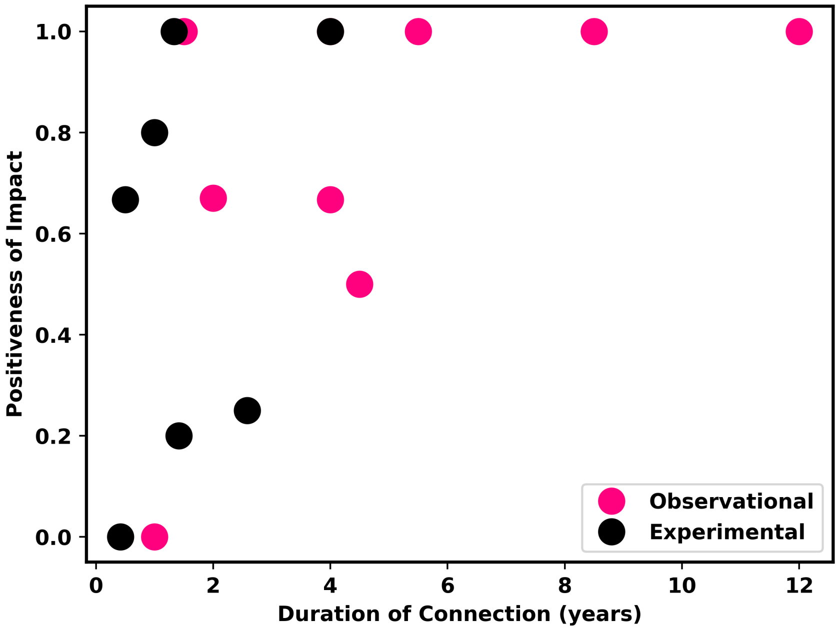


Figure 1: Positiveness of Impact Versus Duration of Connection. The impact is 1 for a variable if a significant positive impact was observed on any of the chosen variables, -1 for a negative impact, and 0 for a neutral result. These values are then averaged on a scale from -1 to 1, giving us what we call the positiveness of impact for each of the sixteen studies. For studies with no negative impacts, this is the equivalent of the frequency of positive impacts. Source: Bayer et al. (2020)

grid in this period. This aggressive roll-out of new electricity connections, predominantly in rural areas, in the World's second-most populous country (at that time), makes for an ideal setting to estimate the effect of duration of connection.

We use a subset of the households surveyed as a part of the IHDS in 2004-5 and 2011-12 that had previously been surveyed in 1994-95 in the Human Development Profile of India (National Council on Applied Economic Research, 1994) survey (HDPI). This allows us to classify households based on whether they got connected between 2004-5 and 2011-12 (short-term connection) or between 1994-95 and 2004-5 (long-term connection), which we analyze as two types of treatment, as well as households that had still not been connected by 2011-12, which constitute our control group.

Since improved electricity access is both the result of and a potential driver of development, making causal inferences regarding the impacts of electrification is challenging. We use difference-in-differences to estimate the effects of short- and long-term connections compared to the control group. However, not all households in a connected village are themselves connected and the selection of villages to be connected may be non-random. We use propensity-score-weighted regressions (Imbens, 2000) to address selection bias in the assignment of households to the control and two treatment groups. An alternative to using propensity score weighted regressions would have been to use an instrumental variable. However, we do not believe that the instruments typically used in the literature satisfy the exclusion restriction because either they may be correlated with development in general (Lee et al., 2020a) or because of spillovers of electrification to unconnected households (van de Walle et al., 2017).

Spillover effects are the effects of electricity access due to the village itself or other households in the village being connected, due to which even households that have not been connected to the grid themselves, can also benefit from the village having access. Therefore, simply comparing outcomes in treated and control households can bias the estimate, even in RCT studies. To avoid this bias, we control for the fraction of electrified households in each village in our regression analysis. The effect of this variable is then the external effect of electrification beyond the household and should be included in the assessment of the benefits of electrification.

We find that access to electricity has statistically insignificant impacts on households that have had access to electricity for less than seven years. However, for households connected for a longer period (7-17 years), we find statistically significant increases in per capita consumption (18%) and education (0.43 years) and less of an increase in the time spent by women collecting fuels compared to the control group (92 minutes less per week). We conclude that the effects of electricity access grow over time. To check for robustness, we use two alternative designs, which suggest that most of our findings, particularly that impacts grow over time are robust. In our first test, we add households connected before 1994 and find that the effects grow over time for almost all variables, despite the inclusion of the third group which nearly doubles the size of the sample for most of our dependent variables. In the second test, we use 1994-5 characteristics instead of 2004-5 ones to estimate propensity scores, and again find that are results are fairly robust.

The next section of the paper reviews the related literature. The third section provides historical context, details our data sources, and provides a descriptive analysis. The fourth section outlines our empirical strategy, the fifth the results, and the sixth some robustness checks. Finally, we provide some conclusions.

2 Related Literature

There are likely multiple channels through which electrification provides benefits that increase over time. Electrification may increase the time that household members have to allocate to new activities, due to a reduction in, if not a total elimination of, the time burden of undertaking various domestic tasks that are rendered redundant with the advent of electricity, such as gathering firewood or other fuels (Dinkelman, 2011). Empirical studies find that connection to the grid results in a significant reduction in the time spent on biomass collection (Samad and Zhang, 2016) and increased participation in the labor force (Dinkelman, 2011). The benefits of electrification are likely to grow with time if participation in the labor force for a longer period has benefits for the individual. Since the surplus time can also be used to augment existing household incomes with other productive activities such as businesses, growth in the business over time would translate to greater returns of electricity access in the long term.

Similarly, access to electricity can increase the duration of hours in which children can study after school (Samad and Zhang, 2016; Aguirre, 2017), providing education benefits. The benefits of education grow with years of schooling, and some benefits, such as the effect of schooling on wages through enhanced employment, only occur after many years. Reduced exposure to indoor air pollution may have similar long-term effects. Poor households are not likely to have adequate resources to make the best of an electricity connection when they first receive it. A large fraction of the benefits may, therefore, be unrealized if households, but over time they may be able to invest in the necessary appliances and machines to fully maximize the benefits of electricity.

Both experimental and observational methods have been used to evaluate the impact of electrification on development outcomes. RCTs have mixed results (Lee et al., 2020a). Among RCTs that evaluate the impacts of grid connections, Lee et al. (2020b) find mostly neutral impacts of electricity connections in rural Kenya on primary economic and non-economic variables, although notably, they find a statistically significant impact on the number of hours worked, in line with the observational study of Dinkelman (2011). Barron and Torero (2017) find a significant reduction in kerosene expenditure and a large reduction in particulate matter concentration in Northern El Salvador. Studies evaluating off-grid connections (e.g. Aevarsdottir et al., 2017) also have mixed results. However, RCTs have several limitations. The sample size is often small and localized to a small region and population, which may make it difficult to extend the results to other populations in different contexts (Pritchett and Sandefur, 2015). Studies are also typically of short duration. Finally, if there are spillover benefits to unconnected households, these are deducted from the estimated benefit of the intervention rather than being added to the total benefit. We use a quasi-experimental observational approach, which allows us to investigate a longer period of 17 years for a large sample of households from 700 villages spread across India. We can also estimate the extent of spillovers.

Observational or quasi-experimental studies often show positive results including in Bangladesh (Khandker et al., 2012), India (Chakravorty et al., 2014; Khandker et al., 2014; Samad and Zhang, 2016; van de Walle et al., 2017), South Africa (Dinkelman, 2011), and Vietnam (Khandker et al., 2009, 2013). In a systematic review, Bayer et al. (2020) report that observational studies report more positive results than experimental studies. In addition to the effect of the duration of connection, discussed above, this could be due to more lax assumptions in observational designs and the absence of proper randomization. Lee et al. (2020a) argue that the instrumental variables used in the literature so far, such as geographic cost-based instruments, may not always satisfy the exclusion criteria. Therefore, instead, we use a propensity-score-weighted-differences-in-differences regression approach by weighting households to simulate a sample where the treatment (here, grid connections) was assigned approximately randomly.

On the other hand, some observational studies such as Cook (2005) (Thailand), Bensch et al. (2011) (Rwanda), and Burlig and Preonas (2016) (India) do not find strong positive impacts. Burlig and Preonas's study is especially relevant given it investigates the effectiveness of the Rajiv Gandhi Grameen Vidyutikaran Yojana (RGGVY), a massive rural electricity roll-out scheme that was launched in India in 2004-5, which overlaps significantly with our period of study. The authors find using nighttime lights data, that although the prevalence of electricity has indeed expanded, its impacts on economic welfare are limited, in the short-to-medium run. On the other hand, van de Walle et al. (2017) study the long-term impacts of household electrification using a DID and an IV design. The authors use the 1981–82 and 1998–99 waves of the India Rural Economic and Demographic Survey (REDS) which covers a period of 17 years, which is coincidentally the exact same length of time that we study, though it refers to an earlier period. They find that electricity brings significant positive changes in the consumption of households that were electrified. As mentioned above, they also find positive external effects on unconnected households. We attempt to resolve the disparities between these studies by focusing on the duration of the electricity connection to distinguish between short-term (0-7 years), and long-term connections (7-17 years).

The effects of short- versus long-run connections have been studied in a developed country context. Lewis and Severnini (2020) found that there were significant benefits to long-run connections in rural American counties that were electrified in the mid-twentieth century. They found that counties that were electrified earlier had significantly larger improvements to median dwelling value, farmland value, retail and manufacture payroll per worker, and farm revenue per worker, compared to those that were electrified later.

Chakravorty et al. (2014) and Samad and Zhang (2016) previously used the HDPI-IHDS surveys to study the impacts of rural electrification in India. Chakravorty et al. (2014) use an instrumental variable approach to assess the impact of electricity access on income using the HDPI survey and the first IHDS survey round. Samad and Zhang (2016) use the two IHDS survey rounds to study the impact of electricity access on a variety of well-being outcomes using a propensity-score-weighted regression. Our research differs from these articles by focusing on the difference between the effects of short-term and long-term electricity access. Chakravorty et al. (2014) present OLS and IV estimates, where the instrument is based on the density of transmission lines in the relevant district. While the OLS estimate of the percentage increase in income per capita as a result of grid connection is 6.7%, the IV estimate is 55.4%, which is surprisingly large. Samad and Zhang (2016) estimate the increase in per capita income from connections as 11.5%.

Our design is closer to that of Samad and Zhang (2016) as we both use differences in differences with propensity score weighting. However, our methodology differs in three major ways. We use two treatment groups of households with short- and long-term access; we only consider households that had not been connected by 1994-95, so that our control group is restricted to households that never had electricity; and, finally, we use more variables for estimating the propensity scores, which changes the results substantially.

Both Chakravorty et al. (2014) and Samad and Zhang (2016) focus on the effects of electricity reliability.[3] Samad and Zhang (2016) control for reliability using a continuous variable interacted with connection status. They find significant negative effects of increased outages on total and non-farm income. We control for reliability in the same way as Samad and Zhang (2016).

3 Institutional Setting and Data

3.1 Historical Background

India has had an aggressive electrification program and the number of electrified villages and households grew significantly in the last decade of the twentieth century and the first decade of the twenty-first century – the period that we use in our study. A large number of households were electrified over a short period of time, though a sizeable number of households still remained unelectrified in 2011-12. This period also followed the liberalization of India's economy when

[3] Questions about reliability were different in the 1994 and subsequent surveys and so Chakravorty et al. (2014) construct a qualitative variable to measure the quality of electricity. The constructed variable may not be ideal as the HDPI measures quality in terms of the frequency of disruptions while the IHDS measures quality in terms of the duration of disruptions, which may not be translatable.

economic growth accelerated.

By the turn of the 21st century there remained only about 100,000 villages with a population of at least 100 people each that were still unelectrified. To connect these villages, the Government of India launched the Rajiv Gandhi Grameen Vidyutikaran Yojana (RGGVY) or the Rajiv Gandhi Rural Electrification Initiative in 2005. RGGVY targeted electrifying all rural households and providing electricity to poor households free of cost. However, there are conflicting reports on the effectiveness of the program. While government sources and a report from the World Bank (Pargal and Banerjee, 2014) indicate that there was a significant increase in the electrification rate, particularly among rural consumers, other studies, such as Burlig and Preonas (2016) who use nighttime lights data, find that although there was a significant increase, it was not as expansive as thought. This suggests that while electricity access may have been brought to a large number of households, many of these households may not have electricity of sufficient quality or may not have access to the resources to be able to utilize the grid connection adequately. India's final five-year plan (2012-2017) aimed to electrify all villages in India, and the government claimed to have achieved that objective by April 2018. According to government statistics, by 2019 only 18,734 households, remained to be electrified, all in the state of Chattisgarh (Saubhagya, 2023).[4]

Rural electrification in India has involved the electrification of villages and subsequently the electrification of households within villages. Therefore, when we correct for selection bias in assignment to treatment, we account for both village-level and household-level characteristics. A major concern in establishing causality between electricity access and development outcomes arises from the possibility that improvements in electricity access often coincide with the development of other government-provided infrastructure and institutions. The IHDS surveys include a comprehensive array of variables that describe the presence of roads, schools, primary healthcare centers, development groups, and the proximity to banks and markets, which can then be controlled for.

3.2 Dataset

Our data form a panel of households surveyed in three waves. The first wave of the sample comes from the HDPI survey, conducted in 1994-95. The second wave comes from the India Household Development Survey - I (Desai et al., 2005) from 2004-5, and the third comes from the India Household Development Survey - II (Desai et al., 2011-2012). The original HDPI survey covered

[4]Villages such as Parcheli and Tetam in the Maoist-affected Dantewada district received electricity as late as 2023.

	Means (standard deviation)		Correlation with % of households electrified	
	2004-5	2011-12	2004-5	2011-12
Village has access†	0.93	0.99		
	(0.25)	(0.10)		
Village fully electrified†	0.22	0.23		
	(0.41)	(0.42)		
Electrified Households in Village (%)	68.62	79.25		
	(34.13)	(26.80)		
Years since First Connected	25.37	31.16	0.3367***	0.4249***
	(15.54)	(15.84)		
Reliability (Hours of Access in a Day)	12.98	13.67	0.0907*	0.3265***
	(7.08)	(6.68)		
Presence of metalled roads†	0.67	0.87	0.2694***	-0.3550***
	(0.47)	(0.33)		
Distance to nearest bank branch office or credit cooperative (km)	4.69	5.06	-0.0822**	-0.0529
	(5.46)	(5.08)		
Distance to the closest general market shop (km)	2.23	2.57	-0.0516	-0.0420
	(4.02)	(5.03)		
Presence of Development Group or NGO †	0.14	0.13	-0.0095	-0.1032***
	(0.35)	(0.34)		
Number of Primary Healthcare Centers	0.13	0.11	0.0475	0.0720*
	0.40	0.31		
Number of Government Primary Schools	1.73	1.70	-0.0408	-0.1099***
	(1.62)	(1.60)		
Number of Government Middle Schools	0.66	0.87	0.1641***	-0.0396
	(0.62)	(0.72)		
Number of Government Secondary Schools	0.32	0.41	0.1464***	0.0648*
	(0.51)	(0.66)		
Number of Government Higher Secondary Schools	0.13	0.17	0.1657***	0.0717*
	(0.34)	(0.42)		

Table 1: Descriptive Statistics for Villages. Sample includes 727 villages. Source: HDPI, IHDS I, and IHDS II surveys. The correlation reported is Pearson's correlation coefficient, and significance has been calculated with the two-sided alternate hypothesis.
† Dummy variable which takes 1 for "yes" and 0 for "no".
[1] The drop in standard deviation is due to one village that reports 40 private higher secondary schools in 2004-5.
*Significant at the 10% level, **Significant at the 5% level, ***Significant at the 1% level

a random sample of 33,230 households located in 16 states, 195 districts, and 1,765 villages, while the IHDS-I survey covered 41,554 households from 384 districts, 1,503 villages, and 971 urban blocks. IHDS-II covers the same villages and urban blocks as IHDS-I but interviewed 42,152 households. Some households which had been interviewed in the previous wave could not be contacted in later periods, given that the last round of surveys was 17-18 years after the first, while some other households had split.

We restrict our analysis to the set of rural households present in all three surveys, that had not split. This results in 9233 households from 727 villages, with a mean of 12.70 households per village and a standard deviation of 7.05. Since the original HDPI survey was selected randomly, this sample can be assumed to be representative at the country level.

3.3 Sample Characteristics

Table 1 presents village-level characteristics. 93% villages had access to electricity by 2004-5, and 99% by 2011-12, with the fraction of fully electrified villages remaining roughly constant at about 22-23%. However, there was a substantial rise in the fraction of households connected

	Means (standard deviation)					
	2004-5			2011-12		
	Electricity	Short-term Electricity	No Electricity	Electricity	Short-term Electricity	No Electricity
Number of Adult Men (21+)	1.80	1.69	1.56	1.71	1.60	1.42
	(0.96)	(0.89)	(0.80)	(0.86)	(0.78)	(0.67)
Number of Adult Women (21+)	1.71	1.57	1.48	1.69	1.57	1.39
	(0.84)	(0.75)	(0.69)	(0.79)	(0.72)	(0.62)
Number of Adolescent Boys (15-21)	1.26	1.25	1.24	1.22	1.21	1.20
	(0.49)	(0.47)	(0.49)	(0.46)	(0.42)	(0.45)
Number of Adolescent Girls (15-21)	1.26	1.25	1.22	1.24	1.23	1.18
	(0.51)	(0.50)	(0.47)	(0.49)	(0.47)	(0.43)
Number of Boy Children (<15)	1.67	1.70	1.75	1.51	1.58	1.61
	(0.90)	(0.92)	(0.97)	(0.77)	(0.81)	(0.83)
Number of Girl Children (<15)	1.68	1.72	1.77	1.53	1.66	1.66
	(0.98)	(0.99)	(1.00)	(0.83)	(0.94)	(0.93)
Presence of Water Source inside the House †	0.45	0.34	0.26	0.44	0.27	0.26
	(0.50)	(0.47)	(0.44)	(0.50)	(0.44)	(0.44)
Presence of Flush Toilets †	0.17	0.09	0.03	0.30	0.14	0.06
	(0.38)	(0.28)	(0.16)	(0.46)	(0.35)	(0.24)
Presence of Separate Kitchens †	0.61	0.50	0.37	0.55	0.39	0.24
	(0.49)	(0.50)	(0.48)	(0.50)	(0.49)	(0.43)
(Log) per capita Consumption (Rs.)	9.99	9.75	9.53	10.18	9.89	9.76
	(0.63)	(0.58)	(0.52)	(0.62)	(0.52)	(0.56)
(Log) Agricultural Income (Rs.)	10.08	9.69	9.21	10.09	9.47	9.12
	(1.53)	(1.48)	(1.44)	(1.58)	(1.44)	(1.40)
(Log) Agricultural Land Holding (acres)	1.64	1.58	1.68	1.63	1.56	1.34
	(1.27)	(1.39)	(1.49)	(1.35)	(1.47)	(1.35)
Schooling of the Highest-educated Adult (years)	7.28	5.70	4.08	7.62	5.49	3.80
	(4.82)	(4.75)	(4.37)	(4.94)	(4.69)	(4.39)
Kerosene Consumption (Litres)	3.33	3.47	3.64	2.74	3.25	3.13
	(3.01)	(2.62)	(2.12)	(2.29)	(2.06)	(1.54)
Time spent by Women in Collecting Fuel (mins per week)	293.25	283.52	264.68	238.39	272.25	312.17
	(296.96)	(273.37)	(270.50)	(387.17)	(404.90)	(424.77)

Table 2: Descriptive Statistics for Household Characteristics 2004-5 and 2011-12. Includes 9163 households. Source: HDPI, IHDS I, and IHDS II surveys.
† Dummy variable which takes 1 for "yes" and 0 for "no".

to the grid.[5]

There is a noticeable improvement between the two periods in the presence of paved roads and the number of both government and private schools (see Appendix B) in villages. However, there is little change in the presence of bank branch offices/credit cooperatives, general market (kirana) shops, and development groups/NGOs. In fact, these characteristics marginally decline between 2004-5 and 2011-12. The observation that, despite an increase in the rate of electricity access (Table 1) several other variables such as the proximity to banks, credit cooperatives, markets, development groups, and primary healthcare centers have not shown improvements, makes this period suitable for our analysis, as we can rule out the possibility of overstating the benefits of electrification by attributing improvements due to these factors to electricity.

Table 2 presents descriptive statistics at the household level. Short-term electricity indicates that the household was first connected between this survey and the previous round, after 1994-95 for the 2004-5 data and after 2004-5 for the 2011-12 data. Households that do not have electricity have poorer household infrastructure in both rounds of the survey. Furthermore, households without electricity (and with poorer household infrastructure on average) tend to be smaller in size, reflected in the smaller number of adult and adolescent men and women in these households. This may be because obtaining electricity access may be a function of the ability to pay, and larger households would have more disposable income to afford getting

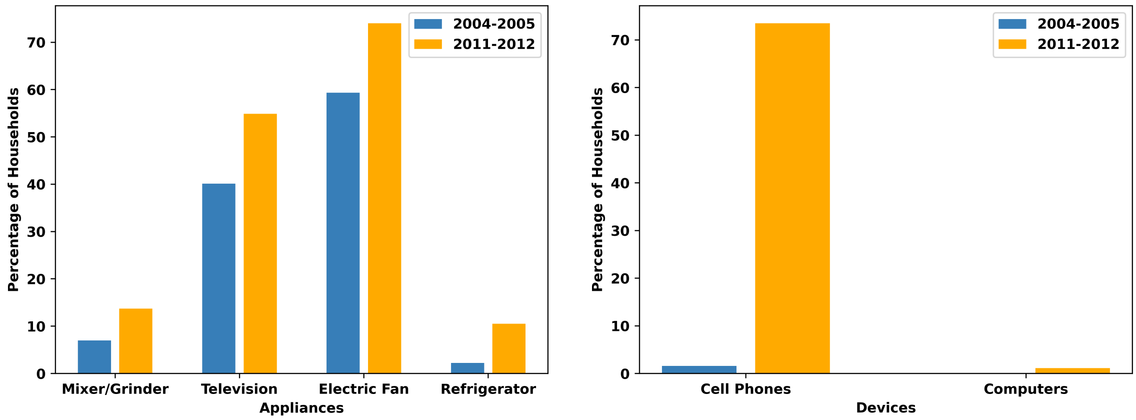
[5]See Appendix B for a detailed discussion of the household electrification data.

connected and acquire the material resources to utilize the connections.

The statistics in Table 2 indicate that the average per capita consumption in 2012 ranged from about Rs. 17,300 in households without electricity to about Rs. 24,100 in households with electricity (about Rs. 19,700 in households with short-term access). To put this in perspective, consumption worth 1 US dollar per day, would have amounted to approximately Rs. 19,500 over the year (at 2012 conversion rates). Poor households are unlikely to have adequate resources to make the best of an electricity connection when they first receive it. A large fraction of the benefits may, therefore, be unrealized by households immediately, but over time they may be able to invest in the necessary appliances and machines to fully maximize the benefits of electricity. We can see this in the ownership of electrical appliances by rural Indian households that were connected to the grid, prior to 2004-5 (Figure 2(a)). These households, although already connected to the grid, saw their ownership of appliances grow considerably from 2004-5 to 2011-12. The growth in ownership is more remarkable for more expensive appliances such as televisions, mixers/grinders, and refrigerators, compared to electric fans. This suggests that affordability has a significant role to play in the lag between getting electrified and being able to reap its full benefits.

For example, computers and cell phones were extremely rare in rural Indian households at the beginning of the twenty-first century, even among those that had been electrified. Advancements in telecommunication infrastructure have made cell phones extremely commonplace in households that have electricity, allowing households to make better use of their electrical connections. Similarly, computer ownership has increased as well, although it is still rare. This is visible in Figure 2(b) where there is an immense rise in the ownership of cell phones. Similarly, not a single household in our sample owned a computer in 2004-5, despite already having been connected to the grid, while more than 1% of households owned computers in 2011-12.

Clearly, benefiting from access to electricity may be a more gradual and continuous process, with various outcomes occurring at different paces. Therefore, in our analysis, we use multiple outcome variables, each of which may have a different relationship with electricity access and thus respond differently. These include consumption, which could start showing benefits faster as a result of increased labor force participation or increased domestic production, and the education level of the household's decision maker, which one would expect to respond much more slowly to electricity access.



((a)) Electrical Appliances ((b)) Technological Devices

Figure 2: Ownership of household electrical appliances and technological devices by rural households in India which were electrified between 1994-95 and 2004-5 ($N = 2121$). Source: HDPI, IHDS I, and IHDS II surveys.

3.4 Characterizing Household Well-being

Table 2 also includes descriptive statistics for our outcome variables. As of 2011-12, there are considerable differences in the levels of outcome variables between households that have access to electricity and those that do not. Households connected to the grid have higher per capita consumption, agricultural income, agricultural land holding, better education, lower consumption of kerosene, and less time spent by women in fuel collection - all these differences are significant at the 0.1% level.

There is also typically a small improvement (increase where desirable or decrease where that is desirable such as in kerosene consumption or time spent in collecting fuels) in the levels of variables between the two surveys, particularly among households that have electricity, with the only exceptions being agricultural land holding which shows marginal decreases. By contrast, households without electricity were typically worse off by these measures in 2011-12 than in 2004-5, with the exceptions of per capita expenditure and kerosene consumption which increased and decreased, respectively. This may be because only the poorest still did not have access to electricity by 2011-12. The reason for the stark difference between the two groups of households, particularly in 2011-12, could be partly because poorer and less educated households tended to be electrified last, but also because of the benefits of being electrified. Therefore, we need to control for these composition effects while studying the effect of connection on the outcomes. In order to study the composition effects, we employ a difference-in-difference approach, with a large set of control variables, to attribute the changes in the levels of well-being appropriately to electricity access. The empirical strategy for the estimation of benefits is described in detail in the following section.

4 Empirical Strategy

4.1 Differences in Differences

While the two panels of the IHDS data have information on when villages were first electrified, there is no data on precisely when individual households were first connected to the grid. We estimate the approximate timing by using the households common to the HDPI and IHDS surveys. Since all three surveys ask households whether they have electricity, we know the interval during which households were connected.

We classify households into three groups - those who did not have electricity in 1994-95 and continued to not have electricity in 2004-5 and 2011-12, those who did not have electricity in 1994-95 but got connected before 2004-5, and those households who did not have electricity in 1994-95 or 2004-5 but were connected by 2011-12. These three groups are our control group, long-term treatment, and short-term treatment groups, respectively. Our analysis excludes all households that had electricity in 1994-95.[6]

We restrict the observations to 2004-5 and 2011-12 alone because we do not have data prior to 1994-95, and so there is a lot of variation in the duration of connection for households that were already connected in 1994-95. Furthermore, we have no data on village-level variables or characteristics in 1994-95. Among other problems, this means that we cannot study spillovers for this wave. In our main results, we only use the 1994-95 data to determine whether households that had electricity in 2004-5 were connected between 1994-95 and 2004-5 or prior to 1994-95. Later, to check the robustness of our results, we also use the 1994-95 levels of variables to estimate propensity scores.

Our model can be formulated as a dynamic two-way fixed effects specification: (Roth et al., 2023):

$$y_{ijt} = \alpha_i + \gamma_t + \nu_j + \beta_1 R_{1,ijt} + \beta_2 R_{2,ijt} + \delta' X_{ijt} + \epsilon_{ijt}, t = 2005, 2012, \tag{1}$$

where y_{it} is the outcome variable (such as the logarithm of total expenditure), for household i in village j at time t. We construct two dummy variables R_1 and R_2 for treatment one period and two periods ago. The former takes the value of 1 if the household gained access to electricity between the current period and the previous wave of the survey and 0 otherwise. The

[6]We also exclude a small number of households that had electricity in 2004-5 but lost connection by 2011-12 as there is no reason to assume that the effect of losing access will be the exact opposite of getting access.

latter variable is equal to 1 if the household gained access to electricity prior to the previous round of the survey but after the round before that. Therefore, in 2011-12 $R_{1,ijt} = 1$ for those households who gained access after 2004-5 and zero for other households, and $R_{2,ijt} = 1$ for those who gained access between 1994-95 and 2004-5.

In Equation 1, the γ_t are common time fixed effects, α_i are household-specific effects, ν_j are village-specific effects, and ϵ_{ijt} is the idiosyncratic error term. The vector of variables X_{ijt} denotes village- and household-level characteristics. The control variables used in the regression are the number of hours of electricity available to a household in a day, the fraction of households electrified in the village, the number of adult men in the household, the number of adult women in the household, whether the household has a water source, flush toilet, and separate kitchen, the population of the village (dummy variables for whether the village has a population less than 1000, and whether the village has a population more than 5000), the presence of metalled roads, proximity to bank branch offices/credit cooperatives, general market ships, presence of NGOs/development organizations, the number of primary health care centers, and the number of government primary, middle, secondary, and higher secondary schools.

Roth et al. (2023, p14) state that: "Unlike the static specification, [where the effect of duration of treatment is not estimated] the dynamic specification yields a sensible causal estimand when there is heterogeneity only in time since treatment ... when there are heterogenous dynamic treatment effects across adoption cohorts, the coefficients ... become difficult to interpret." Clearly, it is possible that the impact in the first period after a household is connected could vary across our two cohorts. We address this by taking differences of both sides of Equation 1:

$$\Delta y_{ij} = \Delta\gamma + \beta_1 \Delta R_{1,ij} + \beta_2 \Delta R_{2,ij} + \delta' \Delta X_{ij} + \Delta\epsilon_{ij} \tag{2}$$

We now have a single cross-section of differences and so we drop the time subscript. For households connected between 2004-5 and 2011-12, $\Delta R_{1,ij} = 1$ while $\Delta R_{2,ij} = 0$. However, for households connected between 1994-95 and 2004-5 $\Delta R_{1,ij} = -1$ while $\Delta R_{2,ij} = 1$. Therefore, the effect of a short-run connection is β_1, but the effect of a long-run connection is $\beta_2 - \beta_1$. To simplify the presentation of results we re-parameterize the model:

$$\Delta y_{ij} = \Delta\gamma + \theta_S D_{S,ij} + \theta_L D_{L,ij} + \delta' \Delta X_{ij} + \Delta\epsilon_{ij} \tag{3}$$

where $\theta_S = \beta_1$, $\theta_L = \beta_2 - \beta_1$, D_S is a dummy variable equal to one for households connected between 2004-5 and 2011-12 and zero otherwise, and D_L is a dummy variable equal to one for households connected between 1994-95 and 2004-5 and zero otherwise. Thus we estimate the effect of short-term connection using only data for households connected between 2004-5 and 2011-12 and avoid the issues raised by Sun and Abraham (2021). The coefficients θ_L and θ_S then measure the average treatment effects (ATE) for long-term and short-term connections, respectively.

4.2 Propensity Scores

However, these coefficients would be the ATEs only under the assumption that each household was equally likely to be assorted into the treatment and control groups, which is improbable. More developed villages may be more likely to be connected to the grid earlier. Similarly, wealthier or better-educated households may be connected preferentially. To find the true ATE, we should account for selection bias in assignment to treatment. To eliminate selection bias, we use a two-stage propensity score weighted regression, using the generalized propensity score (Imbens, 2000). The propensity score is a measure that quantifies the probability of a household being assigned to the treatment that it received. We would like to balance our sample such that every household was equally likely to receive treatment. By weighting each observation by the inverse of its propensity score, we obtain a sample corrected for selection bias due to covariates. In the first stage, we estimate propensity scores for each household being assigned to either control, short-term treatment, or long-term treatment using a multinomial logistic regression. In the second stage, we estimate the weighted difference in differences regression.

We use a generalized propensity score because we have three categories of the treatment, including the control group. The outcome variable for the multinomial logistic regression is the category of treatment that a household receives – households with long-term connections, households with short-term connections, and households that are yet to be connected. The multinomial logistic regression model estimates the probability that the i^{th} household is assigned the k^{th} of K (here, $K = 3$) treatments. As the probabilities sum to one, if there are K categories, we can run $K - 1$ parallel regressions while using one category as a reference or a pivot. The probability of being assigned to a treatment, k, s given by:

$$P(Z_i = k) = \frac{e^{\beta_k \cdot X_i}}{1 + \sum_{k=1}^{K-1} e^{\beta_k \cdot X_i}}, \quad k < K \tag{4}$$

where Z_i is the treatment given to individual i, X_i is the vector of the levels of the independent variables which determine the propensity for household i being assigned treatment k. The coefficients are estimated by maximum likelihood. The household and village characteristics that we use as explanatory variables are the pre-treatment levels of the log of the total household consumption, the number of adult men and women, the presence of water sources, flush toilets, and separate kitchens in the house, whether the village has a population of less than or equal to 1000, whether the village has a population of over 5000, presence of metalled roads and development groups/NGOs, distance to the nearest bank branch office/credit cooperative, distance to nearest shop/market, the number of primary, middle, secondary, and higher secondary schools, both government and private, the number of primary healthcare centers, the fraction of households electrified in the village, and the number of years since the village was first connected, and the 2001 census levels of the fraction of Brahmins, non-Brahmin forward castes, other backward castes, scheduled castes, and scheduled tribes in the village, whether the village is small (less than 1000 individuals), and whether the village is large (more than 5000 individuals). All the variables are taken from the 2004-5 panel. While some households (those with long-term treatment) had already been assigned to treatment by this time, propensity scores can still be calculated. This is because these scores need not be the actual metrics used by governments and agencies in assigning households to treatments. Instead, it is the actual variation in initial observable characteristics between households.

There are two ways selection bias might occur. In the first approach, we assume there is a selection bias at the level of the village, but assignment to households within villages is random. For this, we use village-level characteristics only in the model. In the second approach, we assume that there is endogeneity at both the level of village assignment and household assignment. Therefore, we use the village-level determinants listed above but also use household-level determinants. While we tried using both approaches, we found that several household-level variables prove to be extremely significant factors in determining connection to the grid.[7] Thus, we conclude that the best specification includes both village and household characteristics.

We use Equation (4) to predict propensity scores for each household from the estimated coefficients. We then compute the weights according to the formula:

$$w_i = \frac{1}{\hat{P}(Z_i = k)}, \tag{5}$$

where k is the actual treatment assigned. By weighting each household by the inverse of its propensity score, we scale the contribution of each household to the estimation of the second stage by the probability of being assigned to a particular treatment.

[7] See Table 11 in Appendix B.

In the second stage, we use Equation 3 to perform a propensity-score-weighted regression to calculate the average treatment effects. The outcome variables we use are consumption (log), agricultural income (log), agricultural land ownership (log), years of education of the highest educated adult, number of meals eaten in a day, domestic kerosene consumption, and the time spent collecting fossil fuels.

In using propensity scores, we are assuming that the selection bias in assignment to treatment would arise from observable covariates rather than unobservables. This is likely a reasonable assumption, as government electrification schemes prioritize households and villages to be electrified based on characteristics such as the population of the village and whether the household was identified to be below the poverty line, as explicitly stated for RGGVY (Programme Evaluation Organization, 2014). Village sizes and affluence are variables we have accounted for. [8] Aklin et al. (2021) have argued that the caste composition of villages plays an important role in the implementation of schemes, and thus we have incorporated caste compositions as well.

4.3 Spillover Analysis

If we find that there are statistically significant spillovers from treated to untreated households, then we ought to include a spillover variable – the fraction of households that are electrified in a village – in our difference in differences regressions. But we should also take into account this effect when assessing the overall benefits of electrification. Thus there are internal effects measured by the coefficients θ_S and θ_L but also external effects measured by the coefficient of the electrification fraction variable.

Suppose the outcome for treated households is $Y(1)$ and the expected outcome on untreated households is $Y(0)$, then naively we would expect the average treatment effect (ATE) to be:

$$ATE = E[Y(1) - Y(0)] \tag{6}$$

However, $Y(0)$ would include benefits that untreated households receive through spillovers. Suppose the spillover benefits on untreated households are denoted as $Y_S(0)$, then the unbiased ATE is given by:

$$ATE = E[Y(1) - Y(0) + Y_S(0)] \tag{7}$$

To evaluate whether such spillover benefits exist, we study how household consumption in both treated and untreated households, a proxy for affluence, varies with the fraction of

[8] Earlier schemes were designed to prioritize the adoption of electric pumps in agriculture, and the interventions may have been influenced by affluence.

households electrified in a village. We use household and village-level data from IHDS and a two-stage propensity score-weighted-difference-in-differences design to estimate the spillover effects.

We perform a DID analysis using the changes in the levels of the outcome, treatment, and control variables between 2004-5 and 2011-12. As in our analysis of the time-associated benefits, we also use the 1994-95 survey round to classify households based on whether they had short-term, long-term, or no access. The outcome variable we use is the per capita consumption expenditure of households, and to ensure that our results are robust and not anomalous, we study the impact on both changes in the levels and logs of consumption. We assume that spillover benefits primarily operate through other households. Therefore, we use the fraction of households with access to electricity as our main treatment variable. Recognizing that the spillover may only accrue to unconnected households or that there may be different effects on the two groups (van de Walle et al., 2017), we consider three designs. In the first of these variants, which is also the most general, we write the treatment in terms of three indicator variables A_{STjt}, A_{LTjt} and A_{Ujt}, where the first takes the value 1 if the household is connected on the short-term (0-7 years), and 0, otherwise, the second takes the value 1 if connected in the long-term (7-17 years) and 0, otherwise, and A_{Ujt} takes the value 1 if the household never receives access. The indicator variables are then multiplied by the change in the fraction of households connected (ΔE_{jt}) to arrive at the two different types of treatment for the two groups of households. Similarly, we include lagged effects, i.e., the fraction of households already connected before the survey which is the interaction between A_{STjt}, A_{LTjt}, or A_{Ujt}, and the pre-survey fraction of electrified households in the village (E_{jt-1}). This helps us estimate the effect of the overall number of connected households, and not just the change. To be able to attribute the effects to electricity alone, we use several other household and village-level control variables, as listed in 4.1, along with the pre-treatment level of reliability to control for the overall level rather than just changes. We model the first difference of per capita consumption (Δy_{ijt}) of unelectrified households using the following equation,

$$\begin{aligned}\Delta y_{ijt} = \alpha + (\beta_{E,ST}A_{STjt} + \beta_{E,LT}A_{LTjt} + \beta_{E,U}A_{Ujt})\,\Delta E_{jt} \\ + (\beta_{EL,ST}A_{STjt} + \beta_{EL,LT}A_{LTjt} + \beta_{EL,U}A_{Ujt})\,E_{jt-1} + \gamma.\Delta X_{ijt} + \Delta\epsilon_{ijt} \quad (8)\end{aligned}$$

Here, $\beta_{E,ST}$ measures the spillover effect on households that are connected in the short-term, $\beta_{E,LT}$ measures the spillover effect on households that are connected in the long-term, and $\beta_{E,U}$

measures the spillover effect on households that are not connected. Similarly, $\beta_{EL,ST}, \beta_{EL,LT}$, and $\beta_{EL,U}$ measure the corresponding lagged effects. γ is a vector of coefficients for control variables, and ϵ_{ijt} is the idiosyncratic error term.

However, spillovers may not affect all households differently, or may not affect all households at all. Therefore, we consider two other possible models of spillover effects. In the first of the two, the coefficients are constrained to be the same across households categories, i.e., $\beta_{E,ST} = \beta_{E,LT} = \beta_{E,U} \equiv \beta_E$, and similarly, for the lagged effects, $\beta_{EL,ST} = \beta_{EL,LT} = \beta_{EL,U} \equiv \beta_{EL}$, which simplifies into the equation,

$$\Delta y_{ijt} = \alpha + \beta_E \Delta E_{jt} + \beta_{EL} E_{jt-1} + \gamma.\Delta X_{ijt} + \Delta\epsilon_{ijt} \tag{9}$$

It may also be the case that households that receive electricity access themselves do not receive any indirect benefits and only those households that are not connected benefit from spillovers, as is assumed by van de Walle et al. (2017). In this case, $\beta_{E,ST} = \beta_{E,LT} = \beta_{EL,ST} = \beta_{EL,LT} = 0$. Equation 8 then simplifies to

$$\Delta y_{ijt} = \alpha + \beta_{E,U} A_U \Delta E_{jt} + \beta_{EL,U} A_U E_{jt-1} + \gamma.\Delta X_{ijt} + \Delta\epsilon_{ijt}, \tag{10}$$

We also estimate each of these regressions using $\Delta \ln y_{ijt}$ as the dependent variable.

Still, controlling for other variables need not be sufficient to arrive at an unbiased estimate of the causal effects. A simple DID design assumes that the assignment of households to treatment is random. There could be several factors that could lead to a rapid increase in the fraction of households with access to electricity resulting in a selection bias. To address the selection bias, we weight households with the inverse of generalized propensity scores to remove selection bias due to observables. Since the treatment in this case, can go from -100% to +100%, as the fraction of households that are electrified can decline, we use a multinomial regression with continuous treatment doses for the first stage estimation of the propensity scores. The observables that we use are the same variables that we use to estimate propensity scores in the main analysis, as listed in Subsection 4.2.

5 Results

The results of our analysis of spillover effects are presented in Appendix C. We estimate a (mostly) positive spillover effect of electrification on both logs and levels of per capita household expenditure. Based on our results, we conclude that it is important to control for spillover effects. We use two variables – the change in the fraction of electrified households in each village and the fraction of households already electrified aby 2004-5. Based on our estimates, we also find that it is reasonable to model a common size of spillover on all households. Thus, when we control for spillovers in our analysis of short-term and long-term impacts, we employ the model in equation 9.

The presence of spillovers implies that future studies must incorporate strategies to eliminate biases induced by spillovers. The presence of spillover benefits also implies that RCTs are faced with an additional challenge – the potential invalidity of SUTVA. In experimental impact evaluation studies, the sample is usually restricted to a village or a small group of villages. If connected households are measured against unconnected households in a village connected to the grid, the presence of a non-uniform spillover effect may lead to biased estimates. The only alternative is to find pure controls – households in villages that are yet to be connected to the grid. But then village-level fixed effects would become difficult to account for.

The results of the first stage estimation of propensity scores are presented in Appendix D, and unweighted difference-in-differences estimates without propensity score weighting are presented in Appendix E.

The results of the propensity-score-weighted-difference-in-difference regressions, which are our preferred estimates, are presented in Table 3. We find significant differences between the short-term and long-term effects between the six outcome variables considered. The effects are also distinct from those measured in the simple DID design (Table 12), which highlights the importance of correcting for selection bias in assignment to treatment.

We find that electricity access only has a statistically significant effect on consumption for the long-term connected. The long-term impact is large – 18 percentage points more income growth over the seven years than in the control group – and about two and a half times larger than the statistically insignificant effect for the short-term connected. This implies that the consumption benefits of electrification against time may be a convex function when plotted against time and continue to grow for a considerable number of years after a household receives a connection. This particular result contrasts our findings with other studies in India using the HDPI-IHDS data such as those by Chakravorty et al. (2014) and Samad and Zhang (2016) both of whom find a larger effect in short-term gains in affluence (Chakravorty et al. (2014) use income, not consumption) , although neither of these two studies looks at long-term effects.

In contrast, agricultural income and land holdings do not show any statistically significant improvement. While agricultural income shows short-term gains with electricity access in the

	Δ Outcome Variables					
	Log per capita Consumption (Rs.)	Log Agricultural Income (Rs.)	Log Agricultural Land Holding (Acres)	Schooling of Highest Educated Adult (years)	Kerosene Consumption (liters per month)	Time spent by Women in Fuel Collection (minutes per week)
	$N = 3739$	$N = 1882$	$N = 1968$	$N = 3731$	$N = 3738$	$N = 1675$
Intercept	0.0933	0.3728	-0.6747**	0.1307	-0.3242	142.4327*
	(0.0710)	(0.2752)	(0.3081)	(0.2631)	(0.2357)	(73.8914)
Short-term Access	0.0613	0.0514	0.0817	0.3637	0.0373	-7.7538
	(0.0600)	(0.2389)	(0.1831)	(0.2550)	(0.1920)	(60.5635)
Long-term Access	0.1662***	-0.0668	-0.0086	0.4311*	0.0725	-91.6618**
	(0.0465)	(0.1893)	(0.1759)	(0.2207)	(0.1570)	(43.6885)

Table 3: Time-associated causal effects of electrification on consumption. Estimates using propensity-score-weighted-difference-in-differences. Robust standard errors clustered at the village level.
*Significant at the 10% level, **Significant at the 5% level, ***Significant at the 1% level

simple DID, neither agricultural income nor agricultural land-holding shows any statistically significant change with electricity access when weighted by the inverse of the propensity scores, either in the long-term or the short-term, in line with the findings of Samad and Zhang (2016). The fact that consumption grows, but agricultural income does not, may hint that electricity access enables households to diversify their earnings and pursue economic activities apart from agriculture.

Years of schooling of the highest educated individual shows a borderline significant ($p = 0.0509$) improvement in the long-term, but the short-term effect is not statistically significant ($p = 0.1539$). Intuitively, because households with a short-term connection received electricity less than seven years before the survey date and full schooling takes longer than a decade, the long-term benefits are likely to be much more pronounced than the short-term. There is no significant effect on kerosene consumption, suggesting that households may stack fuel and increase their energy consumption and carbon footprint after being connected to the grid. This may be because while households can use electricity for less power-demanding activities such as lighting, they may not have connections strong enough for more energy-intensive activities, and may resort to using fossil- or bio-fuels. Households also may not have the necessary appliances to displace kerosene with electricity for uses other than lighting. For instance, households may not have electrical immersion rods for heating water and may be using stoves in their absence, thus demanding fuels such as kerosene or petroleum gas. However, getting access to electricity significantly reduced the increase in time spent by women in collecting fuel (5% level). However, the reduction in collection time due to a long-term connection is less than the increase indicated by the intercept. Adding the intercept and long-term access coefficients we find that the long-term connected households increased their fuel collection time by 51 minutes with a standard error of 81 minutes, which means that it is obviously not statistically significant. This contrasts with the increase of 142 minutes per week ($p = 0.052$) in the control group. Since collected fuel such as firewood is typically used for heating and cooking, while electricity is not used for these purposes, the reduction in the additional time spent in collecting fuel may be an effect operating through an electricity-driven increase in affluence.

At first glance, some of our results may seem at odds with those of Samad and Zhang (2016), even though we use the same survey and the same period, and we both use propensity-score-weighted-fixed-effects regressions to control for selection bias. For instance, Samad and Zhang (2016) find significant short-term improvements in consumption, larger than the simple fixed effects results, while we do not. They also find a considerable reduction in kerosene consumption, which we again do not find after controlling for household-level characteristics. However, there are several differences between our samples and methods that may lead to these outcomes. As discussed earlier, our control group only includes households that were never connected to the grid, while Samad and Zhang (2016) include all households apart from our short-term treatment group in a single control group. This also includes households that were connected prior to 1994-5. Since Samad and Zhang do not study both short and long-term access as we do, they are not restricted by the set of households that are common between three panels, allowing them a sample of over twenty thousand households as opposed to our regression sample of less than four thousand. There are many differences in the variables we use as controls and for the estimation of the propensity scores[9].

6 Robustness Checks

6.1 Approach

In our main analysis, we make two crucial assumptions:

1. We reduce our sample to those households that were not yet connected by 1994-5 in order to appropriately classify households into treatment and control groups, which considerably reduces the size of our sample.

2. We use 2004-5 levels of household and village-level characteristics to estimate propensity scores, despite the long-term access group having already been connected to the grid by

[9] The authors do not explicitly list the variables used in the first-stage regression, or discuss the first-stage results, making the two studies difficult to compare.

then.

To check to what degree our results are affected by these assumptions, we employ two robustness checks, one where we relax the condition of excluding households connected prior to 1994-95, and one where we estimate propensity scores using household-level characteristics from 1994-95 instead of 2004-5. We find that the nature of the effects and the relationships between short-term and long-term effects are largely unaffected by these changes.

Our first robustness check adds households electrified before 1994-5 – we call this treatment "very long-term access." Despite making up over half the households in the survey, households connected before 1994-95 were excluded from our main analysis because the period in which these households were actually connected is unknown, which makes these households extremely heterogeneous in terms of how long they have had access to electricity. This can make these results harder to interpret if, for example, the impacts of electrification are non-monotonic after a certain duration of connection. Nevertheless, this group makes for a good test of the robustness of our main results. If the results were to change completely upon the inclusion of this group, such as showing that impacts are higher in the short-term and lesser in the long term, then we would be less confident about our main results. Ideally, the inclusion of a fourth category of households, should not change the results much qualitatively and should preserve the broad trends observed.

To include the very-long term treatment group, we add a new treatment variable, $\Delta D_{V,ijt}$, to Equation 3 equal to 1 if the household received a connection before 1994-95 and zero otherwise:

$$\Delta y_{ij} = \Delta\gamma + \beta_V \Delta D_{V,ij} + \beta_L \Delta D_{L,ij} + \beta_S \Delta D_{S,ij} + \delta' \Delta X_{ij} + \Delta\epsilon_{ij} \tag{11}$$

However, we still only use the differences in the outcome and control variables between the 2004-5 and 2011-12 surveys. We now estimate propensity scores for four types of treatment using a modified version of Equation 4 and Equation 5. To estimate propensity scores, we use the 2004-5 levels, as before. Since the very long-term access group is so large and covers so many years when the households could have been connected, it is possible that households within this group are very different from each other, and some households in this group may have characteristics that are more similar to households our long- or short-term treatment groups. For instance, households that were electrified in the late eighties or early nineties may have characteristics closer to a household connected in the late nineties (which is in a different category), rather than a household connected in the sixties (which is in the same category). This makes it difficult to estimate propensity scores from characteristics and could lead to a suboptimal estimation of propensity scores.

Our second robustness check explores whether our results are sensitive to the data we use in propensity score estimation. We do this by using 1994-5 levels of the variables to estimate propensity scores. In this case, the characteristics that we use may be the characteristics that are actually used by governments and decision-makers to connect households at later dates.

There are some drawbacks, however of using data from 1994-5 to estimate propensity scores. The panel from 1994-5 comes from the HDPI survey which does not have data on village-level characteristics nor some important household-level variables, such as per capita expenditure, which is found to be a statistically significant determinant in our main analysis (Table 11). Secondly, the assignment of connections between 2004-5 and 2011-12 may be influenced very weakly by 1994-5 characteristics and thus, the short-term access group and the control group may not differ much based on their characteristics as of 1994-5.

The variables we use in the first stage regression in this analysis are the 1994-95-levels of the households' agricultural land holding, agricultural income, the ownership of a house, adequacy of drinking water, the presence of a separate kitchen in the household, the presence of a toilet in the household, the number of adult men, and the number of adult women. We also use the caste composition of the village in the 2001 census - which includes the fraction of households that belong to Brahmins, other forward castes, other backward castes, scheduled caste, and scheduled tribe families. The rationale for using data from 2001 is that the caste composition of villages is unlikely to have changed significantly in the period between 1994-95 and 2001.

Once the propensity scores have been estimated, the rest of the analysis is identical to our main approach.

6.2 Results: Very Long-term Connections

Table 4 shows the results for the six outcome variables. These results are somewhat similar to our main results. Per capita consumption and education have some statistically significant results, while agricultural income, agricultural land-holding, and kerosene consumption do not. While the time spent in fuel collection does not show a statistically significant response, both the long-term and very long-term effects are much larger in absolute value than the short-term effect and close to the main results in magnitude. Both the coefficients are somewhat smaller and the standard error for long-term connection is somewhat larger than in Table 3. But there are no dramatic differences between the two. There are some other differences between our main analysis and the robustness tests. The inclusion of very-long term connections

	Δ Outcome Variables					
	Log per capita Consumption (Rs.)	Log Agricultural Income (Rs.)	Log Agricultural Land Holding (Acres)	Schooling of Highest Educated Adult (years)	Kerosene Consumption (liters per month)	Time spent by Women in Fuel Collection (minutes per week)
	$N = 7892$	$N = 4269$	$N = 4596$	$N = 7881$	$N = 7890$	$N = 2928$
Intercept	0.1905***	0.1559	-0.4722	0.0727	-0.1207	138.8177**
	(0.0685)	(0.2232)	(0.2520)	(0.2503)	(0.2566)	(59.3044)
Short-term Access	-0.0158	0.1101	0.0414	0.4415	0.0573	28.5557
	(0.0826)	(0.1809)	(0.1416)	(0.2745)	(0.2193)	(53.2977)
Long-term Access	0.0598	0.0730	0.0427	0.6384**	0.0038	-74.9955
	(0.0616)	(0.1620)	(0.1907)	(0.2726)	(0.2177)	(60.1571)
Very Long-term Access	0.1450**	-0.0103	0.0830	0.4457*	0.0129	-77.1719
	(0.0615)	(0.1467)	(0.1887)	(0.2551)	(0.2089)	(55.7566)

Table 4: Robustness Check I: Time-associated causal effects of electrification on consumption, including households with very long connections. Estimates using propensity-score-weighted-difference-in-differences. Robust standard errors clustered at the village level.
*Significant at the 10% level, **Significant at the 5% level, ***Significant at the 1% level

reduces the estimated magnitudes of the coefficients of short-term and long-term connections on consumption, rendering them statistically insignificant. However, the effect of very long-term connections is similar to that estimated for long-term connections in our main analysis. So, the idea that impacts increase with the length of connection is not refuted.

In contrast, the impact on the schooling of the highest-educated adult is magnified by the inclusion of households connected before 1994-95. This is also the only variable that does not show a monotonic increase or decrease, with maximal impact for long-term connections and similar size effects for short-term and very long-term access. Possibly, the smaller effect on households with very long-term connections may be because these households have already reaped benefits over a long period, and there may be little left to improve, compared to households that had not been connected until much more recently. Overall, the similarity between the two sets of results implies that our results are fairly robust, with some minor concessions.

6.3 Results: Selection Bias Due to 1994-95 Characteristics

Table 5 presents the results of the second robustness check. For all variables except per capita consumption, the results agree strongly with the main results, finding statistically significant long-term impacts on education and the time burden of fuel collection, but no statistically significant impact on agricultural income, agricultural land-holding, and kerosene consumption.

With respect to the change in the years of schooling for the highest educated adult, using 1994-95 propensity scores yields a short-term effect of a 0.32-year increase that is not statistically

	Δ Outcome Variables					
	Log per capita Consumption (Rs.)	Log Agricultural Income (Rs.)	Log Agricultural Land Holding (Acres)	Schooling of Highest Educated Adult (years)	Kerosene Consumption (liters per month)	Time spent by Women in Fuel Collection (minutes per week)
	$N = 3739$	$N = 1882$	$N = 1968$	$N = 3731$	$N = 3738$	$N = 1675$
Intercept	0.1136*	0.1021	-0.5182	0.2702	-0.2140	162.0078**
	(0.0676)	(0.1626)	(0.3302)	(0.2355)	(0.2203)	(68.8091)
Short-term Access	0.0808*	0.1368	0.0018	0.3222	0.0246	-10.4996
	(0.0462)	(0.1477)	(0.2131)	(0.2417)	(0.1645)	(52.9152)
Long-term Access	0.0666*	0.0940	-0.1149	0.4594**	-0.1028	-79.6033**
	(0.0371)	(0.1172)	(0.2080)	(0.2215)	(0.1389)	(36.4303)

Table 5: Robustness Check II: Time-associated causal effects of electrification on consumption, with first stage estimates from 1994-5 characteristics. Estimates using propensity-score-weighted-difference-in-differences. Robust standard errors clustered at the village level.
*Significant at the 10% level, **Significant at the 5% level, ***Significant at the 1% level

significant and a long-term effect of 0.46-year increase that is statistically significant at the 5% level. Notably, these numbers are similar to our main analysis where the short-term increase is found to be 0.36 years and the long-term increase is found to be 0.43 years. Similarly, we find a statistically significant (at the 5% level) long-term reduction relative to the control group in the time spent by women in fuel collection by approximately 80 minutes per week and a short-term insignificant impact of a 10-minute reduction. As in the case of education, these effects are similar to our primary results which show a short-term reduction of close to 8 minutes per week and a long-term effect of approximately 90 minutes per week.

The only variable where the robustness check does not entirely agree with our primary results is per capita consumption. While we found, using the 2004-5 propensity scores, that there was a large statistically significant long-term effect of an 18 percentage-point-increase and a small statistically insignificant short-term increase of 6.3 percentage points, weighting by the 1994-95-propensity-scores yields both short-term and long-term effects that are significant at the 10%-level. Additionally, the short-term effect (8.4 percentage points) is found to be marginally greater than the long-term effect (6.9 percentage points) although the long-term effect is found to be slightly more statistically significant. One reason for this may be the absence of expenditure data in the estimation of the propensity scores, because of which poorer households may not have been weighted as much, and therefore the estimates may not be as large and are found to be closer to the simple DID estimates (see Appendix D).

7 Conclusions

We use India's IHDS and HDPI surveys conducted between 1994-95 and 2011-12 to estimate whether the long-term (7-17 years) benefits of connection to the electricity grid are greater than the short-term (up to 7 years) benefits. We find that the increase in household consumption between 2004-5 and 2011-12 is 18 percentage points greater for households connected between 1994-5 and 2004-5 than for those that were still not connected in 2011-12. Households that were connected between 2004-5 and 2011-12 did not have significantly more income growth than the control group. We found similar but less statistically significant results for education of the most educated adult. Similarly, time spent by women in fuel collection increased significantly less in households connected in the earlier period. We address endogeneity issues using a difference in differences design and inverse propensity score weighting. In contrast to some previous studies, we are careful to only include never connected households in our control group. These results are robust in broad outline to also include households that were connected prior to 1994-95, some of whom may have been connected for a very long time, and to using alternative data to construct propensity scores.

With no statistically significant change in kerosene consumption and an increase in time spent by women in fuel collection (though also not statistically significant), our results show fuel stacking (Cheng and Urpelainen, 2014) rather than ascent of the fuel ladder (van der Kroon et al., 2013).

The results have implications for RCTs that typically collect data from participants after only a relatively short time. Significant benefits will take time to be realized. This may be because households need to gradually acquire appliances to use electricity and the income and savings to purchase them. Ownership of all appliances increased between 2004-5 and 2011-12 among the households that were electrified between 1994-95 and 2004-5. Similarly, education is a slow process and benefits will only be realized over time. Our results support the findings of our meta-analysis of previous studies that found that the duration of connection rather than the type of study determined whether they found statistically significant benefits of electricity access.

Our results also show that per capita expenditure grew more rapidly the greater the share of households that were connected in a village at the start of our period of analysis. This suggests that there are spillovers from connected households. As these benefits are related to the number of connected households at the beginning of the period rather than the change in households, we cannot simply add the two effects together. Instead, they imply that our headline estimate of the benefits of connection is an underestimate. These additional benefits should also be taken into account in future analyses, whether observational or experimental.

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A Meta-Analysis

To test whether the difference in impacts measured by experimental and observational studies may be due to a confounding duration of connection variable, we compare the results of different impact evaluation studies and the duration of the connection in each case. To avoid biases and facilitate reproducibility, we use articles from the systematic review conducted by Bayer et al. (2020) on impact evaluations between 2000 and 2020. Of the 31 studies shortlisted in it, we choose those studies which have data on the duration of the connection.

Experimental studies, RCTs in particular, typically have information on the duration of the experiment, which serves as a direct measure for the duration of the connection. There are seven such studies. Among observational studies, we select the eight studies which use difference-in-differences (DID) methods, and we estimate the duration of the connection as half the time between the cross-sections (median time), since there is seldom information regarding the exact time at which households were electrified. We also use an observational study that does not use experimental or DID methods, but has data for the duration of the connection, taking the tally of our sample to nine observational studies and 16 studies in total.

Study	Methodology	Technology	Duration of Connection (years)	Positiveness of Impact
Aevarsdottir et al. (2017)	Experiment	off-grid	1	0.8
Aklin et al. (2017)	Experiment	off-grid	1.42	0.2
Barron and Torero (2017)	Experiment	grid	4	1
Bensch et al. (2011)	Observational	off-grid	4	0.67
Chakravorty et al. (2014)	Observational	grid	5.5	1
Chaplin et al. (2017)	Observational	grid	4	1
Cook (2005)	Observational	grid	4.5	0.5
Furukawa (2014)	Experiment	off-grid	0.42	0
Grimm et al. (2015)	Observational	grid	1	0
Grimm et al. (2017)	Experiment	off-grid	0.5	0.67
Imai and Palit (2013)	Observational	grid	12	1
Khandker et al. (2013)	Observational	grid	1.5	1
Kudo et al. (2019)	Experiment	off-grid	1.33	1
Lee et al. (2020b)	Experiment	grid	2.58	0.25
Lenz et al. (2017)	Observational	grid	2	0.67
van de Walle et al. (2017)	Observational	grid	8.5	1

Table 6: List of studies used in the meta-analysis to study the impact of methodology, duration, and type of connection on the positiveness of impact. Positiveness of impact is the average impact on the various outcome variables in a study after assigning 1 to a positive impact, 0 to a neutral impact, and -1 to a negative impact. There are a total of 16 studies.

We consider the same five outcome variables as Bayer et al. (2020): total income/expenditure, savings, energy expenditure, business creation, and education. The authors cite their rationale for choosing these variables as that they have been frequently assessed at the household level. We then denote positive impacts as 1, neutral impacts as 0, and negative impacts as -1. Lastly, we average over all the measured outcomes in each study, to avoid biasing our analysis by discriminating between studies and projects and giving more weight to studies that measured more outcomes. We call the averaged quantity the Positiveness of Impact. For studies with only non-negative impacts, this quantity is the same as the frequency of a non-neutral impact.

Among the 16 selected studies, the seven experimental studies had an average duration of connection of 1.61 years, while the nine observational studies had an average duration of connection of 4.77 years, which is nearly three times as large. Furthermore, we can observe in Figure 1 that while studies with a low duration of connection reported both positive and neutral results, studies with long durations reported only positive results.

We then use a simple linear regression to study the effects of methodology, technology, and duration of connection on the positiveness of impact of the i^{th} study y_i:

$$y_i = \alpha + \beta_1\ METHOD_i + \beta_2\ TECHNOLOGY_i + \beta_3\ LOG\left(DURATION_i\right) + \epsilon_i \tag{12}$$

Outcome: Positiveness of Impact	OLS (1)	(2)	(3)	(4)
Intercept	0.4976***	0.5596***	0.5557***	0.5389***
	(0.1078)	(0.1409)	(0.1533)	(0.1464)
Logarithm of Duration of Connection (Years)	0.4905**			0.5755*
	(0.1998)			(0.2876)
Methodology (Observational = 1, Experimental = 0)		0.2009		-0.0128
		(0.1879)		(0.2353)
Technology (Grid = 1, Off-grid = 0)			0.1863	-0.1029
			(0.1939)	(0.2486)

Table 7: Determinants of Impact of Electrification. Sample of 16 impact evaluation studies. Each cell presents the coefficient (and standard error) measured in the respective regression.
*Significant at the 10% level, **Significant at the 5% level, ***Significant at the 1% level

Table 7 presents the results of four regressions. We find that when each explanatory variable is used separately, only duration of connection has a statistically significant effect (at the 5% level). Additionally, when all three explanatory variables are used together, the duration of connection is the only variable that has positive effect which is statistically significant at the 10% level ($p = 0.069$). Given the small size of the sample, we would not expect very statistically significant results. This, however, means that after controlling for duration, methodology is, effectively, of no consequence to the positiveness of impact. On the other hand, off-grid projects tend to find a higher positiveness than grid projects (although this is not significant).

B Additional Electricity Statistics

B.1 Household Electrification Statistics

Table 8 presents descriptive statistics at the household level. Among the 9,233 households, 4,746 households (51.4%) already had electricity connections prior to the 1994-95 wave. By 2004-5, 6,256 households (68.27%) out of 9,163 households had electricity (there was no data for 70 households), and 7,766 households (84.39%) out of 9,202 households were connected by 2011-12, leaving 1436 households (15.61%) unelectrified at the end of the period. By contrast, over 93% of the villages that these households belong to had been connected by 2004-5 and close to 99% of the villages had grid access by 2011-12.

	Means (standard deviation) 1994-95 (1)	2004-5 (2)	2011-12 (3)	Δ (3)-(2)
Electricity Access†	0.51	0.68	0.84	0.16
	(0.50)	(0.47)	(0.36)	(0.46)
Reliability (Hours of Access in a Day)[1]		14.79	14.08	-0.45 [3]
		(6.68)	(6.69)	(7.50)
Pay for electricity†[1]		0.86	0.81	-0.03 [3]
		(0.34)	(0.39)	(0.45)
Pay to the company†[1]		0.79	0.72	-0.03 [3]
		(0.41)	(0.45)	(0.52)
Monthly Expenditure on Electricity [2] (Rs.)		348.84	259.60	-56.61 [3]
		(486.03)	(364.82)	(505.90)

Table 8: Descriptive Statistics for Household Electrification 1994-95, 2004-5 and 2011-12. Includes 9,233 rural unsplit households. The data are unweighted. Source: HDPI, IHDS I, and IHDS II surveys.

[1] Among those connected, [2] Among those who pay for the connection, [3] Households already connected by 2004-5. † Dummy variable which takes 1 for "yes" and 0 for "no"

The mean and the standard deviation of reliability changed little between 2004-5 and 2011-12. However, despite the marginal drop in mean reliability, the standard deviation in the change of reliability for households connected by 2004-5 is unusually large, illustrating that some households with very poor reliability could have seen their reliability improve considerably, and households with good reliability may have seen a decline in quality. Average household expenditure on electricity drops considerably (29.17%) between the two periods, which could imply that poorer households had received access to electricity. It is also evident that fewer households were paying for their electricity connection to electricity distribution companies, and a larger fraction of households were connected who did not receive bills or were connected by government schemes. Furthermore, since the average decrease in payers among those who were already connected to the grid is lower than the decrease overall, a large fraction of new connections may have been connected by government schemes, which makes this period ideal for impact evaluation.

B.2 Village Sizes and Electrification

Table 9 presents village electrification statistics for villages of different sizes. It suggests that the size of the village (in terms of population) may have played a role to play in the fraction of households connected. While there is no observable trend in the relationship between whether a household was already connected by 2004-5 and the size of the village, the percentage increase in electrified households in small villages (with a population of less than 1000 in the 2001 census) was almost twice as much as the increase in larger villages. This factor may, therefore, may have influenced whether a household was more likely to receive treatment in this period, and should be included the propensity score estimate.

		Means (standard deviation)		
		Electrified Households in Village (%)		
		2004-2005	2011-2012	Δ
		(1)	(2)	(2) - (1)
Population of Village	More than 5000	68.12	76.57	8.45
		(33.19)	(28.00)	(32.21)
	1001-5000	70.30	79.02	8.86
		(32.97)	(27.31)	(29.13)
	Less than 1000	65.18	81.79	16.50
		(36.87)	(24.39)	(32.41)

Table 9: Variation in Village Electrification with Size 2004-2005 and 2011-2012. Includes 725 villages. The data on sizes are from IHDS I, which uses the 2001 census (village). The data are unweighted. Source: HDPI, IHDS I, and IHDS II surveys.

C Spillover Effects

Table 10 presents the results of the spillover effect analysis. We test the effect for both logs and levels, in each case estimating separate spillover effects for households with short-term, long-term, and no access (Column 1), the average spillover effect on all households (Column 2), and the spillover effect on unconnected households alone (Column 3). When we look at the indirect effects on log per capita expenditure in households with short-term, long-term, and no access separately, we find that there is no significant impact of the change in the fraction of households. There is, however, a significant effect on the level of consumption for households with short-term connections.

However, we find a statistically significant (1% level) lagged effect on log expenditure for households with short-term connections of 0.3% for every 1% more households electrified by

$N = 3739$	Δ per capita household expenditure (2012 Rs.)					
	Logs			Levels		
	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	0.2030*	0.2288**	0.2219*	4418.4986	4091.0718*	4581.2795
	(0.1213)	(0.1165)	(0.1308)	(3585.7597)	(2468.0257)	(3550.8934)
Spillover Effect (Long-term)	-0.0025*	-0.0003		-34.4893	12.5400	
	(0.0013)	(0.0009)		(34.9205)	(23.2731)	
Spillover Effect (Short-term)	0.0011	-0.0003		43.7219**	12.5400	
	(0.0008)	(0.0009)		(21.0783)	(23.2731)	
Spillover Effect (No access)	0.0012	-0.0003	0.0012	33.8212	12.5400	26.2854
	(0.0013)	(0.0009)	(0.0011)	(42.7625)	(23.2731)	(41.0778)
Lagged Effect (Long-term)	0.0020	0.0020*		96.2827*	65.4751**	
	(0.0015)	(0.0011)		(54.5602)	(32.9458)	
Lagged Effect (Short-term)	0.0030***	0.0020*		66.5479***	65.4751**	
	(0.0009)	(0.0011)		(25.4258)	(32.9458)	
Lagged Effect (No access)	0.0031	0.0020*	0.0025	65.1700	65.4751**	43.3804
	(0.0020)	(0.0011)	(0.0019)	(66.3401)	(32.9458)	(65.4548)

Table 10: Spillover effects of rural electrification on households without access. Spillover effect is the effect of the change in the fraction of households connected to the grid. Lagged effect is the effect of the level in 2004-5. Robust standard errors clustered at the village level in parentheses. *Significant at the 10% level, **Significant at the 5% level, ***Significant at the 1% level

2004-5. The lagged effect is similar for households with no access (0.31% increase), but this is not statistically significant as the standard errors are large. In levels, there are significant effects for households with both short- and long-run connections. Likewise, when we assume spillover effects only impact households without electricity access, however, we find that the effect is not statistically significant effect. Assuming a common effect, we find no statistically significant impact of the change in the fraction of households connected to the grid on the per capita consumption of households, but we do find a positive lagged effect of village electrification. For every 1% more households connected in a village, prior to the survey, there is a 0.2% increase (significant at the 10% level) in the per capita consumption of households. From the levels regression, we find that a 1% increase in the fraction of electrified households prior to the survey increases the per capita expenditure of a household by over Rs.65 (significant at the 5% level).

One reason why the standard errors of the coefficients estimated for households without access are larger may be because the set of households without access is the smallest of the three groups with only 777 out of 3739 households. Furthermore, given that the coefficients of the spillover and lagged effects on households with no access are so similar to the coefficients for households with short-term access, the possibility of all households experiencing a common effect may be realistic.

Khandker et al. (2009) found similar results in Bangladesh. But, despite finding positive spillover effects, our results do not agree with those of van de Walle et al. (2017). While they find that there is a positive spillover effect of the number of years that the village has been connected (which we use as a control variable), we find that this variable has no statistically significant effect. This may be because Van de Walle et al. considered only one channel for spillover effects - the number of years since the village was first connected, while we consider multiple factors such as the fraction of households with access in the village, the quality of electricity available to the village and the years since the village was first connected. It is likely that the length of time that a village has been connected is positively correlated with the fraction of households connected. For instance, in our regression sample for consumption, the correlation between the fraction of households connected and the years since the village was first connected has a correlation coefficient of 0.2752 ($p = 5.97 \times 10^{-66}$). As a result of this, van de Walle et al.'s analysis may suffer from omitted variables bias. Thus the time since the village was first connected may show an impact in their study but not in ours where each aspect of electrification is considered individually.

D First Stage Results

Table 11 presents the results of the multinomial logistic regression analysis used in the calculation of the propensity scores. The sample is the same as that used in the difference in differences analysis of the logarithm of per capita consumption. The results show that several of the variables have a significant effect in determining which households receive the treatments - short-term and long-term connections, while the control is the reference level. We might expect particularly significant coefficients for the long-term access category as these households may have already benefited from short-term gains of electrification. However, we observe that there are also statistically significant coefficients for the assignment to short-term electricity access confirming that the assignment was non-random.

We find that the logarithm of total household consumption is an important determinant of the assignment to treatment. Both the coefficients of the pre-treatment log of total household consumption, for short-term and long-term assignments, are significant at the 0.1% level, with the coefficient greater for long-term connections, perhaps due to a compounded effect of more affluent households being connected earlier, and gaining additional benefits from electricity access. Other variables that were statistically significant determinants of both short-term access and long-term access were proximity to banks and credit cooperatives, proximity to markets and shops, the fraction of households in a village that were already connected to the grid by 2004-5, the fraction of the population of the village that belonged to the Brahmin caste - all significant at least at the 1% level for both short-term and long-term access categories. Caste seemed to be a particularly important determinant for short-term access where the fraction of Brahmins, other forward castes, scheduled tribes (ST), scheduled castes (SC), and other backward castes (OBC) are all statistically significant determinants of short-term connections, increasing the propensity to being connected over villages with a higher presence of those not classified by these groups. The baseline group may be Hindus who do not fall into these caste groups or members of other religions who do not identify as members of the mentioned groups.

Note that the results presented in Table 11 are for estimates on the sample of households for which we have data on the log of per capita household consumption. Other outcome variables use slightly different samples, which may at times even be influenced by different factors. For instance, agricultural households which roughly make up the set of households for which we have data on agricultural income and land holding may have their decision to get connected determined slightly differently, when compared to the full set of households. Thus, there could be slight variations in the coefficients and significance measured for each independent variable across samples. Therefore, we estimate propensity scores separately for each outcome variable's sample.

E Unweighted Difference-in-Differences Results

Table 12 presents the results of a simple unweighted DID regression of Equation 3. We find no significant impact of short-term or long-term access to electricity on per capita consumption, agricultural land holding, or kerosene consumption. We find a borderline significant (10% level) short-run impact on agricultural income, even though the effect is large in magnitude (about a 31% increase). Similarly, we find a weak long-term effect on the years of schooling, which is significant at the 10% level. The only variable with a highly significant response to access is the time spent by women in fuel collection, which shows a reduction of over 80 minutes per week with long-term access, significant at the 5% level.

Comparing these results to those in Table 3, we can see that most of the estimates change substantially after weighting by the inverse of the propensity scores. While there is no statistically significnat impact on agricultural land holding and kerosene consumption even after

Independent variable	Estimate (Standard Error)	
	Short-term electricity access	Long-term electricity access
Intercept	6.570×10^{-1}	$-1.012\times10^{01***}$
	(1.089×10^{00})	(8.916×10^{-1})
Pre-treatment level of log-total household consumption (2012 Rs.)	$-3.249\times10^{-1***}$	$6.247\times10^{-1***}$
	(8.999×10^{-2})	(7.517×10^{-2})
Pre-treatment level of the number of adult men in the household	-3.059×10^{-2}	5.044×10^{-2}
	(6.593×10^{-2})	(5.522×10^{-2})
Pre-treatment level of the number of adult women in the household	-3.407×10^{-2}	7.614×10^{-2}
	(7.793×10^{-2})	(6.506×10^{-2})
Pre-treatment level of water source presence inside the house	2.190×10^{-1}	$2.825\times10^{-1**}$
	(1.204×10^{-1})	(1.014×10^{-1})
Pre-treatment level of flush toilet presence in the house	-2.948×10^{-2}	$4.745\times10^{-1***}$
	(1.008×10^{-1})	(7.314×10^{-2})
Pre-treatment level of separate kitchen in the house	4.783×10^{-2}	-5.786×10^{-2}
	(6.426×10^{-2})	(5.613×10^{-2})
Pre-treatment level of metalled road presence in the village	$2.274\times10^{-1**}$	9.667×10^{-2}
	(8.160×10^{-2})	(6.935×10^{-2})
Pre-treatment level of the number of government primary schools	1.503×10^{-2}	-2.199×10^{-2}
	(3.055×10^{-2})	(2.971×10^{-2})
Pre-treatment level of the number of private primary schools	6.768×10^{-2}	$-1.272\times10^{-1**}$
	(4.836×10^{-2})	(4.548×10^{-2})
Pre-treatment level of the number of government middle schools	-1.453×10^{-1}	-1.312×10^{-1}
	(1.042×10^{-1})	(8.712×10^{-2})
Pre-treatment level of the number of private middle schools	$-2.449\times10^{-1*}$	-2.788×10^{-2}
	(1.134×10^{-1})	$9.054\times10^{-2})$
Pre-treatment level of the number of government secondary schools	3.386×10^{-1} *	$2.695\times10^{-1*}$
	(1.410×10^{-1})	(1.182×10^{-1})
Pre-treatment level of the number of private secondary schools	-3.991×10^{-1} *	1.179×10^{-1}
	(1.826×10^{-1})	(1.141×10^{-1})
Pre-treatment level of the number of government higher secondary schools	1.194×10^{-1}	-1.089×10^{-1}
	(2.175×10^{-1})	(1.860×10^{-1})
Pre-treatment level of the number of private higher secondary schools	-1.518×10^{-2}	1.612×10^{-3}
	(6.555×10^{-2})	(2.904×10^{-2})
Pre-treatment level of the distance to the closest bank/credit cooperative	$2.306\times10^{-2**}$	-9.433×10^{-3}
	(8.704×10^{-3})	(9.156×10^{-3})
Pre-treatment level of the distance to the closest shop/market	$-6.236\times10^{-2***}$	2.863×10^{-4}
	(1.426×10^{-2})	(1.065×10^{-2})
Pre-treatment level of the presence of NGOs in the village	-7.097×10^{-2}	-1.652×10^{-1}
	(1.472×10^{-1})	(1.343×10^{-1})
Pre-treatment level of the presence of primary healthcare centers in the village	5.297×10^{-2}	7.829×10^{-2}
	(1.628×10^{-1})	(1.337×10^{-1})
Pre-treatment level of the fraction of households in the village with electricity access (%)	$-8.480\times10^{-3***}$	$2.138\times10^{-2***}$
	(1.655×10^{-3})	(1.463×10^{-3})
Years since Village was connected	-3.748×10^{-3}	$7.307\times10^{-3*}$
	(4.237×10^{-3})	(3.600×10^{-3})
Whether the Village is small	-1.355×10^{-1}	-3.552×10^{-2}
	(1.285×10^{-1})	(1.093×10^{-1})
Whether the Village is big	2.195×10^{-1}	-2.463×10^{-1}
	(1.516×10^{-1})	(1.402×10^{-1})
Fraction of population Brahmin (%)	$3.702\times10^{-2***}$	$1.590\times10^{-2**}$
	(7.426×10^{-3})	(6.013×10^{-3})
Fraction of population Forward (%)	$2.352\times10^{-2***}$	6.724×10^{-3}
	(5.601×10^{-3})	(4.163×10^{-3})
Fraction of population OBC (%)	$2.442\times10^{-2***}$	$1.420\times10^{-2***}$
	(5.556×10^{-3})	(4.078×10^{-3})
Fraction of population SC (%)	$2.009\times10^{-2**}$	$1.324\times10^{-2**}$
	(6.166×10^{-3})	(4.735×10^{-3})
Fraction of population ST (%)	$2.093\times10^{-2**}$	$1.672\times10^{-2**}$
	(5.753×10^{-3})	(4.331×10^{-3})

Table 11: Results of the first-stage regression for the estimation of coefficients of independent variables, in the households being assigned short-term and long-term treatment. The reference level of the estimates is the control group which still lacks access. The sample of households used in the estimation includes households used to estimate the impact of electricity access on consumption.

*Significant at the 5% level, **Significant at the 1% level, ***Significant at the 0.1% level

	Δ Outcome Variables					
	Log per capita Consumption (Rs.)	Log Agricultural Income (Rs.)	Log Agricultural Land Holding (Acres)	Schooling of Highest Educated Adult (years)	Kerosene Consumption (liters per month)	Time spent by Women in Fuel Collection (minutes per week)
	$N = 3739$	$N = 1882$	$N = 1968$	$N = 3731$	$N = 3738$	$N = 1675$
Intercept	0.1106*	-0.0066	-0.6100*	0.2852	-0.2116	143.2309**
	(0.0633)	(0.1465)	(0.3183)	(0.2136)	(0.2217)	(64.8349)
Short-term Access	0.0675	0.2699*	0.0710	0.3282	-0.0375	-27.5712
	(0.0451)	(0.1382)	(0.1822)	(0.2157)	(0.1648)	(55.2097)
Long-term Access	0.0349	0.1190	-0.1110	0.3043*	-0.1675	-80.9539**
	(0.0346)	(0.1058)	(0.1777)	(0.1798)	(0.1350)	(37.6996)

Table 12: Time-associated causal effects of electrification on consumption. Estimates using simple difference-in-differences. Robust standard errors clustered at the village level.
*Significant at the 10% level, **Significant at the 5% level, ***Significant at the 1% level

propensity score-weighting, the effect of short-term access on agricultural income falls to a fifth of the unweighted estimate, rendering it statistically insignificant. In addition, the two variables which showed significant impacts in the long run – education and time spent in fuel collection – see their coefficients increase in magnitude. The greatest change with propensity score weighting is for the per capita consumption variable where the weighting brings about an extremely significant long-term impact of around an 18% increase in per capita consumption.

A major reason for the propensity-score-weighted regression showing larger long-term benefits may be that poorer households are less likely to get connected to the grid earlier (as seen in Table 11 in Appendix D), and therefore, they are weighted more heavily in the weighted regression. Poor households are also likely to be slower to extract the full benefits from electricity as they may take longer to be able to afford the necessary appliances and capital to make optimal use of the electricity. Consistent with this hypothesis, we did not find a large significant impact of long-term connection when we do not use the level of consumption in estimating propensity scores, which was the case when we estimated effects using only village-level characteristics to control for selection bias in assignment to treatment.

The fact that some variables do better after accounting for selection bias implies that the households which benefit the most after electrification are the ones that are, unfortunately, least likely to be electrified. This has important policy consequences and indicates that progressive electrification programs which target poorer households may yield larger improvements in household well-being and human development.