Powering the Future: The Long-Term Human Capital Effects of Rural Electrification

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Abstract

In low- and middle-income countries, electricity often suffers from coexisting affordability, reliability, and stability challenges, yet empirical work jointly examining these dimensions remains limited. This paper studies the long-run human capital effects of a rural electrification program implemented in China in the late 1990s that simultaneously improved all three dimensions. Using a cohort difference-in-differences design with treatment intensity measured by county-level electricity price reductions, I find that exposure during middle childhood (ages 6–11) leads to substantial long-run gains in human capital. In contrast, exposure only after middle childhood yields negative and statistically insignificant effects. Analysis of mechanisms shows that improved agricultural productivity is an important channel and that increased public investment in education may also play a role. Extended study hours due to improved lighting, however, are unlikely to be relevant. China's experience offers insights for current rural electrification efforts in developing countries.

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1 Introduction

Over the past two decades, rural electrification has expanded rapidly across developing countries. In Bangladesh, the share of rural population with access to electricity rose from 17% in 2000 to 99% in 2022; in Kenya, from 7% to 66%; and in India, from 49% to nearly universal coverage.[1] Despite this remarkable progress, recent evidence shows that simply connecting households to the grid yields only modest or even negligible economic benefits (Lee, Miguel, and Wolfram, 2020b; Burlig and Preonas, 2024), raising concerns about the effectiveness of rural electrification investments.

A possible explanation for this limited impact lies in the distinction between connection and meaningful access. Electrification is not only about whether households are connected but also about how electricity is delivered—particularly its reliability, affordability, and stability (Bhatia and Angelou, 2015; Lee, Miguel, and Wolfram, 2020a).[2] These dimensions continue to constrain economic activity and daily life in many developing countries (Onishi, 2015; World Bank, 2024). The central question is whether improving electricity delivery along these margins can generate meaningful economic outcomes.

My paper answers this question by studying the long-term human capital effects of a rural electrification program implemented in China in the late 1990s that explicitly targeted these key electricity delivery factors. At the time, China had the world's largest rural population, widespread grid coverage,[3] and a development level comparable to that of many developing economies today, making it a useful and generalizable setting. In late 1998, China's central government launched the "Two Reforms and One Price" (TROP) program,[4] which aimed to: (1) upgrade rural electricity infrastructure, (2) reform rural electricity administration, and (3) equalize rural and urban electric-

[1] Source: World Bank DataBank.

[2] Burgess et al. (2020) shows that unconditional grid expansion can lead to inefficient outcomes and welfare losses.

[3] China's rural household connection rate reached 85% by 1991 and 97% by 1998. Source: https://kjpj.bit.edu.cn/docs/20150119212311371518.pdf and https://ncdqh.com.cn/interpretation/200109/132007.html (in Chinese).

[4] "Two Reforms and One Price" (TROP), known in Chinese as *Liang Gai Yi Tong Jia*, was announced in October 1998. Accordingly, 1999 is designated as the starting point of the treatment period in this paper.

ity pricing.[5] TROP was exactly designed for addressing electricity delivery issues. The program covered China's entire rural population, making it the largest electrification effort in global history. Its nationwide rollout offers a rare opportunity to study the general equilibrium effects of rural electrification while mitigating concerns about spillovers and selection bias.

To examine the long-run human capital effects of rural electrification empirically, I construct a dataset by manually collecting county-level rural electricity prices before and after TROP implementation from local gazetteers and linking them to individual and household data from the 2014 China Family Panel Studies (CFPS). The final sample includes 142 counties across multiple provinces. To explore underlying mechanisms, I also assemble prefecture-level economic indicators and county-level public expenditure data.

This paper uses a cohort difference-in-differences (DiD) approach to estimate the effects. The treatment timing is 1999. I compare cohorts who were exposed to the program during primary school age (born between 1988–1994, treatment group) with older cohorts (born between 1979–1984, control group) within the same county.[6] I then leverage cross-county variation in the absolute level of electricity price reductions to estimate the effects. I measure treatment intensity using electricity price reductions because they are consistently measured, directly linked to the program, and generate meaningful cross-county variation. The design assumes that counties with larger and smaller price cuts would have followed similar trends after TROP, and that control cohorts' human capital was not significantly affected by the program. The first assumption is supported by an event study framework. For the second, my paper provides evidence demonstrating that this assumption is also satisfied.

I find that a one standard deviation increase in treatment intensity (0.21 CNY/kWh, 1 CNY $\approx$ 0.12 USD in 2000) raises children's educational attainment by 0.605 years. This effect is comparable to the estimate in Lipscomb, Mobarak, and Barham (2013), but more than six times the impact

[5] Before TROP, rural electricity prices were significantly higher than in urban areas due to institutional distortions and poor infrastructure. After the program, rural prices were reduced and equalized to urban levels.

[6] Treatment cohorts had at least one year of overlap with TROP during primary school. A three-year buffer reduces concerns about partial exposure and variation in school starting age.

of China's Send-Down Movement during the late 1960s and 1970s (Chen et al., 2020a),[7] and more than twice that of China's 1986 Compulsory Education Law (Chen and Park, 2021). The results remain robust across various settings. At the aggregate level, about 74 million rural children were of primary school age when TROP began in 1999. A one standard deviation increase in treatment intensity therefore implies approximately 45 million additional person-years of schooling (74×0.605).

In addition to years of schooling, later adult cognitive outcomes also improve: a one standard deviation increase in treatment intensity raises math test scores by 0.164 standard deviations, and Chinese word recognition scores by 0.142 standard deviations. School completion rates show consistent gains across levels: a one standard deviation increase in treatment intensity raises primary school completion by 3.7 percentage points, junior high by 5.12 points, and senior high by 6.17 points.

While I find no significant heterogeneity by sibling status or hydropower potential, the effects are larger for males (marginally significant), in drier regions, and in more agriculturally dependent counties. This pattern suggests that improved agricultural productivity may be a key channel through which TROP operates.

This paper examines three channels through which rural electrification may affect human capital: enhanced agricultural productivity, increased public educational investment, and improved lighting. Prior literature suggests that rural electrification increases agricultural productivity through irrigation and mechanization (Kitchens and Fishback, 2015; Assunção et al., 2017; Lewis and Severnini, 2020; Fried and Lagakos, 2021). Consistent with this, measuring agricultural productivity by agricultural GDP per unit of arable land, I find that a one standard deviation increase in treatment intensity raises productivity by 824 CNY per acre ($\approx$ 97 USD in 2000), equivalent to about 12.7% of the average per capita net annual income of rural households in 1999.[8] Additional ev-

[7] The Send-Down Movement (1968–1978) was a campaign during China's Cultural Revolution where over 17 million urban youths were sent to rural areas to work and learn from peasants.

[8] In 1999, the average per capita net annual income of rural households in China was 2,210 CNY ($\approx$ 265 USD in 2000). Source: The National Bureau of Statistics of China. https://www.stats.gov.cn/sj/ndsj/zgnj/2000/J16c.htm. In the same year, the national average per capita arable land operated by rural households was 2.07 mu (China's metric,

idence from comparisons of children born before and after 1999 supports this channel, showing improvements in health outcomes consistent with the established link between income and health (e.g., Deaton, 2003; Banerjee, Deaton, and Duflo, 2004; Deaton, 2008).

For the second channel, cheaper and higher-quality electricity likely facilitated the use of school equipment and infrastructure, encouraging governments to allocate more resources to education. Consistent with this prediction, I find that a one standard deviation increase in treatment intensity raises the share of public expenditure devoted to education by 0.42 percentage points.

Rural electrification can also increase education by improving lighting, thereby extending study hours for children. To examine this channel, I collect data on daily sunrise and sunset timing and calculate the daily average daylight duration for my sample counties. However, counties with shorter daylight duration show no significantly larger gains compared to those with longer duration, providing no support for the lighting channel.

A key assumption in my identification strategy is that children who were exposed only after primary school age, referred to as older children, are not significantly affected by the program and therefore serve as the control group. Consistent with this assumption, I find that the impact on older children's education is negative and statistically insignificant. This raises a natural question: why do the effects differ by age at exposure?

Given the central role of agriculture in rural livelihoods in late-1990s China,[9] a plausible explanation is that rural electrification raises household income (income effect) while simultaneously increasing the opportunity cost of schooling for older children (substitution effect), who are closer substitutes for adult labor in agriculture and more likely to operate machinery. The higher opportunity cost can therefore offset or even surpass the income-driven positive effect on education. Consistent with this interpretation, older boys are more negatively affected, although the estimate is imprecise, reflecting their closer substitutability with adult labor relative to older girls. To formal-

where 1 mu equals 0.165 acres), equivalent to 0.34 acres. Source: https://www.stats.gov.cn/sj/ndsj/zgnj/2000/L13c.htm.

[9]In the late 1990s, about 70% of the population was rural, and agriculture accounted for roughly 17% of GDP, compared with a world average agricultural GDP share of about 4% at the time. Data source: World Bank DataBank.

ize this intuition, I develop a simple household decision-making model in the spirit of Shah and Steinberg (2017), in which electrification acts as a productivity shock in agriculture. Appendix Section E provides the model details.

The framework does not rule out other channels through which rural electrification may affect long-run human capital. Increased public investment in education can reinforce the positive effects of improved agricultural productivity for children exposed during primary school age. For older children, however, such benefits may be offset by a strong substitution effect, leading to an overall effect that remains negative and statistically insignificant.

This paper makes two contributions to the literature. First, it contributes to the literature on the effects of rural electrification in developing economies. A large literature examines rural electrification across South and Southeast Asia, Sub-Saharan Africa, and Latin America.[10] However, most of these studies focus on whether households are connected, with much less attention to how electricity is delivered. While a growing literature explores the impacts of reliability,[11] empirical work that jointly examines key electricity delivery factors like affordability, reliability, and stability, remains scarce.[12] This gap is important because, in many low- and middle-income countries, these factors typically coexist (Kojima and Trimble, 2016; World Bank, 2024), and improving a single dimension may be insufficient to generate meaningful and sustained economic benefits.[13]

[10]These include India (Rud, 2012; Khandker et al., 2014; Van de Walle et al., 2017; Thomas et al., 2020; Burgess et al., 2023; Fetter and Usmani, 2024; Burlig and Preonas, 2024), Indonesia (Kassem, 2024), Vietnam (Khandker, Barnes, and Samad, 2013), the Philippines (Barnes et al., 2002), Brazil (Lipscomb, Mobarak, and Barham, 2013), Peru (Dasso Arana, Fernandez, and Ñopo, 2015), South Africa (Dinkelman, 2011), Kenya (Lee, Miguel, and Wolfram, 2020b; Koima, 2024), Ghana (Akpandjar and Kitchens, 2017), Rwanda (Lenz et al., 2017), Ethiopia (Bernard and Torero, 2015; Fried and Lagakos, 2021), Sub-Saharan Africa (Bernard, 2012), and Africa as a whole (Peters and Sievert, 2016).

[11]These include impacts on firm performance (Steinbuks and Foster, 2010; Fisher-Vanden, Mansur, and Wang, 2015; Allcott, Collard-Wexler, and O'Connell, 2016; Cole et al., 2018; Hardy and McCasland, 2021; Abeberese, Ackah, and Asuming, 2021), electricity consumption (Carranza and Meeks, 2021), employment (Mensah, 2024), agricultural wages (Nag, 2024), and household income (Burlando, 2014) in developing countries. Jha, Preonas, and Burlig (2022) studies the relationship between wholesale markets and blackouts. For a comprehensive review of the economics of electricity reliability, see Borenstein, Bushnell, and Mansur (2023).

[12]For affordability, Abeberese (2017) studies the effects of electricity prices on firm performance in a developing setting. For stability, Meeks et al. (2023) documents short-run consumption responses to voltage quality improvements in Kyrgyzstan. Related U.S.-based work includes Cong et al. (2022) on energy poverty, Davis (2025) on how electricity prices shape household heating choices, and Borenstein (2012) and Levinson and Silva (2022) on the distributional effects of pricing.

[13]Meeks and Mahadevan (2025) shows that subsidies alone often fail: even with low tariffs, weak infrastructure

In addition, empirical evidence from China, which had the world's largest rural population at the time of TROP, remains limited.[14] My paper fills these gaps by studying a rural electrification program that simultaneously addressed key electricity delivery issues in late-1990s China, when its development level was comparable to that of many developing economies today (see Figure 1).[15] China's experience can offer insights for ongoing rural electrification efforts in many low- and middle-income countries.

Second, this paper contributes to the literature on the broader socioeconomic impacts of electrification. While several studies find electrification yields only negligible benefits and modest welfare gains (Peters and Sievert, 2016; Lenz et al., 2017; Lee, Miguel, and Wolfram, 2020b; Burgess et al., 2023; Burlig and Preonas, 2024; Koima, 2024), these authors focus on short-term outcomes, potentially missing longer-term effects. A small but growing body of literature documents long-term impacts, including increased female labor force participation (Greenwood, Seshadri, and Yorukoglu, 2005; Vidart, 2024),[16] improved firm productivity (Fiszbein et al., 2020; Fried and Lagakos, 2023), better infant health and reduced fertility (Lewis, 2018), faster economic growth (Lewis and Severnini, 2020), and higher household consumption (Van de Walle et al., 2017). My paper adds to this literature by documenting the long-term human capital effects. Unlike Lipscomb, Mobarak, and Barham (2013), which studies the impact of electrification from hydropower dam construction on human capital development, my paper examines a nationally implemented program using individual-level data in a highly generalizable context, strengthening both identification and external validity.

The remainder of this paper is organized as follows: Section 2 provides background on China's rural electrification. Section 3 describes the data. Section 4 outlines the empirical strategy. Section

can lead to intermittent service and poor power quality. Berkouwer et al. (2024) finds that improving voltage quality has limited economic effects.

[14] There are few exceptions. He (2019), Lin and Xu (2024), and Jiao (2024) (in Chinese) focus on grid connection in 1980s China, whereas my paper examines reforms targeting electricity delivery in the 1990s. Ding, Qin, and Shi (2018) studies the short-run effects of TROP on agricultural income; my paper instead examines long-run human capital outcomes.

[15] Lee, Miguel, and Wolfram (2020a) highlights the importance of understanding cross-country heterogeneity in electrification impacts for effective policymaking.

[16] Vidart (2024) highlights the role of human capital accumulation in linking electrification to female labor force participation, but does not directly examine educational outcomes.

5 presents the main results. Section 6 conducts robustness checks. Section 7 analyzes heterogeneity. Section 8 examines possible mechanisms. Section 9 justifies a key identification assumption and explains age-specific effects. Section 10 concludes.

2 Background

2.1 Rural Electricity Management in China (1979–1999)

Following the launch of China's Reform and Opening-up policy in 1979, the country began transitioning from a centrally planned economy to a market-oriented system. This transformation extended to the electricity sector, where funding constraints posed a major obstacle to reform.

To address the shortage of capital for power infrastructure, the Ministry of Electric Power introduced measures such as joint investment by government departments, local fundraising, and the use of foreign capital. It also shifted from uniform pricing to cost-recovery pricing,[17] moving toward a more market-oriented system (Chen, 2018). As a result, national and local electricity networks operated in parallel until the late 1998 launch of the "Two Reforms and One Price" (TROP) program.

Before 1998, among more than 2,400 county-level power supply enterprises across China, roughly one-third were directly managed and supplied by the national grid—typically in relatively rich counties (or county-level districts) or suburban areas. The remaining counties were locally managed, either purchasing electricity in bulk from the national grid or running their own independent systems.[18] In most cases, electricity was delivered to rural households via township and village-level substations, with prices marked up at each stage of distribution (Section 4.1 provides an example).

Before 2012, China implemented a categorized electricity pricing system based on usage types (e.g., household lighting, agricultural, industrial). During my study period, rural electricity prices can therefore be considered linear. After 2012, there was a shift to a tiered pricing system in which

[17] Cost recovery refers to the principle of recouping the costs associated with providing a product or service.
[18] Source: https://finance.sina.com.cn/g/20060110/15592263755.shtml (in Chinese). In the empirical analysis, I include county fixed effects to absorb variation in electricity management systems.

rates vary by consumption level.[19] For rural households, electricity prices for lighting (referring to daily life usage) and agricultural production were typically similar (see Appendix Table A2 for an example from Yangzhou City in 2000). In practice, it is often difficult to distinguish between household and productive uses of electricity in rural areas, as many residents use electricity at home to pump water for nearby farmland or to process agricultural products.

2.2 Two Reforms and One Price (TROP)

In October 1998, the State Council of China launched the TROP program to improve rural living and production conditions. The initiative aimed to (1) upgrade rural power grids, (2) reform rural electricity administration, and (3) equalize rural and urban electricity pricing, hence the name "Two Reforms and One Price" (TROP). The program was explicitly designed to address key electricity delivery factors, particularly reliability, stability, and affordability.

The upgrading of electricity infrastructure included the installation of more dependable transmission lines and transformers. TROP also removed administrative intermediaries, streamlined local personnel, curbed corruption, and standardized electricity pricing. Rural households were connected directly to the national grid managed by the State Grid Corporation.[20] Electricity usage was measured by newly installed meters, with prices transparently posted in each village. Retail electricity prices are set by distributors but require government approval. In most cases, pricing reflects long-run marginal supply costs. However, inefficiencies remain in the pricing system (International Energy Agency, 2002). Funding for TROP primarily came from the State Grid Corporation, which is owned by the central government, while rural households were responsible only for wiring behind the meter.[21]

Some suggestive evidence confirms that the program achieved improvements in these areas. For example, the electricity gazetteer of Luoding City (a county-level city in Guangdong Province)

[19] Source: National Development and Reform Commission of China. https://www.ndrc.gov.cn/xwdt/xwfb/201206/t20120614_956502.html (in Chinese).

[20] The State Grid Corporation, owned by the central government, was later split into the State Grid and China Southern Power Grid, both managing different regions of the national grid.

[21] Government regulations prohibited additional charges beyond household wiring, except in cases where residents contributed to the cost of electricity meters due to insufficient renovation funds. Source: https://www.gov.cn/gongbao/content/2001/content_61344.htm.

reports that "supply reliability improved from 75% to 99.5%, with a voltage increase of 30 volts.[22] By 2002, rural electricity prices fell from 1.19 CNY per kWh before the 'Two Reforms and One Price' initiative to 0.99 CNY, and then further to 0.79 CNY (the ultimately unified price)."(Luoding City Gazetteer Compilation Commission, 2003). It's important to note that TROP did not involve new power generation.

TROP was implemented in two phases. The first phase (1999–2001) was more intensive, accounting for most infrastructure upgrades and price reductions. For example, in Hubei Province, over 8 billion CNY was invested during the first phase, compared to 3.6 billion CNY in the second phase (2002–2005).[23] Most provinces achieved unified electricity pricing between rural and urban areas by around 2001. The program also delivered measurable improvements at the provincial level. In Guangdong Province, rural electricity prices dropped by 0.49 CNY (a 35.6% reduction), and voltage levels rose from 150–180 volts to the standard 220 volts.[24]

2.3 The Structure of Electricity Consumption and Development Indicators in China around 1999

Electricity Consumption

China's rural household connection rate reached 85% by 1991 and 97% by 1998.[25] Despite this widespread access, per capita electricity consumption in rural areas remained low—just 235 kWh in 1998.[26] For comparison, this was only 5.8% of U.S. per capita electricity consumption in 1960.[27]

Appendix Figure B1 shows the breakdown of rural electricity consumption in Henan Province, a major agricultural region in central China, in 1997. Use is divided into three categories: irrigation and drainage (37.3%), daily life (26.4%), and agricultural processing (36.2%). In practice,

[22] China's standard household voltage is 220V. Most gazetteers, however, do not include electricity quality data.

[23] See pages 340–341 of Hubei Electric Power Industry Gazetteer Compilation Commission (2012).

[24] See pages 550–551 of Guangdong Electric Power Industry Bureau (Group Corporation) (2004).

[25] Source: https://kjpj.bit.edu.cn/docs/20150119212311371518.pdf and https://ncdqh.com.cn/interpretation/200109/132007.html (in Chinese).

[26] Author's calculation based on National Bureau of Statistics of China (1999). This figure includes industrial usage within a county.

[27] The earliest year with available data. Source: World Bank DataBank.

however, daily life and agricultural uses are hard to distinguish, as households live near farmland and often process crops at home.

In 1999, annual per capita expenditure in rural China was approximately 2,390 CNY.[28] In the same year, electricity consumption per rural capita in Henan was 194 kWh. At a unit price of 0.85 CNY per kWh, this translates to an annual electricity cost of 164.9 CNY, which accounted for about 6.9% of total per capita expenditure. This share matches the 6.8% of household spending that U.S. consumers allocated to utilities, fuels, and public services in 2020.[29]

Development Indicators: China 1998 vs. the World in 2022

In 1998, China had the world's largest rural population—820 million—and key development indicators comparable to many low- and middle-income countries today. Figure 1 plots rural population share against GDP per capita (in constant 2015 USD). The circles (except China) represent 61 low- and lower-middle-income countries with rural population shares exceeding 40% in 2022. Circle sizes are in proportion to rural population.

These countries (excluding China) had a combined rural population of 2.2 billion. Among them, 40 countries (66%) had per capita GDP levels below China's 1998 level, while 12 countries (20%) had per capita GDP within 80–120% of China's 1998 value. These 12 countries alone accounted for 1.25 billion rural residents, with an average rural electrification rate of 80.4%, comparable to rural China in the 1990s. China's late-1990s experience may thus offer insights for rural electrification in many low- and middle-income countries today.

[28] Source: National Bureau of Statistics of China. https://www.stats.gov.cn/zt_18555/ztsj/hjtjzl/1999/202303/t20230302_1923300.html.

[29] Source: U.S. Bureau of Labor Statistics. https://www.bls.gov/opub/reports/consumer-expenditures/2023/.

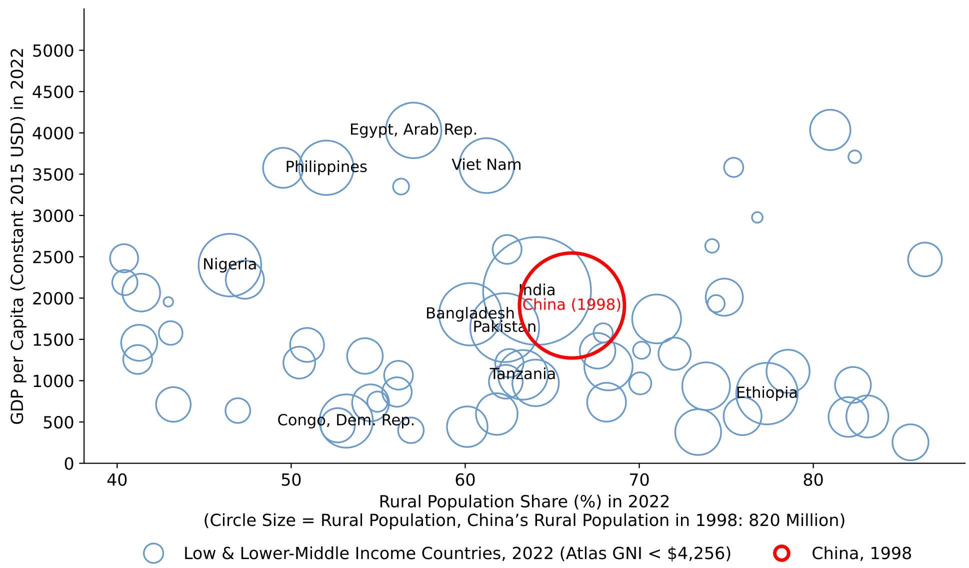

Figure 1: GDP per Capita and Rural Population Share: China 1998 vs. the World in 2022. *Note*: This figure plots GDP per capita (constant 2015 USD) against rural population share. Each circle (except China) corresponds to one of 61 low- and lower-middle-income countries with a rural population share above 40% in 2022. Circle sizes are proportional to each country's rural population. The 10 countries with the largest rural populations are labeled. A total of 12 countries had per capita GDP between 80 and 120% of China's 1998 value. These countries alone accounted for 57% of the total rural population across the 61 countries and had an average rural electrification rate of 80.4%, comparable to rural China in the 1990s. Data are from World Bank DataBank.

3 Data

My paper draws data from three sources: (1) individual-level data from the 2014 wave of the China Family Panel Studies (CFPS), with missing outcome variables supplemented using the 2010 wave; (2) electricity prices from local gazetteers and newspapers; and (3) regional economic indicators from various statistical yearbooks.

3.1 Individual and Household Survey Data

This paper uses individual and household data from the China Family Panel Studies (CFPS), a nationally representative longitudinal survey launched in 2010 by the Institute of Social Science Survey at Peking University.[30] The baseline survey in 2010 includes 14,797 households across

[30] See https://opendata.pku.edu.cn/dataverse/CFPS?language=en.

162 counties in 25 provinces. After excluding 20 county-level districts in Beijing, Shanghai, and Tianjin (due to limited rural representation), this paper covers 142 counties.[31]

Sample Construction. I construct my baseline sample as follows. First, I start with all individuals from the 2010 CFPS baseline born between 1979 and 1994 who have rural Hukou at both ages 3 and 12.[32] Second, I restrict the sample to individuals aged 20 or older in 2014, as their educational attainment is largely complete.[33] I focus on cohorts born 1979–1994 to avoid the influence of major national reforms (the Cultural Revolution 1966–1976, Reform and Opening-up in 1978, and the Compulsory Education Law in 1986). This yields the full sample used in the event study (Figure 3). For the baseline regression, I further exclude cohorts born 1985–1987 (the "gap cohorts") to address concerns about partial treatment exposure and variation in school starting ages. The final baseline sample contains 4,150 individuals from 142 counties.

Outcome Variables and Missing Data. My primary outcome is years of education, supplemented by cognitive test scores.[34] Among the 4,150 individuals in the baseline sample, approximately 76% appear in both the 2010 and 2014 waves; for these individuals, I use their 2014 outcomes. The remaining 24% were surveyed in 2010 but are missing from the 2014 wave.[35] For these individuals, I use their 2010 educational outcomes, following Bianchi, Lu, and Song

[31] The analysis of county-level data in this paper was conducted in the restricted data laboratory of the Institute of Social Science Survey at Peking University.

[32] Hukou is China's household registration system that historically restricted rural-to-urban migration; these restrictions began easing after 1993 (Chan and Zhang, 1999). Because TROP targeted rural areas, I restrict the analysis to individuals with rural Hukou at both ages 3 and 12, ensuring they most likely resided in rural areas during the treatment period.

[33] I use the 2014 wave rather than later waves (2016, 2018, 2020) to minimize cumulative sample attrition from migration and other policy changes, which increase substantially in later waves and could introduce selection bias. Word and math test scores, my supplemental outcome variables, are only available every four years since 2010. Census data like the 2015 census are also less appropriate due to migration concerns and the absence of cognitive test scores. Most individuals in the 2014 wave can be traced back to 2010, when migration was less likely. This choice also avoids potential bias from the post-2012 Hukou reform, which significantly relaxed rural-to-urban migration restrictions.

[34] During the Cultural Revolution (1966–1976), China's education system temporarily adopted a 5-2-2 structure before reverting to the standard 6-3-3 system: six years of primary school, followed by three years each of junior and senior high school (Chen, Jiang, and Zhou, 2020). Note that my empirical analysis includes county fixed effects and province-by-year fixed effects to account for policy changes. Leveraging this policy shift, Chen, Jiang, and Zhou (2020) estimates a 12.7% return to education, while Giles, Park, and Wang (2019) uses the Cultural Revolution as an exogenous shock to estimate a 37.1% return to college education.

[35] Appendix Figure B3 compares the birth year distributions (1979–1994) for individuals present in both waves and those missing in 2014. While the latter group skews slightly younger, the overall distribution remains balanced. Appendix Figure B4 shows that the distributions of years of education are also similar across groups.

(2022).[36] Critically, this imputation is applied equally across treatment (born 1988–1994) and control (born 1979–1984) cohorts. Section 6 shows that results are robust to excluding imputed observations.

Note that for the baseline sample, I assume individuals lived in rural areas by age 12 and remained in the same county until the time of the survey. However, some individuals might have moved to urban areas within the county, relocated to other counties, or changed their Hukou status. Section 6 tests whether my main findings are robust to these migration-related issues.

3.2 Electricity Prices

I manually collect county-level rural electricity prices before and after TROP primarily from local gazetteers. In China's administrative hierarchy, a prefecture is referred to as a city and governs several counties. In some cases, a city may be designated as a county-level city, which is administratively distinct from a prefecture-level city despite the shared terminology. Local gazetteers (Di Fang Zhi, in Chinese) serve as encyclopedic records, documenting a region's history, economy, governance, and infrastructure. Gazetteers are compiled at the provincial, prefectural, and county levels and are a key source for historical and institutional research. While traditional gazetteers were typically produced through collaboration between local officials and elites, modern versions are compiled by government agencies.

As TROP was announced at the end of 1998 and its first phase ran through approximately 2001, I select gazetteers that cover this period. Most of the gazetteers used were published in the 2000s and 2010s, reflecting variation in publication timing across regions. I focus on records describing the implementation and consequences of TROP. When reporting post-reform prices, these gazetteers typically also cite pre-reform prices, allowing price changes to be directly inferred. Some counties provide detailed documentation that facilitates data collection. For example, the electricity gazetteer of Luoding City (a county-level city in Guangdong Province) reports: "By

[36] To mitigate concerns about migration, my analysis includes only individuals from the 2010 baseline, excluding new respondents added in 2014. Control variables such as gender, ethnicity, and parental education are taken from the 2010 baseline, as they are time-invariant. Treatment assignment (described below) is based on county of residence at age 12. Source: https://www.ndrc.gov.cn/wsdwhfz/202206/t20220628_1328962.html.

2002, rural electricity prices fell from 1.19 CNY per kWh before the 'Two Reforms and One Price' initiative to 0.99 CNY, and then further to 0.79 CNY (the ultimately unified price)."(Luoding City Gazetteer Compilation Commission, 2003) In this case, I use 1.19 CNY and 0.99 CNY as the pre- and post-TROP prices, respectively, as this period aligns with the most intensive phase of implementation (see Section 2.2).[37]

Some counties may not have specific records of TROP, while more information could be found in the prefectural gazetteers. For instance, the electricity prices of Jiangdu City (a county-level city) of Yangzhou Prefecture in Jiangsu Province are collected from the gazetteer of Yangzhou electric power industry, in which page 227 notes that "in July 1998, the average electricity price for rural household lighting in the entire prefecture of Yangzhou was around 1 CNY per kWh. Starting from March 1, 2001, the first batch of three rural power grid renovation counties in Yangzhou, i.e., Jiangdu City, Yizheng City, and Hanjiang County, implemented the same electricity rate for both urban and rural residents, with a price of 0.52 CNY per kWh."(Yangzhou Power Supply Company, 2012) In such cases, 1 and 0.52 are collected as the pre- and post-TROP prices of Jiangdu City.

If county and prefecture records are unavailable, I use information from local newspapers or provincial-level sources. Electricity price data are then matched to CFPS survey sites, resulting in a dataset that covers 142 counties. Among these, 83 counties (58.5%) have data from county or prefectural records, while the remainder use provincial-level sources.[38] Additionally, price data for 4 counties come from county or prefectural newspapers, and for 5 counties from provincial newspapers. Importantly, the price data exhibit substantial within-province variation, as at least one county in each province in the sample is matched to county- or prefectural-level prices. This variation is crucial because the empirical specification includes province $\times$ year fixed effects, which require within-province variation for identification. Appendix Table A3 summarizes the data sources.

To address potential selection bias from using provincial-level prices when county-level data

[37] According to local records, some counties completed price unification slightly earlier or later than the planned 2001 endpoint. For instance, Qiyang County implemented unified rural-urban pricing in 2002. Appendix Section C presents robustness checks using a staggered DiD framework to account for variation in implementation timing across counties.

[38] Potential bias from measurement error is addressed in the robustness checks.

are unavailable, I examine the correlation between provincial-level price use and county socioeconomic characteristics in 1999. Appendix Table A4 shows no significant correlations between provincial-level price use and these characteristics.

Before TROP, some regions made small adjustments to rural electricity prices in certain years, but weak electricity infrastructure and poor management limited their effectiveness, leaving pre-TROP prices largely stable. After TROP, although the central government adjusted electricity prices a few times, such as a 0.025 CNY increase in 2006, these changes were relatively small.[39]

Appendix Table A5 presents correlations between pre-TROP electricity prices and county-level characteristics in 1999.[40] It shows that counties with higher pre-TROP electricity prices tend to have higher rural transmission losses at the provincial level, higher agricultural GDP shares, and larger rural populations. These patterns imply that high prices were concentrated in more underdeveloped counties with weaker infrastructure.

3.3 Regional Data

To check the robustness of my results and explore the economic impact of electrification, I collect prefecture-level indicators—including GDP, foreign direct investment, sectoral output, population composition, and arable land—from the China City Statistical Yearbook (1995–2008).[41] Note that in these yearbooks, "cities" are administratively equivalent to prefectures, each covering several counties. I also gather county-level data on public expenditure and agricultural vs. industrial GDP shares from the National Finance Statistics of Cities, Counties, and Districts (1993–2007).[42]

[39] These adjustments reflected rising input costs and pressure to repay the program's funding. Source: https://www.gov.cn/banshi/2006-06/30/content_324013.htm.

[40] Data on transmission loss is drawn from local gazetteers. Most counties don't have these records. In these cases, I use provincial level records instead. Other county-level indicators are drawn from 1999 National City and County Financial Statistics (Quanguo Di Shi Xian Caizheng Tongji Ziliao, in Chinese). Note that most of these indicators are missing for the pre-treatment period. I use 1999 data as it is closest to TROP implementation and most complete.

[41] Due to data limitations, county-level variables are often unavailable or incomplete. The yearbooks report data from 1994–2007. Source: National Bureau of Statistics of China, Urban Social and Economic Survey and Statistical Division (1995–2008).

[42] Source: Ministry of Finance of the People's Republic of China, Budget Department (1993–2007).

3.4 Summary Statistics

Appendix Table A6 reports summary statistics for electricity prices before and after TROP. On average, rural prices declined by 0.24 CNY, a 29% reduction. The standard deviation of post-TROP prices is half that of pre-TROP prices.[43] To illustrate provincial variation while adhering to CFPS restrictions, Appendix Figure B5 shows a dumbbell plot of pre- and post-TROP prices using data from provincial gazetteers and newspapers.

Three counties experienced small price increases after TROP. Located in Liaoning Province near prefectural centers with high industrial output, these counties had among the lowest pre-TROP prices in the sample. Price increases in these counties may reflect local efforts to recoup infrastructure investments.

Table 1 shows summary statistics for key variables from the 2014 CFPS. Columns (1) and (2) compare counties with small (bottom 25th percentile) and large (top 25th percentile) price reductions. Counties with larger reductions tend to have lower average education levels and a higher share of minority populations.[44] These patterns, consistent with Appendix Table A7, suggest that larger price reductions were concentrated in poorer regions. This reinforces the notion that electricity price changes were not random but correlated with local conditions. However, as discussed in the next section, my identification strategy relies on the parallel trends assumption rather than exogenous price reductions. I provide supporting evidence for this assumption using an event-study framework. Because years of education are bounded above, baseline differences between columns (1) and (2) could mechanically generate an apparent effect even in the absence of a true treatment impact. Section 6 addresses this concern directly. Columns (3) and (4) compare control and treatment cohorts, showing that the latter has higher average years of schooling.

[43] Additional details on price data are omitted to comply with CFPS confidentiality rules.

[44] The robustness checks consider whether differences in baseline education levels pose a concern. Minority populations refer to non-Han ethnic groups, often residing in remote or underdeveloped areas.

Table 1: Summary Statistics for Key Variables

Sample (rural residents)	Counties: small vs. large price reductions		All counties	
	Bottom 25th percentile (1)	Top 25th percentile (2)	Control group (1980–1984) (3)	Treatment group (1988–1994) (4)
Years of education	10.39	8.56	8.38	10.35
	(3.24)	(4.36)	(4.03)	(3.72)
Gender (male = 1)	0.49	0.47	0.47	0.48
	(0.50)	(0.50)	(0.50)	(0.50)
Ethnic (Han = 1)	0.98	0.82	0.88	0.90
	(0.15)	(0.38)	(0.33)	(0.30)
Number of siblings	1.30	1.87	1.87	1.41
	(0.94)	(1.40)	(1.29)	(1.03)
Father's years of education	7.19	5.29	5.82	6.69
	(3.61)	(4.31)	(4.20)	(3.94)
Mother's years of education	5.18	3.34	3.35	4.44
	(4.09)	(3.96)	(3.96)	(4.12)
Observations	1057	845	1742	2408

Note: This table includes only rural residents, defined as those with a rural Hukou at ages 3 and 12. Columns (1) and (2) represent counties that experienced small (bottom 25th percentile) and large (top 25th percentile) price reductions in electricity prices after TROP, respectively. Standard deviations are in parentheses.

4 Treatment Intensity and Empirical Strategy

4.1 Treatment Intensity

This paper uses the absolute reduction in rural electricity prices following TROP as a measure of treatment intensity.[45] Treatment intensity is measured using price reductions for three reasons. First, electricity prices provide the most reliable and consistently measured outcome directly linked to the program. Second, price reductions were explicitly program-induced and varied across counties, generating meaningful cross-sectional variation. Third, and most importantly, price reductions proxy a bundle of reforms: TROP combined tariff reform with infrastructure upgrades

[45]One of TROP's goals was to eliminate the rural–urban electricity price gap. Therefore, observed price reductions are an outcome of the program's implementation. See Appendix Table A6 for summary statistics and Section 3.4 for a discussion of counties with negative price reductions.

and improvements in electricity management.[46] Accordingly, the coefficient on price reductions should be interpreted as capturing the impact of improved electricity delivery, rather than the effect of price changes alone.

Appendix Tables A5 and A7 provide suggestive evidence of the interconnection between weak infrastructure and electricity prices. They show that both electricity prices before TROP and the subsequent reductions are negatively correlated with GDP per capita and the industrial share of GDP, and positively correlated with low-voltage transmission losses (provincial level), the agricultural share of GDP, and the rural population share. These patterns indicate that areas with larger price reductions tended to have weaker infrastructure, which is closely associated with poor electricity delivery, including unstable voltage and unreliable supply.

4.2 Empirical Strategy

Since there are insufficient observations for a given region before and after TROP, this paper uses a cohort difference-in-differences (DiD) approach with continuous treatment to estimate the effects.[47] I compare cohorts who were of primary school age or younger (under 12) at the time of TROP implementation in 1999 to those who were older than middle school age (aged 15 or above) in the same county. To mitigate concerns related to partial exposure, variation in school starting ages, and to allow time for the policy's effects to materialize, I introduce a three-year gap between the two groups.[48] The treatment group includes individuals born between 1988 and 1994, all of whom had at least one year of overlap with TROP (1999) during their primary-school ages. The control group consists of cohorts born between 1979 and 1984, the youngest of whom had already surpassed middle school age. The underlying assumption is that TROP had no significant effect

[46]In China in 1999, as in many low- and middle-income countries today, weak infrastructure, inefficient management, and unaffordable electricity were tightly interconnected (Kojima and Trimble, 2016; World Bank, 2024). As a result, lowering prices alone without infrastructure and management reforms would not have been sustainable (McRae, 2015; Trimble et al., 2016; World Bank, 2024; Meeks and Mahadevan, 2025). This interdependence explains why China's central government implemented the "Two Reforms" together with electricity price equalization.

[47]This methodology follows Duflo (2001), Chen et al. (2020a), and Chen (2025), and aligns with the continuous treatment framework discussed in Callaway, Goodman-Bacon, and Sant'Anna (2024).

[48]For example, if the policy takes a few years to have full effect, the control cohorts were already beyond high school age and therefore unlikely to be affected, assuming the treatment effect operates mainly during primary school age or at most through middle school age. Section 9 provides evidence for this assumption.

on the human capital of the control cohorts. Section 9 provides supporting evidence. Figure 2 illustrates the cohort definitions.

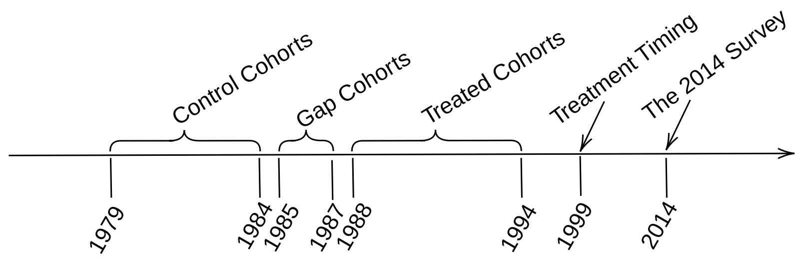

Figure 2: Definition of Cohorts. *Note*: Cohorts born between 1985 and 1987 are classified as "gap cohorts." They are included in the event study but excluded from the baseline regression. To ensure at least one year of overlap with TROP, the oldest cohort in the treatment group is those born in 1988, who were 11 years old in 1999.

Because TROP was implemented nationwide,[49] there is no clean untreated group. Instead, I exploit variation in pre- and post-TROP electricity prices across counties, which led to heterogeneous price reductions after implementation.[50] I compare counties that experienced larger price drops to those with smaller reductions. Because electricity prices are correlated with local characteristics shown in Appendix Table A5, treatment intensity, measured by price reductions, is plausibly endogenous. However, my identification strategy does not require the exogeneity of treatment; it relies on the assumption that, in the absence of TROP, counties with larger and smaller price reductions would have followed similar trends in the post-treatment period. To further address this concern, Section 6 examines whether time-varying unobservables bias the estimates.

The identification relies on two sources of variation: first, cohorts born before 1984 and after 1988 differed in their exposure to TROP during primary school age; second, counties experienced varying magnitudes of electricity price reductions.

[49] Although implementation timing may have varied slightly across regions, the program was broadly simultaneous. Appendix Section C presents robustness checks using a staggered DiD framework.

[50] Post-TROP prices show significantly less variation than pre-TROP prices. See Appendix Table A6.

4.3 Setup for Event Study

Although the parallel trends assumption cannot be tested directly because it involves a counterfactual: what treated counties would have looked like in the absence of treatment, one can assess its empirical plausibility by studying the pre-treatment period using an event study framework. Eq. 1 presents the event study specification. As noted earlier, cohorts born between 1985 and 1987 are classified as "gap cohorts" due to concerns such as variation in school starting ages. These cohorts are included in the event study to assess trends but are excluded from the baseline regression. The 1984 birth cohort serves as the reference group in Eq. 1.

$$\begin{aligned} Y_{i,t,c} =& \alpha + \sum_{\lambda=1979}^{1983} \beta_\lambda \times Price\_Reduction_c \times \mathbf{1}\{t = \lambda\} \\ &+ \sum_{\lambda=1985}^{1994} \beta_\lambda \times Price\_Reduction_c \times \mathbf{1}\{t = \lambda\} + \varphi X_{i,t,c} \\ &+ \gamma_c + \mu_{prov} \times \tau_t + \varepsilon_{i,t,c} \end{aligned} \tag{1}$$

$Y_{i,t,c}$ denotes an outcome of interest, such as years of education, for an individual i born in year t and residing in county c. $Price\_Reduction_c$ measures treatment intensity, defined as the reduction in electricity prices following TROP implementation. The function $\mathbf{1}\{t = \lambda\}$ is an indicator that equals 1 when an individual's birth year t matches the specified parameter λ. X includes individual-level controls: ethnicity, gender, number of siblings, and parental education. γ_c captures county fixed effects.

To address potential policy confounders during the study period—such as the staggered implementation of the 1986 Compulsory Education Law across provinces (Chen and Park, 2021)–I incorporate province $\times$ birth year fixed effects ($\mu_{prov} \times \tau_t$) in the model.[51] As previously noted, 44% of the counties in my sample use provincial-level price data, and adding these interaction terms takes away all provincial-level variation. Despite this, my paper opts to include these terms for two reasons: first, my price reduction data have within-province variation, as not all coun-

[51] Another policy concern is the provincial-level rural tax and fee reform pilot, launched in 2002 to ease farmers' financial burdens and improve the rural economy.

ties in a province use provincial-level data; second, these terms effectively control for potential time-varying confounding factors at the provincial level.

While this paper makes efforts to control for varying trends, some unobservables, such as within-province differences in the implementation of China's Compulsory Education Law in the 1980s, may still affect the estimates. To address this concern, I conduct a series of robustness checks in Section 6. The error term is denoted as ε, with standard errors clustered at the county level.

4.4 Baseline Model

To estimate the causal effect of rural electrification on human capital, I adopt a cohort-DiD approach. I compare cohorts who were of primary school age (ages 6–11) when TROP was implemented to those who were of senior high school age or older (15+) in the same county. I exploit variation from county-level differences in electricity price reductions to identify the effects. The baseline estimation equation is given by:

$$\begin{aligned} Y_{i,t,c} =& \alpha + \beta \times Price\_Reduction_c \times \mathbf{1}\left\{1988 \leq t \leq 1994\right\} + \varphi X_{i,t,c} \\ &+ \gamma_c + \mu_{prov} \times \tau_t + \varepsilon_{i,t,c} \end{aligned} \tag{2}$$

All notation in Equation 2 is consistent with that in Equation 1. The coefficient of interest, β, captures the differential effect of a one-unit increase in treatment intensity on the outcomes of the treated cohorts relative to the control group. As mentioned earlier, the gap cohorts born between 1985 and 1987 are excluded from the baseline regression.

5 Empirical results

5.1 Results of Event Study

Figure 3 presents the event study results, with the 1984 cohort serving as the reference. Cohorts born before 1984 show similar pre-treatment trends, supporting the parallel trends assumption. However, as Roth (2022) cautions, event studies may lack sufficient power to detect violations

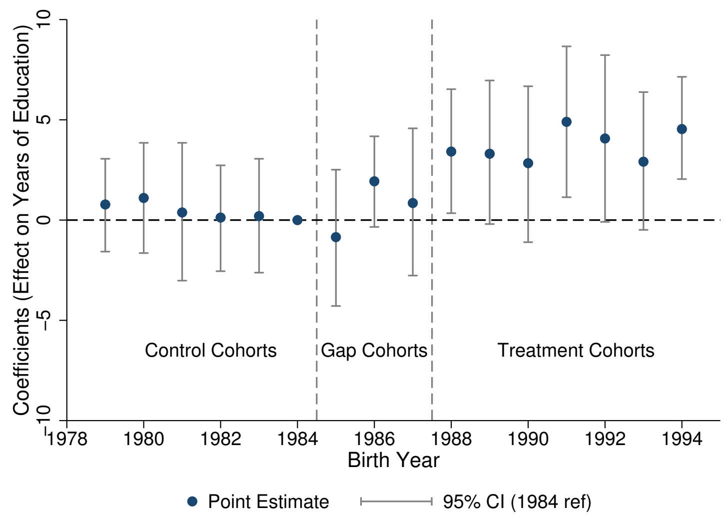

Figure 3: Event Study Results. *Note*: The y-axis represents the coefficients estimated from Eq. 1. The "gap cohorts," shown between the two dashed lines, are included in the event study but excluded from the baseline regression.

of parallel trends, even when pre-trends appear statistically insignificant. Following the author's recommended procedure, I conduct a power analysis for my setting. Assuming 50% power (the probability of detecting a significant pre-trend under a hypothesized trend), the analysis yields a Bayes factor of 0.61 and a likelihood ratio of 0.03, both well below 1.[52] These results indicate that the observed insignificant pre-trend in Figure 3 provides strong support for the parallel trends assumption.

5.2 Results of Baseline Specification

In the baseline specification, I compare cohorts born between 1988 and 1994 with those who were born between 1979 and 1984. Column (1) of Table 2 reports the baseline results: a one standard deviation increase in treatment intensity (0.21 CNY, about 25% of the average pre-TROP price) raises educational attainment by 0.605 years (0.21×2.879).[53] This effect is large—more than six

[52]The Bayes factor is the ratio of the probability of "passing" the pretest under the hypothesized trend relative to under parallel trends. The likelihood ratio compares the likelihood of the observed coefficients under the hypothesized trend versus under parallel trends. See https://github.com/mcaceresb/stata-pretrends?tab=readme-ov-file#pretrends for implementation details.

[53]This effect size is comparable to Lipscomb, Mobarak, and Barham (2013), which shows that a one standard deviation increase in treatment intensity due to grid expansion raises educational attainment by 0.667 years (0.33×2.022).

times the impact of China's Send-Down Movement in the 1960s–70s (Chen et al., 2020a),[54] and more than twice the effect of China's 1986 Compulsory Education Law (Chen and Park, 2021).[55] At the aggregate level, about 74 million rural children were of primary school age when TROP began in 1999. A one standard deviation increase in treatment intensity therefore implies roughly 45 (74 × 0.605) million additional years of schooling. Because TROP was permanent, this corresponds to a sustained annual gain of about 7 (12 × 0.605) million person-years of schooling in rural areas after 1999.

Columns (2) and (3) of Table 2 examine the effects on cognitive ability, measured by standardized math and Chinese word recognition test scores from the 2014 CFPS.[56] To ensure comparability, I standardize the original scores into z-scores.[57] The results indicate that a one standard deviation increase in treatment intensity raises math and word test scores by 0.164 (0.21 × 0.78) and 0.142 (0.21 × 0.676) standard deviations, respectively.

Columns (4)–(6) of Table 2 use primary and secondary school completion as alternative educational outcomes. The results show that a one standard deviation increase in treatment intensity raises primary school graduation rates by 3.7 percentage points (0.21 × 0.176). The same increase raises junior high and senior high school graduation rates by 5.12 (0.21 × 0.244) and 6.17 (0.21 × 0.294) percentage points, respectively.

The final two columns of Table 2 present falsification tests, based on the premise that TROP primarily targeted rural areas and had limited direct impact on urban households.[58] If this holds, TROP should not significantly affect urban counterparts of the treated rural cohorts. I conduct these tests using urban residents from the same counties as the rural sample, assigning them the same treatment intensity based on rural electricity price reductions. Column (7) of Table 2 examines

[54]Effect sizes correspond to a one-standard-deviation change. Calculations are based on data from Chen et al. (2020b). The Send-Down Movement (1968–1978) was a campaign during China's Cultural Revolution where over 17 million urban youths were sent to rural areas to work and learn from peasants.

[55]Treatment in Chen and Park (2021) is binary.

[56]The CFPS math and word tests are based on the Guttman Scale in psychometrics (Guttman, 1944). More details are available on the CFPS website: https://www.isss.pku.edu.cn/cfps/cjwt/cfpsxkt/1295348.htm.

[57]z-score=(x-μ)/δ, where x represents the value being evaluated, μ is the mean, and δ is the standard deviation.

[58]Anecdotal evidence suggests that urban electricity prices might have risen slightly in some regions to offset rural price reductions, but such instances appear limited and economically insignificant—widespread increases would likely have prompted public complaints from urban residents.

individuals with urban Hukou at both ages 3 and 12. The effect is statistically insignificant, though the sample size is relatively small. Column (8) focuses on individuals residing in the urban area of their birth county at the time of the survey, including those with longstanding urban Hukou and those who acquired it later, typically through the reclassification of suburban areas. These individuals likely had consistent access to urban electricity infrastructure. The effect is again small and statistically insignificant.

Table 2: Results of Baseline Specification and Falsification Tests (2014 CFPS)

Dependent var:	(1) Years of education	(2) Math test (z-score)	(3) Word test (z-score)	(4) Complete primary (edu≥6)[a]	(5) Complete junior (edu≥9)[a]	(6) Complete senior (edu≥12)[a]	(7) Falsification (urban Hukou at ages 3 and 12)	(8) Falsification (urban residence at survey)
Price reduction × affected cohorts (1988–1994)	2.879***	0.780***	0.676***	0.176***	0.244***	0.294**	-0.149	-0.034
	(0.716)	(0.175)	(0.142)	(0.041)	(0.083)	(0.135)	(0.913)	(1.164)
Observations	4,145	4,144	4,144	4,145	4,145	4,145	647	1,510
R-squared	0.438	0.384	0.395	0.388	0.329	0.304	0.442	0.404
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province × birth year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep var	9.525	0	0	0.913	0.742	0.399	13.07	11.54

Note: Columns (1) present the baseline results. Columns (2) and (3) use cognitive test scores from the 2014 CFPS as dependent variables. "Word test" refers to Chinese word test. Columns (4)–(6) examine the completion of primary and secondary school as alternative outcome variables. College completion is excluded, as many in the treatment group were still of college age in 2014. Columns (7) and (8) present falsification tests: column (7) includes individuals with urban Hukou at both ages 3 and 12, while column (8) includes those residing in the urban area of their birth county at the time of the survey. Columns (7) and (8) use urban residents from the same counties as the rural sample and assign treatment intensity based on rural electricity price reductions. Standard errors are clustered at the county level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

[a] "edu" represents years of education.

Figure 4 plots the estimated effects of TROP exposure during primary school age on alternative outcome variables: completion rates by grade. The results show that the largest gains occur between Grades 7 and 12, corresponding to junior and senior high school. This pattern suggests that children exposed to lower treatment intensity during middle childhood were more likely to remain in school through later stages. The effects on earlier grades are smaller, likely because most children complete primary school regardless of treatment, while effects on college (Grade 13) are imprecisely estimated due to low baseline enrollment and high opportunity costs.

The jump at Grade 7 is particularly noteworthy. There are at least two reasons. First, secondary school tuition is higher than primary school, so increases in household income can substantially influence decisions about human capital investment at this transition. Second, rural electrification may enhance not only the quantity but also the quality of education—children in primary school may perform better, which encourages parents to continue investing in their schooling after graduation.

Note that the results here reflect primary-school-age exposure and subsequent educational progression. For these cohorts, early improvements in learning and human capital may accumulate over time, leading to larger effects at later schooling stages. As a result, gains in junior and senior high school completion can exceed those observed at earlier grades.

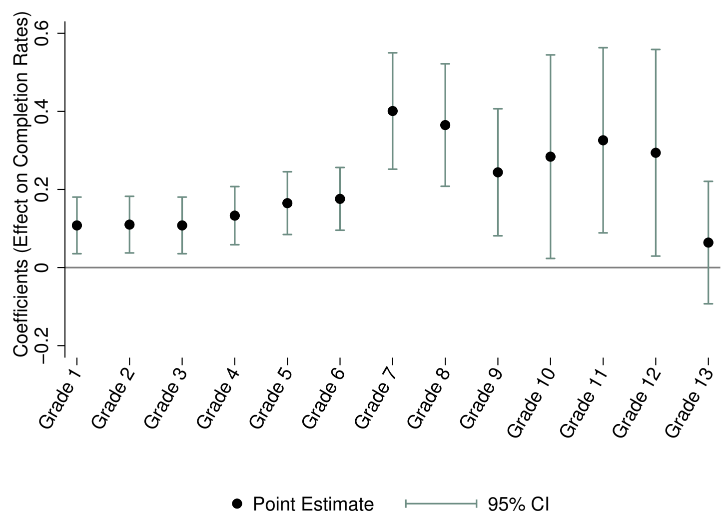

Figure 4: Effects of Treatment on Completion Rates by Grade. *Note*: This figure shows the effects of middle childhood exposure on completion through primary, junior high, senior high, and the first year of college. Estimates are from regressions analogous to the baseline specification but using binary indicators for each grade level as outcomes. China's 6-3-3 system includes six years of primary, three years of junior high, and three years of senior high education.

Note that control cohorts were also exposed to TROP, but only from senior high school age onward. This paper shows that such late exposure has little effect on human capital, as discussed in Section 9. Section 6 also examines whether variation in exposure length during primary school age among treatment cohorts biases the estimates.

6 Robustness

This section demonstrates the robustness of the key result in column (1) of Table 2 through a series of checks. These include: (1) rural school consolidation around 2000; (2) China's accession to the WTO in 2001; (3) differences in baseline socioeconomic characteristics and heterogeneous trends; (4) higher education expansion; (5) migration; and (6) issues related to continuous treatment, alternative treatment measures, and staggered DiD settings. It also addresses potential biases related to electricity price data availability, exposure duration, treatment timing, and missing observations.

6.1 School Consolidation

Starting in the early 2000s, China launched a rural school consolidation initiative, which primarily merged small village primary schools into larger, centralized ones.[59] The impacts of consolidation on education are mixed (Hannum, Liu, and Wang, 2021; Hannum and Wang, 2022), and treated cohorts in my sample might be affected.

I measure consolidation intensity using the ratio of primary schools in 2000 to 2007 at the prefectural level (from the China City Statistical Yearbook). I interact this measure with the treatment cohort dummy and add it to the baseline model. Column (1) of Table 3 shows that this adjustment does not substantially change the coefficient of interest.

Additionally, CFPS asks whether a primary school exists near an individual's village (community). If school consolidation confounded the impact of TROP, controlling for this variable should significantly affect the results. Column (2) of Table 3 shows that the coefficient of interest remains consistent, further suggesting that my findings are not driven by school consolidation.

[59]In the 1980s and 1990s, village-run schools played a key role in expanding access to basic education. However, as quality concerns grew and rural enrollment declined, the government began consolidating schools. Between 2000 and 2005, the number of primary schools dropped by about 34%, and junior high schools by about 1.3%. Source: China Youth Daily. https://zqb.cyol.com/html/2015-09/14/nw.D110000zgqnb_20150914_1-10.htm.

6.2 WTO Accession

China's WTO accession in 2001 spurred export growth and foreign direct investment (FDI), which may influence household education decisions (Erten and Leight, 2021). To account for this, I measure FDI intensity as the ratio of prefecture-level FDI in 2007 to that in 2000 and interact it with the treatment group indicator. Column (3) of Table 3 shows that controlling for this factor does not significantly affect the main result.

6.3 Baseline Socioeconomic Characteristics and Heterogeneous Trends

Appendix Tables A5 and A7 show that price reductions correlate with baseline (1999) socioeconomic characteristics at the county level. If trends in these characteristics evolve differently due to TROP or other contemporaneous policy changes across counties, my estimates would be biased. This concern is particularly salient given policies like the Nine-Year Compulsory Education Campaign (1998–2000), which targeted underdeveloped counties to increase basic education (Zhang, 2026). To address this, I interact county-level socioeconomic conditions in 1999—including agricultural GDP share, public education expenditure per capita, and rural population share—with birth year fixed effects and add these to the regression model (Eq. 2). Column (4) of Table 3 reports the results. The coefficient of interest is smaller but remains comparable to the baseline estimate in column (1) of Table 2. The standard error and R-squared do not differ significantly from those in column (1) of Table 2, indicating that these interaction terms do not substantially increase explanatory power but reduce variation in the independent variable.

Although my paper controls for province × birth year fixed effects to account for the implementation of the Compulsory Education Law in the 1980s, within-province variation in implementation timing remains. To further address this concern, I add county baseline education × birth year fixed effects to the model in addition to the socioeconomic indicators. Baseline education is defined as the average years of education among rural cohorts born between 1970 and 1978 in each county, using data from the 2000 China Census.[60] This measure proxies for local implementation discrep-

[60] The 2000 China census data is obtained from the Integrated Public Use Microdata Series (IPUMS) (Minnesota

ancies, which correlate with pre-TROP electricity prices as shown in Appendix Table A5. Column (5) of Table 3 reports the results. The coefficient of interest remains comparable to the baseline estimate.

Appendix Figure B6 presents the corresponding event study results after controlling for these interaction terms. The results continue to support the parallel trends assumption. Nevertheless, if additional time-varying unobservables correlate with price reductions but are not captured by the interaction terms—or if such trends are nonlinear—the estimates may still be subject to some bias.

6.4 Baseline Education Differences

The first two columns of Table 1 show that counties with larger price reductions have lower average education levels. Because years of education are bounded at the top, such baseline differences could mechanically generate an apparent effect even in the absence of a true treatment impact. To address this concern, I apply Coarsened Exact Matching (CEM) to balance high- and low-treatment counties by baseline education. CEM trims and reweights the sample so that the two groups are comparable in their baseline education distribution. Columns (1) and (2) of Appendix Table A8 report the results based on CEM using different baseline cohorts, while column (3) excludes counties in the lowest 25th percentile of treatment intensity (price reduction), which tend to have higher baseline education levels. The baseline result remains robust across these specifications.

6.5 Higher Education Expansion

China's higher education expanded rapidly in the late 1990s to stimulate the economy. The gross college enrollment rate among 18–22 year-olds increased from 9.8% in 1998 to 24.2% in 2009 (Che and Zhang, 2018). This expansion might change education return expectations, confounding TROP's impact on children's education.

To address this, I construct a prefectural-level measure of higher education expansion as the ratio of students enrolled in 2007 to those in 1999 and interact it with the treatment group indi-

Population Center, 2020).

Table 3: Robustness to Confounding Factors

Dependent var:	Years of education				
	(1)	(2)	(3)	(4)	(5)
Robustness check:	School consolidation	School nearby	WTO accession	Baseline econ trends	Baseline econ + base edu
Price reduction × affected cohorts (1988–1994)	2.869***	2.925***	2.8820***	2.342***	2.356***
	(0.711)	(0.737)	(0.719)	(0.705)	(0.689)
Consolidation intensity × affected cohorts (1988–1994)	0.004				
	(0.010)				
FDI intensity × affected cohorts (1988–1994)			-0.0007		
			(0.0064)		
Observations	4,145	4,061	4,145	3,785	3,620
R-squared	0.438	0.439	0.438	0.455	0.457
County FE	Yes	Yes	Yes	Yes	Yes
Province × birth year FE	Yes	Yes	Yes	Yes	Yes
Baseline econ [a] × birth year FE	No	No	No	Yes	Yes
Baseline econ[a] + base edu[b] × birth year FE	No	No	No	No	Yes
Mean of dep var	9.525	9.539	9.525	9.503	9.403

Note: Column (1) addresses China's rural school consolidation initiative around 2000, which primarily involved merging primary schools in rural and remote areas. Column (2) indirectly controls for the impact of this initiative by including a control for whether a primary school was located near the respondent's village (or community). Column (3) accounts for China's WTO accession by controlling for changes in foreign direct investment. Column (4) includes interactions between 1999 socioeconomic characteristics and birth year fixed effects. Column (5) additionally includes baseline education by birth year fixed effects. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

[a] "Baseline econ" indicates county-level economic indicators in 1999.

[b] Base education is measured as the county-level average years of education among individuals born between 1970 and 1978, calculated using the 2000 census.

cator and a dummy for birth years 1982–1984.[61] Treatment cohorts (1988–1994) should be more affected, as they made college decisions during the expansion, while 1982–1984 cohorts were already in high school when it began.

Column (4) of Appendix Table A8 shows treatment cohorts are indeed more affected by the expansion. The coefficient of interest is positive and significant but larger than the baseline estimate due to sample size reduction from missing enrollment data. Column (5) confirms this by

[61] Although higher education institutions enroll students nationwide, significant quotas are given to local residents.

running the baseline specification on the same restricted sample. These results indicate that higher education expansion in late 1990s China does not drive my main findings.

6.6 Migration

Migration presents another potential concern. The substantial urban-rural divide in China creates strong incentives for labor migration from rural to urban areas, particularly among more educated individuals (Zhao, 1997). Such migration could result in selective attrition, creating a rural brain drain. To alleviate this concern, the baseline sample includes only individuals with rural Hukou during middle childhood, rather than relying on their current Hukou status. However, some individuals may have moved after TROP implementation or lived elsewhere at the time of the survey. To mitigate these concerns, I use additional information on residence at age 12, residence at the time of the survey, and Hukou status at the time of the survey to test whether stricter sample restrictions affect the results.

Columns (1) to (3) of Table 4 implement incremental restrictions. In addition to the baseline requirement of rural Hukou at ages 3 and 12,[62] column (1) restricts the sample to those who also lived in their birthplace at age 12. Column (2) further limits the sample to those still living in their birthplace at the time of the survey. Column (3) adds a third condition: holding rural Hukou at the time of the survey. Across these increasingly restrictive samples, the estimated effects remain consistent, suggesting that migration does not substantially bias the main results.

Since 2000, an increasing number of suburban counties in China have been reclassified as districts. Compared to regular counties, districts generally exhibit higher urbanization and stronger economic development. Rural Hukou holders at ages 3 and 12 in these areas—many of which were formerly suburban counties—may be more likely to migrate to urban centers. To address this concern, column (4) of Table 4 drops all county-level districts, regardless of when they were designated. The coefficient of interest is smaller than in the baseline specification, likely due to reduced sample size, but the difference is not substantial. The findings remain robust.

[62]In the baseline regression, my sample includes only individuals with rural Hukou at both ages 3 and 12.

Table 4: Migration

Dependent var:	Years of education			
Robustness check:	(1) Live in birthplace at age 12	(2) (1) + live in birthplace at survey	(3) (2) + rural Hukou at survey	(4) Drop county-level districts
Price reduction × affected cohorts (1988–1994)	2.932*** (0.722)	2.925*** (0.654)	3.154*** (0.745)	2.701*** (0.714)
Observations	4,011	3,469	3,228	3,116
R-squared	0.442	0.457	0.485	0.467
County FE	Yes	Yes	Yes	Yes
Province × birth year FE	Yes	Yes	Yes	Yes
Mean of dep var	9.513	9.519	9.295	9.217

Note: In addition to the Hukou restriction at ages 3 and 12 used in the baseline regression, columns (1) to (3) apply increasingly strict sample restrictions. Column (1) limits the sample to individuals who lived in their birthplace at age 12. Column (2) further restricts the sample to those still living in their birthplace at the time of the survey. Column (3) adds an additional condition, keeping only individuals who also held rural Hukou at the time of the survey. Column (4) drops all county-level districts. Standard errors are clustered at the county level. *** p<0.01, ** p<0.05, * p<0.1.

6.7 Alternative Treatment Measure and Treatment Timing

Issues with Continuous Treatment. This paper uses a DiD strategy with continuous treatment to estimate the effects. However, Callaway, Goodman-Bacon, and Sant'Anna (2024) cautions that a two-way fixed-effects (TWFE) regression with continuous treatment does not identify a clean average causal effect unless very strong assumptions hold. Following the authors' recommended procedure, I discretize the treatment measure (price reduction) into five categories based on quintiles and interact the category dummies with the dummy indicating treated cohorts. The model specification is shown in Eq. C1.

The omitted group consists of individuals in the lowest-intensity counties (first quintile). As shown in Appendix Figure B7, I find no statistically significant gains in educational attainment in counties with modest price reductions (second and third quintiles). In contrast, exposure in counties experiencing large price reductions yields substantial and statistically significant effects: individuals in the fourth quintile gain 1.20 additional years of schooling, while those in the highest quintile gain 1.65 years. This monotonic and nonlinear pattern indicates that human capital gains arise only when treatment intensity is sufficiently large. Column (1) of Appendix Table C1 shows the details.

Alternative Treatment Measures. Local governments may lack the ability to implement the program, resulting in endogenous price reduction. Additionally, they may exaggerate price reduction levels for political promotion motives. To mitigate these concerns, I use the pre-TROP price as the treatment measure, which is predetermined and less likely to be endogenous than price reduction. Column (2) of Appendix Table C1 shows the results. The coefficient of interest remains statistically significant, with an effect size smaller but comparable to the baseline estimate.

My paper also considers (1) the price reduction ratio as an alternative treatment measure, (2) the choice of timing for post-TROP electricity prices, (3) staggered DiD settings, and (4) calculating price reductions using the unified price as the post-TROP benchmark. As shown in Appendix Table C1, the findings remain robust. Further details are provided in Appendix Section C.

6.8 Additional Robustness Checks

In addition to the checks discussed above, I also examine: (1) potential bias from measurement error due to the use of prefectural or provincial electricity prices when county-level data are unavailable; (2) variation in lengths of exposure; and (3) the impact of missing observations in the 2014 survey. Appendix Table C2 presents the results. My findings remain robust across these alternative specifications. Further details are provided in Appendix Section C.

7 Heterogeneity

I explore heterogeneity in the effects of TROP across household and regional characteristics. The program increased educational attainment for both boys and girls, with larger gains for males (marginally significant), likely reflecting rural preferences for boys and the limited availability of household appliances that reduce girls' domestic workload. The effects are also stronger in drier regions and in counties more dependent on agriculture, suggesting that improved agricultural productivity, such as through irrigation, is an important channel. By contrast, variation in hydropower potential across provinces or family size does not appear to meaningfully alter the results. Overall, these patterns indicate that the educational benefits of rural electrification are closely linked to local agricultural conditions. Further details are provided in Appendix Section D. These findings motivate a closer examination of the agricultural income channel as a potential mechanism in the next section.

8 Mechanisms

This section investigates potential mechanisms through which TROP affected human capital. Existing literature suggests that rural electrification increases crop yields, improves productivity, and supports agricultural expansion (Kitchens and Fishback, 2015; Assunção et al., 2017; Lewis and Severnini, 2020; Fried and Lagakos, 2021). If electrification operates as a productivity shock in agriculture, it should raise agricultural productivity and household agricultural income. Higher income may then relax household budget constraints and increase investment in children's education, given the central role of agriculture in rural livelihoods in 1990s China.[63] I first test this mechanism by documenting prefectural-level changes in agricultural productivity.

If higher agricultural productivity translates into income gains, child health should improve among cohorts born after the program, given the close link between household income and child health. I present evidence consistent with this prediction. This evidence is interpreted as suggestive, as it reflects improvements in health outcomes rather than direct measures of household income.

Since electricity is also a critical input for educational infrastructure, I also examine whether TROP increased public investment in education as an additional mechanism. Finally, rural electrification may also improve educational outcomes by extending study hours through improved lighting. I investigate this channel by exploiting variation in daylight duration.

[63] In the late 1990s, about 70% of China's population was rural, and agriculture accounted for roughly 17% of GDP, compared with a global average of about 4%. Source: World Bank DataBank.

8.1 Rural Electrification and Enhanced Agricultural Productivity

Direct Evidence

To examine TROP's impact on agricultural productivity, I collect data on electricity prices, agricultural GDP,[64] industrial GDP, and arable land area at the prefectural level for the period 1994–2007.[65] Columns (1) and (2) of Table 5 show the impact of TROP on agricultural productivity, measured by agricultural GDP per unit of arable land.[66] Column (2) includes weather controls; column (1) does not. As shown in column (2), higher treatment intensity significantly increases agricultural productivity (measured by agricultural GDP per hectare). A one standard deviation increase in treatment intensity raises agricultural productivity by 2,037 CNY per hectare (0.21 $\times$ 0.97 $\times$ 10^7/1,000), or approximately 240 USD in 2000. This is equivalent to 824 CNY—or about 97 USD—per acre. The effect size is substantial: it represents a 12.7% increase (or 280 CNY) in the annual per capita net income of rural households in 1999.[67] For context, annual tuition in 2000 was about 200 CNY for primary school and 400 CNY for middle school (Liu, 2000).

A valid concern is that the observed increase in agricultural productivity may be driven by a decline in arable land rather than a rise in agricultural output. Columns (3) and (4) of Table 5 report the effects of TROP on agricultural GDP and arable land area, respectively. The results indicate that the productivity gains are driven by higher agricultural output, not by reductions in cultivated land. Column (5) reports TROP's effect on the agricultural share of GDP; higher treatment intensity significantly increases this share, further supporting the agricultural productivity channel.

Another concern is that TROP might stimulate industrial development, creating additional opportunities for parents or children and confounding the agricultural channel. To address this con-

[64]In China's statistical classification, the broader definition of agriculture refers to the primary industry, encompassing farming, forestry, fishing, and animal husbandry. Electricity is vital to these sectors, enabling production, processing, and modernization.

[65]For prefectures where electricity price data are unavailable, I use provincial-level data instead.

[66]GDP is reported in 10 million CNY, and one unit of arable land equals 1,000 hectares.

[67]In 1999, the average annual per capita net income of rural households in China was 2,210 CNY ($\approx$ 265 USD in 2000). Source: The National Bureau of Statistics of China. https://www.stats.gov.cn/sj/ndsj/zgnj/2000/J16c.htm. In the same year, the national average per capita arable land operated by rural households was 2.07 mu (China's metric, where 1 mu is equal to 0.165 acres), equivalent to 0.34 acres. Source: https://www.stats.gov.cn/sj/ndsj/zgnj/2000/L13c.htm.

cern, column (6) reports the effect on the industrial GDP share, which is statistically insignificant. This finding is consistent with the fact that TROP primarily targeted rural areas.

These results provide strong support for the mechanism that TROP enhances children's human capital by improving agricultural productivity. The findings also align with existing literature mentioned above. I next present suggestive evidence related to this mechanism.

Table 5: TROP's Impact on Agricultural Productivity at the Prefectural Level

Mechanism	Boost agricultural productivity					
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent var:	Agricultural GDP per unit arable land	Agricultural GDP per unit arable land	Agricultural GDP	Areas of arable land	Agricultural GDP share	Industrial GDP share
Price reduction	1.103**	0.970**	470.380***	89.872	0.093*	-0.040
× after TROP	(0.431)	(0.435)	(169.562)	(106.535)	(0.054)	(0.043)
Observations	1,475	1,461	1,627	1,463	1,502	1,502
R-squared	0.87	0.871	0.852	0.888	0.806	0.849
Controls	No	Yes	Yes	Yes	Yes	Yes
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep var	2.592	2.592	600.3	341.1	0.194	0.456

Note: Sample period: 1994–2007. GDP is in 10 million CNY, and one unit of arable land equals 1,000 hectares or 2,471 acres. In addition to fixed effects, columns (2) – (6) control for prefectural annual precipitation and average temperature. Standard errors are clustered at the prefectural level. *** p<0.01, ** p<0.05, * p<0.1.

Suggestive Evidence

It's well established that household income is positively correlated with children's health (e.g., Case, Lubotsky, and Paxson, 2002; Currie and Stabile, 2003; Condliffe and Link, 2008). If higher agricultural productivity translates into greater agricultural income, we should also expect improvements in children's nutrition and health, at least in the short term. While direct data on short-run nutrition and health status for individuals in my main sample are unavailable, the 2010 CFPS includes information on birth weight and hospital visits in the previous year for children under age 16.[68] I use this child sample to examine whether TROP improved birth weight and reduced

[68]Note that the baseline analysis in this paper focuses on adults, defined by CFPS as individuals aged 16 or older.

hospital visits, providing suggestive evidence of an income effect.[69]

Specifically, I compare children born between 1999 and 2005 to those born between 1995 and 1998. Columns (1) and (2) of Table 6 present the results. TROP significantly increased birth weight and reduced hospital visits during the year prior to the 2010 survey. A one standard deviation increase in treatment intensity raises birth weight by 39 grams (0.21 $\times$ 0.187 $\times$ 1000), and a decrease in the probability of visiting hospital at least once last year by 3.8 (0.21 $\times$ -0.18) percentage points.

These results are consistent with an income-driven improvement in child health. Alternatively, health gains may reflect improved access to health-related infrastructure, such as heating or cooling. While this channel cannot be separately identified, it is plausibly linked to household income, as higher income relaxes constraints on adopting such improvements. Taken together with the direct evidence above, they support the mechanism that TROP enhances children's human capital by boosting agricultural productivity and, in turn, raising rural household income. As mentioned earlier, household income is a key determinant of educational attainment.

8.2 Rural Electrification and Increased Public Educational Investment

Electricity is essential for operating schools effectively, yet unreliable or unavailable electricity is a common issue in developing countries (Sovacool and Vera, 2014). By improving electricity delivery, TROP might facilitate the use of lighting, fans, and educational equipment, thereby encouraging local governments to allocate more resources to education. Note that nearly all primary schools in China are publicly funded and administered by local governments.

To investigate whether TROP encouraged public educational investment, I collect county-level fiscal data from 1993 to 2007. The available data on educational expenditure covers both urban and rural schools; separate figures for rural schools are unavailable. Columns (3) and (4) of Table 6 present the results. Column (3) includes all counties in the sample, while column (4) excludes county-level districts, which typically have a higher concentration of urban schools.[70] If TROP

[69]Higher household income may enhance maternal health and parental care, both of which are important determinants of child health (Currie and Cole, 1993; Warner, 1995).

[70]Since the education expenditure data are aggregated across urban and rural schools, excluding districts likely

Table 6: TROP's Impact on Birth Weight, Hospital Visits, Public Expenditure on Education, and Lighting

Mechanism	Agricultural productivity	Agricultural productivity	Public edu expenditure	Public edu expenditure	Lighting
	(1)	(2)	(3)	(4)	(5)
Dependent var:	Birth weight (kg)	Hospital visits (at least once=1)	Public edu expenditure share	Public edu expenditure (drop districts)	Years of Edu
Price reduction × affected cohorts (1999–2005)	0.187***	-0.180***			
	(0.068)	(0.054)			
Price reduction × after TROP			0.019*	0.020**	
			(0.010)	(0.010)	
Price reduction × affected cohorts (1988–1994)					3.099***
					(1.013)
Price reduction × affected cohorts (1988–1994) × daylight duration					-0.263
					(0.860)
Observations	2,221	2,815	1,472	1,287	4145
R-squared	0.256	0.283	0.83	0.811	0.438
County FE	Yes	Yes	Yes	Yes	Yes
Year FE	–	–	Yes	Yes	–
Province × birth year FE	Yes	Yes	No	No	Yes
Mean of dep var	3.169	0.434	0.266	0.270	9.525

Note: Columns (1) and (2) report the impact on birth weight and hospital visits for cohorts born between 1995 and 2005 from the 2010 wave children sample, which differs from the baseline. Discrepancy in sample size is due to missing values. Columns (3) and (4) present the results of the impact on public expenditure share on education. The first two columns control for gender, parents' ethnicity, parents' ages, and parents' education. Column (3) includes all counties in my sample, while column (4) drops all county-level districts as they have larger portion of urban schools. Both columns (3) and (4) don't include province × birth year fixed effects but control for rural population and the ratio of agricultural GDP and industrial GDP. Column (5) examines whether TROP improved lighting conditions by exploiting variation in daylight hours. Standard errors in columns (1), (2), and (5) are clustered at the county level. Standard errors in columns (3) and (4) are clustered at the provincial level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

truly affected public investment in rural education, the estimate excluding county-level districts should be more precise.

The results confirm this. Column (3) shows that TROP significantly increased the share of public expenditure allocated to education, and column (4) provides a more precise estimate. In column (4), a one standard deviation increase in treatment intensity raises the education share of

improves the precision of the estimate. Additionally, columns (3) and (4) do not include province × birth year fixed effects but instead control for rural population and the ratios of agricultural and industrial GDP.

public spending by 0.42 percentage points (0.21 $\times$ 0.02). The effect is modest in absolute size, but meaningful given the large base of public educational spending and the relatively small share typically devoted to electricity-related infrastructure. Since the data include urban schools, the true effect on rural schools may be even larger.

8.3 Rural Electrification and Improved Lighting

Rural electrification can also increase education by improving lighting, thereby extending study hours for children. To examine this channel, I collect daily sunrise and sunset timing in 2000 and calculate the daily average daylight duration for my sample counties.[71] Summer and winter vacation periods are dropped, as students have limited coursework during these times.[72] If the improved lighting channel operates, we would expect counties with shorter daylight duration to show larger human capital gains.

Column (5) of Table 6 presents the results. Counties with shorter daylight duration show no significantly different gains compared to those with longer duration, providing no support for the lighting channel. This may be because lighting is more closely tied to electrification at the extensive margin (grid connection) than to the intensive margin (electricity delivery quality) studied here.

9 Are Older Children Valid Comparison?

A key assumption underlying my identification strategy is that children who were exposed to the program only after primary school age—hereafter referred to as older children—are not significantly affected by the program, as they serve as the control group. To assess the validity of this assumption, I conduct an event study and a cohort difference-in-differences (DiD) regression analogous to the baseline specification, focusing on cohorts who were of secondary school age or older when TROP was implemented in 1999. The treatment group consists of cohorts born between 1982 and 1987, who were of secondary school age at the time of implementation, while the control

[71] Source: U.S. Navy. https://aa.usno.navy.mil/data/Dur_OneYear. I use the daylight duration at a county centroid to represent the whole county.

[72] In China, summer vacations are typically in June and August, while winter vacations are usually between mid-January and mid-February.

group includes cohorts born between 1975 and 1979, who were older than secondary school age. To increase the size of the control group, I use a two-year cohort gap rather than the three-year gap employed in Section 5.

Figure 5 presents the event study results, using the 1979 birth cohort as the reference. The treatment year remains 1999. The figure shows no significant pre- or post-trends, lending strong support to the assumption. Appendix Table A9 reports DiD regression results. The coefficient of interest in column (1) is small (-0.391) and statistically insignificant, indicating that exposure during secondary school age has little impact on educational attainment. These findings suggest that older children constitute a valid comparison group.

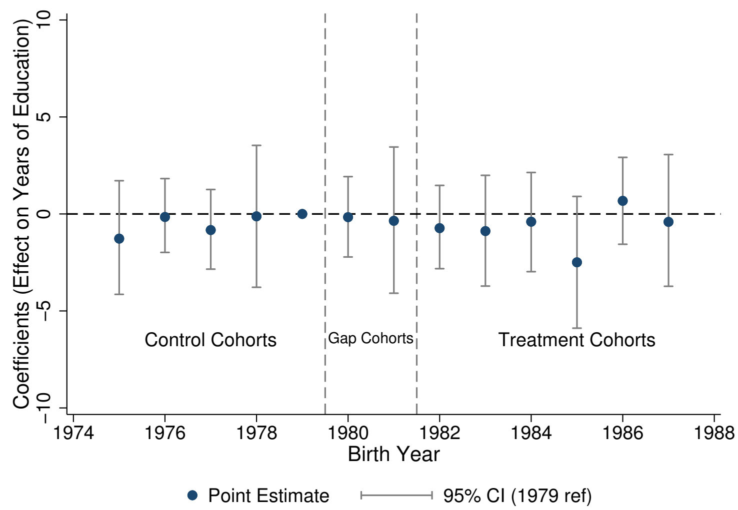

Figure 5: Event Study Results of Secondary-School-Age Exposure to TROP. *Note*: This figure presents event study estimates of secondary-school-age exposure to TROP. The treatment year is 1999. Estimates follow the baseline specification. The “gap cohorts,” shown between the dashed lines, are included in the event study but excluded from the DiD regression reported in Appendix Table A9.

Why the effects differ by age at exposure? The main results in Table 2 show that exposure to TROP during primary school age generates substantial human capital gains, whereas exposure during secondary school age produces negative and statistically insignificant effects. How can these differences across age groups be reconciled? Prior literature suggests that electrification improves agricultural productivity through irrigation and mechanization.[73] Given the central role of agriculture in rural livelihoods in late-1990s China, a plausible explanation is that rural electrification represents a technical change (productivity shock) that benefits primarily adult labor.[74]

The intuition is as follows. Improved access to electricity enables households to adopt electric-powered machinery, which primarily raises adult productivity, as reflected in higher wages. In contrast, the productivity of primary-school-age children changes little, as they are generally less able to operate such machinery due to physical constraints and task suitability. As a result, the opportunity cost of schooling for these children remains low, while household income rises. This generates a strong income effect—higher household resources allow children to remain in school—combined with a weak substitution effect, since children's labor value does not increase substantially. The net effect on their education is therefore positive.

For older children, however, the substitution effect is stronger because they are closer substitutes for adult labor in farm work and are more likely to operate machinery themselves. This raises their opportunity cost of schooling, which may offset the income effect, producing an insignificant or even negative overall effect.[75] This mechanism helps explain why the estimated effect for older children is negative and statistically insignificant. If the substitution effect is indeed at work, we would expect more negative effects for older boys, who typically serve as closer substitutes for adult labor than girls. Column (2) of Appendix Table A9 supports this expectation: the effect for boys is negative and larger in magnitude, although still imprecisely estimated. The findings here are in line with Shah and Steinberg (2017), which shows that a higher opportunity cost of schooling reduces human capital.

To formalize this reasoning, my paper develops a simple household decision-making model in

[73] See Kitchens and Fishback (2015), Assunção et al. (2017), Lewis and Severnini (2020), and Fried and Lagakos (2021).

[74] Other possible explanations for the limited impact on older children include longer adjustment periods for effects to materialize or dependence on human capital accumulated at earlier developmental stages.

[75] In practice, rigidities in the education system, such as high-stakes entrance exams, further limit the effect for secondary-school-age children. Statistics from the Ministry of Education of China show that the gross enrollment rate for the 15–17 age group (high school) was 41% in 1999, while for the 18–22 age group (higher education) it was only 10.5%. These numbers include both urban and rural areas; rural rates were likely lower. Source: http://www.moe.gov.cn/jyb_sjzl/moe_560/moe_566/moe_588/201002/t20100226_7844.html.

the spirit of Shah and Steinberg (2017), in which electrification represents a productivity shock in agriculture that primarily benefits adult labor. The model's predictions align with the empirical results. Details of the model are provided in Appendix Section E.

Note that the framework does not rule out other channels through which rural electrification may affect long-run human capital. Increased public investment in education can reinforce the positive effects of improved agricultural productivity for children exposed during primary school age. For older children, however, such benefits may be offset by a strong substitution effect, leading to an overall effect that remains negative and statistically insignificant.

10 Conclusions

Recent empirical evidence suggests that rural electrification programs focus solely on expanding grid connections often generate negligible economic benefits, at least in the medium term. Electrification involves more than just grid connection—it encompasses affordability, reliability, and quality. In many developing countries, aging and inadequate infrastructure limits the effectiveness of electrification efforts. Despite these challenges, little is known about whether rural electrification programs that improve access beyond grid connection can generate meaningful economics outcomes. This paper addresses this gap by examining the long-term human capital effects of China's 1999 "Two Reforms and One Price" (TROP) program, focusing on key electricity delivery factors in a context where most households were already connected to the grid.

I use a cohort difference-in-differences (DiD) approach to compare cohorts who were of primary school age (middle childhood) during the implementation of TROP with those who had already passed junior high school age. By leveraging regional variation in electricity price reductions, I identify the effects of TROP.

The findings reveal that better electricity access during middle childhood significantly increases educational attainment, school completion, and later adult cognitive performance. In contrast, exposure only after middle childhood yields negative and statistically insignificant effects. Two channels are identified that drive these gains: (1) increased agricultural productivity; and (2) greater government investment in education, reflecting electricity's role in enabling effective school infrastructure. The evidence suggests that extended study hours due to improved lighting are unlikely to be relevant. China's experience in the late 1990s offers insights for rural electrification efforts in developing countries today.

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Online Appendix

Powering the Future: The Long-Term Human Capital Effects of Rural Electrification

Pan Chen

Appendix A: Tables

Table A1: Key Differences: Electrification Versus Rainfall

	This paper	Shah and Steinberg (2017)
Shock	Electrification	Rainfall
Affected wage	Adults only	Adults and children
Child labor productivity	Unchanged	Increased
Empirical proxy for wage change	Electricity price reduction	Rainfall change

Table A2: Rural Electricity Sales Prices in Yangzhou, 2000 (Unit: CNY/kWh)

Region (county)	Residential Lighting	Other Non-standard Lighting	Industrial Use	Agricultural Use
Gaoyou	0.60	0.98	0.90	0.64
Baoying	0.64	0.99	0.89	0.66
Jiangdu	0.60	0.99	0.86	0.64
Hanjiang	0.61	0.98	0.86	0.62
Yizheng	0.64	0.99	0.89	0.65
Suburbs	0.59	0.99	0.89	0.60

Note: This table reports rural electricity prices in Yangzhou, Jiangsu Province, in 2000. At the time, 1 CNY was approximately equal to 0.12 USD. Data source: page 227 of Yangzhou Power Supply Company (2012).

Table A3: Sources of Electricity Prices

Administrative level of price	Number	Source
county	27	local gazetteer
	2	local newspaper
prefecture (city)	52	local gazetteer
	2	local newspaper
province	54	local gazetteer
	5	local newspaper
In total	142	

Note: This table provides a summary of the price data sources. Note that in China, the county is a administrative level below the prefecture (or city), which differs from the structure in the United States.

Table A4: Correlation between Missing Data and County-Level Socioeconomic Characteristics

Dependent var:	Indicator = 1 if the county uses provincial-level data			
	(1)	(2)	(3)	(4)
GDP per capita	0.000			
	(0.007)			
Agricultural GDP share[a]		0.065		
		(0.153)		
Industrial GDP share[a]			0.058	
			(0.063)	
Rural population share				-0.012
				(0.179)
Observations	127	124	127	123
R-squared	0.000	0.002	0.007	0.000
Mean of dep var	0.433	0.435	0.433	0.431

Note: County-level socioeconomic indicators are drawn from 1999 National City and County Financial Statistics (Quanguo Di Shi Xian Caizheng Tongji Ziliao, in Chinese). Most of these indicators are missing for the pre-treatment period. I use 1999 data as it is closest to TROP implementation and most complete. Robust standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

[a] Since industrial/agricultural GDP is unavailable, GDP share here is measured as the ratio of gross industrial/agricultural output to GDP. This measure is imprecise because gross output includes intermediate goods, while GDP reflects only value added.

Table A5: Correlation between Electricity Price pre-TROP and County-Level Characteristics in 1999

Dependent var:	Pre-TROP price				
	(1)	(2)	(3)	(4)	(5)
Rural transmission loss	0.822***				
	(0.291)				
GDP per capita		-0.001			
		(0.002)			
Agricultural GDP share[a]			0.152**		
			(0.073)		
Industrial GDP share[a]				-0.028	
				(0.028)	
Rural population share					0.214***
					(0.068)
Observations	136	123	120	123	119
R-squared	0.033	0.001	0.024	0.004	0.036
Mean of indep var	0.275	5.326	0.475	0.999	0.734

Note: Data on transmission loss is drawn from local gazetteers. Most counties don't have these records. In these cases, I use provincial level records instead. Other county-level indicators are drawn from 1999 National City and County Financial Statistics (Quanguo Di Shi Xian Caizheng Tongji Ziliao, in Chinese). Most of these indicators are missing for the pre-treatment period. I use 1999 data as it is closest to TROP implementation and most complete. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

[a] Since industrial/agricultural GDP is unavailable, GDP share here is measured as the ratio of gross industrial/agricultural output to GDP. This measure is imprecise because gross output includes intermediate goods, while GDP reflects only value added. This also explains why the mean of the independent variable in column (4)—industrial GDP share—is close to 1.

Table A6: Summary Statistics for Electricity Price before and after TROP

	Mean	Std. dev.	Obs
Pre-TROP price	0.83	0.29	142
Post-TROP price	0.59	0.14	142
Price Reduction	0.24	0.21	142

Note:This table presents statistics on rural electricity prices before and after the implementation of TROP. Prices are measured in CNY (Chinese yuan, where 1 CNY $\approx$ 0.12 USD in 2000). A positive price reduction indicates that TROP led to a decrease in electricity prices. Three counties in my sample show a negative price reduction, meaning their electricity prices increased slightly due to TROP.

Table A7: Correlation between Electricity Price Reduction and County-Level Characteristics in 1999

Dependent var:	Price reduction				
	(1)	(2)	(3)	(4)	(5)
Rural transmission loss	0.172				
	(0.264)				
GDP per capita		-0.002*			
		(0.001)			
Agricultural GDP share[a]			0.097*		
			(0.057)		
Industrial GDP share[a]				-0.052**	
				(0.021)	
Rural population share					0.169***
					(0.043)
Observations[b]	136	123	120	123	119
R-squared	0.002	0.006	0.019	0.028	0.043
Mean of indep var	0.143	5.326	0.475	0.999	0.734

Note: Data on transmission loss is drawn from local gazetteers. Most counties don't have these records. In these cases, I use provincial level records instead. Other county-level indicators are drawn from 1999 National City and County Financial Statistics (Quanguo Di Shi Xian Caizheng Tongji Ziliao, in Chinese). Most of these indicators are missing for the pre-treatment period. I use 1999 data as it is closest to TROP implementation and most complete. Robust standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

[a] Since industrial/agricultural GDP is unavailable, GDP share here is measured as the ratio of gross industrial/agricultural output to GDP. This measure is imprecise because gross output includes intermediate goods, while GDP reflects only value added. This also explains why the mean of the independent variable in column (4)—industrial GDP share—is close to 1.

[b] Observation counts vary due to missing data for some counties.

Table A8: Robustness to Baseline Education Imbalance

Dependent var:	Years of education				
	(1) CEM (1979–1984)	(2) CEM (1970–1978)	(3) Lowest 25th percentile of treatment dropped	(4) Higher education expansion	(5) Higher education expansion
Price reduction ×	2.824***	2.815***	3.024***	4.3929***	3.9975***
affected cohorts (1988–1994)	(0.585)	(0.682)	(0.760)	(1.1468)	(1.1114)
Higher education expansion ×				0.0536**	
affected cohorts I (1998–1994)				(0.0268)	
Higher education expansion ×				0.0115	
affected cohorts II (1982–1984)				(0.0325)	
Observations	4,145	4,145	3,035	2,533	2,533
R-squared	0.442	0.443	0.467	0.365	0.364
County FE	Yes	Yes	Yes	Yes	Yes
Province × birth year FE	Yes	Yes	Yes	Yes	Yes
Mean of dep var	9.525	9.525	9.236	10.19	10.19

Note: This table reports results after applying Coarsened Exact Matching (CEM) to balance high- and low-treatment counties by average years of education. Column (1) reweights estimates using CEM based on average education among control cohorts (1979–1984), while column (2) uses CEM based on average education among older rural cohorts (1970–1978) from the 2000 China Census. Column (3) drops counties in the lowest 25th percentile of treatment intensity (price reduction). Column (4) incorporates the impact of higher education expansion, while column (5) uses the same sample as column (4) to check the impact of sample size. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A9: Results of Secondary-School-Age Exposure

Dependent var:	Years of education	
	(1) Overall	(2) Gender difference
Price reduction × affected cohorts (1982–1987) × male		-0.563
		(1.064)
Price reduction × affected cohorts (1982–1987)	-0.391	-0.012
	(0.811)	(0.768)
Observations	3,582	3,582
R-squared	0.446	0.450
County FE	Yes	Yes
Province × birth year FE	Yes	Yes
Mean of dep var	8.396	8.396

Note: This table reports the impact of secondary-school-age exposure to TROP on years of education, using cohorts born 1982–1987 as the treatment group and those born 1975–1979 as the control group. The treatment year is 1999. The oldest treatment cohort had at least one year of overlap with TROP during senior high school, while the youngest control cohorts were already 20 years old in 1999. Column (1) shows the overall effect, while column (2) presents heterogeneous effects by gender. Robust standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Appendix B: Figures

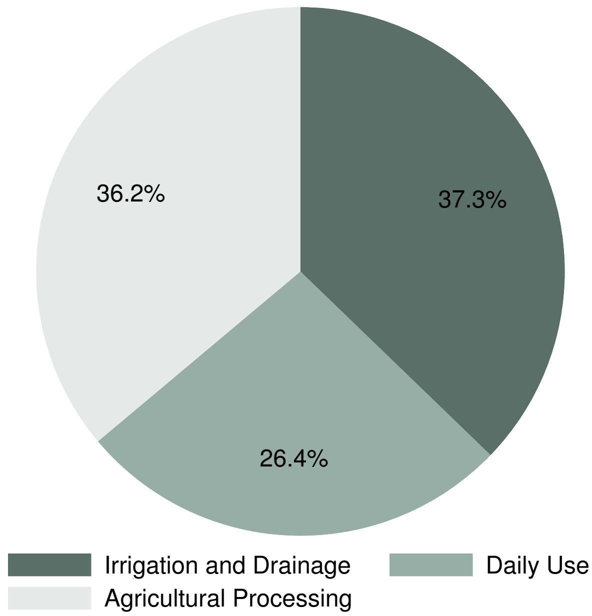


Figure B1: Rural Electricity Use Breakdown in Henan Province, 1997. *Note*: This figure shows rural electricity consumption in Henan Province in 1997. Although the category includes both drainage and irrigation, it primarily reflects irrigation, as most areas in Henan—located in northern China with limited water resources—rely heavily on irrigation. The average rural electricity price in Henan in 1998 was 0.85 CNY ($\approx$ 0.10 USD in 2000), which fell to 0.53 CNY in 2001 following TROP implementation. Source: page 312 of Henan Electric Power Industry Gazetteer Compilation Commission (2010).

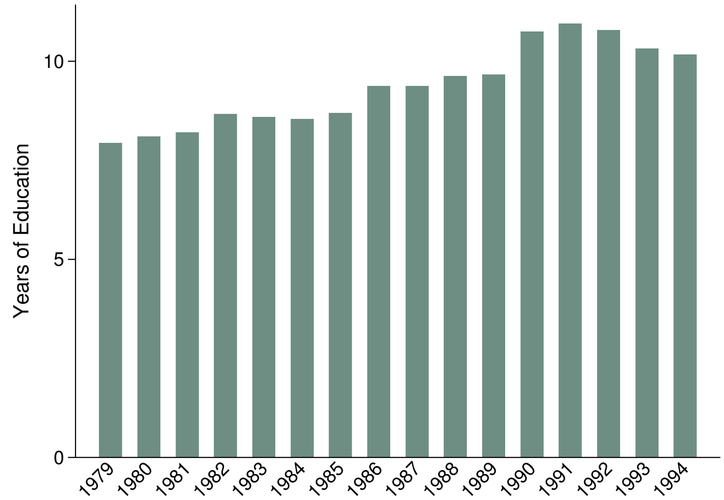
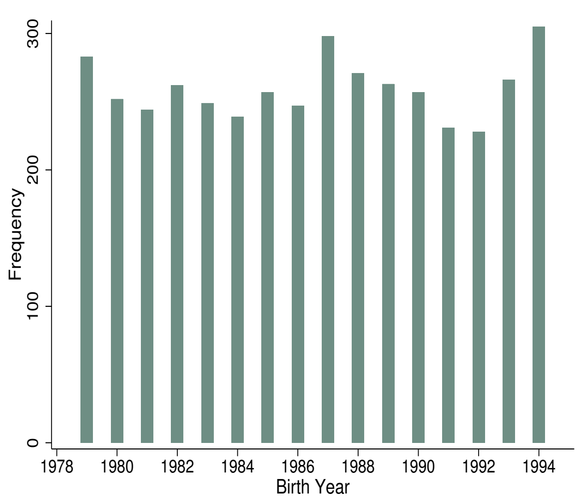
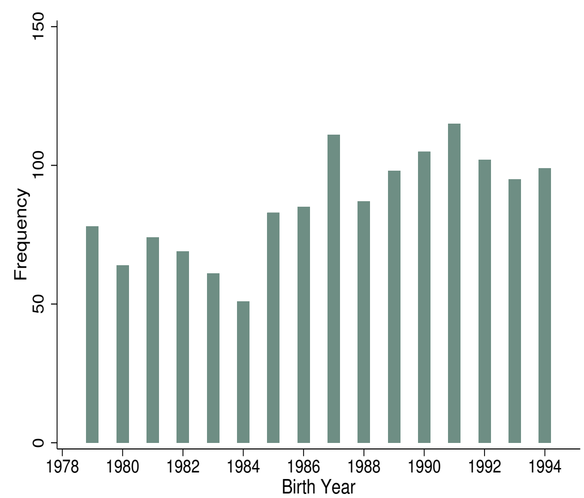


Figure B2: Years of Education by Birth Year. *Note*: This figure illustrates the average years of education by birth year in my sample.

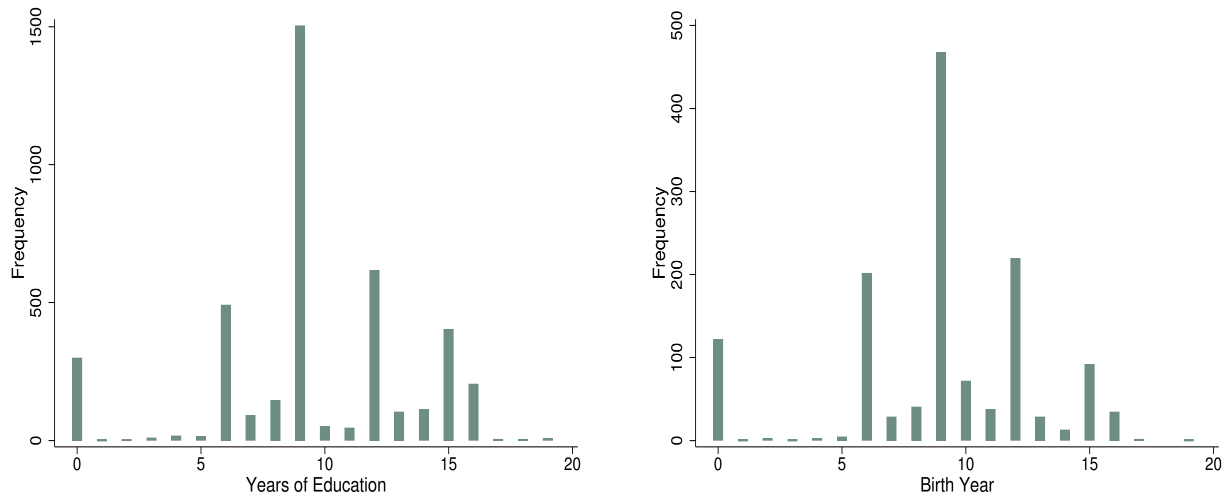


(a) Distribution of Birth Year in the 2014 Wave Matched to the 2010 Wave (n = 4,152)



(b) Distribution of Birth Year for the Missing Sample in the 2014 Wave (n = 1,377)

Figure B3: Sample Distribution of Birth Year. *Note*: Panel (a) shows the distribution of birth year for individuals present in both 2010 and 2014 waves, while panel (b) displays the distribution of birth year for individuals surveyed in the 2010 wave but missing in the 2014 wave. Both panels include cohorts born between 1979 and 1994 with a rural Hukou at ages 3 and 12.



(a) Distribution of Years of Education in the 2014 Wave Matched to the 2010 Wave (n = 4,152)

(b) Distribution of Years of Education for the Missing Sample in the 2014 Wave (n = 1,377)

Figure B4: Sample Distribution of Years of Education. *Note*: Panel (a) shows the sample distribution of years of education in 2014 for individuals present in both the 2010 and 2014 surveys, while panel (b) displays the sample distribution of years of education in 2010 for individuals surveyed in the 2010 wave but missing in the 2014 wave. Both panels include cohorts born between 1979 and 1994 with a rural Hukou at ages 3 and 12.

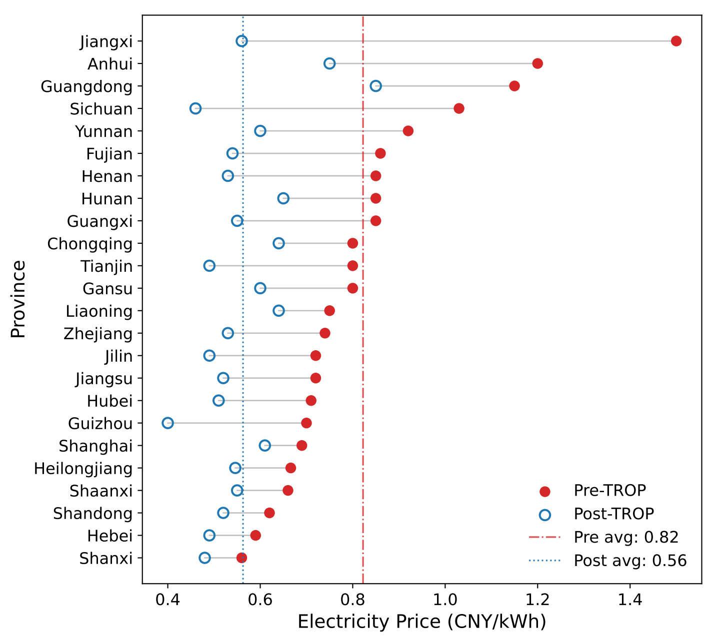
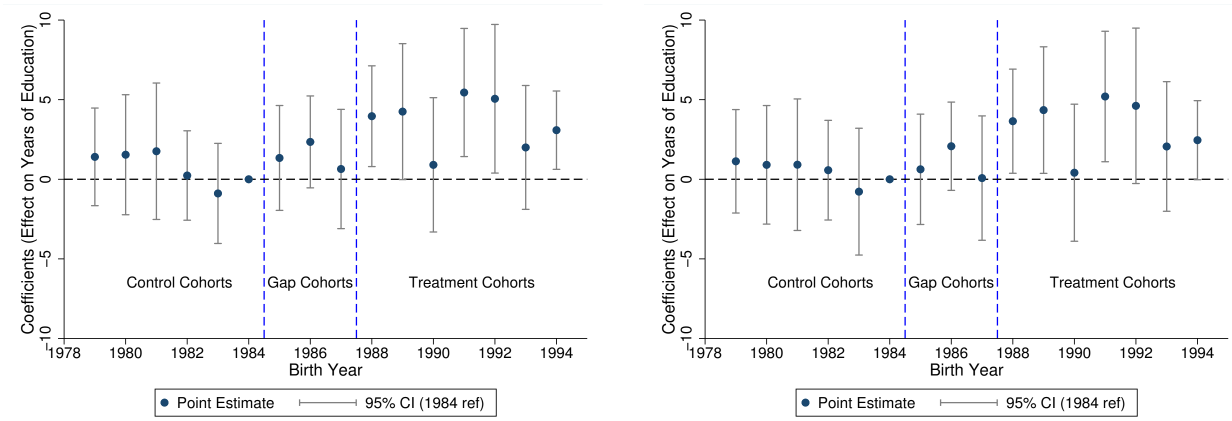


Figure B5: Provincial Electricity Prices: Pre- vs. Post-TROP.



(a) Controlling for 1999 Socioeconomic Characteristics by Birth Year Fixed Effects

(b) Controlling for 1999 Socioeconomic Characteristics and Base Education by Birth Year Fixed Effects

Figure B6: Event Study Results—Controlling for Heterogeneous Trends across Counties. *Note*: Panel (a) includes interactions between 1999 socioeconomic characteristics and birth year fixed effects. Panel (b) additionally includes baseline education by birth year fixed effects. The y-axis represents the estimated coefficients of interest. "Gap cohorts" (between the dashed lines) are included in the event study but excluded from the baseline regression.

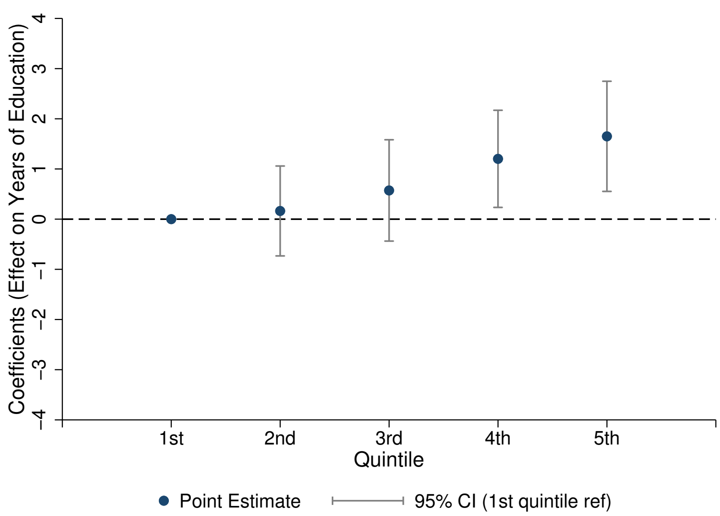


Figure B7: Treatment Effects by Quintiles. *Note*: This figure shows treatment effects by quintile bins, where the first quintile serves as the comparison group.

Appendix C: Additional Robustness Checks

Discrete Treatment. This paper uses a DiD strategy with continuous treatment to estimate the effects. However, Callaway, Goodman-Bacon, and Sant'Anna (2024) cautions that a two-way fixed-effects (TWFE) regression with a continuous treatment interaction does not identify a clean average causal effect unless very strong assumptions hold. Following the authors' recommended procedure, I discretize the treatment measure (price reduction) into five categories based on quintiles and interact the category dummies with the dummy indicating treated cohorts. The model specification is as follows:

$$Y_{i,t,c} = \alpha + \sum_{q=2}^{5} \beta_q \left(\mathbf{1}\{Q_c = q\} \times \mathbf{1}\left\{1988 \leq t \leq 1994\right\}\right) + \varphi X_{i,t,c} + \gamma_c + \mu_{prov} \times \tau_t + \varepsilon_{i,t,c} \tag{C1}$$

where the lowest 20th percentile serves as the comparison group. β_q represents the coefficient of interest, and Q_c denotes the quintile for price reduction in county c. All other notations remain consistent with those in the baseline specification.

Column (1) of Appendix Table C1 reports estimates from a difference-in-differences specification that discretizes county-level electricity price reductions into quintiles. The omitted group consists of individuals in the lowest-intensity counties (Q1). Among cohorts exposed during primary school, I find no statistically significant gains in educational attainment in counties with modest price reductions (Q2–Q3). In contrast, exposure in counties experiencing large price reductions yields substantial and statistically significant effects: individuals in the fourth quintile gain 1.20 additional years of schooling, while those in the highest quintile gain 1.65 years. This monotonic and nonlinear pattern indicates that human capital gains arise only when treatment intensity is sufficiently large.

Alternative Treatment Measures. Local governments may lack the ability to implement the program, resulting in endogenous price reduction. Additionally, they may exaggerate price reduction levels for political promotion motives. To mitigate these concerns, I use the pre-TROP price as the treatment measure, which is predetermined and less likely to be endogenous than price reduction. Column (2) of Appendix Table C1 shows the results. The coefficient of interest remains statistically significant. The effect size is smaller but comparable to the baseline estimate.

I use the price reduction ratio as another alternative treatment measure. The results in column (3) of Appendix Table C1 remain statistically significant. A one standard deviation increase in the price reduction ratio (17%) leads to an increase in children's educational attainment by 0.747 years, which is comparable to the baseline effect of 0.605 years.

Price Selection and Treatment Measure Construction. As discussed in Section 3, there are two key considerations in selecting post-TROP electricity prices. First, in some regions, rural electricity prices were lowered soon after TROP implementation (post-TROP price 1), and later, rural-urban price unification was completed (post-TROP price 2, or the unified price). This paper uses post-TROP price 1, which aligns with the most intensive phase of TROP. In my sample, 83% of counties document post-TROP price 1 between 2000 and 2002; this share rises to 97% when 2003 is included. Second, although full price unification did not occur uniformly in 2001, most regions completed this change around that time, with some doing so earlier or later. More than 87% of counties in the sample completed unification between 2001 and 2003. To test the robustness of price selection, I introduce the following two regressions:

$$\begin{aligned} Y_{i,t,c} =&\alpha + \beta_1 \times Price\_Reduction_c \times \mathbf{1}\left\{Age \leq 11\right\} + \beta_2 \times \mathbf{1}\left\{Age \leq 11\right\} \\ &+ \varphi X_{i,t,c} + \gamma_c + \mu_{prov} \times \tau_t + \varepsilon_{i,t,c} \end{aligned} \tag{C2}$$

where the indicator function equals 1 if individual i in county c was 11 or younger in the year the post-TROP price is chosen. All other notations follow the baseline specification (Eq. 2).

$$\begin{aligned} Y_{i,t,c} =&\alpha + \beta_1' \times Price\_Reduction'_c \times \mathbf{1}\left\{Age \leq 11\right\} + \beta_2' \times \mathbf{1}\left\{Age \leq 11\right\} \\ &+ \varphi X_{i,t,c} + \gamma_c + \mu_{prov} \times \tau_t + \varepsilon_{i,t,c} \end{aligned} \tag{C3}$$

In Eq. C3, $Price\_Reduction'_c$ represents the difference between the pre-TROP price and the unified price, regardless of the year the unification occurred. The indicator function equals 1 if individual i in county c was 11 or younger when the price was unified. All other notations follow the baseline specification (Eq. 2).

Eq. C2 and Eq. C3 are both based on a staggered DiD framework,[1] though a full econometric discussion is beyond the scope of this paper. The coefficients β_1 and β_1' capture the treatment effects. As in the baseline, I exclude cohorts aged 12 to 14 in the year the post-TROP price is selected.[2]

Column (4) of Appendix Table C1 reports results from Eq. C2, and column (5) shows results from Eq. C3. Both estimates are statistically significant and close to the baseline. These findings confirm that the core results are robust to variation in the timing of post-TROP prices. The baseline specification remains preferred because Eq. C2 and Eq. C3 rely on additional assumptions and involve more complex econometric frameworks.

I finally use the unified price as the post-TROP price, regardless of timing, to calculate price reductions and run the baseline regression. The effect in column (6) of Appendix Table C1 is statistically significant and comparable to the baseline.

Price Data Availability. One potential concern with the data collection strategy is the use of prefectural or provincial electricity prices when county-level data are unavailable. Variation at higher administrative levels may not fully capture county-level heterogeneity in electricity prices, leading to mismeasurement of the true treatment intensity. This could bias the estimated effect toward zero.

To assess the robustness of my results to potential measurement error in treatment intensity, I conduct two additional checks. First, I include interaction terms between the administrative level of the price data (county, prefecture, or province) and birth year dummies to allow for heterogeneous trends by data source. As shown in column (1) of Appendix Table C2, the coefficient of interest

[1] If $\beta_1 \times Price\_Reduction_c \times \mathbf{1}\{Age \leq 11\}$ is omitted from Eq. C2, the model becomes a binary-treatment staggered DiD setup. However, my data lacks sufficient variation in the timing of post-TROP prices to identify the effects under this setup.

[2] In practice, the indicator $\mathbf{1}\{\text{Age} \leq 11\}$ is absorbed by birth year fixed effects, since the variation stems from dropped cohorts aged 12–14 in the relevant year. This underscores the limited timing variation in the data.

Table C1: Robustness to Alternative Treatment Measures

Dependent var:	Years of education					
	(1) Discrete treatment	(2) Pre-TROP price as treatment	(3) Price reduction ratio as treatment	(4) Varying timing of post-TROP Price	(5) Varying timing of unified price	(6) Unified price as post-TROP price
Treatment (bin2 = 1) × affected cohorts (1988–1994)	0.163					
	(0.457)					
Treatment (bin3 = 1) × affected cohorts (1988–1994)	0.572					
	(0.515)					
Treatment (bin4 = 1) × affected cohorts (1988–1994)	1.202**					
	(0.494)					
Treatment (bin5 = 1) × affected cohorts (1988–1994)	1.651***					
	(0.560)					
Pre-TROP price × affected cohorts (1988–1994)		2.190***				2.534***
		(0.565)				(0.571)
Price reduction ratio × affected cohorts (1988–1994)			4.396***			
			(1.366)			
Price reduction × affected cohorts (age≤12 when price chosen)				2.924***		
				(0.781)		
Price reduction × affected cohorts (age≤12 when price unified)					2.677***	
					(0.707)	
Observations	4,145	4,145	4,145	4,136	4,196	4,145
R-squared	0.438	0.438	0.438	0.446	0.443	0.437
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Province × birth year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep var	9.438	9.438	9.438	9.405	9.328	9.438

Note: This table reports robustness check results for alternative treatment measures. Column (1) bins price reduction into quintiles and includes a dummy variable for each quintile bin. Column (2) uses pre-TROP electricity prices as treatment intensity. Column (3) uses the price reduction ratio as an alternative measure. Column (4) defines treated cohorts as those age $\leq$ 12 when post-TROP electricity prices were chosen. Column (5) uses the unified price to calculate treatment intensity and defines treated cohorts as those age $\leq$ 12 when the unified price was implemented. Column (6) simply redefines treatment intensity using the unified price as the post-TROP price. Standard errors are clustered at the county level. *** p<0.01, ** p<0.05, * p<0.1.

increases slightly to 3.036, compared to the baseline estimate of 2.879. Second, I drop all counties where treatment is based on province-level prices, which are more prone to measurement error. Column (2) of Appendix Table C2 shows the coefficient rises to 3.293, though the difference remains modest. These results suggest that bias from measurement error is limited, and that the baseline estimate can be interpreted as a conservative lower bound.

Lengths of Exposure. Treatment cohorts experienced varying lengths of exposure to TROP during primary school age. For example, children born in 1988 were exposed for only one year, while those born in 1994 had six years of exposure. To assess whether this variation affects the precision of the estimated effect, I calculate the share of primary school years each cohort was exposed to TROP. The 1988 cohort, for instance, had 1/6 exposure, while cohorts born in 1994 and later were fully exposed. Cohorts born before 1988 had zero exposure. I then construct a new treatment intensity measure by multiplying this exposure share by the electricity price reduction and re-estimate the model using the baseline specification. Column (3) of Appendix Table C2 shows that the coefficient remains highly comparable to the baseline estimate under full exposure.

Missing Observations. Section 3 indicates that about 24% of individuals in the 2010 wave are missing from the 2014 wave. In the baseline regression, I follow Bianchi, Lu, and Song (2022) and use observations from the 2010 wave to fill in missing outcome variables for these individuals. To check if my results are robust to missing observations, I first add a dummy variable indicating whether an individual in the 2010 wave is missing from the 2014 wave to the baseline regression. Column (4) of Appendix Table C2 reports the results, where the coefficient of interest remains statistically significant and close to the baseline. In column (5), I further interact the dummy variable with birth year fixed effects and add them to the baseline regression. The coefficient remains consistent with the baseline.

Additionally, I restrict the sample to individuals present in both the 2010 and 2014 waves. Column (6) of Appendix Table C2 shows that the coefficient is slightly smaller than in the baseline but still statistically significant. The smaller effect size may be due to a smaller sample size and the fact that many treatment cohorts had not yet completed schooling in 2010.

Table C2: Price Selection and Missing Observations in the 2014 Survey

Dependent var:	Years of education					
	(1)	(2)	(3)	(4)	(5)	(6)
Robustness:	Control price administrative level	Drop province-level prices	Years of exposure	Control missing obs I	Control missing obs II	Matched sample in 2010 and 2014
Price reduction × affected cohorts (1988–1994)	3.036***	3.293***	2.740***	3.018***	3.037***	2.794**
	(0.703)	(0.687)	(0.752)	(0.692)	(0.626)	(1.220)
Observations	4,145	2,511	4,145	4,145	4,145	3,154
R-squared	0.441	0.493	0.436	0.448	0.456	0.474
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Province × birth year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep var	9.438	9.611	9.438	9.438	9.438	9.709

Note: This table reports the results of additional robustness checks. Column (1) includes interaction terms between the administrative level of the price data and birth year dummies. Column (2) drops counties where treatment is based on province-level price changes. Column (3) accounts for variation in lengths of exposure. Column (4) adds a dummy for individuals in the 2010 wave who are missing in 2014, and column (5) interacts this dummy with birth year fixed effects. Column (6) restricts the sample to individuals observed in both 2010 and 2014. Standard errors are clustered at the county level. *** p<0.01, ** p<0.05, * p<0.1.

References

Bianchi, Nicola, Yi Lu, and Hong Song. 2022. "The effect of computer-assisted learning on students' long-term development." Journal of Development Economics 158:102919.

Appendix D: Heterogeneity

The impact of electrification may differ by gender. In developed countries, electrification is shown to reduce the burden of household chores for women (Greenwood, Seshadri, and Yorukoglu, 2005), enabling them to invest in human capital earlier in life (Vidart, 2024). In developing economies, however, the evidence on gender-specific impacts remains mixed.[1]

Column (1) of Appendix Table D1 interacts the treatment variable with gender (1 = male, 0 = female). The results indicate that TROP significantly improved educational attainment for both genders, with a larger effect for males (marginally significant). This pattern likely reflects rural conditions in China, where boys are often favored and, given low household incomes, few families could afford appliances that substantially reduce girls' domestic workload. As a result, electrification likely benefited boys more.

Column (2) of Appendix Table D1 examines heterogeneity by family size, using a binary variable equal to 1 if the individual has siblings. Consistent with theories on the quantity-quality trade-off in child-rearing (Becker, 1960; Becker and Lewis, 1973; Becker and Tomes, 1976), the results suggest a smaller effect for individuals with siblings, though again the difference is statistically insignificant.

Beginning in the Seventh Five-Year Plan period (1980s), China implemented multiple rounds of rural hydropower electrification programs that continued until very recently (Lin and Xu, 2024). Because my study period falls in the middle of this long-running program, I examine whether hydropower electrification moderates the effects of TROP. Column (3) of Appendix Table D1 reports results from interacting the treatment variable with the provincial-level hydropower remaining ratio in 2005,[2] which also assesses the sensitivity of my baseline estimates to hydropower electrification. The results show no significant differences between regions with high and low hydropower

[1] Some studies suggest girls benefit more (Khandker, Barnes, and Samad, 2013; Dasso Arana, Fernandez, and Ñopo, 2015; Van de Walle et al., 2017), while others find no significant effects on girls (Lee, Miguel, and Wolfram, 2020b; Burlig and Preonas, 2024). Lipscomb, Mobarak, and Barham (2013) and Khandker et al. (2014) find positive impacts on both boys' and girls' education. See Lee, Miguel, and Wolfram (2020a) for a comprehensive review.

[2] Hydropower remaining ratio is obtained from https://www.ndrc.gov.cn/fggz/fzzlgh/gjjzxgh/200709/P020191104623242600508.pdf#page=10.09.

potential.

As a large share of rural electricity consumption is used for irrigation,[3] rural electrification may have a greater impact in areas more dependent on precipitation, particularly drier regions. In such areas, TROP might facilitate water pumping for irrigation, potentially boosting agricultural income. I collect annual county-level precipitation data from 1999 to 2007 using the ERA5-Land reanalysis dataset via Google Earth Engine.[4] Based on average annual precipitation, I classify counties as non-humid (binary variable = 1) or humid (binary variable = 0), following the classification of the central government of China.[5] Column (4) of Appendix Table D1 reports the results, showing that the treatment has a larger impact on educational attainment in non-humid areas. This effect is statistically significant.

Column (5) of Appendix Table D1 uses an alternative measure of drought: the total number of moderate or worse drought months from 1999 to 2007, based on the Standardized Precipitation Evapotranspiration Index (SPEI).[6] Note that the number of drought months is normalized. The results show that the educational gains from electrification are significantly larger in counties that experienced more drought months during this period.

I also examine heterogeneity by baseline economic conditions by interacting agricultural GDP per rural capita in 1999 (normalized) with the treatment variable. Column (6) of Appendix Table D1 indicates that regions more dependent on agriculture experience larger human capital gains. The final column includes all interaction terms, and most of the above conclusions remain unchanged.

Taken together, the heterogeneity analysis suggests that the educational benefits of rural electrification are closely linked to agricultural production. In drier areas, the opportunity cost of school-

[3] Section 2 provides an example of rural electricity consumption patterns in China.

[4] Source: https://developers.google.com/earth-engine/datasets/catalog/ECMWF_ERA5_LAND_MONTHLY_AGGR.Monthly data are aggregated to annual values. The period 1999–2007 aligns with the primary school years of the treatment cohorts.

[5] See https://www.gov.cn/test/2005-06/24/content_9220.htm.

[6] Source: Beguería, Vicente-Serrano, and Angulo-Martínez (2010), Vicente-Serrano et al. (2010), and https://spei.csic.es/spei_database_2_10/#map_name=spei01. I use the SPEI at the nearest grid point to each county center. The timescale is one month.

ing is lower, so electrification-induced agricultural gains are more likely to encourage children to remain in school. Similarly, in regions more dependent on agriculture, TROP likely generates larger gains through improved productivity. These findings motivate a closer examination of the agricultural income channel as a potential mechanism in the next section.

Table D1: Heterogeneity

Dependent var:	Years of education						
Heterogeneity:	(1) Gender difference (male=1)	(2) Siblings (have siblings=1)	(3) Hydropower potential	(4) Annual precipitation (non-humid area=1)	(5) Drought months	(6) Ag GDP per rural capita	(7) All included
Reduction × affected cohorts (1988–1994)	2.133***	3.578***	2.835***	2.521***	3.418***	3.127***	1.926*
	(0.749)	(1.050)	(0.866)	(0.625)	(0.769)	(0.711)	(1.144)
Reduction × affected cohorts (1988–1994) × male	1.491						1.816*
	(0.947)						(0.951)
Reduction × affected cohorts (1988–1994) × siblings		-1.272					-0.458
		(1.099)					(1.420)
Reduction × affected cohorts (1988–1994) × hydropower potential			-0.114				-0.369
			(0.624)				(0.826)
Reduction × affected cohorts (1988–1994) × non-humid area				5.494**			10.220**
				(2.606)			(2.958)
Reduction × affected cohorts (1988–1994) × drought months					1.447**		0.876
					(0.698)		(0.702)
Reduction × affected cohorts (1988–1994) × ag GDP per rural capita						2.214***	2.774***
						(0.823)	(0.734)
Observations	4,145	4,145	4,145	4,145	4,145	3,785	3,785
R-squared	0.441	0.432	0.439	0.442	0.442	0.448	0.453
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province × birth year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep var	9.525	9.525	9.525	9.525	9.525	9.503	9.490

Note: Column (1) includes interaction terms between gender (0 for female, 1 for male) and the treatment variable. Column (2) adds a binary variable indicating whether the individual has siblings (1 if yes, 0 otherwise). Column (3) interacts provincial-level hydropower remaining ratio with the treatment variable. Column (4) classifies counties as humid or non-humid based on annual precipitation. Column (5) interacts the number of moderate or severe drought months with the treatment variable. Column (6) interacts agricultural GDP per rural capita with the treatment variable. The final column includes all interaction terms. Standard errors are clustered at the county level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Appendix E: A Model of Human Capital Investment

Rural electrification may affect children's long-run human capital through multiple channels. This section outlines a simple framework that focuses on one important channel, agricultural productivity, without ruling out other mechanisms.

Existing literature shows that rural electrification improves irrigation and agricultural productivity (Kitchens and Fishback, 2015; Assunção et al., 2017; Lewis and Severnini, 2020; Fried and Lagakos, 2021). Building on this evidence, and given the central role of agriculture in rural livelihoods in 1990s China, rural electrification is expected to raise household income and affect children's education through two opposing channels: higher income may increase investment in schooling, while more profitable farming can raise the opportunity cost of schooling. The net effect depends on which force dominates.

To formalize this trade-off, I develop a household decision-making model in which electrification represents a productivity shock in agriculture, in the spirit of Shah and Steinberg (2017). Unlike Shah and Steinberg (2017) that assumes adult and child labor are perfect substitutes conditional on human capital, I allow for imperfect substitutability. Even with similar levels of human capital—such as five years of schooling—children are less productive than adults in agricultural work due to differences in physical strength, stamina, and task suitability. Accordingly, my model assumes that adult labor is more productive than child labor.

The Model

The household consists of one parent and one child. The parent maximizes lifetime utility over three periods (periods 1, 2, and 3). In period 1 (early childhood), the child is too young to attend school or work and only consumes. In period 2 (middle childhood and adolescence), the child consumes and allocates one unit of time between schooling (or study) and productive (manual) labor. Let s_2 denote time spent in school. In period 3 (adulthood), the household benefits from the child's accumulated human capital. Borrowing and saving are ruled out throughout the framework.

Let c_t denote household consumption in period $t \in \{1, 2\}$, and $u_t(c_t)$ the corresponding utility, with $\frac{\partial u_t(c_t)}{\partial c_t} > 0$ and $\frac{\partial^2 u_t(c_t)}{\partial c_t^2} < 0$. Let e_t denote the child's human capital in period t, and e_p the parent's human capital (assumed constant). Let $V(e_3)$ represent the utility derived from the child's human capital in period 3. The household's total utility function is given by:

$$U = u_1(c_1) + \beta u_2(c_2) + \beta^2 V(e_3), \tag{E1}$$

where $\beta \in (0, 1)$ is the discount factor.

In period 1, consumption is financed solely through parental (or adult) income:

$$c_1 \leq w_1 e_p, \tag{E2}$$

where w_1 is the wage per unit of parental human capital, with $w_t \in (0, \bar{w})$ and $\bar{w}$ finite.

In period 2, consumption is financed by both the parent and the child. The child's human capital e_2 affects the return to child labor. Let w_2 and $w_{c,2}$ denote adult and child wage rates, respectively. The period 2 budget constraint is:

$$c_2 \leq w_2 e_p + w_{c,2}(1 - s_2)e_2, \tag{E3}$$

where $1 - s_2$ represents the time allocated to working.

Wages are treated as exogenous and interpreted as shifters of agricultural productivity. In contrast to Shah and Steinberg (2017), where $w_2 = w_{c,2}$,[1] my paper assumes that child labor is less productive ($w_{c,2} < w_2$). In rural settings, children—especially younger ones—typically work within the household rather than in external labor markets. As a result, their "wages" are not market-determined but instead reflect the household's internal valuation of their time.

When adult productivity rises because of electrification, the household's valuation of child work also rises. In this sense, child wages co-move with parental wages. Formally, let the child

[1]In Shah and Steinberg (2017), $c_2 = w_2[e_p + (1 - s_2)e_2]$, so if the child works full time ($s_2 = 0$), the marginal returns to child and adult human capital are equal. This implies perfect substitutability between child and adult labor conditional on human capital.

wage $w_{c,2}$ depend on the adult wage w_2, such that $\frac{\partial w_{c,2}}{\partial w_2} > 0$.[2] Note that in the empirical analysis, changes in adult wages are proxied by electricity price reductions.

Following Cunha and Heckman (2007), the child's human capital in period t depends on prior human capital and investments made through consumption or schooling. In this three-period model, early-life human capital is normalized to zero ($e_1 = 0$). The human capital evolution is given by:

$$e_1 = 0, \quad e_2 = f_2(c_1), \quad e_3 = f_3(e_2, c_2, s_2).$$

The production functions satisfy:

$$\frac{\partial f_2}{\partial c_1} \geq 0, \quad \frac{\partial f_3}{\partial e_2} \geq 0, \quad \frac{\partial f_3}{\partial c_2} \geq 0, \quad \frac{\partial f_3}{\partial s_2} \geq 0,$$

and

$$\frac{\partial^2 f_2}{\partial c_1^2} \leq 0, \quad \frac{\partial^2 f_3}{\partial e_2^2} \leq 0, \quad \frac{\partial^2 f_3}{\partial c_2^2} \leq 0, \quad \frac{\partial^2 f_3}{\partial s_2^2} \leq 0.$$

Maximization Problem

Following the literature, let $V(e_3) = e_3$ for simplicity. In period 2, the parent chooses s_2 to maximize lifetime utility. Since utility is strictly increasing in consumption and intertemporal saving is not allowed, both budget constraints bind. Substituting $c_2 = w_2 e_p + w_{c,2}(1 - s_2)e_2$ into the total utility function and omitting c_1 (as it is predetermined from the perspective of period 2), the household solves:

$$\max_{s_2 \in [0,1]} \; u_2[w_2 e_p + w_{c,2}(1 - s_2)e_2] + \beta f_3[e_2, w_2 e_p + w_{c,2}(1 - s_2)e_2, s_2]. \tag{E4}$$

[2] If $\frac{\partial w_{c,2}}{\partial w_2} < 0$, the substitution effect is positive, and the overall effect on education is unambiguously positive.

Assume the following boundary and curvature conditions for an interior solution:

$$\lim_{s_2 \to 0^+} \frac{\partial f_3}{\partial s_2} = +\infty, \quad \lim_{s_2 \to 1^-} \frac{\partial f_3}{\partial s_2} = 0,$$

$$\left(\frac{\partial^2 u_2}{\partial c_2^2} + \beta \frac{\partial^2 f_3}{\partial c_2^2} \right) \cdot \left(\beta \frac{\partial^2 f_3}{\partial s_2^2} \right) > \left(\beta \frac{\partial^2 f_3}{\partial s_2 \partial c_2} \right)^2 .$$

The first-order condition (FOC) is:

$$w_{c,2} e_2 \frac{\partial u_2}{\partial c_2} = \beta \Phi(e_2, c_2^*, w_2, w_{c,2}, s_2^*), \tag{E5}$$

where

$$\Phi = \frac{\partial f_3}{\partial s_2} - w_{c,2} e_2 \frac{\partial f_3}{\partial c_2}. \tag{E6}$$

Eq. E5 implies that, at an interior optimum, the parent equates the marginal utility gained from consumption with the net long-term benefit of schooling. Given that $\partial u_2 / \partial c_2 > 0$, $w_{c,2} > 0$, and $e_2 > 0$, it follows that $\Phi(e_2, c_2^*, w_2, w_{c,2}, s_2^*) > 0$: the human capital return to schooling exceeds the return to working and consuming.

Effect of Parental Wage on Schooling and Long-Term Human Capital

Effect of Second-Period Wage on Schooling

Differentiating the FOC with respect to w_2, the comparative static result yields:

$$\begin{aligned} \frac{\partial s_2^*}{\partial w_2} \propto & \underbrace{-e_2 \frac{\partial w_{c,2}}{\partial w_2} \left(\frac{\partial u_2}{\partial c_2} + \beta \frac{\partial f_3}{\partial c_2} \right)}_{\text{Substitution effect}(-)} \underbrace{- \left[e_p + e_2 (1 - s_2^*) \frac{\partial w_{c,2}}{\partial w_2} \right] w_{c,2} e_2 \frac{\partial^2 u_2}{\partial c_2^2}}_{\text{Income effect } (+)} \\ & \underbrace{+ \beta \left[e_p + e_2 (1 - s_2^*) \frac{\partial w_{c,2}}{\partial w_2} \right] \frac{\partial \Phi}{\partial c_2}}_{\text{Effect of } c_2 \text{ on net impact of schooling (weakly } +)} . \end{aligned} \tag{E7}$$

The overall effect of electrification, operating through adult wages, on the optimal level of child schooling depends on three components in Eq. E7. The first term represents the substitution effect: higher wages increase the returns to working by raising child labor income, period-2 utility, and the child's human capital in period 3. The second term captures the income effect: as w_2 rises, household consumption c_2 increases, reducing the marginal utility of consumption and thereby lowering the relative value of child labor. The third term reflects how changes in c_2 affect the net return to schooling.

Following the literature, this paper assumes $\frac{\partial \Phi}{\partial c_2} \geq 0$.[3] Intuitively, when children are well nourished and not constrained by basic needs, additional schooling becomes more productive relative to consumption. The net effect of electrification on schooling therefore depends on whether the income effect dominates the substitution effect, or vice versa.

Effect of Second-Period Wage on Long-Term Human Capital

$$\frac{de_3}{dw_2} = \frac{\partial f_3}{\partial c_2}\left[e_p + e_2(1 - s_2^*)\frac{\partial w_{c,2}}{\partial w_2}\right] + \frac{\partial s_2^*}{\partial w_2}\Phi. \tag{E8}$$

Given that $\frac{\partial w_{c,2}}{\partial w_2} > 0$, the overall effect of electrification on long-term human capital e_3 is theoretically ambiguous and depends on the sign of $\frac{\partial s_2^*}{\partial w_2}$. If electrification primarily raises adult wages (w_2), while child wages respond only weakly, the income effect dominates in Eq. E7, implying $\frac{\partial s_2^*}{\partial w_2} > 0$ and a positive effect on long-term human capital.This case is more likely for primary-school-age children, who are poor substitutes for adult laborers in farm work.

By contrast, if rural electrification substantially increases child wages through higher adult productivity, the substitution effect may offset or dominate the income effect, yielding a zero or even negative impact on e_3. This scenario is more likely for older children, who are closer substitutes for adult laborers in agricultural production.

[3] Shah and Steinberg (2017) argues that as consumption increases, schooling becomes relatively more effective at converting time into human capital. Nutrition literature shows that improved diet enhances cognitive function (e.g., Bryan et al., 2004; Gómez-Pinilla, 2008), while economic research links better nutrition to improved academic performance (e.g., Anderson, Gallagher, and Ritchie, 2018; Chakraborty and Jayaraman, 2019). Work on dynamic complementarities (e.g., Cunha, Heckman, and Schennach, 2010; Aizer and Cunha, 2012; Nandi et al., 2017; Johnson and Jackson, 2019) further highlights how early health and human capital complement each other in producing later human capital.