Available Online: 11 March 2026 DOI: 10.54254/3049-7248/2026.32206

The heterogeneous impact of a distance education pilot program on primary school enrollment: county level evidence from China

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Abstract. This paper aims to examine the impact of China's Modern Remote Education Project (MREP) on primary school enrollment using county-level panel data from Shaanxi Province between 2000 and 2008. Since the pilot program was rolled out at different times and in different places, the study employs a Difference-in-Differences (DID) design with an event-study specification to identify dynamic policy effects. Using the logarithm of enrollment as the main outcome, the results show that the distance education pilot did not lead to immediate enrollment expansion but was associated with a short-run decline in primary school enrollment. Heterogeneity analysis reveals that these effects vary substantially across regions. Counties with stronger economies saw significant drops in enrollment, while poorer counties showed smaller effects that were not statistically significant. These findings suggest that the impacts of education informatization policies depend heavily on local fiscal and institutional capacity. The study shows the importance of complementary investments and digitally inclusive policy design to avoid unintended distributive consequences.

Keywords: difference-in-difference, education, China, policy, education informatization

1. Introduction: literature review

1.1. MREP and education informatization in rural China

The Modern Remote Education Project for Primary and Secondary Schools (MREP) was formally proposed by the State Council in 2003 and was gradually piloted across different regions nationwide between 2004 and 2005, with the first phase of construction largely completed around 2010. The project mainly introduced Information and Communication Technology (ICT) to address the shortage of educational resources in rural areas by equipping rural primary and junior secondary schools with CD playback devices, instructional CDs, and satellite-based teaching reception points [1]. As one of the largest education informatization policies since the founding of the People's Republic of China, MREP aimed to promote balanced development between urban and rural education and to enhance the overall competencies of rural students, such as expressive and critical thinking skills [2].

Because MREP was implemented relatively early, existing studies have largely examined its policy significance and implementation challenges from a macro-level perspective. There have been limited systematic evaluation of its concrete educational outcomes at the micro level. Previous research has tended to focus on indicators related to educational quality, such as test scores, cognitive abilities [3], or classroom participation, etc. However, these studies have paid little attention to the most fundamental indicator of students' continued participation in the education system—student enrollment. At the compulsory education stage, enrollment size not only reflects the quantity and accessibility of educational provision, but also captures improvements in students' learning experiences and their level of trust in the school system. Therefore, student enrollment constitutes a key outcome variable for assessing whether an education policy truly takes effect.

1.2. ICT and educational outcomes: evidence from China and abroad

From the broader literature on education informatization, a large body of research suggests that the introduction of ICT can improve rural educational provision through multiple mechanisms. First, information technology can partially alleviate shortages of teaching staff. Through recorded and live-streamed classes as well as shared teaching resources, instructional content from resource-rich areas can be transmitted to resource-poor regions. This reduces the burden on individual teachers who must simultaneously attend to many students. A study based on data from Qinghai Province finds that schools integrating ICT into classroom teaching plans significantly improve students' exam performance [4], and this effect does not depend on students' initial learning levels. In this study, the ICT intervention consisted of a learning software package providing animated English review materials and gamified remedial exercises. Although the study mainly focuses on academic outcomes, its findings suggest that informatization may enhance overall learning experiences by increasing teaching engagement and classroom interaction [4].

Second, education informatization may also operate by influencing students' psychological and behavioral mechanisms. Existing studies indicate that the application of ICT can strengthen students' sense of school belonging, which is typically associated with higher academic motivation, self-esteem, and academic achievement [5]. In rural areas, such improvements in school belonging are particularly important, as students' decisions to remain in school are partly shaped by their subjective evaluations of the school environment. As a result, informatized education may not only improve learning outcomes but also affect enrollment size by reducing dropout risks and delaying students' exit from the education system.

International experience likewise supports this argument. Several distance education or technology-based intervention programs designed specifically for rural educational contexts show that technological tools can play a complementary role in settings characterized by multigrade teaching and limited classroom interaction. For example, instructional support tools developed for multigrade classrooms in rural India, as well as projects using augmented reality to improve teacher-student interaction, have been found to increase student engagement and reduce the likelihood of early dropout [6]. Although these studies are largely based on specific technological interventions and are institutionally different from China's nationally standardized implementation of MREP, they collectively suggest that education informatization policies have the potential to influence students' continued participation in basic education by improving learning experiences.

However, the effects of MREP have not manifested evenly across regions. Much research points out that non-material factors significantly constrain the effective use of informatization equipment. Teachers' perceptions of remote education and their technical competencies are widely regarded as key determinants of policy effectiveness. Surveys conducted in western China show that, despite the provision of relevant equipment in project schools, a considerable proportion of teachers failed to effectively transform their teaching mindset, viewing informatization tools merely as means for computer classes or for playing recorded classroom sessions [7]. Moreover, rural primary and secondary school teachers generally face challenges such as lower educational attainment and insufficient ICT skills. This results in significant variation across schools in the intensity and manner of using remote education resources, which in turn affects policy outcomes.

At the same time, local governments' preferences regarding education spending and incentives in project fund management also exert an important influence on the actual benefits derived from MREP. Related studies find that regions with stronger preferences for education investment tend to more actively pursue project types with higher equipment costs and potentially greater returns, whereas regions with weaker preferences are more inclined to select project schools with lower requirements for local matching funds [8]. This implies that a uniformly implemented education informatization policy may generate highly heterogeneous outcomes at the county level.

1.3. Evaluation of education informatization and infrastructure policies

After discussing the concept of ICT and its potential effects on teaching quality and learning experiences, it is necessary to further examine a related body of literature that evaluates education informatization and educational infrastructure policies using causal inference methods. Such studies typically use more rigorous methodological approaches, such as program eligibility rules or randomized experiments, to identify policy effects, providing more convincing empirical evidence for policy evaluation.

In examining the effects of technology-aided instruction, Banerjee et al. use randomized experiments in rural India to evaluate computer-assisted remedial education programs. They find that ICT-based interventions significantly improve students' test scores, with particularly strong effects among academically weaker students [9]. A later study further investigated technology-aided instruction delivered through digital platforms and showed that information technology can enhance student engagement and improve learning outcomes [10]. Together, this line of research provides strong evidence that ICT can improve teaching quality and classroom interaction. However, the outcome variables in these studies are largely confined to academic performance, with little attention to whether such improvements lead to students' continued participation in the education system.

From a broader public policy evaluation perspective, studies on investments in educational infrastructure reach similar conclusions. Goolsbee and Guryan use variation in the implementation of internet subsidy programs across U.S. public schools to analyze the impact of government-led investments in information infrastructure on school informatization. They find that the policy greatly increased schools' internet connectivity, while its short-term effects on students' academic outcomes were relatively limited [11]. An important contribution of this study is its treatment of informatization policies as institutional public investments rather than narrowly defined instructional tools. This perspective closely aligns with the institutional context of China's remote education initiatives. Nevertheless, this research likewise does not treat student enrollment or retention as a core evaluation outcome.

Related research on educational infrastructure further suggests that improvements in school conditions may influence schooling decisions by shaping household educational expectations. Using a large-scale school construction program in Indonesia as a quasi-natural experiment, Duflo finds that the expansion of educational infrastructure significantly increased years of schooling and generated positive long-term labor market outcomes [12]. Although the policy instrument examined in this study differs from China's remote education programs, the underlying mechanism it identifies that visible improvements in educational provision can strengthen households' confidence in the returns to education and thereby promote educational participation. This mechanism is relevant for understanding how informatization policies may affect enrollment size.

In summary, the existing literature shows imitations in three respects. First, most studies focus on educational quality rather than educational participation. There is a lack of analysis of student enrollment as a basic outcome variable. In areas where there is still a risk of dropout, the enrollment scale not only shows the ability of education supply, but also shows the recognition of the school system by students and their families. Second, previous studies are mostly limited to national or regional comparisons, with little causal identification at the county level. Third, studies focusing on a single province, especially at the primary school level in relatively resource-limited areas, are rare. As an important province in central and western China and a key beneficiary of MREP, Shaanxi Province offers a good context for examining county-level policy effects.

Based on these research gaps, this paper treats the 2005 pilot implementation of MREP in Shaanxi Province as a quasi-natural experiment and employs a difference-in-differences approach to systematically identify the impact of the remote education pilot policy on county-level primary school enrollment. This paper contributes to the literature in three ways. First, it shifts the focus from educational quality to educational participation by examining primary school enrollment as a core policy outcome. Second, it uses county-level variation in the pilot implementation of MREP to provide causal evidence using a DID design. Third, it documents heterogeneous policy effects across counties with different initial conditions and implementation capacities, shedding light on the institutional mechanisms underlying education informatization policies.

2. Theoretical framework

Building on the preceding review of the literature on education informatization policy evaluation, this section analyzes the potential channels through which the Modern Remote Education Project (MREP) may affect primary school enrollment. Overall, this section argues that MREP may influence students' enrollment and retention decisions through a combination of supply-side mechanisms and demand-side mechanisms.

2.1. Supply-side mechanism

On the supply side, MREP aims to solve shortages in teaching staff and instructional content in rural primary schools by providing remote education equipment and standardized teaching resources. Existing research suggests that educational resources and institutional capacity not only shape instructional quality but also determine schools' ability to deliver basic educational services in a stable and continuous manner. In resource-constrained areas, remote education initiatives can raise the minimum level of instructional provision, which improves overall school functioning [13].

Improvements in school capacity may, in turn, reduce the likelihood that students withdraw from the education system due to inadequate teaching conditions. Studies on school withdrawal indicate that unstable or low-quality learning environments significantly increase students' dropout risk [14]. Therefore, even if remote education policies do not immediately lead to substantial gains in academic performance, they may still exert a positive effect on primary school enrollment by enhancing instructional stability and reducing disengagement.

2.2. Teaching-side mechanism

Beyond improvements in material conditions, MREP may also affect student enrollment indirectly through changes in teachers' instructional capacity and teaching practices. A large body of research demonstrates that teacher quality is a key factor in students' learning experiences and educational outcomes, particularly in resource-constrained settings [15]. By providing standardized instructional materials and teaching demonstrations, remote education resources can help reduce teachers' preparation burden and improve classroom organization.

However, prior studies also emphasize substantial differences in teachers' attitudes toward information technology, teaching beliefs, and technical skills across regions. These factors play a critical role in shaping how, and to what extent, informatization tools are integrated into classroom practice [16]. As a result, even under a uniform policy framework, the actual effects of MREP may vary considerably across counties. This observation provides the theoretical basis for examining heterogeneous policy effects associated with teacher capacity and local implementation conditions in the empirical analysis.

2.3. Demand-side mechanism

On the demand side, remote education policies may influence enrollment and retention decisions by improving students' learning experiences and psychological attachment to school. Research in educational psychology highlights the importance of students' sense of school belonging in shaping continued participation in education. A stronger sense of school membership is generally associated with higher learning motivation and lower dropout risk [17]. Multimedia instructional materials and more diverse classroom formats may enhance student engagement and strengthen emotional attachment to school.

Further research suggests that student engagement plays a critical mediating role between classroom experiences and educational participation. A comprehensive review shows that behavioral and emotional engagement are strongly correlated with students' persistence in schooling [18]. This means that education informatization policies may affect enrollment even without short-term improvements in academic achievement by enhancing the overall learning experience.

In addition to student-level factors, household decision-making also plays an important role in enrollment at the compulsory education stage. Families' educational investments depend on their expectations of returns to education [19]. Improvements in school informatization and educational infrastructure serve as observable signals of educational quality, potentially increasing households' confidence in the value of continued schooling. From a signaling perspective, visible enhancements in educational provision may alter expectations about educational returns and thereby promote educational participation [20].

2.4. Hypothesis

Based on the theoretical discussion above, this paper proposes the following hypotheses:

Hypothesis 1 (H1):

The implementation of the Modern Remote Education Project (MREP) significantly increases county-level primary school enrollment.

This hypothesis captures the overall effect of remote education policy on educational participation through improvements in educational provision, learning experiences, and institutional reliability.

Hypothesis 2 (H2):

The positive effect of MREP on primary school enrollment is stronger in counties with weaker initial educational resources.

In these counties, pre-existing supply-side constraints are more binding, and the marginal impact of remote education resources is expected to be larger.

Hypothesis 3 (H3):

Counties with stronger local education investment capacity or higher teacher implementation capacity experience larger enrollment gains from MREP.

This hypothesis highlights the moderating role of local institutional conditions and human capital in shaping the effectiveness of education informatization policies.

3. Research design

This study employs panel data from 60 counties in Shaanxi Province covering the period 2000–2008 for empirical analysis. The dataset was preprocessed in several steps.

3.1. Outlier detection and correction

Obvious outliers were identified and corrected to avoid bias in estimation. Observations were flagged as outliers if their values deviated from the county mean by more than three standard deviations (± 3 SD). Implausible records, such as negative values for enrollment or population, were checked and corrected based on historical trends or administrative reports.

The ± 3 SD threshold here was employed only as a diagnostic tool. All flagged observations were subsequently examined in conjunction with adjacent-year trends and available administrative documentation. Implausible records, such as negative values for enrollment or population, or abrupt discontinuities inconsistent with historical patterns, were corrected where corroborating information was available. Observations that could not be independently verified were retained in the dataset to avoid arbitrary sample selection and preserve genuine cross-county variation.

3.2. Handling missing values

Observations with missing values for key variables were treated according to their context:

For intermittent missing values within a county's time series, linear interpolation was applied using the average of preceding and subsequent years.

For missing values at the beginning or end of the series, estimates were derived from the county's average annual growth rate during available periods. For variables entirely missing in a county across 2000–2008, the provincial average of the same variable was used as a substitute. This approach assumes limited cross-county heterogeneity for these indicators; to mitigate potential bias, robustness checks were conducted using alternative imputation methods.

After preprocessing, the dataset contained 540 valid observations. County-level population data were obtained from the Express Professional Superior Platform (EPS), and other control variables were obtained from the China Economic and Social Big Data Research Platform and the statistical yearbooks of Shaanxi Province and its counties.

3.3. Variable selection

This paper adopts a Difference-in-Differences (DID) model to evaluate the impact of the distance education pilot policy on primary school enrollment. The core variables are defined as follows:

3.3.1. Dependent variables

Primary_students: Number of primary school students enrolled in county i in year t, serving as a measure of the scale of primary education (unit: persons).

Graduated_students: Number of students who graduated from primary schools in county i in year t, serving as an alternative dependent variable for robustness check.

3.3.2. Key explanatory variables

Event-study dummies (te_m3, te_m2, te_0, te_1, te_2, te_3): These are constructed as the interaction between the treatment indicator (treat = 1 if a county participated in the pilot program, 0 otherwise) and event time dummies representing the number of years relative to policy implementation.

te_m3 and te_m2 capture trends three and two years before implementation, serving to test the parallel trend assumption. te_0 measures the effect in the implementation year, while te_1–te_3 measure dynamic effects in subsequent years.

Counties were selected as pilot sites based on provincial policy documents prioritizing areas with lower initial access to educational resources.

3.3.3. Control variables

Income_pc: Per capita income, reflecting the level of local economic development. Higher income may increase educational demand and influence primary school enrollment (unit: CNY per person).

Edu_expenditure: Educational expenditure, measuring local government investment in education, which affects educational supply and access (unit: CNY).

Population: Total population of a county, included to control for the direct impact of population size on school enrollment (unit: persons).

Teacher_count: Number of primary school teachers (full-time equivalent), controlling for supply-side conditions in education (unit: persons).

3.4. Model specification

The following model is used to estimate the effect of the distance education pilot policy on primary school enrollment (equation (1)):

$$Primary\_students_{it} = \Sigma_{k\neq-1}\beta_k te_{k,it} + X_{it}\prime\gamma + \mu_i + \lambda_t + +\theta_{it} + \varepsilon_{it} \quad (1)$$

In which $Primary\_students_{it}$ stands for the number of primary school students in county i in year t, the event-time indicators are denoted as $te_{k,it}$, where k indexes the number of years relative to the implementation of the distance education pilot policy. Specifically, for each treated county, $te_{k,it}$ equals one if year t is k years away from the county-specific policy implementation year, and zero otherwise. For example, te_{-3}, te_{-2}, te_0, te_1, te_2, and te_3 correspond to three years before, two years before, the year of implementation, and one to three years after the policy, respectively. Throughout the paper, these indicators are consistently labeled as te_m3, te_m2, te_0, te_1, te_2, and te_3 in the empirical tables. Counties that were never selected into the pilot program serve as control units and take the value zero for all event-time indicators.

X_{it} refers to a set of control variables, including per capita income (income_pc), education expenditure (edu_expenditure), number of teachers (teacher_count), and population size (population). μ_i denotes county fixed effects, controlling for county characteristics that do not change over time. λ_t refers to year fixed effects. Control national macro changes. In addition, the model includes county-specific linear time trends, θ_{it}, to flexibly account for differential underlying trends in enrollment across counties that may evolve over time. ε_{it} is the random error term. This paper also controls for the fixed effect of county and year.

Following standard practice in event-study and difference-in-differences designs, the year immediately preceding policy implementation ($k = -1$) is omitted and serves as the reference period [21]. Omitting $k = -1$ avoids perfect multicollinearity among the time dummies and provides a natural status-quo benchmark immediately before treatment that is least likely to be contaminated by anticipatory adjustments.

To assess this assumption, the pre-treatment coefficients, β_{-3} and β_{-2} are focused on. Under parallel trends, these coefficients should be close to zero and statistically insignificant. However, in empirical applications, it is not uncommon to observe significant pre-treatment coefficients, and such patterns do not necessarily imply a violation of the parallel trends assumption. First, from the perspective of statistical power, standard tests for pre-trends are often underpowered, especially in panels with a limited number of units. As

Rambachan and Roth note, "tests of pre-trends may be underpowered … the lack of a significant pre-trend does not necessarily imply the validity of the parallel trends assumption" [22].

For the above reason, this paper does not mechanically interpret significant pre-treatment coefficients as evidence against parallel trends. Instead, we complement the regression results with graphical evidence from event-study plots and draw on the methodological insights of the literature to assess potential anticipatory effects, baseline heterogeneity, or random fluctuations. This multifaceted approach strengthens the credibility and interpretability of the DID estimates.

Additionally, to address concerns that counties may follow different underlying enrollment trajectories, the baseline specification is augmented by including county-specific linear time trends (equation (2)):

$$Primary\_students_{it} = \Sigma_{(k\neq -1)}\beta_k \cdot te_{k,it} + X_{it}\prime\gamma + \mu_i + \lambda_t + \theta_i \cdot t + \varepsilon_{it} \quad (2)$$

where $\theta_i \cdot t$ denotes a county-specific linear time trend that flexibly captures smooth, unobserved changes in enrollment over time. This specification reduces the risk that estimated policy effects are driven by pre-existing differential trends rather than the policy intervention itself.

To examine heterogeneous policy effects across regions with different economic conditions, counties are classified into high-income and low-income groups based on their median per capita income over the sample period. The baseline model is then estimated separately for each group (equation (3)):

$$Primary\_students_{it}^{g} = \Sigma_{(k\neq -1)}\beta_k^{g} \cdot te_{k,it} + X_{it}\prime\gamma^{g} + \mu_i^{g} + \lambda_t + \varepsilon_{it}^{g} \quad (3)$$

where $g \in \{High-income, Low-income\}$ indexes income groups. This specification allows the policy coefficients to vary across economic contexts and corresponds directly to the heterogeneity results reported in Table 3.

4. Empirical analysis

4.1. Descriptive statistics of key variables

Table 1 presents the descriptive statistics of the main variables. The average number of primary school students (primary_students) is approximately 32,898 with a standard deviation of 20,128, indicating substantial variation in primary education scale across counties and over time. Per capita income (income_pc) averages 13,189 yuan, ranging from 2,245 to 48,602 yuan, reflecting significant differences in economic development levels among counties. Educational expenditure (edu_expenditure) averages 8,917 yuan, with a minimum of 566 and a maximum of 72,373 yuan, suggesting notable disparities in local government investment in education. Total population (population) averages around 303,866, ranging from 27,443 to 775,425, further indicating considerable heterogeneity in county population size.

Table 1. Descriptive statistics

Variables	Observations	Mean	Std. Dev	Min	Max
Primary_students (Number of primary school students)	540	32,898.01	20,127.73	3,288	108,450
Income_pc (Per capita income)	540	13,189.27	7,222.20	2,245.39	48,602
Edu_expenditure (Educational expenditure)	540	8,917.17	7,520.65	566	72,373
Population (Total population)	540	303,865.70	177,299.40	27,443	775,425

4.2. Parallel trend test

To assess the validity of the DID design, this section examines the dynamic effects of the distance education pilot policy using an event-study specification. Figure 1 presents the estimated coefficients based on the logarithm of primary school enrollment, allowing the effects to be interpreted as approximate percentage changes and reducing concerns related to scale heterogeneity across counties.

All standard errors are clustered at the county level to account for within-county serial correlation over time.

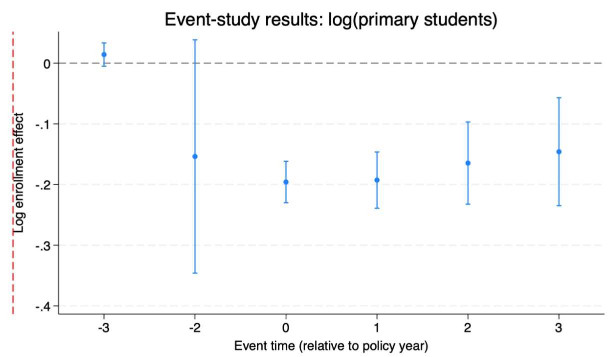

Figure 1. Event-study estimates using log(primary school enrollment)

In the log specification, the pre-treatment coefficients fluctuate around zero and do not exhibit a systematic or monotonic trend. While minor deviations appear in individual pre-policy years, their magnitude is small and does not suggest sustained divergence between treated and control counties. Such short-term fluctuations may reflect early-stage information diffusion or preparatory activities preceding formal implementation, but they are not consistent with a systematic anticipation effect.

To further address concerns regarding differential demographic dynamics across counties, the event-study specification is augmented by allowing county-specific population trends. Under this specification, the pre-treatment coefficients no longer display systematic divergence, while the short-run post-treatment effects remain negative and statistically significant. Taken together, these results provide strong support for the parallel trends assumption and reinforce the credibility of the DID design.

Taken together, the event-study estimates do not exhibit systematic pre-treatment divergence, and this conclusion is reinforced under more stringent specifications allowing for county-specific population trends, providing support for the parallel trends assumption.

4.3. Baseline regression analysis

Having established the validity of the identification strategy, this section examines the baseline effects of the distance education pilot policy on primary school enrollment. Table 2 reports the DID estimates using the logarithm of primary school enrollment as the dependent variable, allowing the coefficients to be interpreted as approximate percentage changes.

Table 2. Regression results with log of primary school enrollment

	(1)	(2)	(3)
	Baseline	High-income counties	Low-income counties
te_m3	0.014	0.000	-0.010
	(0.010)	(.)	(0.021)
te_m2	-0.154	0.000	-0.152
	(0.096)	(.)	(0.106)
te_0	-0.196^{***}	-0.078^{***}	0.000
	(0.017)	(0.010)	(.)
te_1	-0.193^{***}	-0.089^{***}	0.000
	(0.023)	(0.020)	(.)
te_2	-0.165^{***}	-0.102^{***}	0.000
	(0.034)	(0.027)	(.)
te_3	-0.146^{***}	-0.125^{***}	0.000
	(0.044)	(0.034)	(.)
income_pc	-0.000^{***}	-0.000^{***}	-0.000^{***}
	(0.000)	(0.000)	(0.000)
edu_expenditure	-0.000	-0.000	-0.000^{***}
	(0.000)	(0.000)	(0.000)
teacher_count	-0.000	-0.000	0.000^{*}
	(0.000)	(0.000)	(0.000)
population	0.000	-0.000^{***}	0.000^{**}
	(0.000)	(0.000)	(0.000)
_cons	10.438^{***}	11.116^{***}	10.396^{***}
	(0.263)	(0.317)	(0.180)
N	540	300	240

Notes: Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$

Table 2 reports the baseline DID estimates using the logarithm of primary school enrollment as the dependent variable. Overall, the results indicate that the distance education pilot policy is associated with a statistically significant short-run decline in primary school enrollment.

In the baseline specification (Column 1), enrollment decreases by approximately 19.6 percent in the policy implementation year, followed by a further decline of about 19.3 percent in the first post-treatment year. The magnitude of the negative effect gradually attenuates over time, with the estimated coefficients becoming smaller and statistically insignificant by the third post-treatment year. This pattern suggests that the policy impact is concentrated in the short run rather than generating a persistent contraction in enrollment.

Columns (2) and (3) show heterogeneous effects by county income level. In high-income counties, the estimated effects remain negative and statistically significant throughout the post-treatment period, though the magnitude is smaller than in the baseline specification. In contrast, no statistically significant effects are observed in low-income counties. This divergence indicates that local economic conditions play an important role in shaping how counties absorb and respond to education informatization policies.

Regarding the control variables, per capita income presents a statistically significant negative association with primary school enrollment across all specifications. This pattern likely reflects broader demographic and structural dynamics rather than a direct causal effect of income on educational participation. Rising income levels are often associated with declining fertility rates, increased population mobility, and ongoing urbanization processes, all of which may reduce the size of school-age cohorts within a county. Educational expenditure and teacher count show limited explanatory power once county and year fixed effects are included. This suggests that short-run changes in aggregate fiscal inputs do not translate mechanically into enrollment adjustments.

4.4. Heterogeneity analysis

This section examines different effects of the distance education pilot policy across counties with different economic conditions. Following the baseline analysis, counties are divided into high-income and low-income groups based on their county-specific median per capita income over the sample period. Columns (2) and (3) of Table 2 report the corresponding DID estimates for these two subsamples.

The results reveal a clear divergence in policy effects across income groups. In high-income counties, the policy is associated with a statistically significant decline in primary school enrollment in the post-treatment period. Although the magnitude of the effect is smaller than that observed in the full sample, the coefficients remain consistently negative and statistically significant, indicating that enrollment declined by approximately 8-13 percent following policy implementation. This suggests that even relatively well-resourced counties experienced short-run adjustment costs in response to the introduction of remote education infrastructure.

In contrast, no statistically significant policy effects are detected in low-income counties. The estimated coefficients are small in magnitude and statistically indistinguishable from zero across all post-treatment periods. This absence of a detectable effect does not necessarily imply that the policy was neutral in low-income areas. Rather, it may reflect weaker implementation capacity, greater variation in local responses, or offsetting mechanisms that hide average treatment effects in these regions.

Several mechanisms may explain the observed heterogeneity. First, fiscal capacity constraints may play a critical role. In low-income counties, investments in remote education equipment may crowd out other essential educational expenditures, such as school maintenance, teacher support, or student services, limiting the net impact of informatization on enrollment outcomes [23]. In contrast, high-income counties are better positioned to absorb new investments without displacing existing spending, making policy-induced adjustments more visible in enrollment responses.

Second, differences in administrative and teaching capacity may shape how effectively new technologies are integrated into classroom practice. Technology-based education reforms require additional investments in teacher training and institutional support [24]. Where such conditions are absent, new inputs may be underutilized or fail to translate into improvements in perceived school quality, dampening their effects on enrollment decisions.

Third, household responses may further mediate policy impacts. In economically disadvantaged areas, families may show greater uncertainty toward unfamiliar educational technologies, particularly in contexts of weaker institutional trust [25]. Rather than signaling improvements in educational quality, the introduction of remote education equipment may generate skepticism, leading to school transfers, delayed enrollment, or increased reliance on migration-based livelihood strategies.

Finally, demographic dynamics and concurrent policy interventions may also contribute to the observed differences in enrollment responses. In low-income counties, changes in measured enrollment may partly reflect population mobility rather than schooling behavior itself, as the out-migration of working-age adults and accompanying children is a well-documented phenomenon in rural China. In addition, concurrent education policies implemented during the same period, such as school consolidation reforms (Che Dian Bing

Xiao), may have interacted with the distance education pilot in shaping local enrollment patterns. Although the inclusion of county fixed effects and time trends absorbs time-invariant institutional differences, the available data do not allow for a clean separation of these overlapping policy effects. As a result, the estimated treatment effects should be interpreted as net impacts arising from the joint operation of multiple institutional changes during the reform period.

Taken together, the heterogeneity analysis underscores that the effects of education informatization policies are highly contingent on local economic and institutional conditions. Without complementary fiscal support, teacher training, and implementation capacity, such policies may generate uneven or muted impacts across regions. These findings highlight the importance of designing education technology interventions with explicit attention to local constraints, particularly in economically disadvantaged areas. It should be emphasized that the decline in the number of students at school at the county level does not necessarily mean that students withdraw from the education system or that educational opportunities are reduced, but more likely reflects the result of families' migration, school choice, and reconfiguration between different education levels.

5. Robustness checks

To verify the robustness of the empirical findings, two additional checks were conducted. First, standard errors were clustered at the county level to account for within-county correlations. Second, a control variable sensitivity analysis is performed by re-estimating the model with alternative sets of covariates. Specifically, educational expenditure and teacher count are excluded from the control set, leaving only per capita income and total population as controls. This specification addresses potential concerns regarding fiscal measurement error and over-controlling for endogenous educational inputs. The estimated policy coefficients remain stable in both sign and magnitude, indicating that the main results are not sensitive to the choice of control variables.

Table 3. Robustness check

VARIABLES	(1) primary_st~s	(2) graduated_~s	(3) teacher_co~t	(4) primary_st~s	(5) graduated_~s	(6) teacher_co~t
te_m3	-341.6 (390.5)	301.4* (160.8)	86.46*** (31.69)	-178.3 (366.8)	305.8* (158.5)	86.36*** (31.78)
te_m2	-76,66.6* (4,207.1)	4,166.2 (3,853.3)	114.9** (46.02)	-7,451.2* (4,054.1)	4,171.8 (3,849.7)	114.8** (46.09)
te_0	-9,591.2*** (1,738.0)	-665.6** (282.8)	39.01 (37.47)	-9,290.5*** (1,837.2)	-645.0** (292.1)	38.50 (37.25)
te_1	-9,623.1*** (1,952.5)	-1,085.6*** (370.1)	58.85 (42.60)	-9,308.6*** (2,114.9)	-1,065.9*** (385.7)	58.36 (42.12)
te_2	-8,546.6*** (2,330.8)	-1,251.9** (475.0)	57.95 (53.80)	-8,617.9*** (2,511.9)	-1,263.8** (488.1)	58.24 (54.53)
te_3	-8,181.2*** (2,599.9)	-1,313.1** (540.3)	63.50 (69.42)	-7,916.3*** (2,944.5)	-1,298.1** (570.7)	63.13 (69.11)
Observations	540	540	540	540	540	540

Notes: Robust standard errors in parentheses

*** $p < 0.01$, ** $p < 0.05$, *$p < 0.1$

Beyond the standard robustness check, as Table 3 shows, here a placebo test (Table 4) is implemented to further strengthen the causal interpretation. Specifically, a set of dummy policy timing indicators (te_m3_fake to te_3_fake) was constructed by assigning "false" treatment years to all treated counties while keeping all other model specifications unchanged. If the estimated policy effect in the main analysis were driven by spurious trends or other unobserved shocks rather than the actual intervention, similar patterns of significant coefficients around these pseudo policy dates would be potentially observed.

The placebo test reveals that the coefficients on te_m2_fake and te_m3_fake are statistically significant, while no significant discontinuity is observed at the placebo implementation year (te_0_fake), nor are there systematic changes immediately surrounding the fake policy timing. This pattern suggests that the estimated significance in earlier placebo periods is unlikely to reflect a causal policy effect and instead captures broader temporal dynamics unrelated to the actual intervention.

Importantly, these placebo effects do not align with the structure of the true treatment effects observed in the main analysis. In particular, the absence of a discrete shift at the placebo treatment year indicates that the placebo coefficients reflect smooth or gradual changes rather than policy-induced discontinuities. Such patterns are consistent with slow-moving demographic or institutional processes, such as population mobility, cohort size fluctuations, or gradual adjustments in school organization, that evolve independently of the distance education policy.

Crucially, these underlying dynamics are already accounted for, to a large extent, by the inclusion of county fixed effects, year fixed effects, and county-specific linear time trends in the baseline specification. As a result, while the placebo test highlights the presence of non-policy-related temporal variation in enrollment, it does not reproduce the core treatment pattern identified in the main event-study analysis. The lack of a sharp break around the placebo policy date reinforces the interpretation that the estimated post-treatment effects in the baseline model are not artifacts of spurious trends or arbitrary timing.

Taken together, the placebo results suggest that although enrollment exhibits broader temporal movements, these dynamics are orthogonal to the actual policy implementation. Therefore, the placebo test does not undermine the causal interpretation of the baseline findings but instead underscores the importance of cautious interpretation of dynamic coefficients far from the treatment year.

Table 4. Placebo test

	(1)
	primary_students
te_m3_fake	10,113.3***
	(2,359.6)
te_m2_fake	8,070.1***
	(1,913.3)
te_0_fake	-2,245.7
	(3,512.9)
te_1_fake	-1,540.2
	(876.9)
te_2_fake	-3,994.9***
	(694.9)
te_3_fake	-3,992.4***
	(553.4)
income_pc	-0.252
	(0.164)
edu_expenditure	-0.567*
	(0.225)

Table 4. Continued

teacher_count	2.631
	(3.050)
population	-0.0535
	(0.0709)
Constant	53,031.7*
	(21,470.6)
Observations	540

Notes: Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

6. Conclusion

This study examines the effects of China's Modern Remote Education Project (MREP) on primary school enrollment using county-level data from Shaanxi Province. Contrary to conventional expectations that education informatization would automatically expand educational participation, the empirical results show that the distance education pilot did not lead to immediate enrollment growth. Instead, the policy was associated with a short-run decline in primary school enrollment. Importantly, these effects are highly heterogeneous across regions and appear to be strongly conditioned by local fiscal and institutional capacity. Together, the findings suggest that education informatization policies do not operate in a vacuum and may generate unintended distributive consequences when complementary conditions are absent.

The heterogeneity analysis reveals economically meaningful and policy-relevant effects. In low-income counties, the implementation of the distance education pilot policy is associated with a persistent decline in primary school enrollment. Quantitatively, treated low-income counties experienced a reduction of approximately 3,300 to 4,200 primary school students within three years after policy implementation, representing a sizable contraction relative to their baseline enrollment levels. Although these effects are estimated at the county level and should not be interpreted as individual dropout decisions, their magnitude suggests that the policy imposed nontrivial adjustment costs on economically disadvantaged regions. In contrast, no statistically or economically significant effects are detected in high-income counties, indicating that better-resourced regions were largely able to absorb the policy without adverse enrollment consequences.

These findings carry several important policy implications. First, education informatization initiatives should not be implemented as stand-alone infrastructure investments, especially in fiscally constrained regions. In low-income counties, targeted fiscal transfers are essential to prevent resource crowding-out and to ensure that investments in digital equipment do not displace other critical educational expenditures, such as school maintenance, teaching materials, or student support programs.

Second, the results highlight the central role of implementation capacity, especially teacher preparedness. The introduction of remote education technologies should be accompanied by systematic teacher training programs that enhance pedagogical integration and effective classroom use. Without adequate support, digital equipment risks being underutilized or even disrupting instructional processes, undermining the intended benefits of informatization policies.

Third, sustained funding for equipment maintenance and operational support is crucial. Digital education infrastructure requires ongoing financial and administrative commitment beyond initial installation. In the absence of long-term maintenance support, especially in low-income regions, informatization investments may deteriorate quickly, further weakening local education systems.

More broadly, the findings show the importance of digital inclusion as a core principle of education informatization policy. While digital technologies hold promise for reducing disparities in educational resources, their benefits are not automatically realized. Without careful policy design that accounts for regional differences in fiscal capacity and institutional readiness, informatization initiatives may exacerbate existing inequalities rather than promote educational equity. Ensuring that disadvantaged regions can effectively utilize digital infrastructure is therefore central to achieving inclusive and balanced educational development [26].

A specifically important finding of this study is that the MREP is associated with a more pronounced decline in primary school enrollment in high-income counties. At first glance, this result appears counter-intuitive. One plausible explanation is that higher-income households tend to show greater sensitivity to educational quality and possess a stronger capacity to choose across different tiers of the education system. While lower-income households primarily focus on access to schooling, higher-income households are more likely to view education as a high-quality investment in human capital, with education demand displaying greater income elasticity. Consequently, when MREP is introduced as a standardized form of remote instruction, it may not necessarily be interpreted as an improvement in existing teaching conditions. Instead, it may be perceived as a signal that local governments are attempting to substitute traditional, face-to-face instruction with lower-cost digital resources.

Within this framework, the introduction of remote education is likely to trigger a substitution effect. Given their greater financial resources and spatial mobility, higher-income households are better positioned to actively reallocate their schooling choices across different segments of the education market. When local schooling models are perceived as shifting toward lower-cost and more standardized forms of instruction, these households have stronger incentives to transfer their children to higher-level urban schools or private institutions, where educational resources are more concentrated and quality signals are clearer. Such education-related mobility decisions constitute a form of "voting with their feet", and are manifested statistically as a significant contraction in county-level primary school enrollment. It is important to note that this process does not imply a decline in overall educational participation, but rather a reallocation of students across different tiers of the education system. This pattern is consistent with household sorting theory, which predicts that higher-income families exhibit stronger responsiveness to perceived quality differentials and are more likely to reallocate spatially in response to institutional change. In this framework, enrollment decline reflects household re-sorting rather than a contraction of educational demand.

Moreover, high-income counties typically have greater fiscal capacity and administrative capability, making them more likely to integrate education informatization initiatives with contemporaneous school consolidation policies. In some areas, remote education equipment and digital resources were used as compensatory measures to justify the closure or merger of village schools, thereby mitigating resistance to reductions in the number of local schools. However, such institutional arrangements objectively increased physical commuting costs for some students and heightened households' concerns about the stability of schooling arrangements. For higher-income families, who place greater value on educational certainty and long-term returns, this increased uncertainty further strengthened incentives to relocate schooling outside the county, particularly toward urban education systems—thereby amplifying the negative association between informatization investment and county-level enrollment. The resulting pattern, while seemingly counter-intuitive, is thus closely connected to the underlying institutional and behavioral logic. While the data do not allow a clean separation of the effects of school consolidation from the informatization intervention, the estimated coefficients should be interpreted as net effects arising from concurrent institutional adjustments. In this sense, MREP may have interacted with ongoing consolidation reforms to reshape local educational structures, and the observed enrollment decline captures the combined impact of these intertwined processes rather than a standalone technological shock.

Additional evidence supporting this interpretation comes from the observed changes in teacher staffing patterns. As shown in Table 4, the policy is associated with statistically significant adjustments in teacher_count in high-income counties. This suggests that the informatization initiative was accompanied by broader reorganization of local educational resources rather than constituting a purely technological intervention.

Changes in teacher staffing are consistent with a restructuring of the county-level education ecology. If remote education resources were introduced alongside school consolidation efforts, local governments may have reduced or reallocated teachers in response to merged or closed village schools. Such adjustments would alter both the perceived stability and the instructional environment of local schools. For quality-sensitive households in economically advantaged counties, these institutional signals may reinforce concerns that traditional face-to-face instruction is being substituted by standardized digital delivery, thereby strengthening incentives to transfer children to alternative schooling options outside the county.

Taken together, the concurrent movement in enrollment and teacher_count supports the hypothesis that the policy effect reflects systemic institutional adjustment rather than a narrow technology shock.

On the other hand, this study has several limitations. First, the analysis only focuses on a single province, Shaanxi, which may limit the external validity of the findings. The effects of education informatization policies may differ in provinces with distinct demographic, fiscal, or institutional conditions.

Second, the sample period captures an early stage of China's education informatization. Policy effects may evolve over time as technology improves, administrative capacity strengthens, and complementary institutions develop. As such, the estimated impacts should be interpreted as short- to medium-term effects rather than long-run outcomes.

Third, this study does not explicitly account for other contemporaneous education policies, such as the "Two Exemptions and One Subsidy" (Liang Mian Yi Bu) program, which may have interacted with the distance education initiative during the same period. While the inclusion of county and year fixed effects mitigates some confounding influences, future research could adopt alternative identification strategies to better disentangle overlapping policy effects.

Fourth, although this study addresses potential selection concerns through fixed effects and extensive pre-trend diagnostics, the assignment of pilot counties may not have been fully random. Future research could further strengthen causal identification by combining difference-in-differences designs with matching-based approaches, such as propensity score matching, to improve covariate balance between treated and control counties.

Despite these limitations, this study provides evidence on the distributive consequences of education informatization policies. The findings suggest that the observed effects likely reflect joint institutional adjustments, encompassing resource reallocation, school consolidation, and household sorting, rather than a pure technology shock. Education informatization policies operate within complex local institutional environments, and their consequences depend critically on how technological inputs interact with existing governance structures and household behavior.

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