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Fertility and Rural Electrification in Bangladesh

Abstract

We use a household-level panel dataset from Bangladesh to examine the household-level relationship between fertility and the access to electricity. We find that the household's access to electricity reduces the change in the number of children by about 0.1 to 0.25 children in a period of five years in most estimates. This finding also applies to retrospective panel data and is robust to the choice of covariates and estimation methods. Our finding passes falsification test and corroborates with the predictions of our theoretical model on the households' time use and consumption pattern.
JEL classification codes: O20, J13

Keywords: Bangladesh, infrastructure, television, difference in differences, propensity score matching, retrospective panel data

1 Introduction

Access to electricity is essential for development. Provision of welfare-enhancing utilities such as clean water supplies, improved sanitation, and modern health care services can be delivered efficiently with electricity. Electricity enables households to enjoy reliable and efficient lighting and heating equipments, improved cooking facilities, robust mechanical power, better transport and telecommunications services, overall a modern lifestyle. However, we still live in a world where nearly 1.2 billion people currently lack basic access to electricity,[1] particularly in rural areas of developing countries. Over a third of this population lives in South Asia.

In development literature, electricity is found to have impact on income, employment, female empowerment, education and reduction of pollution, to name a few as discussed further in Section 2. One relatively unexplored area of impact is the fertility, which electrification may affect through several potential channels. First, access to electricity alters the way time is used, because electrified households can use additional lighted hours for productive purposes. Second, access to electricity may change consumption patterns, which in turn affect fertility behavior. That is, electricity enables households to enjoy an array of electric appliances and this may induce households to shift resources away from consumption related to children to the goods that operate with electricity. Third, electrification may also create new income opportunities for households, which in turn, may alter the opportunity cost of time, especially for women. Finally, access to electricity makes it easier for households to obtain information through the use of technology such as TV and mobile phones. This may in turn change fertility behavior.

Fertility link of electrification is very important, especially for developing countries, as high fertility rates may result in a lack of human capital investment, which in turn reduces the quality of human resources and may lead to youth unemployment. In the development literature, high fertility is regarded as one of the most important factors hindering long-term economic development (See, for example, Ashraf et al. (2013)). However, academic literature in economics paid little attention to this link, as there exist only a handful of studies on the impact of electrification on fertility.

[1] WEO 2016 Electricity Access Database (`http://www.worldenergyoutlook.org/resources/energydevelopment/energyaccessdatabase/`). Accessed on July 4, 2017.

To fill in this lacuna, we rigorously examine the relationship between fertility and the household's access to electricity using a household-level panel dataset from Bangladesh. Using difference-in-differences (DID) regressions, which control for both observable and unobservable time-invariant household characteristics and observable time-variant characteristics, we find that the access to electricity leads to a statistically and economically significant reduction of fertility by 0.1-0.2 children in a five-year period. This finding remains consistent even when we address the potential endogeneity problem of electricity access by instrumenting the household's access to electricity by the management efficiency of electricity cooperative and the village-level electrification status. Further, we alternatively consider change-on-level specification, which is not affected by the potential presence of the factors that simultaneously affect the access to electricity and fertility. Because the distribution of observable characteristics are different between households, with and without access to electricity, we also use propensity score matching (PSM) for robustness checks. Our main finding remains unchanged under alternative methods and specifications. In addition, we propose a falsification test using a sample of women who have passed their reproductive age. Our results pass these tests, which strengthens the credibility of our results. In exploring the mechanism behind such a finding, we provide suggestive evidence that the negative impact of electrification on fertility partly comes from the increased use of TV.

This paper makes a couple of noteworthy contributions to the literature. First, we use a household-level panel dataset and also construct a retrospective panel dataset to investigate the impact of rural electrification. To the best of our knowledge, this is the first paper that used the household level panel datasets, under the DID estimation setting with time-invariant fixed effects—along with instrumental variables—to address the endogeneity issue of electricity adoption.

Second, we develop a simple household model that incorporates the household's fertility behavior, consumption, and time use. While we make some strong assumptions about the household behavior, the model offers a joint prediction on the direction of change in fertility, time use, and consumption of non-child goods in response to the adoption of electricity. Hence, our theoretical setting allows us to test whether our finding is valid.

This study is organized as follows. Section 2 provides a review of related literature. Section 3 briefly provides some relevant background information on rural electrification in Bangladesh. Section 4 develops a simple model of fertility and electrification. Section 5 describes the data and presents key summary statistics. Section 6 discusses the econometric specifications. Section 7 presents the estimation results. Section 8 offers some discussion.

2 Review of related literature

In recent years, the role of electrification in development has been emphasized in many academic studies. Researchers have found evidence that electrification is associated with income generation and employment creation in Benin (Peters et al., 2011), improved income and educational outcomes in Bangladesh (Khandker et al., 2009a) and in Vietnam (Khandker et al., 2009b), development of manufacturing sector in India (Rud, 2012) and in Brazil (Lipscomb et al., 2013), and improved female employment in South Africa (Dinkelman, 2011) and in Nicaragua (Grogan and Sadanand, 2013). Other impacts of electrification include reduced indoor air pollution (World Bank, 2008), ameliorated medical services (Bensch et al., 2011), increased housing values (Lipscomb et al., 2013), and uptake of modern cooking fuels (Heltberg, 2003, 2004).

While there have been some studies in demography that indicate the existence of a causal link between rural electrification and fertility (e.g., Herrin (1979) and Harbison and Robinson (1985)), rigorous econometric studies on the impact of electrification on fertility have not been available until recently. Some recent studies have explored this topic using aggregate data in developing countries. For example, using microregion-level data in Brazil, Potter et al. (2002) find a strong and consistent relationship between declines in fertility and electrification. Similarly, Grimm et al. (2015) use a pseudopanel data at the district level in Indonesia and find that electrification contributed to a reduction in fertility. They also find that electrification affects fertility through two important channels: exposure to TV and reduced child mortality. Similarly, Grogan (2016) estimates the impact of household electrification on fertility in Columbia and employment with a municipality-level panel dataset. They address the endogeneity of electrification rate using the distance to the nearest operating hydroelectric dam as a key instrumental variable.

However, microeconometric studies on electrification and fertility in developing countries are still limited. One of the first microeconometric studies on this topic is Peters and Vance (2011), who use a household-level dataset for Côte d'Ivoire. They find a negative association between fertility and availability of electricity among rural households but a positive association was found for urban households. They do not attempt to address the potential endogeneity of the availability of electricity.

Another microeconometric study we are aware of is Fetzer et al. (2016), who explore the short-run and long-run impacts of power outages on fertility in urban Columbia. Using a retrospective panel dataset, they find that the outages lead to higher fertility, causing mini baby booms. They also find that the increase in fertility due to the outages is not offset by having fewer children.

Our paper differs from Fetzer et al. (2016) in two important aspects. First, we study the impact of rural electrification and not a temporary shock in urban areas due to outages. Second, even though we also use retrospective panel data, our main analysis is based on proper panel data. Therefore, we can check whether the proper panel data and retrospective panel data lead to similar results. This is important because the retrospective panel data are constructed from some untestable assumptions, a point that is true for both Fetzer et al. (2016) and this study. Therefore, the availability of proper panel data substantially boosts the credibility of the analysis.

In addition to providing empirical evidence on the impact of electrification on fertility, we also develop a simple household model which allows us to make a joint prediction on the direction of change in fertility, time use, and consumption of goods not related to children. Because our data contain information on time use and consumption, we are also able to conduct a theory-based reality check.

Besides the above-mentioned studies, this study is also related to two strands of literature. First, this study ties in with the controversy over the potential causal relationship between modern household technology such as electric appliances and the onset of baby booms in the developed world in the macroeconomics literature. Some scholars argue that the spread of modern household technology can reduce the cost of having children, thereby increasing fertility (Greenwood et al., 2005a), and increase female labor force participation (Greenwood et al., 2005b; Cavalcanti and Tavares, 2008). However, Bailey and Collins (2011) find that levels/changes in county-level appliance ownership and electrification negatively predict levels/changes in fertility rates in the United States between 1940 and

1960, though they do not address the endogeneity of adoption of electricity and appliances (Greenwood et al., 2011). While the context we study is very different, our empirical results are at odds with Greenwood et al. (2005a). This may be explained by the initial level of income when electricity is introduced.

Second, this study also relates to a growing body of literature on the relationship between a specific type of infrastructure and development. Studies have explored the impact of dams (Duflo and Pande, 2007), transportation infrastructure (Fernald, 1999; Banerjee et al., 2012), and telecommunications infrastructure (Röller and Waverman, 2001) among others (See also Gramlich (1994) and Straub (2008) for a review of literature). By emphasizing the impacts of electrification on fertility that have largely been ignored, we underscore the importance of understanding the social impact of infrastructure.

3 Rural Electrification in Bangladesh

In Bangladesh, electricity is generated by five public and several other independent power producers, transmitted through the national grid, and then distributed to end users by different organizations, depending on the region and the purpose of the power usage. For the rural consumers of electricity, the Rural Electrification Board (REB) has been responsible for the distribution of electricity since its establishment as a semi-autonomous government organization in 1977. The REB's responsibility also includes planning and developing the distribution network for each expansion phase of rural electrification. As of January 2017, the REB serves nearly 16 million domestic end users.[2]

The rural electrification program under the REB has been viewed as one of the most successful government programs in Bangladesh (Khandker et al., 2009a) with substantially lower system losses than other major electricity distribution bodies (Alam et al., 2004) and an excellent bill collection record. One critical element of this success is the electricity distribution through rural electricity cooperatives called Palli Bidyut Samities (PBS), whose members are the consumers of electricity and participate in its policymaking through elected representatives serving on its governing body. PBSs own, operate, and manage the rural distribution system within its jurisdiction. The REB approves new PBSs and work with them by providing technical support and training, negotiating the purchase of power from providers, and approving tariffs. As of January 2017, there are 80 PBSs in Bangladesh and each PBS on average covers around 6 subdistricts (upazilas/thanas) and 820 villages.

The establishment of new PBSs depends on various REB's priority criteria such as road infrastructure, number of households, state of industrial and commercial development, existing social and community institutions, number of pumps, rice mills and tube wells for irrigation, and percentage of the area prone to flooding. Accessibility to the Bangladesh Power Development Board's 33kV line and adequate capacity at the grid substation are also considered necessary for the decision to establish a new PBS (Murphy et al., 2002). Therefore, the process of rural electrification is clearly not random. We will address thisp point further in Section 6.

[2] `http://www.reb.gov.bd/` accessed on June 16, 2016. The REB is now called Bangladesh Rural Electrification Board following the Rural Electrification Board Act, 2013. However, since our study period is before this change, we use REB throughout this paper.

4 Model of electrification and fertility

This section develops a simple model of electrification and fertility to underlie our econometric specification in the subsequent analysis. To delineate the idea that the access to electricity affects fertility through the change in time use and consumption pattern, we consider a simple static model with a single decision maker maximizing the following additively-separable utility function U over the consumption of child goods $n \in \mathbf{R}_+$ and nonchild numeraire goods $c \in \mathbf{R}_+$ for the electrification status $e \in [0, 1]$ given exogenously:

$$U(c, n, e) = \omega f(c, e) + (1 - \omega) g(n), \tag{1}$$

where f and g are increasing, concave, and twice differentiable subutility functions for nonchild and child goods, respectively, where child goods include the consumption goods associated with child bearing and rearing such as food, clothes, and education for children and nonchild goods include everything else. The preference parameter $\omega (\in (0, 1))$ represents the weight attached to nonchild subutility. We use prime to denote the derivative with respect to the first argument (e.g., $f' \equiv \frac{\partial f}{\partial c}$ and $g' \equiv \frac{\mathrm{d}g}{\mathrm{d}n}$). The cross derivative of f is denoted by f'_e.

We treat e as a continuous variable in the remainder of this section for simplicity of presentation, even though the household's access to electricity is treated as a binary variable in our empirical analysis. A larger value of e means better electricity service with $e = 0$ and $e = 1$ representing no and full electricity access, respectively. We assume away the quality of children and treat the consumption of child goods and the number of children synonymously in this model.

Each household allocates its effective lighted time (or productive time) to either child-related activities, such as looking after children, or nonchild activities including leisure and work. We denote the *fraction* of the effective lighted time required for each child by $\alpha(e)$, which is a function of electrification, and the fraction of effective lighted time spent on nonchild activities by l. Therefore, households satisfy the following identity of time use:

$$l + \alpha(e) n = 1. \tag{2}$$

Note that the corresponding physical unit of time in eq. (2) may vary across households. For example, households with electric lights or a habit of getting up early would have a longer effective lighted time than other households. Eq. (2) only requires that a fixed proportion of the effective lighted time has to be spent on each child in the household, given its electrification status. In our model, nonlighted hours are assumed to be used only for sleeping or reproductive activities and have no alternative use.

Households also face the budget constraint. Let $I(e)$ be the maximum potential household income, which the household can earn if all of its effective lighted time is spent on work. Assuming that the actual household income earned from work is proportionate to l, we can write the household budget constraint as follows:

$$I(e) l = c + p_n(e) n, \tag{3}$$

where $p_n(e)$ is the "price of having one child," which includes all direct costs of child bearing and rearing, such as food, clothes, and education. Because this is a static model, we ignore the possibility that children potentially contribute to the household income once they grow up.[3]

[3]Alternatively, one can interpret $p_n(e)$ as the net cost of children in present value, which takes into the account the

Households maximize the utility function in eq. (1) subject to the time constraint eq. (2) and the budget constraint eq. (3) over c, n, and l given their electrification status e. We denote the maximizing arguments with an asterisk and explicitly write the argument e to emphasize their dependence on e (i.e., $c_*(e)$, $n_*(e)$, and $l_*(e)$).

To derive our main results, we assume that the following inequalities hold:

$$\alpha'(e) < 0 \tag{4}$$

$$I'(e) > 0. \tag{5}$$

$$p_n'(e) \leq 0. \tag{6}$$

It is reasonable to expect that eq. (4) holds. Because households with better electricity access have more ways to handle child-related matters, the actual number of lighted hours that have to be spent on each child would not increase with electrification. Therefore, even if the access to electricity does not help households spend less time on child-related activities, the *fraction* of the lighted hours that must be spent on each child should decrease with a longer effective lighted time.

Similarly, eqs. (5) and (6) can be expected to hold, because longer lighted hours enable households to (potentially) spend more time on gainful activities (Khandker et al., 2009a,b) and the opportunity to use electrified appliances would not increase the cost of children. As we shall show in Section 7, we have some empirical evidence to support eqs. (4) and (5). We are unable to test eq. (6) due to the lack of data.

Using the notations introduced above, the following proposition can be derived (proof is provided in Appendix A):

Proposition 1 *The necessary and sufficient condition for the optimal number of children $n_*(e)$ to be decreasing with electrification (i.e., $n_*'(e) < 0$) is $V(e) > 0$ for $V(e)$ defined in the following manner*

$$\begin{aligned} V \equiv\ & [f' - (p_n + I\alpha)n_* f'']p_n' + [If' - (p_n + I\alpha)In_* f'']\alpha' + \\ & [\alpha f' + (p_n + I\alpha)l_* f'']I' + [p_n + I\alpha]f_e', \end{aligned} \tag{7}$$

where we dropped the argument e for brevity. Further, when $V(e) > 0$ is satisfied, the following equations hold:

$$l_*'(e) = -(\alpha' n_* + \alpha n_*') > 0 \tag{8}$$

$$c_*'(e) = l_* I' - (p_n + \alpha I)n_*' - (p_n' + \alpha' I)n_* > 0. \tag{9}$$

As seen from its definition, $V(e)$ can be divided into four terms, each involving p_n', α', I', and f_e'. The first and second terms are driven by the price effects induced by electrification through changes in the direct and opportunity costs of children, respectively. It is straightforward to verify that the first term is nonpositive and the second term is negative.

The third term involving I' represents the effect due to changes in potential household income. This effect is ambiguous because $\alpha f' > 0$ and $(p_n + I\alpha)l_* f'' < 0$. The fourth term involving f_e' represents the complementarity effects between electricity and nonchild goods. This negatively affects

contribution of children to the household income.

fertility when $f_e' > 0$.

From Proposition 1, it can be seen that the optimal number of children tends to decrease as a household becomes electrified when at least some of the following conditions are satisfied: (i) the complementarity between electricity and nonchild goods is strong (i.e., f_e' is positive and large), (ii) the direct and opportunity costs of children do not decline much with electrification (i.e., p_n' and α' are small in absolute values), and (iii) the marginal utility from nonchild goods is relatively large and declines only slowly (i.e., f' is large and f'' is small in absolute value), .

Casual observations of prevailing consumption patterns in Bangladesh and elsewhere suggest that conditions (i) is likely to hold under a variety of circumstances. Because access to electricity enables households to enjoy a wide range of additional goods, including electric lights, cooking appliances, refrigerators, fans, and televisions, the marginal subutility of nonchild goods for electrified households is likely to be no smaller than that for nonelectrified households for a given level of nonchild goods consumption. Similarly, condition (ii) is likely to hold under a variety of circumstances because there appear to exist little evidence that the availability of electric appliances drastically reduced the burden of child bearing and rearing.

However, condition (iii) is likely to depend on the context. Condition (iii) is most likely to hold when the household is relatively poor, which is generally the case in rural Bangladesh. This is because the marginal utility from the consumption of nonchild goods is likely to be high and the effect of declining marginal utility is likely to be small when the household is poor. This may also explain why Peters and Vance (2011) find that the effect of electrification is positive in urban area but negative in rural area in Côte d'Ivoire. Similarly, the reason why our empirical finding presented subsequently is at odds with Greenwood et al. (2005a) may be because US in the early 1940s was far richer than Bangladesh in our study period and thus condition (iii) did not hold for the former.[4]

Therefore, the discussion above leads us to expect that the impact of electrification on fertility in rural Bangladesh is negative, but the theoretical prediction is ambiguous as Proposition 1 shows. Nevertheless, it does provide an unambiguous prediction on the relationship between n_*', c_*', and l_*'. That is, when we observe a negative relationship between electrification and fertility, both the consumption of nonchild goods and the fraction of effective lighted time spent on nonchild activities should be positively related to fertility. Therefore, even though we primarily focus on the relationship between electrification and fertility, we can conduct a reality check based on our theoretical model by testing the signs of l' and c'. As elaborated later, our empirical results are consistent with this model prediction.

Because our model is static, one can interpret n_* as the optimal number of children that the household intends to have in the long run. In this interpretation, little difference is expected in the short-run fertility behavior between electrified and nonelectrified households that are otherwise identical, provided that the current number of children is well below their respective optimal number of children. This is because the speed at which households can increase the number of children is largely governed by the biological limit in the short run. Our empirical findings indeed indicate that the cumulative impact of electrification on fertility is indeed larger when we take a longer time horizon.

It should be reiterated that the model presented above takes the household's access to electricity

[4]Acccording to the Madison Project Database 2013 version (`http://www.ggdc.net/maddison/maddison-project/home.htm`), the GDP per capita in 1990 Geary-Khamis dollars in US in 1940 was \$7,010 wheras that in Bangladesh in 2010 was \$1,276.

as given. This is not an issue when we consider a particular household. However, when we try to identify the impact of electrification with data, taking the household's access to electricity as given is potentially problematic because the electricity access may be endogenous. Household characteristics may also alter the optimal number of children because, for example, households may have different values of ω and p_n. Therefore, we mainly use the DID estimation to control for all the household characteristics that are time invariant. To further address the potential concerns for the endogeneity problems, we also use instrumental variables and propensity score matching as detailed in Section 6.

5 Data and Summary Statistics

The main data source for our study is two rounds of the household survey data collected under the *Socioeconomic Monitoring and Impact Evaluation of Rural Electrification and Renewable Energy Programme in Bangladesh*. The first round was conducted in 2005 and the data was collected by a consortium comprising Bangladesh Engineering and Technological Services Ltd. (BETS) and Bangladesh Unnayan Parishad (BUP). The second round of survey, which followed up with a subsample of households, was collected in 2010 by e.Gen Consultants Ltd. Therefore, our dataset is partially panel.

Both rounds cover 45 out of the 70 PBSs operating in Bangladesh at the time of data collection, covering all six divisions of Bangladesh. In round 1, a stratified random sample was drawn according to the electrification status. To understand the impact of electrification, households with access to electricity, including both electricity from the grid and solar panels, were oversampled. Therefore, as with Khandker et al. (2012), it is important to apply the sample weights included in the round 1 data to account for the oversampling of these electrified households. Because no separate weights were provided in round 2, we apply the sample weight for round 1 in the panel data analysis. In the main text, most tables report weighted results but unweighted results, which are provided in Appendix B, are generally similar to weighted results. Further details on our data can be found in Bangladesh Engineering and Technological Services Ltd. and Bangladesh Unnayan Parishad (2006) and Khandker et al. (2012) for the round 1 data and e.Gen Consultants Ltd. (2006) for the round 2 data.

We primarily use the panel households for our analysis. To minimize complications arising from differences in household structure, we only use data for households whose head is male and married to exactly one woman. Therefore, we drop about one percent of polygamous households in each round. Further, because we are interested in the fertility behavior between the two rounds, we restrict our sample to those households in which the wife's age stays between 15 and 49 in both rounds.

Because the survey data we acquired did not contain a unique individual-level identification code, an individual-level panel dataset was constructed by manually matching the names of the husband and wife located between the two rounds for each household. Therefore, we exclude from our sample those households for which the names of the husband and wife could not be matched between the two rounds. Note that the matching of names between the two rounds is not always exact due to variations in English spellings of names. However, only those households that were matched with high confidence were retained for our analysis.

Our dataset unfortunately does not contain the complete history of pregnancy. Therefore, we derive an observable measure of fertility from the number of surviving children born to the spouse (wife) of the male household head, which we denote by NCH_{ht} for household h in round $t (\in \{1, 2\})$.

Therefore, NCH_{ht} is affected not only by the number of children that the wife has given birth to but also by the number of children who died before the time of interview. As a result, the change in the number of surviving children, $\Delta NCH_{h1} (\equiv NCH_{h2} - NCH_{h1})$, can be negative. We retain approximately nine percent of the households that experienced a net decrease in the number of surviving children between the two rounds. This is because only high fertility households that tend to produce more children in the event of child death will be retained if we keep the households for which the change in NCH is nonnegative, leading to a sample selection bias in our estimation. However, about one percent of households for which $|\Delta NCH_{h1}| > 4$ are treated as outliers and dropped from our sample.[5] To keep the presentation simple, we hereafter mostly ignore child deaths and drop the qualifier "surviving" in the remaining discussion. This is reasonable because the probability of death between the two rounds of survey is still relatively small, even though the child mortality in Bangladesh is far from negligible.[6]

After the trimming described above, we have a balanced panel of 2,542 households over the two rounds with a total of 5,084 records in our full panel sample. After accounting for the sample weights, about 47.4 [70.5] percent of households live in electrified villages and about 28.5 [44.5] percent of the households have access to electricity in round 1 [round 2]. The net increase of about 16.0(=44.5-28.5) percentage points in the share of panel households with access to electricity is due to about 17.3 percent of households gaining and about 1.3 percent of households losing access to electricity between the two rounds.

Table 1 provides some summary statistics of key household variables by the household's access to electricity from the national grid ($HELC_{ht}$) for panel households, where $HELC_{ht} = 1$ [$HELC_{ht} = 0$] means that household h has [does not have] access to electricity from the national grid in round $t \in \{1, 2\}$. As shown in Table 1, the head and spouse of electrified households tend to be slightly older than their nonelectrified counterparts. Electrified and nonelectrified households on average have a similar number of children in both rounds. While Table 1 reports weighted results, unweighted results are similar as reported in Table 18 in the Appendix.

Four cautions are in order here. First, our focus is primarily on the grid electricity. Therefore, unless otherwise noted, the electrification status is based on the access to *grid* electricity. This is because the power and reliability of electricity from the grid far exceeded the electricity from the typical Solar Home System available in Bangladesh at the time of surveys. Further, despite the oversampling, only around five percent of the sample households had electricity from solar in both rounds.[7]

Second, the variables of educational attainment are defined as ordered variables to enable easier understanding of the marginal impact of education. For example, if a given household's head has at least some upper secondary education, he automatically has some primary and lower secondary education. Therefore, the proportion of household heads with some primary education but no secondary

[5] Because there are only five years between the two surveys, a woman has to give birth to a child every fifteen months—which is roughly equal to the period of pregnancy and initial lactation in which she is less likely to become pregnant—to achieve $\Delta NCH = 4$ even without child deaths. Thus, it is reasonable to drop the records with $\Delta NCH > 4$. Similarly, we also drop households with $\Delta NCH < -4$, because death is clearly the predominant factor of change in the number of surviving children for those households. Our main results are unaffected by the inclusion of households with $|\Delta NCH| > 4$.

[6] The child mortality rate under five per 1,000 live births in Bangladesh was 68 in 2005 and 47 in 2010 according to the World Development Indicators. Older children tend to survive better.

[7] In 2011, four percent of the household reported to use solar power as a source of electricity in rural Bangladesh (Bangladesh Bureau of Statistics, 2012).

Table 1: Key summary statistics for rounds 1 and 2 by the household's access to electricity (panel households only, weighted).

Survey round	Round 1						Round 2					
Access to grid electricity ($HELC_{ht}$)	$HELC_{h1}=0$		$HELC_{h1}=1$		Total		$HELC_{h2}=0$		$HELC_{h2}=1$		Total	
Description	Mean	(s.d.)	Mean	(s.d.)	Mean	(s.d.)	Mean	(s.d.)	Mean	(s.d.)	Mean	(s.d.)
Age of household head	39.97	(8.11)	41.88	(7.68)	40.52	(8.03)	44.08	(8.60)	46.35	(8.39)	45.09	(8.58)
Age of spouse	32.11	(7.04)	33.83	(6.80)	32.60	(7.01)	36.16	(6.84)	37.53	(6.77)	36.77	(6.84)
Ratio of boys among children under 15†	0.501	(0.364)	0.534	(0.366)	0.510	(0.365)	0.512	(0.346)	0.508	(0.358)	0.510	(0.351)
Number of surviving children	2.636	(1.552)	2.757	(1.519)	2.670	(1.543)	2.933	(1.407)	2.937	(1.451)	2.935	(1.426)
Head has at least some primary educ.	0.578	(0.494)	0.781	(0.413)	0.636	(0.481)	0.669	(0.471)	0.752	(0.432)	0.706	(0.456)
Head has at least some junior sec. educ.	0.372	(0.484)	0.522	(0.500)	0.415	(0.493)	0.368	(0.482)	0.455	(0.498)	0.406	(0.491)
Head has at least some senior sec. educ.	0.174	(0.379)	0.292	(0.455)	0.207	(0.406)	0.196	(0.397)	0.243	(0.429)	0.217	(0.412)
Spouse has at least some primary educ.	0.558	(0.497)	0.715	(0.452)	0.603	(0.489)	0.631	(0.483)	0.724	(0.447)	0.672	(0.469)
Spouse has at least some junior sec. educ.	0.310	(0.463)	0.387	(0.487)	0.332	(0.471)	0.289	(0.453)	0.343	(0.475)	0.313	(0.464)
Spouse has at least some senior sec. educ.	0.089	(0.285)	0.129	(0.336)	0.101	(0.301)	0.090	(0.286)	0.106	(0.307)	0.097	(0.296)
log (HH expenditure per capita)‡	3.294	(0.397)	3.427	(0.388)	3.332	(0.399)	3.972	(0.537)	4.079	(0.600)	4.019	(0.568)
HH has a TV	0.001	(0.030)	0.631	(0.483)	0.181	(0.385)	0.169	(0.375)	0.675	(0.468)	0.394	(0.489)
HH has a mobile phone	0.000	(0.000)	0.070	(0.255)	0.020	(0.140)	0.542	(0.498)	0.815	(0.389)	0.663	(0.473)
Number of observations	1,505		1,037		2,542		1,127		1,415		2,542	

†: The average was taken over those households with at least one child under the age of 15. Therefore, the number of observations used for this row is about 10-15 percent lower than that for other rows in the same column.

‡: Household expenditure is expressed in Bangladesh Taka (BDT) per day. The PPP conversion factor for private consumption is USD 1=BDT 17.88 in 2005 and USD 1=BDT 23.15 in 2010 according to the World Development Indicators (`http://data.worldbank.org/indicator` accessed on May 27, 2016).

education in round 1 is $27.1(= 60.3 - 33.2)$ percent.

Third, the sex ratio of children is likely to influence subsequent fertility decisions because it is not uncommon among Bangladeshi households to prefer boys to girls. However, we observe only the number of surviving children born to the wife (i.e., NCH) but not separate numbers of boys and girls. Therefore, we use the household ratio of boys out of all children under the age of 15, which may include children whose mother is not the spouse of the male household head. For households with no children under 15, we assign a value of half in the regression analysis, but the average reported in Table 1 excludes those households.

Finally, there is a possibility that our panel sample suffers from selective attrition. This may be due to the selective attrition of households or manual matching of names described above. Therefore, we also report in Table 2 the summary statistics of the original sample, which include nonpanel households. It should be noted that Table 2 is unweighted because we do not have sample weights for round 2. As the comparison between Table 1 (or Table 18 in the Appendix) and Table 2 shows, the summary statistics are generally similar and we have no indication that our sample suffers from selective attrition. However, our regression analyses may suffer from attenuation bias if the matching in our sample is imperfect.

Tables 1 and 2 show that there are a few notable differences between nonelectrified and electrified households. First, the educational attainment in electrified households are higher than that in nonelectrified households for both husband and spouse. Second, consumption expenditures per capita for electrified households are on average higher than those for nonelectrified households and the rate of increase in average consumption per capita between the two rounds is also higher for electrified households. Third, there was a substantial increase in the penetration of television and mobile phones between the two rounds of the survey. Given that these two devices allow people to obtain information from outside their villages, they arguably deserve special attention.[8]

Table 3 presents the sample mean of ΔNCH and its standard error by the household's access to electricity and number of children in round 1. For example, households without access to electricity ($\text{HELC}^1 = 0$) on average have 0.316 more children in round 2 than they had in round 1. Based on a two-sided t-test, this figure is significantly positive. Hence, Table 3 shows that nonelectrified households tend to increase their number of children if they have three or fewer children already. For electrified households, the number of children tends to increase when NCH^1 is two or fewer. For electrified households with at least three children, the number of children remains unchanged over time in our data.

The right most column in Table 3 measures the difference in ΔNCH between nonelectrified and electrified households. On average, ΔNCH for electrified households in round 1 is lower than nonelectrified households by 0.180, which can be interpreted as a DID estimator. This estimator uses only the access to electricity in round 1 and not that in round 2 and thus differs from the DID regressions discussed subsequently because the identification in DID regressions rests on the existence of households that change the electrification status between the two rounds. Nevertheless, the difference in ΔNCH reported in the right most column of Table 3 captures the basic idea of DID estimators,

[8]It should be noted that share of individuals using the Internet was only 3.7 [0.2] percent in 2010 [2005] in Bangladesh according to the World Development Indicators (`http://data.worldbank.org/data-catalog/world-development-indicators` accessed July 5, 2017). While the breakdown by urban and rural areas is not available, it is likely that most users are in the urban area. Therefore, the Internet is unlikely to be an important source of information for most rural residents in Bangladesh during our study period.

Table 2: Key summary statistics for rounds 1 and 2 by the household's access to electricity (all households).

Survey round	Round 1						Round 2					
Grid electricity access ($HELC_{ht}$)	$HELC_{h1} = 0$		$HELC_{h1} = 1$		Total		$HELC_{h2} = 0$		$HELC_{h2} = 1$		Total	
Description	Mean	(s.d.)	Mean	(s.d.)	Mean	(s.d.)	Mean	(s.d.)	Mean	(s.d.)	Mean	(s.d.)
Age of household head	40.66	(8.92)	42.26	(8.77)	41.38	(8.89)	43.07	(8.92)	44.80	(8.97)	44.08	(8.99)
Age of spouse	32.75	(7.60)	34.06	(7.63)	33.34	(7.64)	34.89	(7.28)	35.87	(7.48)	35.46	(7.41)
Ratio of boys among children under 15†	0.519	(0.356)	0.521	(0.358)	0.520	(0.356)	0.518	(0.350)	0.512	(0.359)	0.514	(0.355)
Number of surviving children	2.601	(1.632)	2.654	(1.659)	2.625	(1.644)	2.781	(1.498)	2.744	(1.529)	2.759	(1.516)
Head has at least some primary educ.	0.626	(0.484)	0.795	(0.404)	0.703	(0.457)	0.695	(0.460)	0.786	(0.410)	0.748	(0.434)
Head has at least some junior sec. educ.	0.394	(0.489)	0.556	(0.497)	0.467	(0.499)	0.376	(0.485)	0.487	(0.500)	0.441	(0.497)
Head has at least some senior sec. educ.	0.194	(0.395)	0.311	(0.463)	0.247	(0.431)	0.190	(0.393)	0.264	(0.441)	0.233	(0.423)
Spouse has at least some primary educ.	0.560	(0.496)	0.720	(0.449)	0.633	(0.482)	0.677	(0.468)	0.775	(0.417)	0.734	(0.442)
Spouse has at least some junior sec. educ.	0.310	(0.463)	0.437	(0.496)	0.368	(0.482)	0.309	(0.462)	0.405	(0.491)	0.365	(0.482)
Spouse has at least some senior sec. educ.	0.088	(0.283)	0.140	(0.347)	0.111	(0.315)	0.091	(0.288)	0.133	(0.340)	0.116	(0.320)
log (HH expenditure per capita)‡	3.299	(0.389)	3.425	(0.376)	3.356	(0.388)	3.909	(0.490)	4.062	(0.555)	3.998	(0.534)
HH has a TV	0.001	(0.035)	0.665	(0.472)	0.302	(0.459)	0.151	(0.358)	0.693	(0.461)	0.468	(0.499)
HH has a mobile phone	0.000	(0.011)	0.073	(0.260)	0.033	(0.179)	0.561	(0.496)	0.827	(0.378)	0.717	(0.451)
Number of observations	9,030		7,493		16,523		1,760		2,474		4,234	

†: The average was taken over those households with at least one child under the age of 15. Therefore, the number of observations used for this row is about 10-15 percent lower than that for other rows in the same column.

‡: Household expenditure is expressed in Bangladesh Taka (BDT) per day. The PPP conversion factor for private consumption is USD 1=BDT 17.88 in 2005 and USD 1=BDT 23.15 in 2010 according to the World Development Indicators (`http://data.worldbank.org/indicator` accessed on May 27, 2016).

Table 3: The average change in the number of children between the two survey rounds (ΔNCH_{h1}) by the number of children (NCH_{h1}) and the access to electricity ($HELC_{h1}$) in round 1 (weighted).

	$HELC_{h1} = 0$				$HELC_{h1} = 1$				Difference		
NCH_{h1}	μ_0		(s.e.)	Obs	μ_1		(s.e.)	Obs	$\mu_1 - \mu_0$		(s.e.)
0	1.990	***	(0.112)	107	1.849	***	(0.165)	50	-0.141		(0.200)
1	0.683	***	(0.058)	240	0.609	***	(0.068)	147	-0.074		(0.090)
2	0.335	***	(0.052)	443	0.207	***	(0.048)	301	-0.128	*	(0.071)
3	0.225	***	(0.051)	356	0.017		(0.068)	256	-0.209	**	(0.085)
4+	-0.280	***	(0.071)	359	-0.364	***	(0.076)	283	-0.085		(0.104)
Total	0.316	***	(0.034)	1,505	0.136	***	(0.036)	1,037	-0.180	***	(0.050)

Note: The means μ_0 and μ_1 are the mean of the change in the number of children (ΔNCH) for nonelectrified and electrified households, respectively. All figures are weighted by the sample weight. Statistical significance of a two-sided t-test of inequality for the population mean μ with $H_0 : \mu = 0$ and $H_a : \mu \neq 0$ at 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

because it reflects the difference between electrified and nonelectrified households in the differences in the number of children between the two rounds of surveys. Table 3 appear to indicate that the optimal number of children is between three and four for nonelectrified households, whereas it is close to three because households, on average, do not increase the number of children when the initial number of children is at these levels.

In addition to the data described above, we also construct a retrospective panel dataset using a method similar to Jensen and Oster (2009). This has the advantage that we do not need to worry about selective attrition, which may be an issue for our proper panel data. To construct the retrospective panel data, we use the number of years with access to electricity, which is available (only) for the round 1 data. In addition, we need to construct the birth history of the spouse from the observed data.

Therefore, we chose the households suitable for this analysis in the following manner. First, we select only nuclear households in round 1 and count the number of individuals who are either son or daughter of the head of households. We include in the retrospective panel data if this number coincides with the number of surviving children reported by the spouse. We expect that all the children given birth by the spouse are still alive and reside with her for most households in this sample. However, we cannot exclude some other possibilities. For example, there may be some households in which the number of stepchildren that the household head had at the time of his second marriage (after becoming divorced or widowed) is equal to the number of children that the current spouse gave birth to but died by the time of the survey.

However, we ignore this possibility and also the possibility that the households may lose access to electricity to construct retrospective panel dataset. That is, based on the assumption that the spouse gave birth to all the children for these nuclear households, we construct the number of children and the status of electrification in the past retrospectively. As an example, consider a household that has three children aged 0, 2, and 4 and obtained access to electricity two years ago. One year ago, this household had two children and access to electricity. Three years ago, it had only one child and no access to electricity. We will refer to the data constructed retrospectively in this way *round 0* data. Among the 16,523 households used in Table 2, we were able to construct round 0 data for 9,819 households.

It should be noted that we can set round 0 at any year before 2005 in principle. However, we choose to set round 0 up to five years before the first round for three reasons. First, as mentioned by Jensen and Oster (2009), the recollection of longer-past events is likely to be less reliable. Second, the location of residence may change but our retrospective data cannot adequately account for migration. Third, children who were sufficiently old in 2005 were less likely to be residing with their parents than younger children. However, we are only able to include those nuclear-family households in which all children are still living with their parents (their father and his spouse, to be more exact). Therefore, retrospective panel dataset does not represent those households with old children very well and thus it is inappropriate to set round 0 at a long past.

In Table 4, we report the average change in the number of children between rounds 0 and 1 (i.e., $\Delta\text{NCH}_{h0} \equiv \text{NCH}_{h1} - \text{NCH}_{h0}$) when round 0 is set at the year 2000 such that the time intervals between rounds 0 and 1 and between rounds 1 and 2 are both five years. We do not expect Tables 3 and 4 to be similar because of the secular decline in fertility and difference in the demographic composition of households in the initial time period. Further, because of the way we constructed the retrospective panel, none of the households in the retrospective panel dataset experienced a net decrease in the number of children.

Despite these issues associated with the comparison between Tables 3 and 4, a few points are worth highlighting. First, the subsequent increase in fertility is larger when the initial number of children is small. Second, regardless of the initial number of children, the subsequent increase in the number of children is on average higher when the household does not have access to electricity in the initial period. Third, as expected, the increase in the number of children in the five-year period is on average larger for the retrospective panel, which is consistent with the secular decline in fertility. Fourth, consistent with the discussion in Section 4, the change in the number of children between the two rounds (ΔNCH) is not significantly different between electrified and nonelectrified households when the initial number of children is sufficiently small (zero or one in Table 3 and zero in Table 4). Finally, the DID estimators are similar between Tables 3 and 4.

6 Identification strategy

The discussion in section 4 suggests that access to electricity may affect fertility decisions and the summary statistics in Tables 3 and 4. However, these tables do not take into account the heterogeneity across households, including the systematic difference that may exist between electrified and nonelectrified households. Therefore, we discuss in this section our strategy to identify the causal effect of rural electrification.

Difference-in-differences specification

Our main identification strategy is based on the difference-in-differences estimation, which allows us to control for all the time-invariant unobservable household characteristics as well as observable time-variant household characteristics. The dependent variable in our model is the number of children

Table 4: The average change in the number of children between rounds 0 and 1 (ΔNCH_{h0}) by the number of children and the access to grid electricity (HELC_{h0}) in round 0 using the retrospective panel data (weighted).

	$\text{HELC}_{h0} = 0$				$\text{HELC}_{h0} = 1$				Difference		
NCH_{h0}	μ_0		(s.e.)	Obs	μ_1		(s.e.)	Obs	$\mu_1 - \mu_0$		(s.e.)
0	1.023	***	(0.022)	1,284	1.027	***	(0.042)	289	0.004		(0.048)
1	0.791	***	(0.019)	1,772	0.672	***	(0.029)	540	-0.119	***	(0.035)
2	0.452	***	(0.017)	2,176	0.339	***	(0.022)	765	-0.113	***	(0.027)
3	0.308	***	(0.018)	1,414	0.234	***	(0.022)	558	-0.074	***	(0.028)
4+	0.311	***	(0.023)	816	0.176	***	(0.027)	308	-0.135	***	(0.036)
Total	0.593	***	(0.010)	7,462	0.457	***	(0.014)	2,460	-0.136	***	(0.017)

Note: The means μ_0 and μ_1 are the mean of the change in the number of children (ΔNCH) for nonelectrified and electrified households, respectively. The round 0 is the year 2000 in this table. Statistical significance of a two-sided t-test of inequality for the population mean μ with $H_0 : \mu = 0$ and $H_a : \mu \neq 0$ at 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

NCH_{ht}, whereas the key covariate is the indicator variable HELC_{ht} for the household's access to grid electricity. The simplest version of our estimation equation is:

$$\text{NCH}_{ht} = \alpha\text{HELC}_{ht} + \beta\text{HELC}_{ht} \times I_{t2} + \eta_h + \delta_t + u_{ht}, \tag{10}$$

where β is the main coefficient of interest and η_h, δ_t, and u_{ht} are the household-specific, round-specific, and idiosyncratic error terms. The term $I_{tj} \equiv \mathbf{1}(t = j)$ is an indicator variable, which takes one if $t = j$ and zero otherwise (i.e., I_{t2} is the time dummy for round 2). Note that I_{t2} is not needed in the specificatio above because it is absorbed by the round-specific fixed-effects terms. In addition to eq. (10), we may also include a vector of (potentially) time-varying covariates X_{ht} as follows:

$$\text{NCH}_{ht} = \alpha\text{HELC}_{ht} + \beta\text{HELC}_{ht} \times I_{t2} + \gamma X_{ht} + \eta_h + \delta_t + u_{ht}, \tag{11}$$

In the estimation of eqs. (10) and (11), we allow for the clustering of the error terms at the subdistrict level. When we use the retrospective panel data and analyze the data for rounds 0 and 1, we replace I_{t2} with I_{t1} and $t = 0$ will be the base time period.

Table 3 suggests that the change in the number of children depends on the initial number of children. This suggests that the impact of electrification may depend on the number of children that the household has initially. Therefore, we also consider a specification in which $\text{HELC}_{ht}I_{t2}$ is interacted with an indicator that NCH_{h1} is at least some threshold value v suc that the estimation equaliton can be written as follows:

$$\begin{aligned}\text{NCH}_{ht} &= \alpha\text{HELC}_{ht} + \beta\text{HELC}_{ht} \times I_{t2} + \beta^{+}\text{HELC}_{ht} \times I_{t2} \times \mathbf{1}(\Delta\text{NCH}_{h1} \geq v) \\ &\quad + \theta I_{t2} \times \mathbf{1}(\Delta\text{NCH}_{h1} \geq v) + \gamma X_{ht} + \eta_h + \delta_t + u_{ht}.\end{aligned} \tag{12}$$

The term $\mathbf{1}(\Delta\text{NCH}_{h1} \geq v)$ only enters as interaction terms because this term is time-invariant and thus its effect is absorbed by the household-level fixed-effects terms. We will also follow similar specification when the impact heterogeneity along other dimensions is considered.

Instrumental variables

One potentially important concern for these estimation equations is the potential endogeneity of $\text{HELC}_{ht}I_{t2}$. Suppose that there is a shock to the household that are simultaneously correlated with the household's adoption of electricity between the two survey rounds and change in fertility behavior, then the estimates of β will be biased.

If we knew whether the selection is positive [negative], we would know the OLS estimate upwards [downwards]. However, it is not possible to determine the direction of bias because both positive and negative selections are possible. Therefore, we instrument HELC_{ht} and $\text{HELC}_{ht}I_{t2}$ with the indicator variable for the village-level electrification status (i.e., it takes one if the village that household h is located is electrified at round t and zero otherwise) and the system loss from the grid at time t as well as their interactions with I_{t2}.

The village-level electrification status is a relevant instrument because households are not able to get connected to the national grid if their villages are not electrified. Therefore, the village electrification status is a relevant instrument. Because fertility decision is primarily a household decision, using the village-level electrification status is likely to mitigate the concerns for endogeneity. However, it may be still debatable whether the village electrification status strictly satisfies the exclusion restriction, because those villages that are electrified in round 1 may have a favorable development condition than the rest of the villages, which in turn may affect the fertility behavior. This is a relevant concern because the process of rural electrification is not random as noted in Section 3.

Therefore, we also include the system loss from the grid, which was compiled from Annual Reports from the Rural Electrification Board (2006, 2011). To understand the relevance of this instrument, note that excessive system loss may result from technical causes such as suboptimal voltage regulation and circuit configurations as well as nontechnical causes such as theft and nonfunctional meters, even though some loss is unavoidable. This indicates that the management efficiency of the PBS is low when the system loss is high. Because households are less likely to adopt electricity when their PBS is poorly managed, the system loss from the grid is also a relevant instrument. On the other hand, the management of PBS appears to have no clear link to the fertility in its area of operation.

It should be reiterated that the DID estimation is immune to the endogeneity concerns that typically arise in pure cross-sectional regressions. For example, one may argue that local corruption affects both the system loss and quality of local health facilities, the latter of which in turn affect infant mortality and fertility decisions. However, so long as the level of corruption is time-invariant during our study period, we need not worry about endogeneity of this sort in the DID regressions.

Furthermore, even if there is a "corruption shock" that has taken place between the two survey rounds, the impact on fertility through local health facilities is likely to be limited because only a small share of birth deliveries take place in hospitals.[9] Therefore, we argue that the system loss from the grid together with the electrification status of the village plausibly satisfies exclusion restriction.

[9] In 2007, only 10.5 percent of deliveries took place in a health facility (NIPORT et al., 2009).

Change-on-level specification

While the DID estimation is our preferred specification, we also consider a change-on-level specification to show that our results are not driven by a shock that simultaneously affects the adoption of electricity and fertility. To be specific, we use the change in the number of children between the two survey rounds as an alternative dependent variable. In other words, we regress ΔNCH_{h1} on HELC_{h1} and a variety of covariates observed at $t = 1$ such that the estimation equation has the following form:

$$\Delta\text{NCH}_{1h} = b\text{HELC}_{h1} + \phi\text{NCH}_{h1} + \gamma X_{h1} + \iota_{j(h)} + \epsilon_h, \tag{13}$$

where $j(\cdot)$ is a mapping from the household to its subdistrict of residence and $\iota_{j(h)}$ and ϵ_h are the subdistrict-specific and idiosyncratic error terms, respectively. The main coefficient of interest is b in this equation. Even though we control for important household-level observables and include subdistrict fixed effects in our change-on-level regressions, there may be endogeneity concerns. That is, households with access to electricity are self-selected and the fertility behavior may be systematically different from those households without access to electricity. Following the argument we made above, we instrument HELC_{h1} by the village-level electrification status and system loss from the grid in round 1.

The change-on-level specification in eq. (13) has an advantage that the key covariate HELC_{h1} is not affected by the events that took place between the two survey rounds. Therefore, while HELC_{h1} and ΔNCH_h may be simultaneously affected by certain expectations about the future, the type of the endogeneity problems this specification may potentially suffer from is different from those that affect this specification. Thus, if there are endogeneity problems we fail to address, we would be able to detect by comparing the difference-in-differences and change-on-level estimates provided that the difference in the bias from these estimations are sufficiently large. Conversely, if we do not detect any inconsistency between these estimates, we can have some level of confidence in our estimates.

Propensity Score Matching

As a further robustness check, we also adopt propensity score matching (PSM) method in this subsection. One potential advantage of PSM over regressions considered above is that it helps to make the distribution of covariate more balanced between the control group (i.e., households without access to grid electricity in our application) and treatment groups (i.e., households with access to grid electricity in our application). The covariate balance would be irrelevant if the regression models used above are correctly specified. However, there is a possibility that our DID regression results may be confounded with the combination of unbalanced covariate and covariate-dependent time trends. While the change-on-level regressions address the issue of covariate-dependent time trend, it does not address the effect of selection on time-invariant unobservable variables. Therefore, we will also run DID regression with matched sample.

Table 5: Main difference-in-differences regressions (weighted).

Dep var: NCH_{ht}	(1)	(2)	(3)	(4)
Grid electricity $\times I_{t2}$	-0.206**	-0.229***	-0.197**	-0.298**
	(0.0835)	(0.0838)	(0.0809)	(0.132)
Solar electricity $\times I_{t2}$		-0.162		
		(0.161)		
Ratio of boys among children under 15			0.0647	-0.111
			(0.155)	(0.184)
log (HH expenditure per capita)			-0.176***	-0.138
			(0.0644)	(0.0878)
Grid electricity	0.116	0.119	0.107	0.00365
	(0.0924)	(0.0924)	(0.0930)	(0.125)
Solar electricity		0.00171		
		(0.251)		
I_{t2}	0.338***	0.360***	0.456***	0.515***
	(0.0618)	(0.0619)	(0.0710)	(0.121)
R^2	0.870	0.871	0.871	0.877
Observations	5,084	5,084	5,084	2,900

Note: All regressions include household-level fixed-effects terms. $I_{t2} \equiv \mathbf{1}(t = 2)$ is an indicator variable for round 2. Standard errors clustered at the subdistrict level are reported in parentheses. ***, ***, and * denote statistical significance at 1, 5, and 10 percent levels, respectively. Column (4) is based on the households in electrified villages only.

7 Results

Baseline difference-in-difference regressions

We now apply the econometric specifications discussed in Section 6 to the data. Let us start with the main results with panel data. In column (1) of Table 5, we report the OLS regression of the DID specification in eq. (10). The first row ("Grid electricity$\times I_{t2}$") reports the main coefficient of interest β, where "Grid electricity" in the tables presented here and below is NCH_{ht}. This estimate indicates that the electrification reduces the change in the number of children between the two survey rounds by 0.206 children.

It should be noted that α reported in the fifth row ("Grid electricity") does not represent the causal effect of the adoption of electricity. If we assume away the presence of those who lost electricity access between the two rounds, α reflects the difference in the change in the number of children between the early adopter (i.e., those who have adopted electricity by round 1) and late adopter of electricity (i.e., those who have adopted electricity between rounds 1 and 2). Follow this interpretation and assuming that there is no endogeneity issue when the household-level fixed-effects terms are included, the impact of partial electrification between the two survey rounds on fertility is $-0.090(= -0.206+0.116)$. While we focus on β in the rest of our discussion on DID regression results, α is mostly positive and smaller in absolute value than β. Therefore, this result indicates that the cumulative effect of access to electricity depends on the length of electricity access that the household has.

Let us now turn to column (2) of Table 5. In this specification, we also try to identify the effect of access to electricity for the solar. The second row shows the DID estimate of the impact of adopting solar electricity. As with the grid electricity, the point estimate is negative. However, the estimated impact of solar electricity is smaller in absolute value than that of grid electricity, even though the difference between them is insignificant. This is not surprising because solar electricity is less robust and reliable than grid electricity. The comparison between the second row ("Solar electricity$\times I_{t2}$") and sixth row ("Solar electricity") shows that the impact of partial electrification during the two survey periods is smaller than that of full electrification as with the case of the grid electricity.

In column (3), we add the ratio of boys among children under 15 and logarithmic household expenditure per capita as additional covariates based on eq. (11). While we add these covariate in our DID regressions wherever possible in the rest of the paper to reduce potential sources of confounding, results are generally similar both quantitatively and qualitatively even when they are omitted.

It could be argued that households located in electrified and nonelectrified villages may have experienced systematically different shocks that simultaneously affect the household's adoption of electricity and fertility. Therefore, we re-estimated the regression in column (3) with a subsample of households that are located in the villages that were electrified in round 1. As the result reported in column (4) shows, the point estimate of β becomes larger in absolute value even though it is not significantly different from the corresponding point estimate in column (3). Therefore, the estimates reported in Table 5 all indicate that access to electricity reduces the fertility between the two survey rounds by around 0.2 children.

In Table 6, we report the regression results for eq. (10) using a balanced retrospective panel dataset with 9,922 households between rounds 0 and 1. We vary the timing of round 0 from 2004 to 2000 in columns (1) to (5). The point estimates are all negative and significant when the time interval between rounds 0 and 1 is two years or longer. Therefore, the analysis of retrospective panel also indicates that the impact of rural electrification on fertility is negative and the cumulative impact tends to get larger as the household has access to electricity for a longer period of time. This is consistent with the interpretation of our theoretical model discussed in section 4.

It should be reiterated that the retrospective panel data set can be created only for nuclear households that satisfy certain conditions detailed in Section 5. Therefore, the comparability between Tables 5 and 6 may be debatable because the types of households included in the proper and retrospective panel data sets are different. To address this concern, we run the same regression as column (5) but only with a subset of 1,401 panel nuclear households for which round 0 data can be created. As the results reported in column (6) shows, the impact is still negative and significant. The point estimate is smaller in absolute value than that in column (5). This may be because nuclear households may be less traditional than nonnuclear households in the first place or because it is costlier for nuclear households to have a large number of children.

To understand better whether the use of nuclear households alters the results, we also run the regression reported in column (3) of Table 5 with the same 1,401 panel nuclear households. As shown in column (7) of Table 6, the point estimate for the panel nuclear households is not significantly different from that for the full panel households. More importantly, even though the former is smaller in absolute value, the point estimate is still statistically significant. Therefore, negative point estimates in columns (1) to (5) in Table 6 are unlikely to be driven by the fact that we use nuclear households instead of all households in the analysis.

Table 6: Regression results based on synthetic panel (weighted).

Dep var: NCH_{ht}	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Grid electricity $\times I_{t1}$	-0.00822	-0.0556***	-0.127***	-0.253***	-0.417***	-0.123*		-0.114*
	(0.00732)	(0.0186)	(0.0360)	(0.0579)	(0.0875)	(0.0743)		(0.0686)
Grid electricity $\times I_{t2}$							-0.136**	-0.263***
							(0.0663)	(0.0909)
log (HH expenditure per capita)							-0.311***	
							(0.100)	
Ratio of boys among children under 15							-0.0265	
							(0.134)	
Grid electricity	0.0165	0.0210	0.0149	0.0716	0.150	0.0687	-0.0326	0.0532
	(0.0175)	(0.0362)	(0.0534)	(0.0755)	(0.106)	(0.0853)	(0.0949)	(0.0935)
I_{t1}	0.0513***	0.221***	0.522***	0.977***	1.584***	0.655***		0.655***
	(0.00589)	(0.0141)	(0.0274)	(0.0445)	(0.0688)	(0.0406)		(0.0338)
I_{t2}							0.624***	1.087***
							(0.0786)	(0.0645)
Year in Round 0	2004	2003	2002	2001	2000	2000	N/A	2000
Rounds	0-1	0-1	0-1	0-1	0-1	0-1	1-2	0-2
R^2	0.993	0.961	0.899	0.820	0.752	0.930	0.891	0.848
Observations	19,844	19,844	19,844	19,844	19,844	2,802	2,802	4,203

Note: All regressions include household-level fixed-effects terms. $I_{tj} \equiv \mathbf{1}(t = j)$ is an indicator variable for round $j \in \{1, 2\}$. Standard errors clustered at the subdistrict level are reported in parentheses. ***, ***, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

Table 7: Impact heterogeneity by the initial number of children (weighted).

Dep var: NCH_{ht}	(1)	(2)	(3)	(4)
Threshold no children (v)	$v = 1$	$v = 2$	$v = 3$	$v = 4$
Grid electricity $\times I_{t2}$	-0.224	-0.153	-0.163*	-0.183**
	(0.310)	(0.156)	(0.0970)	(0.0811)
Grid electricity $\times I_{t2} \times \mathbf{1}(\text{NCH}_{h1} \geq v)$	0.0621	-0.00676	-0.00571	0.0166
	(0.321)	(0.170)	(0.137)	(0.167)
Ratio of boys among children under 15	0.0145	0.0513	0.0882	0.0449
	(0.141)	(0.157)	(0.152)	(0.147)
log (HH expenditure per capita)	-0.140**	-0.0898*	-0.118**	-0.123**
	(0.0589)	(0.0531)	(0.0575)	(0.0564)
Grid electricity	0.0669	0.0884	0.114	0.127
	(0.0843)	(0.0871)	(0.0863)	(0.0825)
I_{t2}	2.119***	1.122***	0.732***	0.608***
	(0.190)	(0.124)	(0.0851)	(0.0740)
$I_{t2} \times \mathbf{1}(\text{NCH}_{h1} \geq v)$	-1.803***	-0.942***	-0.673***	-0.762***
	(0.195)	(0.125)	(0.107)	(0.141)
R^2	0.891	0.888	0.884	0.884
Observations	5,084	5,084	5,084	5,084

Note: All regressions include household-level fixed-effects terms. $I_{t2} \equiv \mathbf{1}(t = 2)$ is an indicator variable for round 2. Standard errors clustered at the subdistrict level are reported in parentheses. ***, ***, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

In column (8) of Table 6, we again use the same 1,401 panel nuclear households as those used in columns (6) and (7). However, we use rounds 0, 1, and 2 together in this analysis. The first and second rows identify the impacts of access to electricity on the fertility between rounds 0 and 1 and between rounds 0 and 2, respectively. We cannot reject the null hypothesis that the impact on fertility in the five-year period between rounds 0 and 1 is twice the impact on fertility in the ten-year period between rounds 0 and 2. Therefore, the results are broadly consistent across different time periods and the estimated impact of access to grid electricity mostly range between 0.1 and 0.25. Below, we conduct a variety of robustness and reality checks to further bolster this finding.

Impact heterogeneity

The DID estimation builds on the assumption of parallel trends. In our context, this assumption requires that the change in the number of children between the two survey rounds would be the same if no [every] household had access to electricity in either [both] rounds.

This assumption is, however, potentially problematic if the initial number of children affects the subsequent change in the number of children. This is plausible because households without children at the beginning of the study period may try to increase the number of children to reach the optimal number n^* of children, whereas those households with multiple children may have already reached n^* by then. Therefore, it is possible that the estimated impact of access to electricity presented so far may be confounded with the effect of the initial number of children.

To allow for this possibility and the heterogeneity of impact across households with different initial numbers of children, we use eq. (12) in which the interaction term between the indicator variable for having at least v children in round 1 and the round 2 indicator (I_{t2}) as well as its interaction with HELC_{ht} are included in addition to the model used in column (3) of Table 5 for $v \in \{1, 2, 3, 4\}$.

The results of these regressions are reported in Table 7. The first row ("Grid electricity$\times I_{t2}$") provides the estimated coefficient β in eq. (12). The seventh row ("$I_{t2} \times \mathbf{1}(\text{NCH}_{h1} \geq v)$") provides the estimates of θ in eq. (12). Consistent with the argument above, it shows that the the number of children tends to increase faster if the initial number of chldren is lower. However, as the second row ("Grid electricity$\times I_{t2} \times \mathbf{1}(\text{NCH}_{ht} \geq v)$") indicates, the estimates of β^{+} are all insignificant and close to zero. Further, as with our baseline results, the estimates of β are all negative and around 0.2, even though the point estimate is not statistically significant when $v = 1$ or $v = 2$ because of the collinearity between the first two terms in Table 5. Therefore, there is no evidence that the difference in the initial fertility between electrified and nonelectrified households in round 1 is driving our main results reported

So far, we estimated under the implicit assumption that the impact of access to electricity on fertility does not depend on the timing of the adoption of electricity. However, if people's behavior changes only slowly over time, the impact may be greater when the household has access to electricity for a longer period of time. Therefore, we include interaction terms of HELC_{ht} and $\text{HELC}_{ht} \times I_{t2}$ with the indicator variable that the household had access to electricity for at least k years in round 1 in addition to the base specification reported in column (3) of Table 5. We report the regression results for this specification with different values of $k(\in \{5, 10, 15, 20\})$ in columns (1) to (4) of Table 8. Because the second row is negative for the first three columns, the point estimates indicate that the impact of electrification is slightly higher for those households with a longer history (up to 15 years) of electricity access. However, because the coefficients in the second row are all insignificant and mostly small, the impacts of access to electricity on fertility between the two survey rounds are not significantly different between households with and without a long history of electricity access.

It is also plausible that the impact of electricity access depends on the quality of electricity provided. To capture this idea, we would include the information on outage frequency during the entire study period. Unfortunately, the outage information available to us was collected only during the surveys and in a different manner between the two survey rounds. That is, in round 1, each household answered the number of outages and their average duration in the week before the survey. In round 2, the average hours of outage per day was asked in each village. Therefore, the comparability of the outage information across the two rounds is somewhat questionable. Further, because the measurement is based solely on the recollection of respondent, we expect this information to be imprecise. To mitigate these issues, we choose to create an indicator variable for frequent outages in each round, which takes one if the household experienced more than median share of time in which electricity is unavailable (43.9 percent in round 1 and 15.3 percent in round 2).

In column (5) to (7) of Table 8, we report the regression results in which the interaction terms of HELC_{ht} and $\text{HELC}_{ht} \times I_{2t}$ with the frequent outage indicator for round 1, round 2, and both rounds are included. The effect of outage in round 2 is large and positive though not significant as shown in the fourth row. The positive sign is expected, because the impact of rural electrification is expected to be smaller when outage is more frequent. On the other hand, the third rows shows that the effect of outage in round 1 is very close to zero. Taken together, we find that the effect of outages is small

Table 8: Heterogeneity of impact by the duration of electricity access and outage (weighted).

Dep var: NCH_{ht}	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Threshold length of access (k)	$k = 5$	$k = 10$	$k = 15$	$k = 20$			
Grid electricity $\times I_{t2}$	-0.142	-0.187**	-0.192**	-0.217***	-0.185*	-0.219*	-0.208*
	(0.107)	(0.0945)	(0.0882)	(0.0825)	(0.101)	(0.118)	(0.125)
Grid electricity $\times I_{t2} \times$ (at least k years of access)	-0.0897	-0.0244	-0.0193	0.261			
	(0.102)	(0.102)	(0.134)	(0.225)			
Grid electricity $\times I_{t2} \times$ (Frequent outage at $t = 1$)					-0.0250		-0.0243
					(0.113)		(0.113)
Grid electricity $\times I_{t2} \times$ (Frequent outage at $t = 2$)						0.0332	0.0341
						(0.121)	(0.121)
Ratio of boys among children under 15	0.0658	0.0649	0.0645	0.0665	0.0652	0.0635	0.0641
	(0.155)	(0.155)	(0.155)	(0.155)	(0.155)	(0.155)	(0.155)
log (HH expenditure per capita)	-0.174***	-0.176***	-0.176***	-0.178***	-0.179***	-0.176***	-0.178***
	(0.0647)	(0.0643)	(0.0644)	(0.0647)	(0.0652)	(0.0640)	(0.0650)
Grid electricity	0.0651	0.0935	0.102	0.126	0.177*	0.170	0.233
	(0.122)	(0.104)	(0.0969)	(0.0955)	(0.107)	(0.162)	(0.172)
Grid electricity $\times$ (at least k years of access)	-0.125	0.139	0.120	-0.117			
	(0.273)	(0.231)	(0.550)	(0.839)			
Grid electricity $\times$ (Frequent outage at $t = 1$)					-0.132		-0.130
					(0.177)		(0.175)
Grid electricity $\times$ (Frequent outage at $t = 2$)						-0.0950	-0.0859
						(0.193)	(0.190)
I_{t2}	0.451***	0.457***	0.456***	0.458***	0.459***	0.456***	0.459***
	(0.0717)	(0.0717)	(0.0712)	(0.0715)	(0.0717)	(0.0709)	(0.0716)
R^2	0.872	0.871	0.871	0.872	0.872	0.871	0.872
Observations	5,084	5,084	5,084	5,084	5,084	5,084	5,084

Note: All regressions include household-level fixed-effects terms. $I_{t2} \equiv \mathbf{1}(t = 2)$ is an indicator variable for round 2. Standard errors clustered at the subdistrict level are reported in parentheses. ***, ***, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

Table 9: Instrumental variable regression results (weighted).

Dep var	(1) NCH_t	(2) $HELC_t$	(3) $HELC_t \times T_{t2}$	(4) NCH_t
Grid electricity $\times I_{t2}$	-0.165*			-0.240**
	(0.090)			(0.120)
log (HH expenditure per capita)	-0.172***	0.000	0.031*	-0.167***
	(0.047)	(0.017)	(0.018)	(0.0622)
Ratio of boys among children under 15	0.063	-0.005	-0.060**	0.0566
	(0.110)	(0.027)	(0.028)	(0.157)
Grid electricity	0.623***			0.0564
	(0.185)			(0.118)
I_{t2}	0.357***	-0.061	-0.010	0.363***
	(0.065)	(0.101)	(0.091)	(0.0924)
Electrified village		0.261***	-0.328***	0.112
		(0.045)	(0.043)	(0.117)
Electrified village $\times I_{t2}$		0.139***	0.746***	0.126
		(0.019)	(0.023)	(0.132)
System loss from the grid		-1.434*	-0.887	
		(0.852)	(0.877)	
System loss from the grid $\times I_{t2}$		0.598	0.016	
		(0.689)	(0.641)	
Estimation	2SLS (Stg 2)	2SLS (Stg 1)	2SLS (Stg 1)	OLS
First stage F		32.71***	264.05***	
p-Value for OIR	1.997			
Obs	5,084	5,084	5,084	5,084

Note: All regressions include household-level fixed-effects terms. $I_{t2} \equiv \mathbf{1}(t = 2)$ is an indicator variable for round 2. Standard errors clustered at the subdistrict level are reported in parentheses. ***, ***, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

and our main result that rural electrification negative affects fertility remains unchanged even when the effect of outage is taken into account.

IV and spillover regressions

The results presented above consistently indicate that rural electrification negative affects fertility. However, it is possible that our results are driven by some factors that simultaneously affect the adoption of electricity and fertility in the opposite direction. To address this issue, we use the instrumental variables approach discussed in the previous section.

In the first three columns of Table 9, we report the results of the IV version of DID regression where the access to grid electricity and its interaction with the round 2 indicator variable are instrumented by the electrification status of the village and system loss from the grid as well as their interaction with the round 2 indicator variable. As shown in columns (2) and (3), the first-stage regressions indicate that the village electrification status has a positive impact on the household's adoption of electricity. The first-stage F-statistic indicate that our instruments are strong.

The interpretation of the first-stage results require some caution. Even though the point estimate on electrified village is negative and significant in column (3), the sum of the sixth row ("Electrified village") and seventh row ("Electrified village $\times I_{t2}$") show that the total impact of village electrification on the interaction between the household's electrification status and round 2 indicator variable is positive. Similarly, the system loss negatively affect the adoption of electricity though the statistical significance is marginal. Thus, our first-stage results appear to be reasonable.

The second stage regression result is reported in column (1). While both the absolute value and statistical significance of the estimated impact reduced slightly in comparison with our baseline results, the result shows that our main result remains unchanged.

In column (4), we report an alternative specification in which electrified village enters as a regressor. In this specification, we include the village-level electrification status in the same way as the household-level electrification. The reason why this specification may be relevant is because the electrification of village may have impact on nonelectrified households. For example, in electrified villages, members of nonelectrified households may be able to watch TV in their electrified neighbors, which in turn may have impact on their fertility behavior. As with the IV regression results, the inclusion of village-level electrification status as regressors does not alter our main results.

Change-on-level regressions

While we believe that the instrumental variables we use are plausibly exogenous, one may still argue that there is a factor that affects our instruments and fertility simultaneously. Even though we have no evidence that such a factor exists and poses a threat to our identification, it is still prudent to take such possibility into account. Therefore, we also consider a change-on-level specification. As we argued in the previous section, this specification does not suffer from the potential endogeneity arising from the presence of a shock that simultaneously affects the fertility decision and adoption of electricity between the two survey rounds.

The results of change-on-level regressions are reported in Table 10. Column (1) reports the result of a simple subdistrict-level fixed-effects regression. Thus, without controlling for any other covariate, the change in the number of children between the two survey rounds for electrified households is lower than that for nonelectrified households by 0.18 children. Once households' observable characteristics such as the demographics, education of household head and his spouse, and the logarithmic household expenditure per capita are controlled for, the coefficient on grid electricity drops to 0.1 but it is still significant as reported in Column (2).

Columns (1) and (2) are based on OLS regressions and do not take into account of the potential endogeneity issue. This point is potentially important, because those households that have already access to grid electricity in round 1 may be systematically different from those that did not, which in turn may explain the difference in fertility behavior. In this case, the OLS estimates do not represent the causal effect of electrification but the systematic difference between electrified and nonelectrified households in round 1. Therefore, we also report the result of an IV regression where the adoption of electricity in round 1 is instrumented by the indicator variable for electrified village and system loss from the grid in 2005. The first- and second-stage regressions are reported in columns (4) and (3), respectively.

The first-stage result shows that the instruments have expected signs and significant. The second-stage result shows that the point estimate on grid electricity became larger in absolute value than that in column (2), even though the point estimate is no longer significant. This indicates that the OLS

Table 10: Change on level regressions (weighted).

Dep var	(1) ΔNCH_h	(2) ΔNCH_h	(3) ΔNCH_h	(4) $HELC_{ht}$
Grid electricity in round 1	-0.176***	-0.101*	-0.164	
	(0.052)	(0.054)	(0.104)	
Total number of children of married woman		-0.389***	-0.388***	0.007
		(0.026)	(0.025)	(0.008)
Ratio of boys among children under 15		-0.141*	-0.139**	0.026
		(0.073)	(0.069)	(0.019)
Age of household head		-0.026	-0.025	0.023**
		(0.039)	(0.038)	(0.011)
Age of household head squared		0.032	0.030	-0.027**
		(0.043)	(0.041)	(0.013)
Age of spouse		0.039	0.039	-0.016
		(0.051)	(0.049)	(0.013)
Age of spouse squared		-0.051	-0.051	0.027
		(0.073)	(0.070)	(0.018)
Head has at least some primary educ.		0.004	0.010	0.053**
		(0.064)	(0.063)	(0.025)
Head has at least some junior sec. educ.		0.071	0.071	0.024
		(0.078)	(0.075)	(0.024)
Head has at least some senior sec. educ.		-0.050	-0.046	0.026
		(0.067)	(0.066)	(0.025)
Spouse has at least some primary educ.		0.131**	0.135**	0.057**
		(0.057)	(0.056)	(0.022)
Spouse has at least some junior sec. educ.		-0.029	-0.031	0.003
		(0.060)	(0.058)	(0.020)
Spouse has at least some senior sec. educ.		-0.270***	-0.271***	0.034
		(0.073)	(0.071)	(0.030)
log (HH expenditure per capita)		-0.235***	-0.227***	0.129***
		(0.066)	(0.066)	(0.027)
System loss from the grid (%)				-0.016*
				(0.010)
Electrified village				0.591***
				(0.037)
Estimation	OLS	OLS	2SLS (stg 2)	2SLS (stg 1)
Subdistrict Fixed Effects	Yes	Yes	Yes	Yes
R^2	0.092	0.325	0.260	
First Stage F-statistic				244.79***
Hansen J-statistic			2.334	
Observations	2,542	2,542	2,542	2,542

Note: Standard errors clustered at the subdistrict level are reported in parentheses. ***, ***, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

Table 11: Propensity matching estimators.

	(1)	(2)	(3)	(4)	(5)
Est	-0.210***	-0.179***	-0.185***	-0.224***	-0.167**
(s.e.)	(0.065)	(0.053)	(0.060)	(-0.080)	(0.080)
Estimation	PSM	PSM	PSM	PSM	PSM+DID
PS Model	Probit	Probit	Logit	Probit	Probit
Treatment Effect	ATET	ATE	ATET	ATET	ATET
Sample	All	All	All	Elec vill	All
Obs	2,542	2,542	2,542	1,450	4,148

Note: The total number of children of married woman, the ratio of boys among children under 15, the age and education indicators (i.e., "at least some primary", "at least some lower secondary", and "at least some upper secondary") of the head and spouse, and the logarithmic household expenditure per capita are used for estimating the propensity score. Standard errors are reported in parentheses. In columns (1) to (4), Abadie-Imbens standard errors are reported (Abadie and Imbens, 2016). In column (5), standard errors are clustered at the subdistrict level. ***, ***, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

estimate is, if anything, upward biased (biased towards zero because the point estimate is negative). Thus, the results of change-on-level regressions are also consistent with our main finding that the impact of rural electrification on fertility is negative.

Propensity score matching

As a further robustness check, we also adopt propensity score matching (PSM) method in this subsection. One potential advantage of PSM over regressions considered above is that it helps to make the distribution of covariate more balanced between the control group (i.e., households without access to grid electricity in our application) and treatment groups (i.e., households with access to grid electricity in our application). The covariate balance would be irrelevant if the regression models used above are correctly specified. However, because we do not know the correct underlying model, our DID regression results are potentially confounded with the combination of unbalanced covariates and covariate-dependent time trends. While the change-on-level regressions address the issue of covariate-dependent time trend, it does not address the effect of selection on time-invariant unobservable variables. Therefore, we also run a DID regression with matched sample as elaborated below.

In column (1) of Table 11, we report our baseline PSM estimate. In this estimation, each observation in the treatment group is matched with a closest neighbor in the control group (i.e., households without access to grid electricity) as measured by the probit estimate of the probability of getting the treatment. For each treatment observation, we are able to obtain a unique neighbor and the covariate are well-balanced between the (counterfactual) control and treatment groups after matching.[10] Therefore, an impact estimate of the household's access to grid electricity in round 1 on fertility can

[10] We did not use calipers because we were always able to find a close enough observation to each treatment observation. It should also be noted that some control observations were matched with multiple treatment observations.

Table 12: Falsification regression results (weighted).

Dep var: NCH_{ht}	(1)	(2)	(3)	(4)
Grid electricity $\times I_{t2}$	-0.0373	0.0753	-0.0414	-0.878
	(0.407)	(0.523)	(0.391)	(1.219)
Solar electricity $\times I_{t2}$		0.176		
		(0.989)		
Ratio of boys among children under 15			-0.370	0.208
			(0.542)	(0.655)
log (HH expenditure per capita)			-0.359	-0.273
			(0.390)	(0.515)
Grid electricity	0.174	0.239	0.0995	0.902
	(0.604)	(0.632)	(0.605)	(0.828)
Solar electricity		1.900*		
		(1.052)		
I_{t2}	-0.251	-0.388	0.0268	0.750
	(0.295)	(0.444)	(0.431)	(1.235)
R^2	0.817	0.829	0.820	0.838
Observations	362	362	362	212

Note: All regressions include household-level fixed-effects terms. Standard errors clustered at the subdistrict level are reported in parentheses. ***, ***, and * denote statistical significance at 1, 5, and 10 percent levels, respectively. Column (4) is based on the households in electrified villages only.

be obtained by taking the difference in the change in the number of children ΔNCH_h between these two groups. Because the matching is done only for treatment observations, this estimate corresponds to the average treatment effect on the treated (ATET). The estimated results are close to the baseline DID estimate.

In column (2), we report an estimate of the average treatment effect (ATE). This is similar to ATET except that observations in the control groups are also matched with their closest neighbors in the treatment group and used to calculate the estimated treatment effect. Column (3) is the same as column (1) except that logit model is used instead of the probit model. Column (4) is also the same as column (1) except that only those households within electrified villages are used for the analysis. Therefore, the treatment households are matched with a relatively small set of control households in this analysis. The results reported in columns (2)-(4) are similar to column (1) in Table 11.

In column (5), we report a DID regression estimate of β (i.e., the coefficient on $HELC_{ht} \times I_{h2}$) of the impact of electrification using the matched sample used in column (1) of Table 11, where the regression specification is the same as column (1) of Table 5. Because there are 1,037 treatment households in round 1, there are a total of 2,074 households and 4,148 observations over two rounds when the households in the counterfactual control group is included. Because this model includes the household-level fixed effects, it controls for all household-level time-invariant characteristics. All the estimates reported in Table 11 are consistent both qualitatively and quantitatively with our baseline DID estimates.

Falsification test

Once the woman has passed her child-bearing age, we would expect that access to electricity has no impact on the number of surviving children. Therefore, we conduct a falsification test using a sample of panel households in which the spouse's age remain between 51 and 75 between the two rounds, because women in this age group rarely bear a child. We will refer to this sample as the falsificaton sample.

Table 12 presents the results of the same regressions as those presented in Table 5 except that the falsification sample is used. As the first row shows, none of the point estimates is negative and significant and all point estimates except for column (4) are close to zero. Further, all point estimates are small compared with their standard errors. Further, the estimated coefficient on HELC_{ht} is larger in absolute value than that on $\text{HELC}_{ht}T_{ht}^2$. Therefore, if we follow the interpretation mentioned above, the impact of partial electrification on fertility would be positive, which is different from what we observed earlier. Therefore, the results of the falsification test overall show that rural electrification had no impact on fertility of women who have already passed their reproductive period as expected.

Consistency with the theoretical model

Our results strongly indicate that the impact of rural electrification significantly reduces fertility and most estimates are about 0.2 fewer children over the five year period between 2005 and 2010. This finding, in turn, appears to indicate $n_*(e) < 0$. Thus, we now check the consistency of our empirical results with the model assumptions and predictions discussed in section 4. Specifically, we test the signs of α', I', l', and c' based on eqs. (4), (5), (8), and (9).

To test the signs of α' and l', we use the wife's time-use data collected in round 1 survey, including both panel and nonpanel households. However, the time-use data is incomplete in the sense that we only know how many hours a day the wife spent on each of the following 18 activities over the last 24 hours of the survey: (1) listening to the radio, (2) watching TV, (3) processing food, (4) collecting fuel, (5) working as an agricultural worker, (6) working as a nonagricultural worker, (7) engaging in other income-generating activities, (8) fetching water, (9) washing clothes and cleaning, (10) cooking and serving, (11) eating, (12) bathing or caring for one's body, (13) shopping, (14) resting (excluding sleeping), (15) socializing, (16) performing religious practices, (17) reading and studying, and (18) taking care of children. We denote the number of hours spent on the jth activity ($1 \leq j \leq 18$) by τ_j and the total number of hours spent on these activities by $T \equiv \sum_{1 \leq j \leq 18} \tau_j$.

This list presumably covers most important activities that are performed during the effective lighted hours. However, there may exist other activities that are not appropriately covered in this list. For example, if one has to commute to a workplace, time spent traveling may not be captured in this list. Furthermore, activities such as listening to the radio can be done without light or simultaneously with other activities. However, the data limitation leads us to ignore these possibilities and assume that (1) the listed activities are performed only during the effective lighted time, (2) they are the only activities performed during the effective lighted time, and (3) each of them is performed separately. When we have a missing value of τ_j for some j, we treat the missing value as zero. To avoid including those households for which the time-use records appear highly incomplete or seemingly problematic, we dropped 34 panel households (1.3 percent of observations) for which $12 \leq T \leq 22$ was not satisfied.

As is clear from the definition of α, this quantity can be calculated only from those households

Table 13: Testing the consistency with assumptions and theoretical predictions (weighted, only panel households in round 1).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep var	α	α	I	I	l	l	c	c
Grid electricity	-0.144	-0.127	0.159***	0.066**	0.280*	0.251	0.399***	0.317***
	(0.104)	(0.095)	(0.036)	(0.027)	(0.164)	(0.159)	(0.049)	(0.042)
Covariates	No	Yes	No	Yes	No	Yes	No	Yes
R^2	0.151	0.175	0.218	0.345	0.202	0.211	0.210	0.281
Obs	2,356	2,356	2,356	2,356	2,356	2,356	2,356	2,356

Note: All regressions include household-level fixed-effects terms. Covariates include the total number of children of married woman, the ratio of boys among children under 15, the age and age squared of the head and spouse, their education indicators (i.e., "at least some primary", "at least some lower secondary", and "at least some upper secondary"), and the logarithmic household expenditure per capita. $I_{t2} \equiv \mathbf{1}(t=2)$ is an indicator variable for round 2. Standard errors clustered at the subdistrict level are reported in parentheses. ***, ***, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

with at least one child. Therefore, we restrict our sample to the set of households with at least one child and calculate α by $\alpha = \tau_{18}/T/n$ because it corresponds to the proportion of the lighted hours spent on taking care of each child on average. Similarly, because l is the proportion of the lighted hours not spent on taking care of children, we calculate l by $l = 1 - \tau_{18}/T$.

Finding the empirical counterparts of the maximum potential income I and nonchild goods consumption c is also a challenge. For I, it may be computed, in principle, by dividing the income from work by the fraction of the effective lighted time used to generate that income. However, the data does not allow us to clearly distinguish between nonwork and work incomes. Further, the data do not contain time-use information for men, who are generally the main income earner of the household. Therefore, we choose to use the logarithmic household income per capita as a proxy, assuming that the fraction of lighted hours used to generate income does not vary much across households. For c, because we are unable to distinguish between consumption expenditure for children and adults, we use the logarithmic total consumption expenditure exclusive of food, education, and health care as a proxy for the consumption of nonchild goods.

To test the signs of α', I', l', and c', we run OLS regressions of α, I, l, and c on the household's access to electricity in round 1 ($HELC^1$). We report the results of regressions with and without the covariates using a sample of households with at least one child in Tables 13 and 14, where the former only uses panel households and the latter includes nonpanel households as well.

Column (1) of each of these tables shows that the coefficient on α is estimated to be negative, though not significant. Column (2) shows that this point remains valid even with covariates. These results support the assumption that $\alpha' \leq 0$. Columns (3) and (4) show that the coefficient on I is significantly positive whether or not covariate are included, which supports the assumption that $I' > 0$.

Columns (5) and (6) show that the coefficient on l is positive, even though they are mostly insignificant. Columns (7) and (8) show that the coefficient on c is significant and positive. Thus, as expected from the empirical evidence for $n' < 0$ and Proposition 1, we empirically observe $l' > 0$ and

Table 14: Testing the consistency with assumptions and theoretical predictions (weighted, all households in round 1).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep var	α	α	I	I	l	l	c	c
Grid electricity	-0.044	-0.031	0.189***	0.083***	0.109	0.107	0.416***	0.308***
	(0.051)	(0.049)	(0.015)	(0.012)	(0.107)	(0.107)	(0.020)	(0.019)
Covariates	No	Yes	No	Yes	No	Yes	No	Yes
R^2	0.071	0.079	0.177	0.305	0.097	0.098	0.188	0.263
Obs	14,771	14,771	14,771	14,771	14,771	14,771	14,771	14,771

Note: All regressions include household-level fixed-effects terms. Covariates include the total number of children of married woman, the ratio of boys among children under 15, the age and age squared of the head and spouse, their education indicators (i.e., "at least some primary", "at least some lower secondary", and "at least some upper secondary"), and the logarithmic household expenditure per capita. $I_{t2} \equiv \mathbf{1}(t = 2)$ is an indicator variable for round 2. Standard errors clustered at the subdistrict level are reported in parentheses. ***, ***, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

$c' > 0$, even though the former is not statistically significant. Therefore, our empirical results given in Tables 13 and 14 are consistent with our model assumptions and predictions.

So far, we have ignored the endogeneity of the household's access to electricity. Therefore, we also ran (unreported) regressions with the household's access to electricity instrumented again by the indicator variable for electrified village and system loss from the grid in 2005. In this case, the coefficient on α tends to become more negative but the coefficient on c tends to get smaller regardless of whether we include nonpanel households in the analysis. Therefore, while the validity of the instruments for income and consumption may be questionable, we have no evidence that the theoretical prediction in Proposition 1 is empirically violated.

Exploring the causal channels

The analysis presented so far gives robust evidence that the impact of rural electrification on fertility is significantly negative both economically and statistically. Our finding is also consistent with the theoretical predictions and passes the falsification test. However, it does not show why there is a negative impact. In this subsection, we explore a potentially important causal channel. Specifically, we include the possession of televisions and mobile phones in the DID regressions, because televisions and mobile phones would promote the dissemination of important information about family planning, various income-generating opportunities, and other issues, which may affect fertility.

Column (1) of Table 15 shows that the estimated impact of grid electricity on fertility (first row) becomes smaller in absolute value and insignificant once the possession of TV is included but the same cannot be said about mobile phone as column (2) shows. These results do not change even when we simultaneously include both the indicator variables for the possession of TV and mobile phone as column (3) shows. Ignoring the potential endogeneity of the possession of TV, our results indicate that TV is an important channel through which fertility is reduced.

While we do not have measurements of time spent on watching TV that are comparable between the two rounds, we are able to construct an indicator (TVWCH) on whether the spouse watches

Table 15: Difference-in-differences regressions with the indicators for the possession of TV and mobile phone (weighted).

	(1)	(2)	(3)	(4)
Dep var	NCH_{ht}	NCH_{ht}	NCH_{ht}	TVWCH_{ht}
Grid electricity $\times I_{t2}$	-0.0763	-0.210**	-0.0889	0.237***
	(0.105)	(0.0832)	(0.105)	(0.0481)
HH has a TV $\times I_{t2}$	-0.201*		-0.226**	
	(0.105)		(0.109)	
HH had a mobile phone $\times I_{t2}$		0.0357	0.113	
		(0.171)	(0.181)	
Ratio of boys among children under 15	0.0683	0.0643	0.0685	0.120*
	(0.155)	(0.154)	(0.154)	(0.0687)
log (HH expenditure per capita)	-0.166**	-0.180***	-0.172***	0.00560
	(0.0648)	(0.0642)	(0.0645)	(0.0268)
Grid electricity	0.0375	0.111	0.0411	0.213***
	(0.113)	(0.0957)	(0.113)	(0.0611)
HH has a TV	0.0741		0.0763	
	(0.106)		(0.109)	
HH has a mobile phone		0.0106	-0.0301	
		(0.151)	(0.160)	
I_{t2}	0.470***	0.434***	0.433***	0.0272
	(0.0696)	(0.0878)	(0.0876)	(0.0368)
R^2	0.872	0.871	0.872	0.675
Observations	5,084	5,084	5,084	5,084

Note: All regressions include household-level fixed-effects terms. $I_{t2} \equiv \mathbf{1}(t=2)$ is an indicator variable for round 2. Standard errors clustered at the subdistrict level are reported in parentheses. ***, ***, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

TV. Unfortunately, there is a slight discrepancy in its defintion across two rounds because of the survey design. That is, it takes one if the time the spouse spent on watching TV is positive over the last 24 hours [in daily average] in round 1 [round 2]. Ignoring this discrepancy in the definition, we ran a DID regression of this indicator on the access to grid electricity with a specification similar to column (1) of Table 5. As reported in column (4) of Table 15, we find a significantly positive impact of electrification on the probability of watching TV. Therefore, even though our results do not address endogeneity issue, they appear to provide suggestive evidence that rural electrification on fertility is negative because television provides people with more information or because it serves as an alternative form of entertainment.

8 Discussion

Numerous studies have examined the social and economic impacts of rural electrification. However, relatively few studies have investigated the impact of rural electrification on fertility in developing countries. As discussed in Section 2, the idea that there may be a relationship between the availability of electricity and fertility is not new by itself. However, rigorous econometric studies using household-level data remain scarce. To our knowledge, this is the first study that use household-level panel data to study the causal impact of rural electrification on fertility.

Our main finding is that rural electrification negatively affects fertility. This finding is robust with respect to (1) the choice of estimation methods, (2) the choice of sample (i.e., proper or retrospective panel), and (3) potential sources of endogeneity. Moreover, the results passes the falsification test and are consistent with the predictions from our theoretical model. We also provide suggestive evidence that an important causal channel is television, because the inclusion of TV possession in the DID regression makes the estimated effect of electrification smaller by more than half and statistically insignificant. The finding that better access to television tends to lead to lower fertility is consistent with previous studies such as Ferrara et al. (2012), Grimm et al. (2015), and Jensen and Oster (2009). It is also consistent with our theoretical model because people may spend more time watching TV than child-related activities.

The current study made several notable contributions. First, by using the panel data, we were able to control for all the unobserved time-invariant heterogeneity across households, which was not possible with cross-sectional data. We also addressed the potential endogeneity by instrumental variables and used change-on-level regression as an alternative specification. Further, we also checked the robustness of our results by PSM. We consistently showed that the impact of electrification on fertility is negative and both economically and statistically significant. Further, the point estimates are broadly consistent with each other, ranging between 0.1 and 0.25 for most estimates.

Second, to make our results even more credible, we proposed a falsification test using a sample of women who past their reproductive period, which is typically not conducted in the analysis of fertility. This is potentially important because this provides additional, indirect evidence that our approach is reasonable. No other study we are aware of conducted a falsification test based on a sample of women who have past their reproductive period.

Third, to our knowledge, there has been no formal treatment of the impact of electrification on fertility. Even though our model is simple and based on a set of strong assumptions, it does elucidate the factors that determine the direction of the impact of rural electrification on fertility. In particular, the discussion in Section 4 indicates that the impact is likely to be negative in relatively poor areas but may be positive in richer areas. Further, our theoretical model also provides a prediction on consumption and time-use behavior. Our empirical results are consistent with the model prediction.

Finally, this study highlights the possibility that rural electrification has a significant social impact that goes well beyond those typically considered in impact assessment studies. Therefore, this study calls for a broader assessment of rural electrification and potentially other infrastructure projects to fully understand their potential impacts. Full understanding of potential impacts in turn would alter how policy makers approach the policy formulation of large infrastructure investment.

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Appendix A:Proof of Proposition 1

Taking the first-order condition for the utility maximization problem, it can be shown that c_* and n_* satisfy the following condition:

$$\omega[p_n(e) + I(e)\alpha(e)]f'(c_*(e), e) = (1-\omega)g'(n_*(e)). \tag{14}$$

Note that the term $I(e)\alpha(e)$ in the square brackets on the left hand side of eq. (14) can be interpreted as the opportunity cost of having one child because it corresponds to the amount of income that could be earned using the time spent raising one child. Therefore, $[p_n(e) + I(e)\alpha(e)]$ represents the total economic cost of having one child and eq. (14) allows the usual interpretation of first-order condition that the marginal utility per price from child goods equals that from nonchild goods.

Taking a total differentiation of eqs. (2), (3), and (14) with respect to e and solving for $n'_*(e)$, we obtain the following results:

$$n'_*(e) = \frac{\omega V(e)}{(1-\omega)g''(n_*(e)) + \omega[p_n(e) + I(e)\alpha(e)]^2 f''(c_*(e), e)}. \tag{15}$$

Because the denominator of eq. (15) is unambiguously negative from the concavity assumption about f and g, we can see that $n'_*(e)$ has an opposite sign of V.

By taking the total differentiation of eqs. (2) and (3) with respect to e and applying eq. (15), we obtain eqs. (8) and (9). The latter part of Proposition 1 follows from these and eqs.(4)–(6).

Appendix B: Additional tables

In this section, we present additional results that do not apply sample weights. Tables 16 and 17 are unweighted versions of Tables 3 and 4 and present the average change in the number of children between rounds 1 and 2 and between rounds 0 and 1, respectively. Table 18. Table 18 is an weighted version of Table 1 and provides unweighted summary statistics of the panel data. Tables 19–23 correspond to the unweighted version of the DID regressions presented in Tables 5–9. Table 24 provides the unweighted version of the change-on-level regressions in Table 10, whereas the unweighted version of the falsification regressions in Table 25 are given in Table 12. The unweighted versions of the tests of asumptions and model predictions in Tables 13 and 14 are presented in Tables 26 and 27. Finally, Table 28 is an unweighted version of Table 15.

Table 16: The average change in the number of children between the two survey rounds (ΔNCH_{h1}) by the number of children (NCH_{h1}) and the access to electricity (HELC_{h1}) in round 1.

	$\text{HELC}_{h1}=0$				$\text{HELC}_{h1}=1$				Difference		
NCH_{h1}	μ_0		(s.e.)	Obs	μ_1		(s.e.)	Obs	$\mu_1-\mu_0$		(s.e.)
0	2.065	***	(0.111)	107	1.880	***	(0.166)	50	-0.185		(0.199)
1	0.700	***	(0.048)	240	0.612	***	(0.066)	147	-0.088		(0.081)
2	0.354	***	(0.037)	443	0.266	***	(0.045)	301	-0.089		(0.058)
3	0.242	***	(0.050)	356	-0.012		(0.057)	256	-0.253	***	(0.076)
4+	-0.209	***	(0.058)	359	-0.336	***	(0.071)	283	-0.127		(0.092)
Total	0.370	***	(0.028)	1,505	0.160	***	(0.034)	1,037	-0.210	***	(0.044)

Note: The means μ_0 and μ_1 are the mean of the change in the number of children (ΔNCH) for nonelectrified and electrified households, respectively. Statistical significance of a two-sided t-test of inequality for the population mean μ with $H_0:\mu=0$ and $H_a:\mu\neq 0$ at 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

Table 17: The average change in the number of children between rounds 0 and 1 (ΔNCH_{h0}) by the number of children and the access to grid electricity (HELC_0) in round 0 using the retrospective panel data.

	$\text{HELC}_{h0}=0$				$\text{HELC}_{h0}=1$				Difference		
NCH_{h0}	μ_0		(s.e.)	Obs	μ_1		(s.e.)	Obs	$\mu_1-\mu_0$		(s.e.)
0	1.031	***	(0.018)	1,284	1.038	***	(0.038)	289	0.007		(0.042)
1	0.752	***	(0.016)	1,772	0.668	***	(0.027)	540	-0.084	***	(0.031)
2	0.442	***	(0.013)	2,176	0.341	***	(0.020)	765	-0.101	***	(0.024)
3	0.301	***	(0.015)	1,414	0.241	***	(0.021)	558	-0.061	**	(0.025)
4+	0.320	***	(0.020)	816	0.186	***	(0.027)	308	-0.134	***	(0.033)
Total	0.578	***	(0.008)	7,462	0.453	***	(0.012)	2,460	-0.125	***	(0.015)

Note: The means μ_0 and μ_1 are the mean of the change in the number of children (ΔNCH) for nonelectrified and electrified households, respectively. The round 0 is the year 2000 in this table. Statistical significance of a two-sided t-test of inequality for the population mean μ with $H_0:\mu=0$ and $H_a:\mu\neq 0$ at 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

Table 18: Key summary statistics for rounds 1 and 2 by the household's access to electricity (panel households only).

Survey round	Round 1						Round 2					
Access to grid electricity ($HELC_{ht}$)	$HELC_{h1} = 0$		$HELC_{h1} = 1$		Total		$HELC_{h2} = 0$		$HELC_{h2} = 1$		Total	
Description	Mean	(s.d.)	Mean	(s.d.)	Mean	(s.d.)	Mean	(s.d.)	Mean	(s.d.)	Mean	(s.d.)
Age of household head	39.79	(8.01)	41.81	(7.67)	40.61	(7.93)	44.17	(8.21)	46.39	(8.37)	45.41	(8.37)
Age of spouse	32.09	(6.87)	33.82	(6.72)	32.79	(6.86)	36.11	(6.64)	37.59	(6.68)	36.93	(6.71)
Ratio of boys among children under 15†	0.504	(0.363)	0.530	(0.363)	0.515	(0.363)	0.517	(0.351)	0.510	(0.359)	0.513	(0.355)
Number of surviving children	2.582	(1.530)	2.766	(1.540)	2.657	(1.537)	2.957	(1.413)	2.929	(1.450)	2.941	(1.434)
Head has at least some primary educ.	0.577	(0.494)	0.784	(0.412)	0.662	(0.473)	0.668	(0.471)	0.758	(0.428)	0.718	(0.450)
Head has at least some junior sec. educ.	0.364	(0.481)	0.521	(0.500)	0.428	(0.495)	0.366	(0.482)	0.454	(0.498)	0.415	(0.493)
Head has at least some senior sec. educ.	0.165	(0.371)	0.295	(0.456)	0.218	(0.413)	0.192	(0.394)	0.245	(0.430)	0.221	(0.415)
Spouse has at least some primary educ.	0.553	(0.497)	0.724	(0.447)	0.623	(0.485)	0.638	(0.481)	0.735	(0.441)	0.692	(0.462)
Spouse has at least some junior sec. educ.	0.302	(0.459)	0.388	(0.487)	0.337	(0.473)	0.276	(0.447)	0.351	(0.478)	0.318	(0.466)
Spouse has at least some senior sec. educ.	0.082	(0.274)	0.130	(0.337)	0.101	(0.302)	0.082	(0.274)	0.107	(0.310)	0.096	(0.295)
log (HH expenditure per capita)‡	3.274	(0.395)	3.419	(0.389)	3.333	(0.399)	3.926	(0.512)	4.083	(0.587)	4.013	(0.560)
HH has a TV	0.001	(0.036)	0.639	(0.480)	0.262	(0.440)	0.149	(0.356)	0.693	(0.461)	0.452	(0.498)
HH has a mobile phone	0.000	(0.000)	0.077	(0.267)	0.031	(0.175)	0.531	(0.499)	0.818	(0.386)	0.691	(0.462)
Number of observations	1,505		1,037		2,542		1,127		1,415		2,542	

†: The average was taken over those households with at least one child under the age of 15. Therefore, the number of observations used for this row is about 10-15 percent lower than that for other rows in the same column.

‡: Household expenditure is expressed in Bangladesh Taka (BDT) per day. The PPP conversion factor for private consumption is USD 1=BDT 17.88 in 2005 and USD 1=BDT 23.15 in 2010 according to the World Development Indicators (`http://data.worldbank.org/indicator` accessed on May 27, 2016).

Table 19: Main difference-in-differences regressions.

Dep var: NCH_{ht}	(1)	(2)	(3)	(4)
Grid electricity $\times I_{t2}$	-0.235***	-0.250***	-0.226***	-0.314**
	(0.0727)	(0.0757)	(0.0715)	(0.126)
Solar electricity $\times I_{t2}$		-0.121		
		(0.166)		
Ratio of boys among children under 15			-0.00559	-0.0863
			(0.102)	(0.144)
log (HH expenditure per capita)			-0.192***	-0.166*
			(0.0587)	(0.0843)
Grid electricity	0.162*	0.165**	0.158*	0.108
	(0.0824)	(0.0827)	(0.0832)	(0.117)
Solar electricity		0.0660		
		(0.235)		
I_{t2}	0.391***	0.405***	0.517***	0.577***
	(0.0506)	(0.0541)	(0.0630)	(0.115)
R^2	0.869	0.869	0.870	0.874
Observations	5,084	5,084	5,084	2,900

Note: All regressions include household-level fixed-effects terms. $I_{t2} \equiv \mathbf{1}(t = 2)$ is an indicator variable for round 2. Standard errors clustered at the subdistrict level are reported in parentheses. ***, ***, and * denote statistical significance at 1, 5, and 10 percent levels, respectively. Column (4) is based on the households in electrified villages only.

Table 20: Regression results based on synthetic panel.

Dep var: NCH_{ht}	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Grid electricity $\times I_{t1}$	-0.00717	-0.0486***	-0.114***	-0.235***	-0.391***	-0.144**		-0.146***
	(0.00627)	(0.0166)	(0.0311)	(0.0487)	(0.0704)	(0.0592)		(0.0549)
Grid electricity $\times I_{t2}$							-0.112*	-0.265***
							(0.0612)	(0.0762)
log (HH expenditure per capita)							-0.308***	
							(0.0975)	
Ratio of boys among children under 15							-0.179	
							(0.145)	
Grid electricity	0.0229	0.0344	0.0244	0.0871	0.167*	0.0713	0.0113	0.0999
	(0.0172)	(0.0332)	(0.0467)	(0.0665)	(0.0946)	(0.0788)	(0.0900)	(0.0794)
I_{t1}	0.0480***	0.210***	0.501***	0.946***	1.538***	0.671***		0.667***
	(0.00509)	(0.0139)	(0.0265)	(0.0443)	(0.0665)	(0.0365)		(0.0321)
I_{t2}							0.602***	1.085***
							(0.0758)	(0.0607)
Year in Round 0	2004	2003	2002	2001	2000	2000	N/A	2000
Rounds	0-1	0-1	0-1	0-1	0-1	0-1	1-2	0-2
R^2	0.993	0.963	0.902	0.821	0.751	0.929	0.886	0.840
Observations	19,844	19,844	19,844	19,844	19,844	2,802	2,802	4,203

Note: All regressions include household-level fixed-effects terms. $I_{tj} \equiv \mathbf{1}(t = j)$ is an indicator variable for round $j \in \{1, 2\}$. Standard errors clustered at the subdistrict level are reported in parentheses. ***, ***, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

Table 21: Impact heterogeneity by the initial number of children.

Dep var: NCH_{ht}	(1)	(2)	(3)	(4)
Threshold no children (v)	$v = 1$	$v = 2$	$v = 3$	$v = 4$
Grid electricity $\times I_{t2}$	-0.283	-0.199	-0.169*	-0.189**
	(0.301)	(0.153)	(0.0945)	(0.0797)
Grid electricity $\times I_{t2} \times \mathbf{1}(\text{NCH}_{h1} \geq v)$	0.0950	0.0112	-0.0503	-0.0327
	(0.310)	(0.163)	(0.130)	(0.143)
Ratio of boys among children under 15	-0.0248	0.00581	0.0155	-0.0105
	(0.0955)	(0.0941)	(0.0974)	(0.0973)
log (HH expenditure per capita)	-0.153***	-0.115**	-0.138***	-0.154***
	(0.0517)	(0.0465)	(0.0503)	(0.0533)
Grid electricity	0.106	0.121	0.133	0.146*
	(0.0774)	(0.0798)	(0.0824)	(0.0761)
I_{t2}	2.221***	1.203***	0.787***	0.652***
	(0.195)	(0.114)	(0.0780)	(0.0705)
$I_{t2} \times \mathbf{1}(\text{NCH}_{h1} \geq v)$	-1.855***	-0.957***	-0.649***	-0.693***
	(0.203)	(0.119)	(0.0997)	(0.118)
R^2	0.891	0.887	0.883	0.881
Observations	5,084	5,084	5,084	5,084

Note: All regressions include household-level fixed-effects terms. $I_{t2} \equiv \mathbf{1}(t = 2)$ is an indicator variable for round 2. Standard errors clustered at the subdistrict level are reported in parentheses. ***, ***, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

Table 22: Heterogeneity of impact by the duration of electricity access and outage.

Dep var: NCH_{ht}	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Threshold length of access (k)	$k=5$	$k=10$	$k=15$	$k=20$			
Grid electricity $\times I_{t2}$	-0.174*	-0.209**	-0.220***	-0.241***	-0.196**	-0.294***	-0.259**
	(0.103)	(0.0857)	(0.0794)	(0.0740)	(0.0979)	(0.105)	(0.121)
Grid electricity $\times I_{t2} \times$ (at least k years of access)	-0.0831	-0.0447	-0.0241	0.190			
	(0.102)	(0.0930)	(0.117)	(0.177)			
Grid electricity $\times I_{t2} \times$ (Frequent outage at $t=1$)					-0.0555		-0.0783
					(0.107)		(0.101)
Grid electricity $\times I_{t2} \times$ (Frequent outage at $t=2$)						0.121	0.132
						(0.104)	(0.102)
Ratio of boys among children under 15	-0.00621	-0.00491	-0.00563	-0.00293	-0.00519	-0.00844	-0.00826
	(0.103)	(0.102)	(0.102)	(0.102)	(0.102)	(0.102)	(0.102)
log (HH expenditure per capita)	-0.188***	-0.191***	-0.191***	-0.195***	-0.198***	-0.191***	-0.199***
	(0.0585)	(0.0583)	(0.0583)	(0.0587)	(0.0604)	(0.0601)	(0.0616)
Grid electricity	0.136	0.137	0.151*	0.168**	0.205*	0.190	0.225
	(0.104)	(0.0894)	(0.0848)	(0.0840)	(0.115)	(0.132)	(0.145)
Grid electricity $\times$ (at least k years of access)	-0.209	0.103	0.103	0.328			
	(0.282)	(0.252)	(0.663)	(1.148)			
Grid electricity $\times$ (Frequent outage at $t=1$)					-0.0909		-0.0777
					(0.164)		(0.164)
Grid electricity $\times$ (Frequent outage at $t=2$)						-0.0579	-0.0453
						(0.168)	(0.168)
I_{t2}	0.506***	0.517***	0.517***	0.520***	0.522***	0.518***	0.523***
	(0.0634)	(0.0635)	(0.0630)	(0.0634)	(0.0643)	(0.0637)	(0.0649)
R^2	0.871	0.870	0.870	0.870	0.870	0.871	0.871
Observations	5,084	5,084	5,084	5,084	5,084	5,084	5,084

Note: All regressions include household-level fixed-effects terms. $I_{t2} \equiv \mathbf{1}(t=2)$ is an indicator variable for round 2. Standard errors clustered at the subdistrict level are reported in parentheses. ***, ***, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

Table 23: Instrumental variable regression results.

Dep var	(1) NCH_t	(2) $HELC_t$	(3) $HELC_t \times T_{t2}$	(4) NCH_t
Grid electricity $\times I_{t2}$	-0.132*			-0.276**
	(0.069)			(0.120)
log (HH expenditure per capita)	-0.196***	0.010	0.032**	-0.188***
	(0.041)	(0.013)	(0.015)	(0.0587)
Ratio of boys among children under 15	-0.001	-0.007	-0.048*	-0.00887
	(0.071)	(0.024)	(0.025)	(0.102)
Grid electricity	0.414***			0.148
	(0.127)			(0.111)
I_{t2}	0.430***	-0.042	-0.015	0.439***
	(0.060)	(0.064)	(0.069)	(0.0891)
Electrified village		0.358***	-0.344***	0.0329
		(0.040)	(0.039)	(0.112)
Electrified village $\times I_{t2}$		0.081***	0.807***	0.122
		(0.014)	(0.016)	(0.125)
System loss from the grid		-1.631**	-1.807***	
		(0.699)	(0.670)	
System loss from the grid $\times I_{t2}$		0.479	0.156	
		(0.460)	(0.488)	
Estimation	2SLS (Stg 2)	2SLS (Stg 1)	2SLS (Stg 1)	OLS
First stage F		33.6***	681.22***	
p-Value for OIR	0.194			
Obs	5,084	5,084	5,084	5,084

Note: All regressions include household-level fixed-effects terms. $I_{t2} \equiv \mathbf{1}(t = 2)$ is an indicator variable for round 2. Standard errors clustered at the subdistrict level are reported in parentheses. ***, ***, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

Table 24: Change on level regressions.

Dep var	(1) ΔNCH_h	(2) ΔNCH_h	(3) ΔNCH_h	(4) $HELC_{ht}$
Grid electricity in round 1	-0.181***	-0.110**	-0.128	
	(0.052)	(0.052)	(0.087)	
Total number of children of married woman		-0.394***	-0.393***	0.010
		(0.026)	(0.025)	(0.007)
Ratio of boys among children under 15		-0.128**	-0.127**	0.009
		(0.056)	(0.053)	(0.019)
Age of household head		-0.015	-0.014	0.018
		(0.034)	(0.033)	(0.011)
Age of household head squared		0.018	0.018	-0.022*
		(0.040)	(0.038)	(0.012)
Age of spouse		0.024	0.023	-0.016
		(0.045)	(0.043)	(0.013)
Age of spouse squared		-0.027	-0.027	0.028
		(0.067)	(0.064)	(0.018)
Head has at least some primary educ.		0.045	0.047	0.061**
		(0.059)	(0.058)	(0.024)
Head has at least some junior sec. educ.		-0.006	-0.006	0.030
		(0.064)	(0.061)	(0.021)
Head has at least some senior sec. educ.		-0.009	-0.008	0.025
		(0.068)	(0.066)	(0.024)
Spouse has at least some primary educ.		0.101**	0.103**	0.068***
		(0.048)	(0.047)	(0.022)
Spouse has at least some junior sec. educ.		-0.035	-0.035	-0.004
		(0.059)	(0.056)	(0.020)
Spouse has at least some senior sec. educ.		-0.196**	-0.196**	0.027
		(0.079)	(0.076)	(0.026)
log (HH expenditure per capita)		-0.258***	-0.255***	0.126***
		(0.069)	(0.068)	(0.022)
System loss from the grid (%)				-0.026**
				(0.013)
Electrified village				0.688***
				(0.020)
Estimation	OLS	OLS	2SLS (Stg 2)	2SLS (Stg 1)
Subdistrict Fixed Effects	Yes	Yes	Yes	Yes
R^2	0.089	0.315	0.253	
First Stage F-statistic				673.34***
Hansen J-statistic			0.553	
Observations	2,542	2,542	2,542	2,542

Note: Standard errors clustered at the subdistrict level are reported in parentheses. ***, ***, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

Table 25: Falsification regression results.

Dep var: NCH_{ht}	(1)	(2)	(3)	(4)
Grid electricity $\times I_{t2}$	-0.127	-0.0970	-0.134	-0.900
	(0.437)	(0.456)	(0.435)	(1.152)
Solar electricity $\times I_{t2}$		-0.121		
		(0.961)		
Ratio of boys among children under 15			0.0433	0.445
			(0.525)	(0.604)
log (HH expenditure per capita)			-0.241	-0.143
			(0.354)	(0.471)
Grid electricity	0.332	0.382	0.320	1.138
	(0.666)	(0.685)	(0.672)	(0.903)
Solar electricity		1.491		
		(1.245)		
I_{t2}	-0.107	-0.151	0.0478	0.723
	(0.349)	(0.373)	(0.426)	(1.150)
R^2	0.818	0.822	0.818	0.835
Observations	362	362	362	212

Note: All regressions include household-level fixed-effects terms. $I_{t2} \equiv \mathbf{1}(t=2)$ is an indicator variable for round 2. Standard errors clustered at the subdistrict level are reported in parentheses. ***, ***, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

Table 26: Testing the consistency with assumptions and theoretical predictions (only panel households in round 1).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep var	α	α	I	I	l	l	c	c
Grid electricity	-0.106	-0.094	0.159***	0.063***	0.274*	0.253*	0.435***	0.351***
	(0.092)	(0.083)	(0.029)	(0.021)	(0.149)	(0.144)	(0.047)	(0.041)
Covariates	No	Yes	No	Yes	No	Yes	No	Yes
R^2	0.131	0.147	0.213	0.338	0.170	0.176	0.214	0.277
Obs	2,356	2,356	2,356	2,356	2,356	2,356	2,356	2,356

Note: All regressions include household-level fixed-effects terms. Covariates include the total number of children of married woman, the ratio of boys among children under 15, the age and age squared of the head and spouse, their education indicators (i.e., "at least some primary", "at least some lower secondary", and "at least some upper secondary"), and the logarithmic household expenditure per capita. $I_{t2} \equiv \mathbf{1}(t=2)$ is an indicator variable for round 2. Standard errors clustered at the subdistrict level are reported in parentheses. ***, ***, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

Table 27: Testing the consistency with assumptions and theoretical predictions (All households in round 1).

Dep var	(1) α	(2) α	(3) I	(4) I	(5) l	(6) l	(7) c	(8) c
Grid electricity	-0.048	-0.035	0.176***	0.079***	0.127	0.123	0.410***	0.311***
	(0.046)	(0.046)	(0.014)	(0.011)	(0.088)	(0.089)	(0.020)	(0.018)
Covariates	No	Yes	No	Yes	No	Yes	No	Yes
R^2	0.055	0.062	0.155	0.285	0.071	0.072	0.175	0.250
Obs	14,771	14,771	14,771	14,771	14,771	14,771	14,771	14,771

Note: All regressions include household-level fixed-effects terms. Covariates include the total number of children of married woman, the ratio of boys among children under 15, the age and age squared of the head and spouse, their education indicators (i.e., "at least some primary", "at least some lower secondary", and "at least some upper secondary"), and the logarithmic household expenditure per capita. $I_{t2} \equiv \mathbf{1}(t = 2)$ is an indicator variable for round 2. Standard errors clustered at the subdistrict level are reported in parentheses. ***, ***, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

Table 28: Difference-in-differences regressions with the indicators for the possession of TV and mobile phone.

Dep var	(1) NCH_{ht}	(2) NCH_{ht}	(3) NCH_{ht}	(4) TVWCH_{ht}
Grid electricity $\times I_{t2}$	-0.104	-0.225***	-0.109	0.208***
	(0.102)	(0.0772)	(0.104)	(0.0476)
HH has a TV $\times I_{t2}$	-0.197*		-0.207**	
	(0.101)		(0.103)	
HH had a mobile phone $\times I_{t2}$		-0.0236	0.0469	
		(0.128)	(0.134)	
Ratio of boys among children under 15	-0.00386	-0.00584	-0.00351	0.0722
	(0.102)	(0.102)	(0.103)	(0.0500)
log (HH expenditure per capita)	-0.183***	-0.192***	-0.184***	0.0281
	(0.0592)	(0.0588)	(0.0592)	(0.0287)
Grid electricity	0.0809	0.156*	0.0821	0.259***
	(0.111)	(0.0858)	(0.111)	(0.0593)
HH has a TV	0.0953		0.0973	
	(0.103)		(0.106)	
HH has a mobile phone		0.0257	-0.0166	
		(0.114)	(0.120)	
I_{t2}	0.526***	0.516***	0.512***	0.0252
	(0.0626)	(0.0717)	(0.0717)	(0.0360)
R^2	0.871	0.870	0.871	0.682
Observations	5,084	5,084	5,084	5,084

Note: All regressions include household-level fixed-effects terms. $I_{t2} \equiv \mathbf{1}(t = 2)$ is an indicator variable for round 2. Standard errors clustered at the subdistrict level are reported in parentheses. ***, ***, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.