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Fertility and rural electrification in Bangladesh

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Abstract: We use contemporaneous and retrospective panel datasets to examine the household-level relationship between fertility and access to electricity in Bangladesh. We find that access to electricity reduces fertility by about 0.2 children over a period of five years or total fertility rate by about 1.2 in most estimates. This finding is robust with respect to the choice of the estimation method, the choice of sample, and potential presence of endogeneity. The finding also corroborates the theoretical predictions on time use and consumption pattern derived from our model of electrification and fertility. The results also suggest that television is an important impact channel. The study findings underscore the importance of examining a broad and long-term impact of rural electrification and possibly other infrastructure interventions.

Keywords: Bangladesh, Infrastructure, Television, Difference-in-differences, Retrospective panel data

1. Introduction

Access to electricity is essential for development. Welfare-enhancing utilities such as clean water supplies, improved sanitation, and modern health care services can be delivered efficiently with electricity. Electricity also enables households to enjoy reliable and efficient lighting and heating equipments, improved cooking facilities, robust mechanical power, better transport and telecommunications services, and overall a modern living.

A relatively unexplored area of the impact of electrification is fertility. There are several potential channels through which electrification may affect fertility. First, access to electricity may alter the time use, because electrified households can use additional lighted time for productive purposes. Second, electrified households may change the pattern of consumption and shift resources towards the goods that operate with electricity. Third, electrification often creates new income opportunities for households, thereby possibly altering the opportunity cost of time, especially for women. Finally, access to electricity facilitates information acquisition using electricity-powered devices such as television (TV). Such changes in household behavior could have implications for fertility behavior.

The possible link between fertility and electrification is important, especially for developing countries, as high fertility rates may result in underinvestment in human capital. In turn, this may result in low quality of human resources and unemployment. In the development literature, high fertility is regarded as one of the most important factors impeding long-term economic development (Ashraf et al., 2013). Notwithstanding the importance, there are only a few studies focusing on the impact of electrification on fertility.

To address this gap, we rigorously examine the relationship between fertility and access to electricity using household-level panel data from Bangladesh. For most of our analysis, we use the difference-in-

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differences (DID) estimation strategy to identify the change in fertility with access to electricity. The DID strategy allows us to control for both observable and unobservable time-invariant household characteristics and observable time-variant characteristics, thereby substantially mitigating the endogeneity concerns. We find that access to electricity lead to a significant reduction in fertility—both statistically and economically—by about 0.2 children within a five-year period or a reduction of total fertility rate by about 1.2 children during a woman's reproductive years. Putting these figures into perspective, it is observed that the total fertility rate in Bangladesh declined by 0.36 from 2.69 to 2.33 over our study period between 2005 and 2010, during which electricity access had significantly improved.

Our finding remains valid even when we address the potential endogeneity of electricity access using instrumental variables (IVs). While previous studies such as Dinkelman (2011) used the land gradient as the instrument for electrification, it is not a suitable instrument for our study because Bangladesh is a flat country where most of the land lies no more than 10 meter above the mean sea level. Instead, we instrument the household's access to electricity by the management efficiency of electricity cooperatives and village-level electrification status.

Our finding is also similar even when we use a retrospective panel dataset that is constructed based on information such as childbirths and duration of household's access to electricity, as detailed in section 5. To our knowledge, this is the first study to simultaneously use contemporaneous (i.e., nonretrospective) and retrospective panel datasets to evaluate the impact of an infrastructure intervention. We also conduct a battery of robustness checks to address potential concerns about our baseline DID specification to enhance the credibility of our finding.

Furthermore, 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 plausible joint prediction on the direction of change in fertility, time use, and consumption of nonchild goods with the adoption of electricity. Our empirical results are consistent with the model predictions.

Finally, we explore the mechanism that drives our results beyond the changes in time use. Our results suggest that the negative impact of electrification on fertility partly comes from the use of TV. This finding is consistent with previous studies such as Jensen and Oster (2009), Grimm et al. (2015), and Ferrara et al. (2012). Taken together, we present compelling evidence that rural electrification has contributed to the reduction in fertility in Bangladesh, and the use of TV is likely driving this impact.

The empirical findings presented in this paper are highly relevant to Bangladesh and other developing countries in Asia and perhaps elsewhere, since a large number of people are expected to gain access to electricity in upcoming decades. Currently, around one billion people lack access to electricity, particularly in rural areas of developing countries. More than two fifths of this population lives in developing Asia, according to the Energy Access Outlook 2017 by the International Energy Agency (IEA).[1] Nevertheless, this situation is likely to improve. For example, the Government of Bangladesh aims to bring all citizens under power coverage by 2021. The IEA estimates the electrification rate to reach 99 percent by 2030 in developing Asia.[2]

While it can be argued that randomized field experiments provide more credible empirical evidence, it is often difficult to conduct a field experiment to study the long-term impact of a major infrastructure project. Experimental approach may be impractical due to budgetary, logistical, and ethical considerations. Furthermore, when it takes a long time for the impact on the outcome of interest to become detectable, which is likely to be the case of fertility, it is crucial to keep the control and treatment groups separate for an extended period of time to achieve clean identification. However, this can be highly challenging for infrastructure interventions. Thus, observational studies, such as the present study, will continue to play an important role in advancing understanding on large infrastructure programs.

This study is organized as follows. Section 2 discusses the contributions to the existing literature. Section 3 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 the key summary statistics. Section 6 discusses the econometric specifications. Section 7 presents the estimation results. Section 8 offers some discussion.

2. Contribution to the literature

This study contributes to the current literature on the role of electrification in development. Scholars have found evidence that electrification is associated with positive socioeconomic impacts including improved income and educational outcomes in Bangladesh (Khandker et al., 2012) and Vietnam (Khandker et al., 2013), development of manufacturing sector (Rud, 2012), higher consumption and male labor supply in India (van de Walle et al., 2017), improved female employment in South Africa (Dinkelman, 2011) and Nicaragua (Grogan and Sadanand, 2013), and improvement in children's nutritional status in Bangladesh (Fujii et al., 2017). 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).

This study also adds to the demographic literature on electrification. There have been a few studies in demography on the existence of a causal relationship between rural electrification and fertility (Herrin, 1979; Harbison and Robinson, 1985). However, rigorous econometric studies on the impact of electrification on fertility were not available until recently.

Some recent studies have explored fertility and electrification using aggregate data in developing countries. For example, Grimm et al. (2015) use 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. This study also finds that exposure to TV is an important channel through which electrification reduces fertility. Similarly, using aggregate data, Potter et al. (2002) and Grogan (2016) show that electrification has a negative impact on fertility in Brazil and Columbia, respectively, but these results may suffer from aggregation bias. To address this issue, it would be desirable to use household-level data. Nevertheless, microeconometric studies on electrification and fertility based on household-level data are limited to date. Hence, we contribute to this small body of studies.

One of the first studies in this literature is Peters and Vance (2011), who use a household-level dataset from Côte d'Ivoire. They find a negative association between fertility and access to electricity among rural households but find a positive association for urban households. The authors did not attempt to address the potential endogeneity of the availability of electricity. As a result, it is unclear if their results are driven by selection or causation. As elaborated subsequently, the current study uses the DID and IV approaches, among others, to address the potential endogeneity of household's access to electricity.

This study also relates to Burlando (2014) and Fetzer et al. (2018). Using household-level data, they investigate the impact of power outages on fertility in Zanzibar and urban Columbia, respectively. Both studies find that power outages lead to an increase in fertility. Further, Burlando (2014) also suggests that power outages lead to an increase in domestic leisure time for both men and women. These results are broadly consistent with our findings, even though these studies examine from the perspective of temporary loss of electricity rather than permanent electrification and were conducted in different continents.

[1] http://www.iea.org/energyaccess/database/on November 6, 2019.

[2] http://www.iea.org/access2017/accessed on September 12, 2019.

Unlike the studies mentioned above, we use both contemporaneous and retrospective panel datasets. The former uses current observations at the time of survey and the latter contains past observations derived from current observations under certain assumptions. As such assumptions cannot be tested, it is unclear if contemporaneous and retrospective panel data would lead to similar results. We can overcome this issue because we compare the estimation results based on contemporaneous and retrospective panel datasets. This is a distinct advantage over studies that solely rely on retrospective panel datasets such as Fetzer et al. (2018).

This study also relates to two broad 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 (Greenwood et al., 2005a,b; Bailey and Collins, 2011; Greenwood et al., 2011; Lewis, 2018). While the context of our study is very different, we suggest that the negative impact of electrification is partly due to the increased use of TV.

Second, this study also adds to the growing body of literature on the development impact of a specific type of infrastructure, such as dams (Duflo and Pande, 2007), transportation (Fernald, 1999; Banerjee et al., 2012), and telecommunications (Röller and Waverman, 2001). We contribute to this literature by examining the impact of access to electrification on fertility—an outcome that has been largely ignored—and highlight the importance of understanding the social impact of infrastructure development from a broader perspective.

3. Rural electrification in Bangladesh

Bangladesh generates electricity through five public sector organizations and several independent power producers, transmitted through the national grid, and distributes to end users through different organizations, depending on the region and purpose of power usage. The Rural Electrification Board (REB) has been responsible for distributing electricity to rural consumers since its establishment as a semi-autonomous government organization in 1977. Even though the REB was established four decades ago, the initial progress in rural electrification was very slow, with only 21 percent of the rural population having access to electricity 2000 (NIPORT et al., 2001). However, the last two decades have witnessed a significant increase in access to electricity, and the proportion of people with access to grid electricity in the rural areas rose to 51 percent in 2014 (NIPORT et al., 2016).

The rural electrification program under the REB is often viewed as one of the most successful government programs in Bangladesh (Khandker et al., 2009) with substantially lower system losses than other major electricity distribution entities (Alam et al., 2004) and an efficient bill collection record. A critical element of this success is electricity distribution through rural electricity cooperatives called *Palli Bidyut Samities* (PBS), where members are electricity consumers and participate in policy-making through elected representatives serving on the PBS' governing body. The PBS own, operate, and manage the rural distribution system within their jurisdictions. The REB approves new PBS and coordinates by providing technical support and training, negotiating power purchase agreements with providers, and approving tariffs. As of September 2019, the Bangladesh REB had supplied electricity to nearly 25 million domestic connections through 80 PBS and each PBS serves around six subdistricts (upazilas/thanas) and one thousand villages on average.[3]

The establishment of new PBS depends on the 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 area prone to flooding. Accessibility to the Bangladesh Power Development Board's 33 kV line and adequate capacity at the grid substation are also considered necessary for the decision to establish a new PBS (Murphy et al., 2002). The PBS designs a distribution network known as "master plan" for its area of jurisdiction. The annual expansion of the grid is based, among others, on priority areas, annual sanctions made to the master plan, and the assessment of revenue generation potential, where the minimum qualifying revenue potential of 45,000 BDT (about 550 USD) per year per kilometer of line construction is required to cover the operating cost of new connections (Waddle, 2007). Once a village is identified to be electrified, the PBS nominates locations for poles and wiring.

Therefore, the process of rural electrification is clearly not random, even though the factors influencing electrifications do not appear to have obvious causal impact on fertility.[4] Thus, we adopt various approaches to examine the robustness of our results with respect to the potential endogeneity of electrification, as detailed in sections 6 and 7.

4. Model of electrification and fertility

This section develops a simple model of electrification and fertility to underlie our econometric specifications in the subsequent analysis. While our model is built on a few strong assumptions, it helps us understand the possible consequences of electrification that accompany the changes in fertility. Our model aims to derive a joint prediction on the changes in number of children, consumption, and time use in response to electrification. To this end, 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. Child goods include the consumption goods associated with childbearing and rearing such as food, education, and health care for children, and nonchild goods include everything else. We treat the consumption of child goods and number of children synonymously in this model, assuming away the discreteness of number of children and the quality of children. The preference parameter $\omega (\in (0,1))$ represents the weight attached to nonchild subutility. We use the prime notation to denote the derivative of a function of a single variable (e.g., $g' \equiv \frac{dg}{dn}$). We use subscripts to denote the partial derivatives of a function of multiple variables (e.g., $f_c \equiv \frac{\partial f}{\partial c}$).

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 indicates better electricity service with $e = 0$ and $e = 1$ representing no and full electricity access, respectively.

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

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

[3] http://www.reb.gov.bd/accessed on November 6, 2019. 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 the term "REB" in this study.

[4] It is also worth noting that the correlations of the household's access to electricity with the household's land, income, and expenditure are very low (Khandker et al., 2012).

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) requires that a fixed proportion of the effective lighted time be spent on each child in the household, given its electrification status. In our model, nonlighted time is assumed to be used only for sleeping or reproductive activities and have no alternative use.

Households also face a budget constraint. Let $I(e)$ be the maximum potential household income, which the household can earn if its entire effective lighted time is spent on work. It is important to let I depend on e in our model because electrification can support important income-generating activities. For example, agricultural households can use electric water pumps for irrigation (World Bank, 2008) and expand their hours of operation for activities such as sewing, making handicrafts, and selling goods and services under electric lights.

Assuming that the actual household income earned from work is proportionate to l, we can write the household budget constraint as follows:

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

where $p(e)$ is the "price of having one child," which includes all the direct costs of childbearing and rearing. As this is a static model, we ignore the possibility of children contributing to household income once they grow up.[5] We also implicitly assume that the leisure decision is separable to keep the model simple.[6]

Households maximize the utility function in eq. (1) subject to time constraint eq. (2) and budget constraint eq. (3) over c, n, and l taking their electrification status e as given. We denote the maximizing arguments with an asterisk and explicitly write them as a function of 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:

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

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

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

It is reasonable to expect that eq. (4) holds. As households with better access to electricity have more options to handle child-related matters, the actual amount of effective lighted time that must be spent on each child would not increase with electrification. Therefore, even if access to electricity does not help households spend less time on child-related activities, the *fraction* of the effective lighted time 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 effective lighted time enables households to (potentially) spend more time on gainful activities (Khandker et al., 2013), and the opportunity to use electric appliances would not increase the price of having one child. As shown in section 7, we have some empirical evidence to support eqs. (4) and (5). We are unable to test eq. (6) due to data limitation.

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:*

$$\begin{aligned} V \equiv\ & [f_c - (p + I\rho)n_* f_{cc}]p' + [If_c - (p + I\rho)In_* f_{cc}]\rho' \\ & + [\rho f_c + (p + I\rho)l_* f_{cc}]I' + [p + I\rho]f_{ce}, \end{aligned} \tag{7}$$

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

$$l'_{*(e)} = -(\rho' n_* + \rho n'_*) > 0 \tag{8}$$

$$c'_{*(e)} = l_* I' - (p + \rho I)n'_* - (p' + \rho' I)n_* > 0. \tag{9}$$

As seen from the definition in eq. (7), $V(e)$ can be divided into four terms, each involving p', ρ', I', and f_{ce}. The first and second terms are driven by the price effects induced by electrification through changes in 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 effects due to changes in the potential household income. This effect is ambiguous because $\rho f_c > 0$ and $(p + I\rho)l_* f_{cc} < 0$. The fourth term involving f_{ce} represents the complementarity effects between electricity and nonchild goods. This term is positive when $f_{ce} > 0$.

It should be noted that n is not included in the argement of ρ and p in eqs. (2) and (3), respectively. Therefore, Proposition 1 is based on the assumption that there are no economies of scale in the time and monetary requirements for child rearing. While these are strong assumptions, the main results remain the same even in the presence of a modest degree of economies of scale as shown in Appendix D.

Proposition 1 shows that the optimal number of children tends to decrease with household electrification, provided that some of the following conditions are satisfied: (i) complementarity between electricity and nonchild goods is strong (i.e., f_{ce} is positive and large), (ii) direct and opportunity costs of children do not decline much with electrification (i.e., p' and ρ' are small in absolute values), and (iii) marginal utility from nonchild goods is relatively large and declines only slowly (i.e., f_c is large and f_{cc} is small in absolute value).

The casual observations of prevailing consumption patterns in Bangladesh and elsewhere suggest that condition (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 TVs, the marginal subutility of nonchild goods for electrified households is likely to be no less than that for nonelectrified households for a given level of nonchild goods consumption. Similarly, condition (ii) is also likely to hold, because there is little evidence that the availability of electric appliances drastically reduced the burden of childbearing 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 on fertility is positive in the urban areas but negative in the rural areas of Côte d'Ivoire. The discussion above also leads us to expect that the impact of electrification on fertility in rural Bangladesh is negative, which is indeed the case as discussed subsequently. Further, our empirical finding is at odds with Greenwood et al. (2005a) and this may be because the United States (US) in the early 1940s was far richer than Bangladesh during our study period and thus condition (iii) did not hold in their study.[7]

While the theoretical prediction on the sign of n'_* is ambiguous as Proposition 1 shows, it provides an unambiguous prediction on the rela-

[5] Alternatively, one can interpret $p(e)$ as the net cost of children in present value, which takes into the account the contribution of children to the household income.

[6] We can also alternatively assume that l includes leisure time and c includes the value of leisure time, in which case the complementarity between leisure and access to electricity is also implicitly allowed. The empirical results are qualitatively similar under either interpretation.

[7] According to the Madison Project Database 2013 version (http://www.ggdc.net/maddison/maddison-project/home.htm), GDP per capita in the 1990 Geary-Khamis dollars in the US in 1940 was $7,010, whereas it was $1276 in Bangladesh in 2010.

tionship 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 explained later, our empirical results are consistent with the model predictions in Proposition 1.

Because our model is static, we 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 their number of children is largely governed by their biological limit in the short run. Our empirical findings indeed indicate that the cumulative impact of electrification on fertility is larger when we consider a longer time horizon (see the subsequent discussion on Table 4).

We reiterate here that the model presented above takes the household's access to electricity as given. This is not likely to be a major issue when we consider a specific household. However, when we attempt to identify the impact of electrification from the data, taking the household's access to electricity as given is potentially problematic because access to electricity may be endogenous. Therefore, we use the DID approach to control for all the household characteristics that are time-invariant. To further address the potential concerns for endogeneity, we use a variety of other approaches, as detailed in Section 6.

5. Data and summary statistics

5.1. Main contemporaneous panel data

The main data source for our study comprises two rounds of household survey data collected under the *Socioeconomic Monitoring and Impact Evaluation of Rural Electrification and Renewable Energy Programme in Bangladesh*. The first round (round 1) of the survey was conducted in 2005 by a consortium comprising Bangladesh Engineering and Technological Services Ltd. (BETS) and Bangladesh Unnayan Parishad (BUP). The second round (round 2) of the survey, which followed up with a subsample of households, was conducted in 2010 by e.Gen Consultants Ltd. Therefore, our dataset is partially panel and covers 45 out of the 70 PBS from all six divisions of Bangladesh operating at the time of data collection. Both the survey rounds collected various individual and household characteristics such as age, gender, relationship to the household head, and educational attainment of each household member, consumption expenditure, and electrification status.

In round 1 of the survey, a stratified random sample was drawn based on 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 noted by Khandker et al. (2012), it is important to apply the sample weights included in 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, all the tables report the weighted results, but the unweighted results are generally similar to weighted results (see also Appendix B). 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 round 1 data and e.Gen Consultants Ltd (2006) for round 2 data.

We primarily use the panel households that are observed in both rounds 1 and 2 in our analysis. To minimize the complications arising from the differences in household structure, we only use the data for those households whose head is a male and married to exactly one woman. We discarded about one percent of polygamous households from each round. Further, because we are interested in the fertility behavior between the two rounds, we restricted our sample to those households in which the age of the spouse of the male household head (wife) is between 15 and 49 years in both rounds.

As our survey data did not contain a unique individual-level identification code, we constructed an individual-level panel dataset by matching the names of the husband and wife between the two survey rounds for each household. The matching was imperfect because the English spelling of names vary between the two survey rounds. Thus, we excluded from the sample those households for which the names of the husband and wife could not be matched with high confidence between the two survey rounds. As a result, about 40 percent of the households in round 2, which are all supposed to be panel households, were dropped. In addition, households that migrated were not tracked in round 2, even though Bangladesh has low migration rates. Therefore, our main contemporaneous panel data potentially suffer from sample selectivity due to imperfect matching of names and migration.

Another data limitation is the lack of complete history of pregnancy. Hence, we derive an observable measure of fertility from the number of surviving children born to the wife, which we denote by N_{ht} for household h in round $t (\in \{1, 2\})$. Thus, N_{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.[8] As a result, the observed change in the number of surviving children, $\Delta N_{h1} (\equiv N_{h2} - N_{h1})$, 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 households with a high fertility that tend to produce more children in the event of child deaths will be retained if we keep the households for which ΔN is nonnegative, leading to a sample selection bias in our estimation. However, about one percent of households for which $|\Delta N_{h1}| > 4$ are treated as outliers and removed from our sample.[9] To keep the presentation simple, we hereafter ignore child deaths and drop the qualifier "surviving" in the remaining discussion. This is reasonable because the probability of death between the two survey rounds is still relatively small, even though child mortality in Bangladesh is far from negligible.[10]

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

Table 1 provides summary statistics of key household variables based on the household's access to electricity from the national grid (E_{ht}) for panel households, where $E_{ht} = 1$ [$E_{ht} = 0$] indicates that household h has [does not have] access to electricity from the national

[8] The lack of complete history of pregnancy is common in household surveys with the notable exception of Demographic and Health Surveys (DHS) conducted around the world. Even if there is a bias in the reported number of children, our results are not subjected to such a bias so long as the bias is time invariant.

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

[10] Child mortality rate under five per 1000 live births in Bangladesh was 68 in 2005 and 47 in 2010 according to the World Development Indicators.

grid in round $t \in \{1, 2\}$. As shown in Table 1, the head (husband) and spouse (wife) 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. The unweighted summary statistics are generally similar.

Four precautions are in order before interpreting Table 1. First, our focus is on *grid* electricity because the amount and reliability of electricity from the grid far exceeded those of electricity from the typical Solar Home System that was available in Bangladesh during our study period. Further, despite the oversampling, only around five percent of the sample households used electricity from solar in both the survey rounds.[11]

Second, the variables for educational attainment are defined as ordered variables. For example, if a given household head has at least some upper secondary education, he automatically has some primary and lower secondary education. Therefore, the proportion of spouses with some primary education but without any secondary education in round 1 is $27.1 (= 60.3 - 33.2)$ percent.

Third, the child sex ratio is likely to influence the households' subsequent fertility decisions because it is not uncommon among Bangladeshis to prefer boys to girls.[12] However, we observe only the number of surviving children born to the spouse of the male household head (i.e., N_{ht}) but not separate numbers of boys and girls. Therefore, we use the ratio of boys out of all the children aged less than 15 years in the household, which may include children whose mother is not the spouse of the male household head. For households with no children under 15 years, we assign a value of half in the regression analysis, but the average reported in Table 1 excludes such households.

Finally, our contemporaneous sample potentially suffers from sample selectivity as noted earlier. Therefore, we compare the summary statistics of the original sample, which includes nonpanel households, with those of the contemporaneous panel sample (reported in Table 10.E in Appendix E). We find that the summary statistics from both samples are generally similar. However, the null hypothesis for the test of equality of means between the panel and nonpanel households is rejected jointly and individually for most covariates in each of the two rounds. Therefore, the conclusions drawn only from the contemporaneous panel must be interpreted with caution. This, in turn, means that the use of the retrospective panel dataset, described in detail in the next subsection, is critical for verifying that our conclusions are not driven by the sample selectivity of the panel households, because the retrospective panel dataset does not have the same issue.

Table 1 shows that the nonelectrified and electrified households are generally different in observable characteristics. For most characteristics, there is a significant difference between them in each round by the Wald test for equality of means as shown in columns (3) and (6). Specifically, there are three notable differences between the nonelectrified and electrified households. First, in electrified households, educational attainment and age of the household head and spouse are both higher than those in nonelectrified households. However, these differences can be mostly addressed by combining household- and time-specific fixed effects.

Second, consumption expenditure per capita for electrified households is on average higher than that for nonelectrified households and the rate of increase in average expenditure per capita between the two rounds is also higher for electrified households. Therefore, we also include the logarithmic household expenditure per capita, together with the ratio of boys among children under 15 years as an additional time-varying control in most regression specifications.

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

Survey round	Round 1							Round 2						
Grid electricity access (E_{ht})	(1) $E_{h1} = 0$		(2) $E_{h1} = 1$		(3) Total		Wald	(4) $E_{h2} = 0$		(5) $E_{h2} = 1$		(6) Total		Wald
Description	Mean	(s.d.)	Mean	(s.d.)	Mean	(s.d.)	*P*-val[c]	Mean	(s.d.)	Mean	(s.d.)	Mean	(s.d.)	*P*-val[c]
Age of household head	39.97	(8.11)	41.88	(7.68)	40.52	(8.03)	0.000	44.08	(8.60)	46.35	(8.39)	45.09	(8.58)	0.000
Age of spouse	32.11	(7.04)	33.83	(6.80)	32.60	(7.01)	0.000	36.16	(6.84)	37.53	(6.77)	36.77	(6.84)	0.000
Ratio of boys among children under 15[a]	0.501	(0.364)	0.534	(0.366)	0.510	(0.365)	0.042	0.512	(0.346)	0.508	(0.358)	0.510	(0.351)	0.803
Number of surviving children (N)	2.636	(1.552)	2.757	(1.519)	2.670	(1.543)	0.141	2.933	(1.407)	2.937	(1.451)	2.935	(1.426)	0.962
Head has at least some primary educ.	0.578	(0.494)	0.781	(0.413)	0.636	(0.481)	0.000	0.669	(0.471)	0.752	(0.432)	0.706	(0.456)	0.009
Head has at least some junior sec. educ.	0.372	(0.484)	0.522	(0.500)	0.415	(0.493)	0.000	0.368	(0.482)	0.455	(0.498)	0.406	(0.491)	0.013
Head has at least some senior sec. educ.	0.174	(0.379)	0.292	(0.455)	0.207	(0.406)	0.000	0.196	(0.397)	0.243	(0.429)	0.217	(0.412)	0.113
Spouse has at least some primary educ.	0.558	(0.497)	0.715	(0.452)	0.603	(0.489)	0.000	0.631	(0.483)	0.724	(0.447)	0.672	(0.469)	0.003
Spouse has at least some junior sec. educ.	0.310	(0.463)	0.387	(0.487)	0.332	(0.471)	0.014	0.289	(0.453)	0.343	(0.475)	0.313	(0.464)	0.073
Spouse has at least some senior sec. educ.	0.089	(0.285)	0.129	(0.336)	0.101	(0.301)	0.059	0.090	(0.286)	0.106	(0.307)	0.097	(0.296)	0.440
log (HH expenditure per capita)[b]	3.294	(0.397)	3.427	(0.388)	3.332	(0.399)	0.000	3.972	(0.537)	4.079	(0.600)	4.019	(0.568)	0.019
HH has a TV	0.001	(0.030)	0.631	(0.483)	0.181	(0.385)	0.000	0.169	(0.375)	0.675	(0.468)	0.394	(0.489)	0.000
HH has a mobile phone	0.000	(0.000)	0.070	(0.255)	0.020	(0.140)	0.000	0.542	(0.498)	0.815	(0.389)	0.663	(0.473)	0.000
Number of observations	1,505		1,037		2,542			1,127		1,415		2,542		

Note: In both rounds 1 and 2, 171 subdistricts are covered in the data. The sample weights are applied.

[a] The average was taken over 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 reported in the last row.

[b] 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).

[c] *P*-value for the Wald test of the null hypothesis that the mean of the covariate is equal between nonelectrified and electrified households with errors clustered at the subdistrict level.

[11] In 2011, four percent of the households reported using solar power as a source of electricity in rural Bangladesh (Bangladesh Bureau of Statistics, 2012).

[12] Nevertheless, gender-selective abortion is not prevalent in Bangladesh as the sex ratios at birth did not change between 1993 and 2011 (Talukder et al., 2014).

Table 2
Average change in the number of children between the two survey rounds by initial number of children and access to grid electricity.

Panel A: Contemporaneous panel dataset											
	$E_{h1} = 0$				$E_{h1} = 1$				Difference		
N_{h0}	μ_0		(s.d.)	Obs	μ_1		(s.d.)	Obs	$\mu_1 - \mu_0$		(s.d.)
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)	1505	0.136	***	(0.036)	1037	−0.180	***	(0.050)
Panel B: Retrospective panel dataset											
	$E_{h0} = 0$				$E_{h0} = 1$				Difference		
N_{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)	2460	−0.136	***	(0.017)

Note: N_{ht} and E_{ht} are the number of children and access to electricity in round t, respectively. Sample weights are applied in both panels. The means μ_0 and μ_1 reported in Panel A [Panel B] are respectively the means of the change in number of children (ΔN) between rounds 1 and 2 (ΔN_{h1}) [rounds 0 and 1 (ΔN_{n0})] for nonelectrified and electrified households in round 1 [round 0] in the contemporaneous [retrospective] panel dataset. Round 0 is the year 2000 for Panel B. The statistical significance of a two-sided t-test for the population mean μ with $H_0 : \mu = 0$ and $H_a : \mu \neq 0$ at 1, 5, and 10 percent levels are denoted by ***, **, and *, respectively.

Third, there was a substantial increase in the penetration of television and mobile phones between the two survey rounds. Given that these devices allow people to access external information (i.e., from outside their villages), they arguably deserve special attention. Therefore, we will explore the possible causal effect of electricity on fertility through the use of TV and mobile phones.[13]

Panel A of Table 2 presents the mean (μ) of the change in the number of children (ΔN) 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 ($E_{h1} = 0$) 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 statistically significant. Hence, Panel A shows that nonelectrified households tend to increase their number of children when they already have three or fewer children (i.e., $N_{h1} \leq 3$). For electrified households, the number of children will tend to increase when $N_{h1} \leq 2$ but it remains unchanged when $N_{h1} = 3$. Therefore, Panel A indicates that the optimal number of children, at which households stop having additional children, is between three and four for nonelectrified households and around three for electrified households. It should be also noted that the negative averages for households with four or more children in round 1 are broadly consistent with the figure suggested by the under-five child mortality rate in Footnote 10.[14]

The rightmost column in Table 2 measures the difference in ΔN between electrified and nonelectrified households. On average, ΔN for electrified households in round 1 is lower than that for nonelectrified households by 0.180 in Panel A. This figure can be regarded as a naïve DID estimate of the impact of electrification on fertility, because it reflects the difference between electrified and nonelectrified households in the difference in number of children between two rounds of surveys.

5.2. Retrospective panel data

In addition to the contemporaneous panel dataset described above, we also construct a retrospective panel dataset using a method similar to Jensen and Oster (2009). This dataset has the advantage in that it does not suffer from selective attrition that the contemporaneous panel dataset potentially suffers from. 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 reproductive history of the spouse from the observed data.

Because our data do not have a separate identifier for the mother (and father) of each child, we need to rely on the relationship to the household head to identify the mother of each child. Therefore, we selected households suitable for the construction of retrospective panel data in the following manner. First, we select only nuclear households in round 1 and count the number of individuals who are either the son or daughter of the household head for each household. We include the household in the retrospective panel data if this number coincides with the number of surviving children reported by the spouse. For most households in this sample, we expect that all the children the spouse has given birth to are still alive and reside with her. However, we cannot exclude some other possibilities, even though they are likely to be exceptions. For example, there may be some households in which the number of stepchildren (i.e., household head's children from the previous marriage) is equal to the number of the spouse's own children not living with their mother (spouse) at the time of survey.

To construct the retrospective panel dataset, we ignore this possibility and the prospect of some households losing access to electricity. Based on the assumption that the spouse gave birth to all the children in the household during round 1 survey, the number of children and the status of electrification in the past are constructed 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 (aged 1 and 3) and access to electricity.

[13] It should be noted that the 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 live in the urban areas. Therefore, during our study period, the Internet is unlikely to be an important source of information for most rural residents in Bangladesh.

[14] For example, using the figure for 2005, when there are four children who are just born, 0.272(= 4 × 68/1000) children are expected to die within five years.

Three years ago, it had only one child (aged 1) and no access to electricity. We will refer to the data constructed retrospectively in this way as *round 0* data. Among the 16,523 households in the round 1 sample, we were able to construct round 0 data for 9922 households using the procedure described above.

It should be noted that round 0 can be set at any year before 2005. However, we choose to set round 0 only up to five years before the first round for three reasons. First, as Jensen and Oster (2009) indicate, the recollection of long-past events is likely to be unreliable. Second, the location of residence may change but our retrospective data cannot adequately account for migration. Third, children who were sufficiently old in 2005 are less likely to be residing with their parents than younger children. However, we are only including nuclear-family households where all the children are still living with their parents (i.e., their father and his spouse). Therefore, the retrospective panel dataset does not represent households with older children. This, in turn, means that it is inappropriate to set round 0 in a distant past.

In Panel B of Table 2, we report the average change in the number of children between rounds 0 and 1 (i.e., $\Delta N_{h0} \equiv N_{h1} - N_{h0}$), 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 necessarily expect Panels A and B of Table 2 to be similar at least for three reasons. First, there has been a secular decline in fertility. Thus, the demographic composition of households in the initial time period is different between the contemporaneous and retrospective panel datasets. Second, because of the way we constructed the retrospective panel, none of the households in the retrospective panel dataset experienced a net decrease in number of children. Finally, the sample selection issues in the contemporaneous panel dataset discussed earlier do not apply to the retrospective panel dataset.

Despite these differences, a few points are worth highlighting. First, the subsequent increase in fertility is more when the initial number of children is less in both Panels A and B of Table 2. 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 number of children in the five-year period for the retrospective panel data is on average larger than that for the contemporaneous panel data, which is consistent with the secular decline in fertility. Fourth, consistent with the discussion in section 4, the change in number of children between the two rounds (ΔN) is not significantly different between electrified and nonelectrified households when the initial number of children is sufficiently small (zero or one in Panel A and zero in Panel B). Finally, the naïve DID estimates are quantitatively similar between Panels A and B.

6. Identification strategy

The discussion in section 4 and the naïve DID estimates in Table 2 suggest that access to electricity may have a negative impact on fertility. However, the naïve DID estimates suffer from a variety of potential issues. For example, they do not consider the heterogeneity between electrified and nonelectrified households. In this section, we discuss the strategies to identify the causal effect of rural electrification in more credible ways by addressing the potential issues.

6.1. Difference-in-differences specification

Our primary identification strategy is based on the DID estimation, which allows us to control for all the time-invariant unobservable and observable household characteristics and time-variant observable household characteristics. The dependent variable in our model is number of children N_{ht}, whereas the key covariate is the indicator variable E_{ht} for the household's access to grid electricity. We denote the indicator variable for round $j \in \{0, 1, 2\}$ by $I_{tj} \equiv \mathbf{1}(t = j)$, which takes one if $t = j$ and zero otherwise. The simplest version of our estimation equation is:

$$N_{ht} = \beta_0 + \beta E_{h1} \times I_{t2} + \delta_2 I_{t2} + \eta_h + u_{ht}, \tag{10}$$

where β_0 is the intercept, β is the main coefficient of interest, and δ_j, η_h, and u_{ht} are the round-specific, household-specific, and idiosyncratic error terms, respectively. In addition to the basic specification in eq. (10), we may also include a vector of the time-varying covariates X_{ht} as follows:

$$N_{ht} = \beta_0 + \beta E_{h1} \times I_{t2} + \gamma X_{ht} + \delta_2 I_{t2} + \eta_h + u_{ht}, \tag{11}$$

where X_{ht} includes the ratio of boys among children under the age of 15 and the logarithmic household expenditure per capita. Note that we do not include covariates in the analysis of the retrospective panel dataset because we cannot construct the covariates retrospectively.

We estimate eqs. (10) and (11) by ordinary least-squares (OLS) regressions with household-specific and round-specific fixed effects, allowing for the clustering of error terms at the subdistrict level. When we use the retrospective panel data and analyze the data for rounds 0 and 1, we use $t = 0$ as the base time period (instead of $t = 1$) in eqs. (10) and (11) and replace E_{h1}, δ_2, and I_{t2} with E_{h0}, δ_1, and I_{t1}, respectively.

6.2. Instrumental variables estimation

The potential endogeneity of $E_{ht} \times I_{t2}$ is an important concern about the DID specification discussed above. That is, there may be some unobservable factors that affect the number of children N_{ht}, and the distribution of such factors may be different between households with and without access to electricity in round 1. The DID estimation described above is immune to the presence of such factors, provided that their impact on N_{ht} is constant over time. However, if the effect of some unobservable factors on N_{ht} varies over time, the OLS estimate of β will be biased and reflect not only the causal effect of electrification but also the systematic difference in the distribution of such unobservable factors between electrified and nonelectrified households.

The bias in our context could go both ways. For example, it is possible that households with "modern preferences" may choose to obtain access to electricity and stop having additional children early. The presence of such a factor would lead to a downward bias in β. On the contrary, households that like to show off their wealth may choose to adopt electricity early and increase the number of children over time, in which case β would be biased upward. Hence, it is difficult to determine the direction of bias in OLS estimates.

To address the potential endogeneity issue discussed above, we use IVs for the household-level electrification status E_{h1} and run two-stage least-squares (2SLS) regressions. Specifically, we use the indicator variable of the village-level electrification status—which takes one if the village where household is located is electrified and zero otherwise—and the grid system loss—which measures the percentage of electricity lost during transmission and distribution—as instruments. As we argue below, both variables are relevant instruments and plausibly satisfy the exclusion restriction.

Village-level electrification status is a relevant instrument because households are not able to connect to the national grid if their villages are not electrified. As fertility is primarily a private household decision, village-level electrification status would plausibly serve as an instrument that helps us mitigate the endogeneity concern. However, it may be still debatable if village-level electrification status strictly satisfies the exclusion restriction because villages that were electrified in round 1 may have a favorable development condition versus villages not electrified in round 1, which in turn may affect household fertility behavior. This is a relevant concern because the process of rural electrification is not random as noted in section 3.

To address this issue, we also include the grid system loss, which was compiled from the Annual Reports of the Rural Electrification Board (2006, 2011), in the set of IVs. The grid system loss is likely to be uncorrelated with the household preferences discussed earlier. At the same time, the grid system loss is also a relevant instrument. To understand this point, note that excessive system loss may result from technical causes such as suboptimal voltage regulation and circuit configurations and nontechnical causes such as nonfunctional meters. As Khandker et al. (2009) point out, the usual distribution problems, such as theft and illegal connections, that beleaguer other Bangladeshi distributors are almost nonexistent in the REB's operations. Thus, it is reasonable to regard the grid system loss as a measure of the management efficiency demonstrated by the PBS. Because households are less likely to adopt electricity when their PBS is poorly managed, the grid system loss is also a relevant instrument.

It should be underscored here 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 grid system loss and quality of local health facilities, and the latter in turn affects infant mortality and fertility decisions. Therefore, the OLS estimate of β would be potentially biased in pure cross-sectional regressions. However, the DID estimation is still valid if the level of local corruption remains constant during our study period. Even if this is not the case, the impact on fertility through local health facilities is likely to be limited because only a very small share of birth deliveries take place in the local health facilities.[15]

Another potential issue with the grid system loss is related to the formation of the PBS management. Each PBS has a board comprising no more than 15 directors, who are elected for a three-years term through annual elections held on a rotating basis. Further, the Office Bearers such as the president and vice-president are elected annually by ballot within the board.[16] Therefore, if the election results reflect a PBS-specific shock that simultaneously affects both the grid system loss and fertility decisions during the two survey rounds, our IV estimate would be influenced by this shock. However, unless the shock is persistent and omnipresent within each PBS, its effects would be limited because election is on a rotating basis. Further, given that fertility decision is an intrinsically households-level decision, the impact of PBS-specific shock on fertility decisions is likely to be small.

It should be underscored that the grid system loss is relevant only in electrified villages. Therefore, it is important to include both the village-level electrification status and the grid system loss in the set of IVs. While there are potential threats to our identification through these IVs, as discussed above, we can perform an overidentification restriction test because we have two IVs for one endogenous variable. This test allows us to see if the exclusion restriction is violated for either instrument.[17] As shown in section 7, we have no evidence that the exclusion restriction is violated in our analysis of electrification and fertility.

6.3. *Propensity score matching*

As a robustness check, we also employ the propensity score matching (PSM) method. A potential advantage of the PSM method is that the distribution of covariates can be made more balanced between the control group (i.e., households without access to grid electricity in round 1) and the treatment group (i.e., households with access to grid electricity in round 1). The covariate balance would be irrelevant if the regression models discussed above are correctly specified. However, there is a possibility that our DID regression results may be confounded with the combination of unbalanced covariates and covariate-dependent time trends. Therefore, we also run a DID regression with a matched sample to address both problems simultaneously.

6.4. *Impact heterogeneity*

Table 2 and the discussion presented in Section 4 suggest that the change in the number of children may also depend on the initial number of children. This suggests that the impact of electrification may depend on the initial number of children. Therefore, we also consider a specification in which $E_{ht} \times I_{t2}$ is further interacted with an indicator $V_h^\nu (\equiv \mathbf{1}(N_{h1} \geq \nu))$ that the number of children in round 1 (N_{h1}) is at least a given threshold value ν using the following estimation equation:

$$N_{ht} = \beta_0 + \beta E_{h1} \times I_{t2} + \beta^+ E_{h1} \times I_{t2} \times V_h^\nu + \gamma X_{ht} + \delta_2 I_{t2} + \theta I_{t2} \times V_h^\nu + \eta_h + u_{ht}, \quad (12)$$

where β^+ captures the impact heterogeneity. Notice that the term V_h^ν only enters as interaction terms in eq. (12) because this term is time-invariant and its level effect is absorbed by the household-specific fixed-effects terms.

7. Results

7.1. *Baseline difference-in-difference regressions*

We now apply the econometric specifications discussed in section 6 to the data. Let us start with the baseline results with the contemporaneous panel data. In column (1) of Table 3, we report the OLS regression of the DID specification in eq. (10), which regresses the number of children N_{ht} in household h at round t on the household- and round-specific fixed-effects terms. The sample weights are applied to all the regression results reported in this section.

The first row ("$E_{h1} \times I_{t2}$") of Table 3 reports the estimate of β, where E_{h1} is an indicator variable for households electrified in round 1 and I_{t2} is an indicator variable for round 2. The point estimate indicates that the change in number of children between 2005 and 2010 for households with access to electricity in round 1 is smaller than the corresponding change for households without access by 0.180 children. This point estimate is both economically and statistically significant. As shown in column (2), the point estimate remains statistically significant and quantitatively similar even when we include logarithmic household expenditure per capita and ratio of boys among children under 15 in the regression.

7.2. *Instrumental variables regressions*

As discussed in the previous section, the OLS results presented above potentially suffer from the endogeneity issue. Therefore, we use the instrumental variables estimation as described in section 6. Specifically, we run a 2SLS regression with household- and time-specific fixed effects, in which $E_{h1} \times I_{t2}$ is the endogenous variable instrumented by the interaction terms of grid system loss in round 1 and village electrification status in round 1 with I_{t2}. Note that E_{h1} and its instruments (grid system loss and village electrification in round 1) are absorbed by the household-level fixed effects.

We report the second- and first-stage regression results in columns (3) and (4) of Table 3, respectively. Column (4) shows that the IVs have the expected signs. The interaction term between grid system loss in round 1 and I_{t2} has a negative coefficient and the interaction term between the indicator for electrified villages and I_{t2} has a positive coefficient, even though only the latter is statistically significant. The large first stage F-statistic (359.6) suggests that the instruments are indeed relevant. Further, the null hypothesis of the overidentification restriction test cannot be rejected even at a 10 percent level. Therefore, there

[15] In 2007, only 10.5 percent of children were born in a health facility (NIPORT et al., 2009). Further, consistent with the observations made here, Fujii et al. (2017) find that the local health facility is not an important impact channel through which rural electrification affects the nutritional status of children.

[16] See http://www.reb.gov.bd/site/page/abae212c-16fd-4810-b789-9f91c5e63e93/PBS-BOARD (accessed on December 22, 2017) for further details about the PBS Board.

[17] A similar strategy was used in van de Walle et al. (2017).

Table 3
Baseline difference-in-differences regressions.

Dep var	(1) N_{ht}	(2) N_{ht}	(3) N_{ht}	(4) $E_{h1} \times I_{t2}$
$E_{h1} \times I_{t2}$	-0.180^{**}	-0.171^{**}	-0.202^{**}	
	(0.072)	(0.070)	(0.087)	
log (HH expenditure per capita)		-0.176^{***}	-0.175^{***}	0.031^{**}
		(0.064)	(0.045)	(0.012)
Ratio of boys among children under 15		0.066	0.064	-0.055^{***}
		(0.155)	(0.108)	(0.021)
I_{t2}	0.316^{***}	0.434^{***}	0.443^{***}	0.055
	(0.052)	(0.062)	(0.051)	(0.052)
(Grid system loss in round 1) $\times I_{t2}$				-0.560
				(0.388)
(Village electrified in round 1) $\times I_{t2}$				0.600^{***}
				(0.023)
Estimation	OLS	OLS	2SLS (2nd stg)	2SLS (1st stg)
Observations	5,084	5,084	5,084	5,084
R^2	0.870	0.871	0.070	
First Stage F				359.6
P-value for OIR test				0.164

Note: All the regressions include household-level fixed-effects terms and apply sample weights. E_{h1} is an indicator variable for electrified households in round 1 and $I_{t2} \equiv \mathbf{1}(t = 2)$ 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. Overidentification restriction (OIR) test is based on the Hansen J-statistic.

is no evidence that our instruments are invalid. Hence, our 2SLS regression results appear to be credible.

As shown in column (3), the 2SLS estimate of β is -0.202, which is negative and statistically significant. This point estimate is quantitatively close to and slightly larger in absolute value than the OLS estimate. Therefore, the OLS estimates are conservative and biased slightly upwards, if any.

7.3. Propensity score matching and other robustness checks

As a robustness check, we also estimate the impact of electrification by PSM in a DID framework. A potential advantage of PSM is that it helps to make the distribution of covariates more balanced between the control and treatment groups, where the former [latter] is a set of households without [with] access to grid electricity in round 1. The covariate balance would be irrelevant if the regression models used above are correctly specified. However, the combination of unbalanced covariates and covariate-dependent time trends can potentially bias our estimates. To address this potential issue, we also run a DID regression with a matched sample.

To this end, we match each observation in the treatment group with a closest neighbor in the control group (i.e., households without access to grid electricity) as measured by the propensity score, or the probit estimate of the probability of getting the treatment.[18] For each treatment observation, we are able to obtain a unique neighbor and the covariates are well-balanced between the (counterfactual) control and treatment groups after matching.[19] Since there are 1037 treatment households in round 1, there are a total of 2074 households and 4148 observations over the two rounds when the households in the counterfactual control group are included.

The DID regression estimate of β (i.e., the coefficient on $E_{h1} \times I_{h2}$), based on the matched sample and with the same specification as column (1) of Table 3, is -0.210 with a clustered standard error of 0.088. This is very similar to the results reported in Table 3. Inclusion of the ratio of boys among children under 15 years in the household and the logarithmic expenditure per capita in the set of covariates did not change the results much. Thus, the use of PSM does not alter the conclusions derived from the baseline DID estimates discussed above.

We also provide additional robustness checks in Appendix B. We first report results without sample weights, without household fixed effects, and with the allowance for the time-varying treatment (Table B.8). We also report the OLS and IV regression results based on the change-on-level specification (Table B.9). All the estimates indicate that the impact of electrification on fertility between the two survey rounds is around -0.2, and they are statistically significant. In Appendix C, we also report the results of falsification tests to verify that our results are not driven by a factor other than rural electrification.

7.4. DID regressions with the retrospective panel dataset

The results discussed above provide an estimate of the impact of electrification on fertility between the two rounds of survey, or between years 2000 and 2005. We now use the retrospective panel data to investigate if a similar impact is observed for earlier years. In Table 4, we report the DID regression results using the retrospective panel dataset.

In columns (1) to (5) of Table 4, we vary the timing of round 0 from 2004 to 2000 using the same regression specification as column (1) of Table 3, except that the base survey round is round 0 instead of round 1. Regardless of the choice of the timing of round 0, the point estimate is always negative. Further, the estimate is also statistically significant except when round 0 is set at year 2004. Therefore, the analysis of the retrospective panel also indicates that the impact of rural electrification on fertility is negative, and the cumulative impact becomes larger if the household has access to electricity for a longer period of time (up to five years). This is consistent with the interpretation of our theoretical model discussed in section 4.

It should be reiterated that the retrospective panel dataset can be created only for nuclear households that satisfy certain conditions detailed in Section 5. Therefore, the comparability between

[18] To estimate the propensity score, we use a married woman's total number of children, the ratio of boys among children under 15 years, age and educational 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.

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

Table 4
Regression results based on the retrospective panel dataset.

Dep var: N_{ht}	(1)	(2)	(3)	(4)	(5)	(6)
$E_{h0} \times I_{t1}$	−0.009	−0.044***	−0.065***	−0.111***	−0.136***	−0.116*
	(0.007)	(0.013)	(0.019)	(0.022)	(0.030)	(0.062)
$E_{h0} \times I_{t2}$						−0.161
						(0.099)
Round 0	2004	2003	2002	2001	2000	2000
Observations	19,844	19,844	19,844	19,844	19,844	4203
R^2	0.993	0.961	0.899	0.819	0.752	0.847

Note: All the estimates are based on ordinary least-squares regressions with household-level fixed-effects terms. Sample weights are applied in all the regressions. E_{h0} is an indicator variable for electrified households in round 0 and $I_{tj} \equiv \mathbf{1}(t = j)$ 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. Columns (1)–(5) use all 9922 nuclear households in round 1. Columns (6) uses 1401 panel nuclear households.

Tables 3 and 4 may be debatable because the types of households included in the contemporaneous and retrospective panel datasets are different. To address this concern, we created a sample of 1401 nuclear households from the contemporaneous panel households, for which round 0 data can be created.

In column (6) of Table 4, we run a DID regression using this sample of panel nuclear households for rounds 0, 1, and 2, together. The coefficient on $E_{h0} \times I_{t1}$ provides an estimate of the impact of electrification in round 0 on the change in the number of children between 2000 and 2005. Even though the number of households in the sample used for this regression is smaller than that for other columns in this table, the impact is still negative and statistically significant at a 10 percent level. It should be noted that the estimated coefficient on $E_{h0} \times I_{t1}$ will be identical even if we use data only for rounds 0 and 1 and use the same specification as columns (1)–(5) of Table 4, because we have no time-varying covariates.

The estimated coefficient on $E_{h0} \times I_{t2}$ is reported in the second row of column (6). It represents an estimate of the impact of electrification in round 0 on the change in number of children between 2000 and 2010. The point estimate (−0.161) is larger in terms of absolute value than the coefficient on $E_{h0} \times I_{t1}$. This is expected because the time interval is longer. However, the point estimate is not statistically significant. Besides the small sample size, lack of statistical significance can also be attributed to the presence of contamination. That is, households that did not have access to electricity in round 0 may have obtained access to electricity by round 2. While this issue exists even when a five-year interval is used, the impact of contamination is likely to be larger when the time interval is longer. Indeed, when we use a specification that allows for time-varying treatment in a way similar to column (4) of Table B.8 in Appendix B to address this issue, the impact of the treatment is statistically significant for both 5- and 10-year periods. Furthermore, the impact over the 10-year period between 2000 and 2010 is about twice as large as that over the 5-year period between 2000 and 2005 (results available upon request).

7.5. Impact heterogeneity

The analysis so far did not explicitly consider the potentially heterogeneous impact of electrification. However, it is plausible that the initial number of children affects the subsequent change in number of children. That is, households without children may try to increase the number of children to reach the optimal number n^* of children, whereas households with multiple children may have already attained n^* in the base time period ($t = 1$). To account for this potential impact heterogeneity across households with different initial number of children, we run regressions based on eq. (12), in which the interaction terms involving the indicator variable for having at least $v(\in \{1, 2, 3, 4\})$ children in round 1 (i.e., V_h^v) are included in the model used in column (2) of Table 3.

The first row of Table 5 provides estimates of β in eq. (12), whereas the second row gives estimates of β^+. The point estimate of β^+ is very close to zero when $v = 1$ and positive when $v \geq 2$. Regardless of the value of v, the point estimate of β^+ is statistically insignificant. When the point estimate is positive (i.e., $v \in \{2, 3, 4\}$), the absolute value is always smaller than β such that $\beta + \beta^+ < 0$. Therefore, regardless of the initial number of children in the household, access to electricity in round 1 tends to have a negative impact on fertility. The positive point estimates of β^+ suggest that the impact of rural electrification on fertility for households with a large initial number of children may be smaller in absolute value than that for households with no or few children. However, the heterogeneity of the impact appears to be limited because β^+ is generally small in absolute value and statistically insignificant.

Another point to note from Table 5 is that the point estimates of β are all negative regardless of the value of v. Because $E_{h1} \times I_{t2}$ and $E_{h1} \times I_{t2} \times V_h^v$ are highly collinear, particularly when v is small, it is not surprising that the point estimates are statistically insignificant when $v = 1$ or $v = 2$. Nevertheless, as with our baseline results, the estimates of β are all well within one standard deviation from −0.2. Hence, accounting for the potential impact heterogeneity by the initial number of children does not alter our baseline results. In Table E.11 in Appendix E, we additionally investigate the impact heterogeneity by the duration of access to electricity as of round 1 because the impact of electrification may be different between early adopters and late adopters. However, as with the case of initial number of children, we do not find statistically significant impact heterogeneity.

7.6. Consistency with theoretical model

The results presented above strongly indicate that the impact of rural electrification significantly reduced fertility by around 0.2 children over the five year period between 2005 and 2010 in most estimates. This finding, in turn, indicates $n'_*(e) < 0$. 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 ρ', I', l', and c' based on eqs. (4), (5), (8) and (9), respectively.

To test the signs of ρ' and l', we use the time-use data of the wife collected in round 1 survey, including both panel and nonpanel households, to construct the empirical counterparts of ρ and l. However, the time-use data are incomplete because we only know how many hours a day the wife spent on each of the following 18 activities over the last 24 h of the survey: (1) taking care of children, (2) processing food, (3) collecting fuel, (4) working as an agricultural worker, (5) working as a nonagricultural worker, (6) engaging in other income-generating activities, (7) fetching water, (8) washing clothes and cleaning, (9) cooking and serving, (10) shopping, (11) reading and studying, (12) listening to the radio, (13) watching TV, (14) eating, (15) bathing or personal care, (16) resting (excluding sleeping), (17) socializing, and (18) performing

Table 5
Impact heterogeneity by the initial number of children.

Dep var: N_{ht} Threshold number of children (v)	(1) $v = 1$	(2) $v = 2$	(3) $v = 3$	(4) $v = 4$
$E_{h1} \times I_{t2}$	−0.136	−0.146	−0.169*	−0.193***
	(0.164)	(0.106)	(0.090)	(0.114)
$E_{h1} \times I_{t2} \times V_h^v$	−0.001	0.010	0.037	0.108
	(0.142)	(0.157)	(0.152)	(0.146)
$I_{t2} \times V_h^v$	2.070***	1.110***	0.727***	0.602***
	(0.160)	(0.107)	(0.073)	(0.065)
log (HH expenditure per capita)	0.017	0.052	0.089	0.046
	(0.142)	(0.157)	(0.152)	(0.146)
Ratio of boys among children under 15	−1.776***	−0.948***	−0.687***	−0.787***
	(0.164)	(0.106)	(0.090)	(0.114)
I_{t2}	−0.140**	−0.089*	−0.12**	−0.122**
	(0.059)	(0.053)	(0.057)	(0.056)
Observations	5,084	5,084	5,084	5,084
R^2	0.891	0.888	0.884	0.884

Note: All the estimates are based on ordinary least-squares regressions with household-level fixed-effects terms. Sample weights are applied in all the regressions. E_{h1} is an indicator variable for electrified households in round 1 and $I_{t2} \equiv \mathbf{1}(t = 2)$ is an indicator variable for round 2. $V_h^v \equiv \mathbf{1}(N_{h1} \geq v)$ is an indicator variable that the initial number of children is no smaller than v. Standard errors clustered at the subdistrict level are reported in parentheses. ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

religious practices. 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 $\widetilde{T} \equiv \sum_{1 \leq j \leq 18} \tau_j$.

This list presumably covers most of the important activities that are performed during effective lighted time. However, there may be other activities that are not appropriately covered in this list. For example, if one must commute to the workplace, the 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, because of data limitation, we need to 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 about one percent of observations for which $12 \leq \widetilde{T} \leq 22$ was not satisfied. Because we assume away the leisure time in our analysis, we define the total productive (nonleisure) time by $T \equiv \sum_{1 \leq j \leq 11} \tau_j$. In other words, we exclude the time spent on listening to the radio, watching TV, eating, bathing or personal care, resting, socializing, and performing religious activities from the effective lighted time $\widetilde{T}$ to arrive at T.[20]

As evident from the definition of ρ, this quantity can be calculated only from those households with at least one child. Therefore, we restrict our sample to the set of households with at least one child and calculate ρ by $\rho = \tau_1/T/n$, because it corresponds to the average proportion of effective lighted time spent taking care of each child. Similarly, because l is the proportion of the effective lighted time not spent on taking care of children, we calculate l by $l = 1 - \tau_1/T$.

Finding the empirical counterparts for the maximum potential income I and nonchild goods consumption c is also a challenge. For I, it may be computed, in principle, by dividing income earned from work by the fraction of the effective lighted time used to generate that income. However, the data do 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 earners 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 ρ', I', l', and c', we run OLS regressions of ρ, I, l, and c on the household's access to electricity in round 1 (E_{h1}). Table 6 reports the results of the regressions using a sample of households with at least one child. The odd-numbered columns use a sample of panel households with at least one child, whereas the even-numbered columns use all the households with at least one child.

Columns (1) and (2) show that ρ tends to be lower for electrified households, even though the coefficient is insignificant. Therefore, these point estimates are consistent with the assumption on ρ in eq. (4). Columns (3) and (4) show that the coefficient on I is significantly positive, which supports the assumption in eq. (5).

Columns (5) and (6) show that the coefficient on l is positive, even though they are insignificant. Columns (7) and (8) show that the coefficient on c is significant and positive. This suggests that $l' > 0$ and $c' > 0$ hold, even though the former is not statistically significant. Hence, our empirical results are consistent with Proposition 1, given our empirical evidence for $n' < 0$.

Taken together, we find that our empirical results are broadly consistent with the model assumptions and predictions discussed in section 4. This finding is robust because it remains true even when we control for some household characteristics or instrument the household's electrification status by the indicator variable for electrified villages, and grid system loss in 2005, as discussed in Appendix E.

7.7. *Exploring causal channels*

The analysis presented so far provides consistent evidence that the impact of rural electrification on fertility is significantly negative, both economically and statistically. This finding is also consistent with the theoretical predications in Proposition 1. In this subsection, we explore why there is such a negative impact.

Specifically, we investigate the relevance of possessing TVs and mobile phones in DID regressions, because TVs and mobile phones can disseminate important information about family planning, various

[20] While we believe this classification is reasonable, the classification of what qualifies as leisure is admittedly subjective. For this reason, we also define $\widetilde{\rho}$ and $\widetilde{l}$ by replacing T with $\widetilde{T}$ in their definitions discussed below and use them as dependent variables. The conclusions remain unchanged under these alternative definitions.

Table 6
Test of consistency with model assumptions and predictions..

Dep var	(1) ρ	(2) ρ	(3) I	(4) I	(5) l	(6) l	(7) c	(8) c
E_{h1}	−0.202	−0.040	0.159***	0.189***	0.367	0.101	0.399***	0.416***
	(0.155)	(0.070)	(0.036)	(0.015)	(0.244)	(0.144)	(0.049)	(0.020)
Sample	Panel	All	Panel	All	Panel	All	Panel	All
R^2	0.152	0.069	0.218	0.177	0.202	0.093	0.210	0.188
Observations	2356	14,771	2356	14,771	2356	14,771	2356	14,771
Mean of dep var	0.491	0.449	3.620	3.634	99.02	99.00	1.184	1.180

Note: All the estimates are based on ordinary least-squares regressions with subdistrict fixed effects. E_{h1} is an indicator variable for electrified households in round 1. 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
Difference-in-differences regressions with indicators for possessing TV and mobile phone.

Dep var: N_{ht}	(1)	(2)	(3)
$E_{h1} \times I_{t2}$	−0.101	−0.170**	−0.102
	(0.096)	(0.072)	(0.096)
log (HH expenditure per capita)	−0.174***	−0.176***	−0.174***
	(0.065)	(0.064)	(0.065)
Ratio of boys among children under 15	0.068	0.066	0.068
	(0.156)	(0.156)	(0.156)
I_{t2}	0.433***	0.434***	0.433***
	(0.062)	(0.062)	(0.062)
(Have TV in round 1) $\times I_{t2}$	−0.111		−0.112
	(0.010)		(0.10)
(Have mobile phone in round 1) $\times I_{t2}$		−0.007	0.020
		(0.151)	(0.158)
Observations	5,084	5,084	5,084
R^2	0.871	0.871	0.871

Note: All the estimates are based on ordinary least-squares regressions with household-level fixed-effects terms. Sample weights are applied in all the regressions. E_{h1} is an indicator variable for electrified households in round 1 and $I_{t2} \equiv \mathbf{1}(t = 2)$ 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.

income-generating opportunities, and modern lifestyles, all of which may affect fertility. Indeed, previous studies such as Ferrara et al. (2012), Grimm et al. (2015), and Jensen and Oster (2009) indicated the presence of impact of TV on fertility. While we are not aware of any compelling empirical evidence on the impact of mobile phones on fertility, studies have found that mobile phones improved income opportunities for fishermen and farmers (Fu and Akter, 2016; Jensen, 2007; Muto and Yamano, 2009) and various other aspects of life.

Column (1) of Table 7 shows that the estimated impact of access to grid electricity in round 1 on fertility, which is reported in the first row ("$E_{t1} \times I_{t2}$"), declines in absolute value and is insignificant once the possession of TV in round 1 interacted with I_{t2} is included in the regression. However, the same cannot be said about the ownership of mobile phones as column (2) shows. These results do not change when we simultaneously include the ownership of both TV and mobile phone as shown in column (3). Thus, ignoring the potential endogeneity of TV possession, there appears to be a (negative) causal link between TV possession and fertility.

In contrast, no causal link was found between mobile phones and fertility. This result may not be surprising for two reasons. First, most people in Bangladesh during late 2000s had only a basic mobile phone with limited functionality, even if they have a mobile phone. Second, mobile phone ownership is far less dependent on access to grid electricity than TV ownership. The absence of impact through mobile phones arguably corroborates our conjecture that the estimated impact of TV on fertility does not capture the use of modern technology at large. Instead, it specifically reflects the use of TV.

Our conjecture is based not only on the fact that TV serves as an alternative form of entertainment but also that TV provides people with a wide range of information. The latter reason is relevant as the state-run Bangladesh Television broadcasts free-to-air and sometimes sponsored awareness raising programs funded by multilateral and bilateral development partners. For example, the USAID sponsored a popular drama serial during the mid 2000s to promote, among others, the idea of a small family and use of family planning and reproductive health services (Rahman et al., 2017).

Our results are also consistent with World Bank (2008) based on DHS data from nine countries, including Bangladesh. This study finds that access to TV significantly increases women's knowledge about health and family planning, and increased awareness has changed health behaviors and improved health outcomes. Therefore, the negative impact of rural electrification on fertility appears to be driven, at least in part, by the improved understanding of family planning through TV watching.

8. Discussion

A number of studies have examined the social and economic impact 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 causal relationship between electricity availability and fertility is by itself not a new premise. However, rigorous econometric studies using household-level data are still limited. To our knowledge, this is the first study that uses 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 the (1) choice of estimation method, (2) choice of sample (i.e., contemporaneous or retrospective panel), and (3) potential presence of endogeneity. Moreover, our results are consistent with the theoretical predictions on time use and consumption behavior, and pass falsification tests. We also provide suggestive evidence that an important causal channel is the use of TV.

The current study makes several notable contributions that allow us to distinguish from the previous ones. First, we develop a formal model that simultaneously analyzes fertility, consumption, and time use in the context of rural electrification. To our knowledge, we are the first to offer such a model. Even though our model is simple and based on a set of strong assumptions, it provides testable theoretical predictions and elucidates the factors that determine the direction of the impact of rural electrification on fertility. As the discussion in section 4 indicates, the impact is likely to be negative in relatively poor areas but may be positive in more affluent areas. Therefore, given that rural Bangladesh is generally poor, it is not surprising that we consistently find a negative impact. More importantly, the observed relationships of fertility, consumption, and time-use with households' access to electricity are found to be consistent with our model predictions as shown in our empirical analysis in section 7.

Second, to make our results credible, we have considered a variety of potential sources of threats to identification. The approaches we have taken to address them are potentially applicable to other studies. In our baseline DID specification, we control for all unobservable time-invariant characteristics across households using panel data, which was not possible for many existing studies relying on cross-sectional data or panel data at an aggregated level. Addressing the potential endogeneity with IVs does not alter our conclusion. The results obtained from PSM, which addresses the potential confounding with unbalanced covariate distribution and covariate-dependent time trend, also provide consistent results. Further, to boost the credibility of our results, we use the retrospective panel dataset in addition to the contemporaneous panel dataset, because the latter potentially suffers from sample selection issues.

All these results show that the impact of electrification on fertility is negative and economically and statistically significant. While none of the methods we presented are able to simultaneously address all the potential threats to identification, consistency of the results across different specifications and methods strongly indicate that rural electrification indeed had a negative impact on fertility; it appears unlikely that selection on unobservables can explain all of the results presented above in a coherent manner.

Furthermore, the point estimates are generally consistent with each other quantitatively. They mostly indicate that rural electrification reduced fertility by 0.2 children over a five-year period. Assuming that the period of reproductive age is between 15 and 44 years, or 30 years in total, the implied reduction in the total fertility rate due to electrification is about $1.2(= 0.2 \times 30/5)$ for a give household. Multiplying this by the net increase in the electrification rate of 16.0 percentage points (see section 5), we arrive at the contribution of rural electrification to the reduction of total fertility rate of $0.192(= 1.2 \times 0.160)$ in our study area. This amounts to 53.3 percent ($= 0.192/0.36$) reduction in the total fertility rate between 2005 and 2010 in Bangladesh (see also section 1). Even though the national figure also includes areas that are not covered by the rural electrification program (e.g., urban areas) and the back-of-envelope calculation provided here is crude, the order of the magnitude of the estimated impact is clearly sizable.

Finally, besides identifying the impact of rural electrification on fertility, we also explored the impact channels. The finding that TV ownership lowered fertility is broadly consistent with previous studies such as Ferrara et al. (2012), Grimm et al. (2015), and Jensen and Oster (2009). Even though TV is not explicitly modelled, our finding is also consistent with our theoretical model, because people living in electrified households may spend more time on watching TV instead of child-related activities versus those in nonelectrified households.

An important limitation of this study is that we did not examine the impact of rural electrification on child quality. This is because we do not have any credible indicator of child quality in our data. Nevertheless, existing studies appear to indicate that there is indeed a positive impact on child quality in terms of height-for-age z-score, the probability of schooling, and study time (Fujii et al., 2017; World Bank, 2008).

The current 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 broad and long-term assessment of rural electrification and possibly other infrastructure interventions to fully understand their potential impacts. A complete understanding of the potential impact would alter how policy makers approach the practice of policy formulation for large infrastructure investment and prioritize the projects, especially given the limited donor funding and government budget support. This, in turn, would lead to decisions based on a balanced judgement.

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jdeveco.2019.102430.

Appendix A

Proof of Proposition 1. Households maximize the utility function in eq. (1) subject to the time constraint in eq. (2) and budget constraint in eq. (3). From the first-order conditions for this utility maximization problem, c_* and n_* can be shown to satisfy the following condition:

$$\omega[p(e) + I(e)\rho(e)]f_c(c_*(e), e) = (1-\omega)g'(n_*(e)). \tag{A.1}$$

Note that the term $I(e)\rho(e)$ in the square brackets on the left hand side of eq. (A.1) can be interpreted as the opportunity cost of having one child because it corresponds to the income that can be earned using the time spent in raising one child. Therefore, $[p(e) + I(e)\rho(e)]$ represents the total economic cost of having one child and eq. (A.1) permits the usual interpretation of the 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 (A.1) with respect to e and solving for $n'_*(e)$, we obtain the following results:

$$n'_*(e) = \frac{\omega V(e)}{(1-\omega)g''(n_*(e)) + \omega[p(e) + I(e)\rho(e)]^2 f_{cc}(c_*(e), e)}. \tag{A.2}$$

Because the denominator of eq. (A.2) 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. (A.2), we obtain eqs. (8) and (9). The latter part of Proposition 1 follows from these and eqs. (4)–(6).

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